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**WORK MOTIVATION AND WELFARE SANCTIONS —  
EVIDENCE FROM GERMAN SURVEY AND  
ADMINISTRATIVE DATA**

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INGRID HOHENLEITNER  
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*Vorsitzender:* Prof. Dr. Erich Gundlach

*Erstgutachter:* Prof. Dr. Thomas Straubhaar

*Zweitgutachterin:* Prof. Dr. Melanie Krause

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*It is our choices that show what we truly are, far more than our abilities.*

J. K. Rowling



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# List of Abbreviations

## METHODS

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ATE	Average Treatment Effect
ATT	Average Treatment Effect on the Treated
BRM	Binary Regression Model
BS	Balancing Score
CIA	Conditional Independence Assumption
CR	Competing Risks
CS	Common Support
EKM	Epanechnikov Kernel Matching
KM	Kernel Matching
LATE	Local Average Treatment Effect
MPH	Mixed Proportional Hazard
NNM	Nearest Neighbor Matching
OLM	Ordinal Logistic Model
ORM	Ordinal Regression Model
PS	Propensity Score
PSA	Propensity Score Analysis
PSM	Propensity Score Matching
SITA	Strongly Ignorable Treatment Assignment
ToE	Timing-of-Events

## LABOR MARKET & INSTITUTIONS

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Alg II	Arbeitslosengeld II (engl.: UB II)
ALMP	Active Labor Market Policy
BA	Bundesagentur für Arbeit (German FEA)
BMAS	Bundesministerium für Arbeit und Soziales (engl.: Federal Ministry for Labor and Social Affairs)
BIEN	Basic Income Earth Network
FDZ	Forschungsdatenzentrum (engl.: 'Research Data Centre')

FEA	Federal Employment Agency
IAB	Institut für Arbeitsmarkt- und Berufsforschung (engl.: 'Institute for Employment Research')
LMA	Labor Market Access
SA	Social Assistance
SC	Social Code
SCB	Social Code Book
SGB	Sozialgesetzbuch (engl.: SCB)
UA	Unemployment Assistance
UB I	Unemployment Benefits I
UB II	Unemployment Benefits II
UI	Unemployment Insurance
UIB	Unemployment Insurance Benefits

#### DATA SETS

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IEBS	Stichprobe der integrierten Erwerbsbiografien (engl.: 'Integrated Employment Biographies Sample')
PASS	Panel Arbeitsmarkt und Soziale Sicherung (engl.: 'Panel Study Labour Market and Social Security')
SIAB	Stichprobe der Integrierten Arbeitsmarktbiografien (engl.: 'Sample of Integrated Labour Market Biographies')



*This thesis I dedicate to my father, my mother, my brother, and my dear friends Lars, Solveig, Puneet, Pradeep, Cathérine, Katjana, Sybille, and all other, not namely mentioned, but also close, dear, and inspiring persons in my life.*



# Chapter 1

## Introduction

This cumulative dissertation about work motivation and welfare sanctions is based on five articles which present the results of empirical studies, based on two rich German data sets: the panel survey 'Labour Market and Social Security' (PASS), and the spell data set which was merged and prepared exclusively for these studies, based upon the (SIAB) 'Sample of Integrated Labour Market Biographies' (SIAB), and supplemented by administrative data of the (FEA), both provided by the German Institute for Employment Research (IAB) and its Research Data Centre (FDZ).

The first, sole-author article about non-pecuniary work motivation, and the second article, written in collaboration with Andreia Tolciu about work values in Germany, contribute to the research topic 'work motivation'. The remaining three papers, all of which were co-authored with Katja Hillmann, contribute to the topic 'impacts of welfare sanctions'.

The overarching research question behind the topics of these articles is whether, and if so, to what extent, monetary work incentives, or even kinds of institutionalized existential economic pressure are necessary to generate sufficient labor supply in order to maintain economic wealth and prosperity. Or, to put it positively, how much freedom of existential economic pressure and pure monetary work incentives can a society afford without undermining the incentives for individuals to sufficiently contribute to the society's wealth and prosperity?

Within this superordinate topic, a more concrete research question leads to this thesis: how can welfare systems be organized to provide social security which guarantees at least the minimum subsistence level, including social participation, without setting disincentives for people's contributions to the labor market which could possibly threaten the economic wealth of a society.

In this dissertation, two main research topics are addressed: Firstly, the two articles about work motivation and work values in Germany contribute to the question “How important are non-monetary work incentives for a positive labor supply decision of the individuals?” Although both articles basically refer to the whole group of employable people, the second article (on work values) focuses mainly on (employable) welfare recipients. In Germany this concretely means recipients of unemployment benefits II (UB II).

Secondly, the three articles about welfare sanctions contribute to the question of whether existential economic pressure leads to the expected positive effects on labor supply, and which adverse side effects on the labor supply are entailed by that. As current welfare payments are defined to cover just the minimum subsistence level, welfare sanctions in the form of benefit cuts reduce the available income, by definition, to a level *below* the (socially defined) minimum subsistence level; hence, it is self-evident that welfare sanctions do have detrimental effects, at least on the sanctioned individuals. We restrict our analysis to economic aspects with a focus on the individuals’ labor market outcomes.

The results of the five articles in this thesis provide strong evidence for two central findings, contributing to the superordinate research questions. First, the impact of non-monetary work motivation on individual’s labor supply decision is generally underestimated. And work values are one of the central aspects that enhance non-pecuniary work incentives. Furthermore, the pessimistic expectations of a disastrous decrease in labor supply if monetary work incentives decrease are not verified by our findings. To the contrary, especially concerning the non-pecuniary work motivations of poorer people, of people with lower occupational level, and of people receiving welfare benefits, which by the majority are expected to be below-average, we find the opposite to be true: the majority of those people reveal non-pecuniary work incentives which are above the average. And strong work values are one of the factors that drive these findings.

In addition, we find that the existential economic pressure caused by welfare sanctions on the one hand, for part of the affected, indeed enhances their individual labor supply. But this tends to hold for people with already better chances on the labor market. For disadvantaged people, detrimental effects, even on their labor supply, exceeds possible positive effects. On the other hand, even the at times positive effect on labor supply, on average has strong adverse effects on the quality of employment, even in the long run. Such adverse effects,

which are detrimental to the occupational development of the individuals, are also detrimental from a public welfare point of view.

All in all, the political implications of our findings, especially those applicable to labor market and welfare policy, suggest that fewer negative incentives — especially those like existential pressure — would be beneficial, and that the risk of using more positive incentives to foster an individual's non-pecuniary work motivation is not nearly as great as commonly supposed. Or, to be more direct, perhaps it's time to give freedom a chance.

## **1.1 Contributions to the state of research**

This thesis connects two topics in two different research fields which are generally explored separately. Bringing the findings of both of these research topics together leads to insights which can provide new perspectives on labor market and social welfare policy.

### **1.1.1 Work motivation**

The research topic of non-pecuniary work motivation has gone virtually unexplored by economists. One reason might be that economists mainly focus on monetary incentives. And especially in terms of labor supply, the thought that pecuniary incentives, in addition to monetary ones could play a decisive role, is an idea that has, so far, been almost neglected by economic research.

Thus we have to rely on the widely spread literature from other disciplines such as social science, business administration, and work research, which generally focuses on topics other than the labor market supply. With our papers on non-pecuniary work motivation and labor supply we, thus, enter an almost new research topic for economists.

### **1.1.2 Welfare sanctions**

Since the extensive restructurings of the unemployment insurance and welfare systems in many European countries over the last two decades, more and more studies into the effects of different aspects of the newly implemented labor market institutions have been conducted. For many topics in this field, the literature

on unemployment insurance (UI) still outweighs the studies on welfare benefits for employable people.

This is also the case for the topics of UI benefit sanctions and welfare sanctions. Sanctions in form of temporary benefit cuts play an important role within the newly established comprehensive monitoring and sanction regimes of European welfare systems. Since unemployment benefits II (UB II) were introduced in Germany in 2005, there has been a lot of controversy, and even a bitter public debate, with the reforms being heralded as a paradigm shift from welfare towards workfare by its supporters, on the one hand, and criticized as being incompatible with fundamental human rights by its opponents, on the other. Due to the great public interest, this topic also caught the attention of the scientific community who have produced several articles across this range of topics.

Nevertheless, there still remain many huge research gaps. Initially, almost all studies merely focused on the impact of benefit sanctions on the transition rate from unemployment to employment, finding mostly positive effects. Then, a few studies on the impact of UI sanctions looking beyond employment entry came up. Concretely, they found that sanctions also increase the transition from unemployment to exits from labor market, and that sanctions have negative effects on employment quality in terms of wages, income, and job stability.

With our first study about welfare sanctions, we also investigated the option to leave the labor market, the so-called non-employment option, and found positive effects for employment entry as well as for exit from the labor force. When we published the first working paper version of our article in 2012, we provided the first study that addressed this topic of labor market exits in the context of welfare sanctions. Our work got a surprisingly large amount of public attention by scientists, as well as policy advisors, and even print media. We had the opportunity to present our work at three reputable international conferences, and in 2015, we published a revised version of our working paper.

The two current articles about our research on welfare sanctions are based on comprehensive data analysis which investigate various aspects of the impact of sanctions against employable welfare recipients. The first of the two studies presents the extremely extensive results of a dynamic approach of propensity score matching. Besides the analysis of various kinds of exit events such as entering unsubsidized or subsidized work, leaving welfare, and exiting employment, we carried out the analysis for two main groups (unemployed and employed people) and various subgroups; additionally, we consider employable

people who are indirectly affected by sanctioned household members (indirect sanctioned). Furthermore, we applied monthly updated estimations over two years which show us the development of sanction effects over time — something else which is novel for research on welfare sanctions — for welfare recipients as well as for those who are indirectly sanctioned. The monthly updates of treatment effects over two years time, in particular, reveal new and much more differentiated insights into the diverse facets of welfare sanctions' impacts on various exit options.

The second of the two current studies on welfare sanctions uses the same data set, but needs no dynamic approach as the outcome variables are not based on welfare durations but analyze post-welfare-spell outcomes. Specifically, we analyzed the impact of welfare sanctions on the subsequent employment quality. The outcome variables which reflect various aspects of employment quality are wages, income, and cumulative durations in three mutually exclusive employment states: employed without welfare receipt, unemployed, and employed with supplementary welfare receipt. These variables are intended to cover employment stability.

The effects of (mostly UI) sanctions on subsequent employment is a very new research topic, and the empirical literature is still quite rare. Except for one German study from 2015, which focuses on young male welfare recipients, and which also considers post-sanction employment quality reflected by the initial wage, we provide the first broader study about the effect of welfare sanctions on post-sanction employment quality. The set of outcome variables reflecting employment stability, in particular, are quite unique and provide interesting and surprising insights. Similar to the first of the two current studies, we also considered initially employed welfare recipients as well as indirectly affected household members. We found strong evidence for long-term adverse effects of welfare sanctions on the subsequent employment quality over two years, specifically for wages, incomes, and employment stability.

## 1.2 Own contributions

As the bulk of this dissertation is based upon articles with collaborative authorship, the following gives a brief overview of the own [and co-author's] contributions.

## **Chapter 2: *Non-Pecuniary Work Motivation and Labor Supply***

**Full title:** “Non-Pecuniary Work Motivation and Labor Supply: Would your neighbor work if she did not need the money?”

**Author:** Ingrid Hohenleitner

**Completion date:** June 2010

**Data set:** Panel survey of the IAB: ‘Labour Market and Social Security’ (PASS)

**Methods:** Ordinal regression analysis

**Submitted to:** Institute for Employment Research (IAB), Nuremberg, for the 3rd Interdisciplinary Ph.D. Workshop ‘Perspectives on (Un-)Employment’ in November 2010

## **Chapter 3: *Work Values in Germany***

**Article title:** “Same but Different: Work Values in Germany”

**Authors:** Andreia Tolciu (AT), Ingrid Hohenleitner (IH)

**Data set:** Panel survey of the IAB: ‘Labour Market and Social Security’ (PASS)

**Methods:** Ordinal regression analysis

**Own contributions:** Complete data preparation and data analysis with Stata (IH); evaluation and interpretation of the results (IH and AT); [writing the first version of the article (AT)]; proofreading and slight revision of the article (IH)

**Publishing date:** December 2011

**Published at:** *Hamburg Review of Social Sciences* (peer-reviewed)

## **Chapter 4: *Impact of Welfare Sanctions on (Non-)Employment***

**Article title:** “Impact of Welfare Sanctions on Employment Entry and Exit from Labor Force: Evidence from German Survey Data”

**Authors:** Katja Hillmann (KH), Ingrid Hohenleitner (IH)



**Completion dates:** September 2012 (first version), October 2015 (revised version)

**Data set:** Panel survey of the IAB: ‘Labour Market and Social Security’ (PASS)

**Methods:** Timing-of-events (ToE) analysis with a discrete mixed proportional hazard (MPH) framework

**Own contributions:** Data preparation with Stata (IH); [data analysis with Sabre (KH)]; evaluation and interpretation of the results (KH and IH); joint work on the article (KH and IH)

**Presentations on workshops and conferences** (all IH): Workshop in Labour Market and Social Policy of the ifo Institute (Dresden, March 2011), Annual Conference of the Royal Economic Society (RES) (Cambridge, March 2012), Workshop in Empirical Economics at Potsdam University (June 2012), Annual Congress of the Verein für Socialpolitik (V.f.S.) (Göttingen, September 2012), and Annual Conference of the European Association of Labour Economists (EALE) (Bonn, September 2012)

**Published as:** HWWI Research Paper 129 (2012); HWWI Research Paper 168 (revised 2015)

## **Chapter 5: *Welfare Sanctions: Impact on Employment and Benefit Receipt***

**Article title:** “Impact of Welfare Sanctions on Employment and Benefit Receipt: Considering Top-Up Benefits and Indirect Sanctions”

**Authors:** Ingrid Hohenleitner (IH), Katja Hillmann (KH)

**Completion date:** November 2017

**Data set:** Complex merged spell data set based on the (SIAB) ‘Sample of Integrated Labour Market Biographies’ (SIAB) and supplemented by administrative data of the (FEA), exclusively provided by the IAB

**Methods:** Dynamic propensity score matching (PSM) with stratification

**Own contributions:** Data preparation and data analysis with Stata (IH and KH), preparation of the results (IH and KH), evaluation of the results (IH), writing the article (IH)

**Published as:** HWWI Research Paper 189, January 2019

## **Chapter 6: *Impact of Welfare Sanctions on Employment Quality***

**Article title:** “Impact of Welfare Sanctions on the Quality of Subsequent Employment: Wages, Incomes, and Employment Stability”

**Authors:** Ingrid Hohenleitner (IH), Katja Hillmann (KH)

**Completion date:** November 2017

**Data set:** Complex merged spell data set based on the (SIAB) ‘Sample of Integrated Labour Market Biographies’ (SIAB) and supplemented by administrative data of the (FEA), exclusively provided by the IAB

**Methods:** Propensity score matching (PSM)

**Own contributions:** Data preparation and data analysis with Stata (IH and KH), evaluation of the results (IH), writing the article (IH)

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**Further publication:** submission to the *International Journal of Social Welfare* intended (style guidelines considered)

## Chapter 2

# Non-Pecuniary Work Motivation and Labor Supply: Would your neighbor work if she did not need the money?

This chapter is based upon a working paper by Ingrid Hohenleitner, submitted in July 2010 to the 3rd Interdisciplinary Ph.D. Workshop “Perspectives on (Un-) Employment” of the Institute for Employment Research (IAB) in Nuremberg, November 2010.

### 2.1 Motivation

In the vast majority of western European countries, a paradigm shift in social and labor market policy has taken place. During the current decade, more and more European governments have shifted towards a so-called ‘activation policy’, the present form of European workfare policy. Regardless of whether pure workfare policy is already implemented in practice, the legal basis and the paradigm behind it points in that direction. Initially the media massively supported their governments on this way, as well as many members of the public. Meanwhile, after half a decade of practice, more and more critical voices are being raised, including those of former supporters. One of the fundamental points of criticism is that activation policy focuses on the behavior and motivation of jobless people, and neglects economic framework conditions. In fact the term ‘activation’ implies that some are inactive and need to be activated. But is this really the main problem of labor markets, that people are not motivated

enough to work? And if some people are not as motivated as they are expected to be, what are the causes and which solutions could be helpful?

In this debate, the idea of an unconditionally granted basic income is becoming more and more important. A steadily increasing number of supporters come from a wide range of social groups. The idea of a basic income is to change the economic framework conditions towards a guaranteed basis of social security. It enables people to act more freely, particularly on the labor market. On the one hand, people do not have to be employed in order to receive a subsistence income. They can quit the labor market; they can demand higher wages as well as better working conditions. These kinds of labor supply reactions can be expected for inconvenient jobs, especially if earning money is the main motivation for these kinds of jobs. On the other hand, people who are able to work can improve their financial situation by earning money, not just as employees, but also as entrepreneurs. Therefore, monetary working incentives still exist alongside a basic income. For jobless people they could even improve, particularly if a person gets social transfers, because in the majority of cases, current transfers for unemployed are strongly reduced with every Euro earned to supplement one's welfare payments. A basic income reduces the current deduction rates of about 80 to 100% to a common tax level shared by all other workers and income earners. Although a basic income does not remove monetary working incentives, it establishes an exit option to quit the labor market — whether partly for some periods in life, or fully for the rest of one's life. Therefore, the crux of whether a basic income is feasible, is the question of people's labor supply reactions.

If we compare activation policy and the unconditional basic income, we see the antagonism in their principles and their policy implications. The main maxim of the current activation policy, and of every workfare ideology, is the quid-pro-quo principle. It claims that nobody who is able to work should get social payments without being liable to pay back his dues to society. Concretely, welfare recipients are liable to give back to society in the form of work commitment or, if there is no work for them to do, at least in the form of proving willingness to work. Accordingly, current labor market policy sees less of a problem in the framework conditions of the labor market, but focuses mainly on the behavior of unemployed people. Implementing a vast bureaucracy with extensive control mechanisms in order to activate the unemployed is the consequence of this policy.

In contrast, the protagonists of a basic income focus on the framework conditions. A guaranteed level of social security for everybody will enable people to act independently and free them from existential economic pressure. Everybody has the chance to act responsibly — for oneself, for one's social network, and for society in general. Activating people is not a matter for governmental authorities. Basic income supporters assume that the overwhelming majority of people want to contribute to society, and if there are some individuals that do not, perhaps they need assistance. Social responsibility to offer them assistance is not neglected by basic income supporters; they only object in forcing people to accept public assistance, as is the case with activation policy.

Basic income protagonists, however, have a fundamentally different understanding of human beings than supporters of activation policy. The former assume that it is a fundamental characteristic of human beings to be willing to contribute to society, and that forcing people would undermine their motivation; the latter, however, believe that there has to be some external pressure to activate people. In reality, motivation structures differ between individuals and we can assume that both kinds of extreme conditioning could be observed — i.e. the kind of people who will work under almost all conditions, and those who won't work whether they are compensated with a lot of money, or confronted with extreme pressure. But the fundamental principles of social and labor market policy should not be oriented to some extreme individual cases — they should focus, instead, on the vast majority of the society.

As a basic income is obviously much more compatible with free citizens and the principles of free society than all current forms of welfare policies, more and more people sympathize with this idea. Nevertheless, the crucial factor behind whether a basic income is politically and economically feasible, are people's labor supply reactions. In fact, not only opponents of the basic income question whether people would work if they did not need the money. This kind of skepticism dominates large portions of society. However, an amazing paradox appears in the debate: although most skeptical people say that they would continue to work even if they get a basic income, they are convinced that their neighbors — or some other lazy people — won't do so.

With my investigation, I intend to leave this field of speculations and shed light on people's work motivation. Concretely, I want to find out whether people will be willing to work if they can take their subsistence for granted, as is the case with an unconditional basic income.

## 2.2 Focussing the research question

The main purpose of my research is to find out whether an unconditional basic income is an economically feasible alternative to workfare and activation policy. I focus on developed countries, particularly the European welfare policy with its activation paradigm, but also have in mind countries with a long tradition of workfare, like the USA.

On the one hand, a basic income does not undermine monetary working incentives at all; on the other hand, it implements an exit option out of the labor market. Therefore, in order to assess the economic feasibility of a basic income, we have to answer the question “who would use the exit option and to what extent”. In this context, work motivation plays a decisive role. If we know more about the question, “what motivates people to work”, we will be able to assess their labor supply reactions more precisely.

Although every form of work is economically relevant, I will focus my research on paid work, because direct labor market effects are immediately crucial to assessing whether a basic income is financially viable. Nevertheless, I want to stress that non-paid work also contributes to the economic wealth of a society, and will also contribute to the feasibility of a basic income. But the fiscal effects of non-paid work are indirect, and even more difficult to estimate.

My research focuses on a basic income that is unconditionally granted to all on an individual basis, without means test or work requirement. To this definition given by the Basic Income Earth Network (BIEN)<sup>1</sup>, I add two further aspects. Crucial to the effects of a basic income is the amount of the regular payments. I assume a monthly paid basic income that amounts to at least the socio-cultural subsistence level of a society. This definition includes higher amounts than the subsistence income but excludes lower ones. Thus, the definition on which my research is based complies with the fundamental idea of an unconditional basic income as stated by the German Basic Income Network<sup>2</sup>. The reason for excluding lower amounts than the (socio-cultural) subsistence level is that such a regular income, in fact, does not free people from the work requirement, even if it is granted without means tests and any other legal conditions. In the strict sense, such a guaranteed income is not really unconditional. The second aspect

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<sup>1</sup>See <http://www.basicincome.org/bien/>

<sup>2</sup>See <http://www.grundeinkommen.de/die-idee> (in German)

I add to the definition of basic income by BIEN, is that I assume the basic income is granted for all members of a society<sup>3</sup>. Concretely, all inhabitants of a country are included, i.e. native-born and naturalized citizens, as well as foreign citizens having their residence in that country.

The state of research concerning the labor supply effects of a basic income is still affected by a lack of sufficient data. Indeed, some analysis based on theoretical simulation models exist, however the validity of their results still could not be confirmed by comparing them with real data. Although some basic income experiments have been conducted and some forms of basic income have been realized, an unconditional basic income in the pure form, as defined above, has not yet been realized yet.

The Alaska permanent fund is unconditionally granted for the citizens of Alaska, but the amount of income falls far short of the minimum subsistence level. In Brazil, the first steps towards a basic income have been made, and to date, Brazil is the only country in the world that has declared the implementation of a basic income by law. However, the current social transfer in Brazil is restricted to poor families that have to fulfill a series of requirements. The Namibian Basic Income Grant (BIG) is a time-limited basic income experiment, currently conducted in a Namibian village. It provides interesting insights into people's behavior, however the outcomes of the Namibian experiment do not sufficiently contribute to the prediction of labor supply reactions in industrialized countries, which is the focus of my research.

Finally, the US-American experiments conducted in the 1960s and 70s show some tendencies in labor market reactions of individuals, but there are two critical problems with this data. Firstly, the experiments were conducted a long time ago and working behavior and the underlying working moral have probably changed over time. The second point concerns the time constraint: the people who participated in the US-American field experiments knew that the experiments were time-limited. Therefore, there were probably some people who may have wanted to quit their jobs for a while, but did not do that. A reason could be, for example, that they worried whether the re-entry in the labor market would succeed after some years without paid work. In contrast, other people may have left the labor market for a while, who would not have done so with a lifelong basic income. For these people, the reason for quitting their jobs

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<sup>3</sup>This aspect also conforms with the definition of basic income given by the German Basic Income Network, see <http://www.grundeinkommen.de/die-idee> (in German).

during the experiment could be that they saw this as a unique chance to take a break during their working lives. With a basic income, they probably also would leave the labor market for some periods during their lifetime, but this may not be for their whole lifetime. Furthermore, they may not quit the labor market the moment a basic income is implemented, and this is crucial for the feasibility of a basic income. If a huge proportion of the population leave the labor market upon implementing a basic income, massive economic problems can be expected. But if the periods out of the labor market are spread across people's lifetimes, they will cause considerably less — or even no — problems in the labor market. In contrast, it is possible that the voluntary times out of the labor market with a basic income are less than today's sabbaticals and involuntary times off caused by unemployment. All in all, there are some essential problems in deducing labor supply effects of a lifelong basic income from the labor supply behavior of time-limited experiments.

Altogether, the state of basic income research is still characterized by a lack of current data analysis concerning the effects on people's labor supply. I want to fill this gap with an in-depth examination of work motivation of the German population — naturally including all inhabitants, i.e. native-born and naturalized citizens as well as foreign citizens, having their residence in Germany.

I have chosen to use data about the German population for two reasons. Firstly, the basic income debate is penetrating more and more civil groups in Germany. The steadily increasing number of supporters come from many sections of society. An increasing number of people are dissatisfied with the current social system and the current labor market situation — concerned people as well as several experts. Slowly, but inexorably, the political climate seems to be becoming increasingly ready for a paradigm shift towards an *unconditional* basic income. Whether this assessment is true, there is a political and cultural breeding ground of supporters that make it probable that at least field experiments will take place in the near future<sup>4</sup>.

Secondly, and this is the main reason I have focused my analysis on Germany, a rich and up-to-date dataset containing a vast range of essential variables for my research questions is available for this country. The new data set, a panel study called "Labour Market and Social Security" (PASS), provides several variables

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<sup>4</sup>In fact, there are some ardent supporters of the basic income in Germany who take field experiments into consideration, or are about to plan and conduct such experiments.



concerning work motivation. In addition to detailed socio-demographic information in general, it contains extensive information on qualification and employment. Furthermore, it contains information about attitudes to gender roles that affect working behavior. This is particularly important for the question of whether a basic income would especially encourage women to leave the labor market, as some feminist opponents of a basic income fear.

As a first step, I analyze which people will probably work if they get a basic income, and which people will probably not; concretely, I investigate what characterizes people with different degrees of work motivation. Although a basic income does not remove monetary working incentives, it implements an exit option that enables people to leave the labor market. Therefore, I firstly focus on the non-pecuniary work motivation and try to find its essential determinants of impact. Thereby I examine individual effects as well as possible impacts by partners or by the household structure. Secondly, I compare diverse factors of work motivation, especially focussing on the divergence between monetary and non-monetary work motivation.

## 2.3 Data

As mentioned above, my analysis are based on the German panel study “Labour Market and Social Security” (PASS)<sup>5</sup>, because it is the most appropriate for my research question. PASS is an annual household survey, conducted by the Institute for Employment Research (IAB). Access to the data was obtained via a Scientific Use File (SUF), provided by the Research Data Centre (FDZ)<sup>6</sup> of the German Federal Employment Agency (BA) at the IAB.

In the first wave — the wave being used for my analysis — 18,954 individuals living in 12,794 households were interviewed. The survey is designed as a two-stage random sample including 300 postal code areas as primary sampling units. Thus, the sample is clustered. Furthermore, the survey consists of two partial populations divided into two sample units. On the one hand, the so called BA-sample consists of households and individuals in receipt of

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<sup>5</sup>The German name of the panel study is Panel “Arbeitsmarkt und Soziale Sicherheit” (PASS).

<sup>6</sup>See [http://fdz.iab.de/en/FDZ\\_Individual\\_Data/PASS.aspx](http://fdz.iab.de/en/FDZ_Individual_Data/PASS.aspx)

Unemployment Benefit II (Alg II). On the other hand, the so called Microm-sample includes households and individuals registered as residents of Germany. Thereby, the sample is not simple random; it is stratified, where the probability that a household of the primary sampling unit is drawn from differs by a factor of four for households with a low income and a factor of two for a medium income compared to households with high income. The initial interviews with any given household are the household interview, and a personal interview held with the head of the selected households. Subsequently, all other household members from the age of 15 are interviewed; people older than 64 get a reduced questionnaire, referred to as pensioner's questionnaire.

In addition to a vast set of socio-demographic variables, the PASS data set includes a wide range of employment-related as well as benefit-related characteristics. Furthermore, it includes a number of subjective indicators that contain information about several attitudes. However, some of the survey questions, especially those about attitudes, are not asked annually, but only in certain waves; this is also the case with the questions about work motivation that are essential for my analysis.

Apart from the huge spectrum of questions, the PASS panel survey has another essential advantage for my analysis: as a consequence of the complex selection procedure explained above, the sample includes disproportionately more poorer households than richer ones. This kind of disproportional selection improves the analysis especially of poorer households and individuals. Nevertheless, valid analysis of the whole population is possible as well<sup>7</sup>.

The first wave of the panel study (2006/2007) contains several questions about work motivation; of these, the following question fit best to my main research issue:

*“Let us now deal with the topic of work and gainful employment. Regardless of whether you currently work or not: To what extent do you agree to the following opinions on work? Please think very generally about working on a job. Please tell me whether you ‘Strongly agree’, ‘Somewhat agree’, ‘Somewhat disagree’ or ‘Strongly disagree’ to these opinions<sup>8</sup>.”*

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<sup>7</sup>Of course, the complex sample design is taken into account with the weighting factors. See Christoph et al. (2008): Codebook and Documentation of the Panel Study “Labour Market and Social Security” (PASS), Wave 1 (2006/2007), IAB Data Report 5/2008 (EN), p. 6f, [http://doku.iab.de/fdz/berichte/2008/DR\\_05-08\\_I\\_EN.pdf](http://doku.iab.de/fdz/berichte/2008/DR_05-08_I_EN.pdf).

<sup>8</sup>The missing options of answering were ‘Don’t know’ and ‘Refused’. See Question P 23 of personal questionnaire of the first wave of the PASS

- A Work is **only** a means to earn money
- B Having work is the most important thing in life
- C Work is important, because it gives you the feeling to be a part of it
- D I would also like to work, if I didn't need the money."

This question was asked of every member of interviewed households who were between 15 and 64 years of age. The answers to the four statements are contained in four variables that I prepared to an outcome that is ascending ordinal scaled with four categories: 'strongly disagree' (SD), 'disagree' (D), 'agree' (A) and 'strongly agree' (SA). The fourth variable (of statement D), called *arbol*, represents the *non-monetary* work motivation and is the most important for my research question. The first variable (of statement A), called *arbmon*, represents the *monetary* work motivation and is the second most important for my research.

In order to get a better view on the data set ultimately used for my analysis, Table 2.1 presents some descriptive statistics of the most important variables. It includes the *unweighted* means, i.e. the means of the observations in the sample. *Weighted* means of a great part of these variables are shown in Table 5.

In the part of my analysis presented in this text, the variable *arbol*, standing for non-monetary work motivation, is the predictand. Partly for this analysis, but mainly for my further analysis of the structure of work motivation, the variable *arbmon*, standing for monetary work motivation, is also used as a predictand.

The individual characteristics, used as predictor variables in different models, are grouped into so called hard and soft factors. The hard factors consist of a gender dummy (*female*), a dummy for East Germans (*ost*), the age of the questioned person (*alter*), qualification measured in equivalentents of education time (*bildzeit*), a dummy set for several kinds of graduation and professional education (*bbild1* to *bbild9*), the dummy variables for the employment status unemployed (*alogem*) and homemakers (*hausmf*), and finally dummy variables for the religious faiths Roman Catholic (*konfess1*), Evangelical/Protestant (*konfess2*), Islamic (*konfess4*), and no religion (*konfess7*). The soft factors contain a variable for a conservative role model (*rollkons*) and a dummy set of the social position (*sozpos1* to *sozpos10*). The outcomes of these variables are based on self-assessment of the interviewees, who have classified themselves in a range between the lowest (*sozpos1*) and the highest social position (*sozpos10*).

The dummy variables *bbild1* to *bbild9* for occasional qualification represent the

Variable	Mean	Std.Dev.	Min.	Max.
<b>Predictands</b>				
arbol	2.1670	.9120	0	3
arbmon*	1.5970	.9884	0	3
<b>Hard factors</b>				
female	.5430	.4982	0	1
ost	.2868	.4523	0	1
alter	40.55	12.44	15	64
bildzeit	11.68	2.63	7	21
bbild1	.2791	.4486	0	1
bbild2	.5296	.4991	0	1
bbild3	.0620	.2411	0	1
bbild4	.0581	.2340	0	1
bbild5	.0628	.2427	0	1
bbild6	.0067	.0815	0	1
bbild8	.0010	.0315	0	1
bbild9	.0007	.0270	0	1
alogem	.3392	.4735	0	1
hausmf	.0972	.2963	0	1
konfess1	.2308	.4214	0	1
konfess2	.2596	.4385	0	1
konfess4	.0482	.2142	0	1
konfess7	.0482	.2142	0	1
<b>Soft factors</b>				
rollkons	.2847	.4513	0	1
sozpos1	.0485	.2148	0	1
sozpos2	.0408	.1978	0	1
sozpos3	.0864	.2810	0	1
sozpos4	.0972	.2963	0	1
sozpos5	.2799	.4490	0	1
sozpos6	.1571	.3639	0	1
sozpos7	.1577	.3644	0	1
sozpos8	.0981	.2974	0	1
sozpos9	.0190	.1365	0	1
sozpos10	.0153	.1229	0	1
<b>Further Statistics</b>				
Observations	N = 15,121*			
Population	N_pop = 47.6 Mio.			

TABLE 2.1: Descriptive statistics of hard and soft individual characteristics.  
Source: PASS, Wave 1, 2006/2007. \* excluding arbmon: N = 15,085 obs.

following groups of graduation and professional education: 1: non-skilled, semi-skilled workers, 2: apprenticeship, vocational school, 3: foreman, technicians, vocational academy, 4: polytechnic, bachelor, 5: master, other university degrees<sup>9</sup>. Qualification Group 7, containing pupils in their first school education, is not considered in the sample I use, because they have not got a degree yet, and therefore no values for the qualification variable *bildzeit* exist.

## 2.4 Methodology and theory

For the main part of my analysis, I use the ordinal variable *arbol* as the endogenous variable. The variable *arbol*, measuring the non-monetary work motivation, includes the ascending output categories ‘strongly disagree’ (SD), ‘disagree’ (D), ‘agree’ (A) and ‘strongly agree’ (SA), which represents the level of agreement to the statement ‘I would also like to work, if I didn’t need the money’.

As the endogenous variable *arbol* is ordinal scaled, I use several ordinal regression models (ORM)<sup>10</sup> for my data analysis. Because the survey questions about working attitudes are essential for my research but were only asked in the first wave of the PASS survey, I can carry out only a cross section analysis, but not a panel analysis. Hence, I analyze the current status of working attitudes at a particular point of time, but I cannot examine changes over time.

The main part of my investigation is finding out the determinants of non-monetary work motivation. To extract the relevant impact factors, I examined a huge range of variables. These variables, used as exogenous in my models, were grouped into individual characteristics and household characteristics. Within the individual characteristics, I grouped the variables into hard factors that contain socio-demographic characteristics on the one hand, and soft factors that covered several attitudes, on the other hand.

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<sup>9</sup>Because the German qualification degrees cannot in every case exactly be translated into English, here are the German terms for the qualification groups: 1: ohne Abschluss, angelernt; 2: Lehre, Berufsfachschule; 3: Meister, Techniker, Berufsakademie; 4: Fachhochschule, Bachelor; 5: Universitätsabschluss (Diplom, Magister), Master, 6: Promotion, Habilitation; 7: Schüler (allgemeinbildende Schule); 8: anderer deutscher Abschluss; 9: anderer ausländischer Abschluss.

<sup>10</sup>See Long and Freese (2006): Regression Models for Categorical Dependent Variables Using Stata.

As ordinal regression models were used for my analysis, it is helpful to be familiar with some theoretical background about these kinds of models in order to interpret the results, so I give a brief overview of the underlying theory as well as of conceptualizing my models.

Using the PASS data set, I first fitted an ordinal regression model (ORM) with so called ‘hard individual factors’

$$\Pr(\text{arbol} = m|\mathbf{x}) = F(\tau_m - \mathbf{x}\boldsymbol{\beta}) - F(\tau_{m-1} - \mathbf{x}\boldsymbol{\beta}) \quad \text{with } m = 1 \text{ to } 4 \quad (2.1)$$

where

$$\mathbf{x}\boldsymbol{\beta} = \beta_{\text{female}}\text{female} + \beta_{\text{ost}}\text{ost} + \beta_{\text{alterz}}\text{alterz} + \beta_{\text{bildzeit}}\text{bildzeit} + \beta_{\text{alogem}}\text{alogem} + \\ \beta_{\text{hausmf}}\text{hausmf} + \beta_{\text{konfess1}}\text{konfess1} + \beta_{\text{konfess2}}\text{konfess2}$$

These hard factors of individual characteristics, used as exogenous variables  $\mathbf{x}$ , include the socio-demographic characteristics gender (*female*), the dummy variable Eastern/Western Germany (*ost*: stands for Eastern), age centered at its mean (*alterz*), qualification measured in equivalents of education time, also centered at its mean (*bildzeit*), the dummy variables for employment status unemployed (*alogem*) and homemakers (*hausmf*), and the dummy variables for the religious faiths Roman Catholic (*konfess1*), Evangelical/Protestant (*konfess2*), or alternatively using the confession dummies Islamic (*konfess4*), and no confession (*konfess7*).

Alternatively, I fitted models substituting the variable *bildzeit* for qualification by a dummy set, representing several kinds of graduation and professional education (*bbild1* to *bbild9*).

Furthermore I extended the previous models with only hard factors by soft individual factors. Concretely, I examined the impact of a conservative role model with a dummy variable (*rollkons*) and an interaction term for conservative women (*kons\_female*). Another essential soft factor is the social position (*sozpos*). The outcome of this variable is generated by self-assessment of the interviewees, where the interviewed persons classify themselves in a range between the lowest social position 1 (*sozpos1*) and the highest social position 10

(*sozpos10*). For my analysis I use a corresponding dummy variable set, *sozpos* (*sozpos1* to *sozpos10*).

Formula 2.1 shows the predicted probabilities  $Pr$  of the four different outcome categories,  $m$ , of the endogenous variable *arbol*, given the observed values for the exogenous variables  $\mathbf{x}$ .  $\tau_m$  are the cutpoints, also called thresholds, which determine whether a person's answer falls in the lower or in the higher category of non-monetary work motivation. As the endogenous variable has  $m = 4$  output categories, we have  $m - 1 = 3$  relevant cutpoints to estimate. The predicted probabilities  $Pr$  of the outcome categories  $m$  are calculated by the difference between the probability that the cutpoint  $\tau_{m-1}$  is exceeded and the probability that the cutpoint  $\tau_m$  is not exceeded.  $F$  is the cumulative distribution function (cdf) of the random error,  $\varepsilon$ , while the random error is the portion of the variation of the endogenous variable *arbol* that cannot be explained by the model. In ordinal logistic models (OLM), which I use for my analysis, the distribution of  $F$  is logistic with a variance of  $Var(\varepsilon) = \pi^2/3$ .<sup>11</sup>

For interpreting the outcome of my analysis, I use odds ratios as well as marginal effects and discrete changes. Thus, the outcome of ordinal regression models (ORM) can be interpreted much more illustratively than would be possible with the original outcome as logits. Concretely, I use factor changes in odds as well as discrete and marginal changes in predicted probabilities. For a better understanding of these concepts, even for readers who are not quite familiar with ORM, I give a brief introduction into the underlying theory, based on J. Scott Long and Jeremy Freese<sup>12</sup>

### 2.4.1 Ordinal regression models

Ordinal regression models (ORM) are commonly presented as latent-variable models. Defining  $y^*$  as a latent variable ranging from  $-\infty$  to  $+\infty$ , the structural model is defined by:

$$y_i^* = \mathbf{x}_i\boldsymbol{\beta} + \varepsilon_i$$

or for the case of one independent variable,

<sup>11</sup>See Long and Freese (2006), p. 186.

<sup>12</sup>See Long and Freese (2006), Ch. 5.

$$y_i^* = \alpha + \beta x_i + \varepsilon_i$$

where  $i$  is the observation and  $\varepsilon$  is a random error.

In my analysis,  $y^*$  is the real but not observable degree of non-pecuniary work motivation. As it is not observable, it is called a latent variable.

The measurement model for ORM can be seen as an expanded model for binary outcomes. While binary regression models (BRM) divide  $y^*$  into the two categories, 1 and 0, an ORM divides  $y^*$  into  $J$  ordinal categories,

$$y_i = m \quad \text{if } \tau_{m-1} \leq y_i^* < \tau_m, \text{ for } m = 1 \text{ to } J$$

where the thresholds  $\tau_1$  through  $\tau_{J-1}$  are estimated. The lowest and highest thresholds are assumed to be  $\tau_0 = -\infty$  and  $\tau_J = +\infty$ , respectively.

To illustrate the measurement model, we consider the dependent variable *arbol* that I use as endogenous for my analysis. The interviewees were asked to respond to the statement:

*"I would also like to work, if I didn't need the money."*

The possible responses are 1 = Strongly Disagree (SD), 2 = Disagree (D), 3 = Agree (A), and 4 = Strongly Agree (SA). The continuous latent variable,  $y^*$ , can be thought as the propensity to agree with this statement. The observed response categories correspond to the latent variable as defined by the measurement model:

$$y_i = 1 \Rightarrow \text{SD} \quad \text{if } \tau_0 = -\infty \leq y_i^* < \tau_1$$

$$y_i = 2 \Rightarrow \text{D} \quad \text{if } \tau_1 \leq y_i^* < \tau_2$$

$$y_i = 3 \Rightarrow \text{A} \quad \text{if } \tau_2 \leq y_i^* < \tau_3$$

$$y_i = 4 \Rightarrow \text{SA} \quad \text{if } \tau_3 \leq y_i^* < \tau_4 = +\infty$$

Thus when the latent variable  $y^*$  crosses a threshold, the observed category for  $y$  changes. The three cutpoints,  $\tau_1$  to  $\tau_3$  lead to the four levels,  $m$ , of the observed variable  $y$ .



The predicted probability of observing an outcome of  $y = m$  for a given value of  $\mathbf{x}$  can be computed by the formula

$$\Pr(y = m|\mathbf{x}) = \Pr(\tau_{m-1} \leq y^* < \tau_m|\mathbf{x})$$

Substituting  $\mathbf{x}\beta + \varepsilon$  for  $y^*$  and using some algebra leads to the following formula for the predicted probability of an observed outcome for given values of  $x$ , the standard formula for ORM:

$$\Pr(y = m|\mathbf{x}) = F(\tau_m - \mathbf{x}\beta) - F(\tau_{m-1} - \mathbf{x}\beta)$$

As mentioned above,  $F$  is the cumulative distribution function (cdf) for the random error  $\varepsilon$ , and in an OLM,  $F$  is logistic with a variance of  $\text{Var}(\varepsilon) = \pi^2/3$ .

## 2.4.2 Interpretation using odds ratios

Because  $\beta$ -coefficients in an OLM have to be interpreted as changes in logits, which is not very intuitive, it is favorable to transform the results into odds ratios, interpreted as factor changes in odds, or to use marginal and discrete changes.

Firstly, we consider the log of the odds, called logits, using an example. Note that the logit model is *linear* in the logit. This means that the log odds are a linear combination of the exogenous variables  $\mathbf{x}$  and the coefficients  $\beta$ .

For example, let us consider a *logit model with three independent variables*:

$$\ln \left[ \frac{\Pr(y \leq m|\mathbf{x})}{\Pr(y > m|\mathbf{x})} \right] = \ln \Omega(\mathbf{x}) = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \beta_3 x_3 \quad \text{for } m = 1 \text{ to } J - 1$$

where  $J - 1$  of the  $J$  thresholds,  $\tau_1$  through  $\tau_{J-1}$ , are estimated. The coefficients can be interpreted as follows: for a unit change in  $x_k$ , the logit is expected to change by  $\beta_k$ , holding all other variables constant. Although this interpretation could be seen as convenient because logits are independent of the level of the exogenous variables  $\mathbf{x}$ , it is favorable to use marginal and discrete changes or odds ratios instead of log odds for interpretation. The reason is the little substantive meaning of log odds. While odds ratios, interpreted as factor changes,

are also independent of  $\mathbf{x}$ , marginal and discrete changes depend on the values of all exogenous variables.

Now let us transform the logits of the example into odds. The *odds* of observing an outcome *lower versus higher than the threshold*  $\tau_m$ , is defined by the equation

$$\Omega_{\leq m | > m}(\mathbf{x}) = \frac{\Pr(y \leq m | \mathbf{x})}{\Pr(y > m | \mathbf{x})} = \exp(\beta_0) \times \exp(\beta_1 x_1) \times \exp(\beta_2 x_2) \times \exp(\beta_3 x_3)$$

Secondly, we consider the ordinal regression model (ORM) in general. The ORM can also be seen as a *nonlinear probability model* without using the idea of a latent variable. For the *ordinal logit model* (OLM), the *odds* that an outcome is less than or equal to  $m$  versus greater than  $m$ , given the values of  $\mathbf{x}$ , is defined by

$$\Omega_{\leq m | > m}(\mathbf{x}) \equiv \frac{\Pr(y \leq m | \mathbf{x})}{\Pr(y > m | \mathbf{x})} \quad \text{for } m = 1 \text{ to } J - 1$$

For example, we could compute the odds of disagreeing (D) or strongly disagreeing (SD) ( $m \leq 2$ ) versus agreeing (A) or strongly agreeing (SA) ( $m > 2$ ).

The log of the odds, the *logit*, can be estimated by the equation

$$\ln \Omega_{\leq m | > m}(\mathbf{x}) = \tau_m - \mathbf{x}\boldsymbol{\beta}$$

For one independent variable and  $J$  categories while fixing the intercept to equal 0, the *logit* is computed as

$$\ln \Omega_{\leq m | > m}(\mathbf{x}) = \ln \left[ \frac{\Pr(y \leq m | \mathbf{x})}{\Pr(y > m | \mathbf{x})} \right] = \tau_m - \beta_m x_m \quad \text{for } m = 1 \text{ to } J - 1$$

For ordinal logit models (OLM) — but not for ordinal probit models — the results can be interpreted using odds ratios. Accordingly, the OLM can be defined as

$$\Omega_{\leq m | > m}(\mathbf{x}) = \exp(\tau_m - \mathbf{x}\boldsymbol{\beta})$$

where we simultaneously estimate  $J - 1$  equations for  $m = 1$  to  $J - 1$ .

We can calculate the *factor change in odds* for a unit increase in  $x_k$  by the following odds ratio:

$$\frac{\Omega_{\leq m | > m}(\mathbf{x}, x_k + 1)}{\Omega_{\leq m | > m}(\mathbf{x}, x_k)} = \exp(-\beta_k) = \frac{1}{\exp(\beta_k)}$$

This multiplicative effect can be interpreted as follows: for a unit increase in  $x_k$ , the odds of an outcome that is less than versus equal to  $m$ , is changed by the factor  $\exp(-\beta_k)$ , holding all other variables constant.

In interpreting my results, I prefer to use the *inverse* of the odds shown above. In particular, I use  $\Omega_{> m | \leq m}(x)$ , i.e. the probability observing an outcome *higher versus lower* than the cutpoint  $\tau_m$ . Hence, the *factor changes in odds* for an increase in  $x_k$  are calculated by the following *odds ratios*:

$$\frac{\Omega_{> m | \leq m}(\mathbf{x}, x_k + 1)}{\Omega_{> m | \leq m}(\mathbf{x}, x_k)} = \exp(\beta_k)$$

for a unit increase in  $x_k$ , and by

$$\frac{\Omega_{> m | \leq m}(\mathbf{x}, x_k + SD_x)}{\Omega_{> m | \leq m}(\mathbf{x}, x_k)} = \exp(\beta_k \times SD_x)$$

for a standard deviation increase in  $x_k$ .

Transforming  $\beta$ -coefficients into odds ratios, in order to interpret the results of an OLM as factor changes, has the advantage that the interpretation is much more intuitive compared to changes in logits. Furthermore, the odds ratios, interpreted as factor changes, are independent of the exogenous variables  $\mathbf{x}$ .

### 2.4.3 Interpretation using marginal and discrete changes

The *marginal change* in the predicted probability is computed as:

$$\frac{\delta}{\delta x_k} \Pr(y = m | \mathbf{x}) = \frac{\delta}{\delta x_k} F(\tau_m - \mathbf{x}\boldsymbol{\beta}) - \frac{\delta}{\delta x_k} F(\tau_{m-1} - \mathbf{x}\boldsymbol{\beta})$$

which is the slope of the curve relating  $x_k$  to  $\Pr(y = m | \mathbf{x})$ , holding all other variables constant. The *value of the marginal change* depends on the value of  $x_k$ , and

on the values of all other variables  $\mathbf{x}$ . As the marginal change can be misleading when the probability curve is changing rapidly or when an independent variable is a dummy variable, it can be *favorable* to use *discrete changes*.

The *discrete change* in the predicted probability is defined as

$$\frac{\Delta}{\Delta x_k} \Pr(y = m|\mathbf{x}) = \Pr(y = m|\mathbf{x}, x_k = x_E) - \Pr(y = m|\mathbf{x}, x_k = x_S)$$

The discrete change is the change in the predicted probability for a change in  $x_k$  from the start value,  $x_S$ , to the end value,  $x_E$  (e.g., a change from  $x_k = 0$  to  $x_k = 1$  for a dummy variable). It can be interpreted as follows: when  $x_k$  changes from  $x_S$  to  $x_E$ , the predicted probability of outcome  $m$  changes by  $\Delta \Pr(y = m|\mathbf{x})/\Delta x_k$ , holding all other variables at  $\mathbf{x}$ .

The *value of the discrete change* depends firstly on the value at which  $x_k$  starts, secondly on the amount of change in  $x_k$ , and thirdly on the values of all other variables. Usually, each *continuous variable* except  $x_k$ , is held at its *mean*; for independent *dummy variables*, the change could be computed for both values of the variable.

## 2.5 Results

The intention of my analysis was to find out which people would probably work if they get a basic income and which people probably would not. Therefore, I analyzed what characterizes people with different degrees of work motivation, focussing on the non-pecuniary work motivation. Concretely, I carried out a cross sectional analysis with the PASS data set to find the key predictors of non-monetary work motivation, using several ordered logit models.

Table 2.2 presents the results of diverse ordered logit models (OLM) showing the impact of hard and soft individual characteristics on non-pecuniary work motivation and the willingness to work, respectively, even if the person does not need the money. The results are shown as  $\beta$ -coefficients of the exogenous variables.

The first model, including only hard individual factors, shows that women (*female*), Eastern Germans (*ost*), people with Roman Catholic faith (*konfess1*)

Variable	Model 1	Model 2	Model 3	Model 4
<b>Hard factors</b>				
female	0.401***	0.464***	0.465***	0.467***
ost	0.298***	0.291***	0.258 **	0.284 **
alterz	0.004	0.004	0.004	0.002
bildzeit	0.031***	0.025 **	0.023 **	
bbild2				0.230***
bbild3				0.182
bbild4				0.220 **
bbild5				0.202*
bbild6				0.770***
bbild8				1.628*
bbild9				-0.023
alogem	0.232***	0.236 **	0.244***	0.221 **
hausmf	-0.217 **	-0.151	-0.140	-0.158*
konfess1	0.191 **	0.193 **		0.175 **
konfess2	0.192 **	0.189 **		0.176 **
konfess4			-0.460*	
konfess7			-0.140 **	
<b>Soft factors</b>				
rollkons		0.002	0.009	-0.007
kons_female		-0.377***	-0.383***	-0.367***
sozpos2		-0.765 **	-0.769 **	-0.766 **
sozpos3		-0.732***	-0.720***	-0.731***
sozpos4		-0.691***	-0.678***	-0.698***
sozpos5		-0.579***	-0.571***	-0.602***
sozpos6		-0.724***	-0.714***	-0.740***
sozpos7		-0.596***	-0.583***	-0.602***
sozpos8		-0.510 **	-0.497 **	-0.517 **
sozpos9		-0.594 **	-0.585 **	-0.594 **
sozpos10		-0.095	-0.087	-0.116
<b>Statistics</b>				
N	15121	15121	15121	15121
N_pop	47.6e+06	47.6e+06	47.6e+06	47.6e+06
df_r	231	231	231	231

TABLE 2.2: Results of diverse OLM:  $\beta$ -coefficients of hard and soft individual characteristics as predictors of non-pecuniary work motivation. Legend: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$

and Evangelicals or Protestants (*konfess2*) have a significantly higher non-pecuniary work motivation than the rest of the population between 15 and 64 years. A higher qualification (*bildzeit*) also has a significantly positive effect on the non-monetary work motivation. Thus, these groups of people have a significantly higher probability to work with a basic income than the rest of the population. Homemakers, in contrast, have a significantly lower non-monetary motivation to do paid work than other people have. Under control of the other used variables, neither the age nor the migration background of a person has a significant impact on non-monetary work commitment. The variable for the migration background specially gets insignificant if a variable for qualification is taken into the model.

The second model extends the first model by two types of attitudes, called soft factors. Firstly, the impact of a conservative role model is examined with a dummy variable (*rollkons*) and an interaction term for conservative women (*kons\_female*). People with a conservative role model have a strongly significant lower non-monetary work commitment than not conservative people in the age of 15 to 64. This effect is almost completely due to the women. Non-conservative women (*female*) compared to non-conservative men have a strongly significant higher probability to be willing to work, without needing the money. In contrast, conservative women (*kons\_female*) have a strongly negative significant probability in comparison to non-conservative women. Men, however, do not differ significantly between the conservative (*rollkons*) and the non-conservative group.

Secondly, the impact of the social position (*sozpos*) on the endogenous variable is examined by a dummy set that ranges from the lowest (*sozpos1*) to the highest position (*sozpos10*). The outcome of this variable is generated by self-assessment of the interviewees. A remarkable result of the analysis is that all groups of self-assessed social position (*sozpos2-sozpos9*), except the highest position (*sozpos10*), state a significantly lower non-pecuniary work motivation than the lowest social position (*sozpos1*).

Conversely, people who see themselves as belonging either to the lowest or to the highest social position state a significantly higher non-monetary work commitment than other people do. Especially for the lowest social position this is an interesting and surprising outcome that cannot be explained easily. The group of the lowest position includes a higher than average proportion

of East Germans, of older people, of unemployed, of people with lower qualification and of people with migration background. All these characteristics, except the lower qualification and the migration background, positively affect the non-monetary work motivation. But this cannot be the explanation for the phenomenon, because the outcome is under the control of all these variables<sup>13</sup>.

By adding the soft factors to the model, the impact of most of the hard factors do not considerably change. But, the negative effect of the homemakers becomes insignificant by including the variables for the conservative attitude, particularly by controlling for conservative women.

Compared to Model 2, the *third model* includes dummy variables for Islamic faith (*konfess4*) and people without any confession (*konfess7*) instead of Roman Catholics and Protestants. It shows that, in contrast to the second model, people without any confession and people with Islamic confession have a significantly lower probability of being willing to work if they do not need the money, compared to the rest of the population.

The *fourth model*, in comparison to Model 2, substitutes the variable *bildzeit* for qualification by a dummy set, representing several kinds of graduation and professional education (*bbild1* to *bbild9*). Except the variable for homemakers that gets negatively significant, the hard factors do not essentially differ, compared to the second model. Except for the qualification group 3 (foreman, technicians, vocational academy) and the group 9 (another foreign degree), all qualification groups have a significant higher non-monetary work motivation than the reference group 1 (non-skilled, semi-skilled workers). This concretely concerns the qualification groups 2, 4, 5, 6 and 8, including the following degrees of qualification: apprenticeship, vocational school (*bbild2*), polytechnic, bachelor (*bbild4*), university degree, master (*bbild5*), PhD, habilitation (*bbild6*), and other German degrees (*bbild8*)<sup>14</sup>. The qualification group 7 (pupils) is not considered in this analysis, because pupils have not received a degree yet, and therefore no values for the qualification variable *bildzeit* exist. The lack of significance of qualification group 9 (other foreign degrees) could be explained by the small number of only 11 observations. All in all, we see that having any degree normally increases the probability of working, even if the person does not need the

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<sup>13</sup>The variable for the migration background is not included in the models presented in Table 2.1, because it has not been significant. But I have also analyzed models including that variable that verifies this result.

<sup>14</sup>Because the German qualification degrees cannot in every case exactly be translated into English, the German terms for the qualification groups are presented in Footnote 9.

money. Accordingly, having any degree normally increases the non-pecuniary work motivation. The only exception is the qualification group 3 (foreman, technicians, vocational academy), whose level of non-monetary work motivation does not significantly exceed the non-skilled and semi-skilled ones.

Table 2.3 presents the factor change in odds of non-pecuniary work motivation (*arbol*) for a unit respectively standard deviation (SD) increase in  $x_k$ , while holding  $\mathbf{x}$  constant, where  $\mathbf{x}$  is a vector of hard and soft individual factors as predictors of the variable *arbol* included in Model 2. The endogenous variable *arbol* can also be interpreted as the willingness to work, even if the person does not need the money.

Model Individual Characteristics	Model 2		
	Factor change in odds for a unit increase in $\mathbf{x}$	SD increase in $\mathbf{x}$	SD of $\mathbf{x}$
<b>Hard factors</b>			
female	1.5906***	1.2612***	0.5000
ost	1.3382***	1.1262***	0.4079
alterz	1.0039	1.0501	12.6582
bildzeit	1.0257**	1.0702**	2.6699
alogem	1.2662**	1.0688**	0.2819
konfess1	1.2132**	1.0934**	0.4621
konfess2	1.2081**	1.0880**	0.4461
<b>Soft factors</b>			
rollkons	1.0022	1.0010	0.4381
kons_female	0.6860***	0.8859***	0.3214
sozpos2	0.4655**	0.9120**	0.1205
sozpos3	0.4811***	0.8713***	0.1883
sozpos4	0.5012***	0.8583***	0.2212
sozpos5	0.5605***	0.7762***	0.4377
sozpos6	0.4847***	0.7450***	0.4065
sozpos7	0.5508***	0.7793***	0.4182
sozpos8	0.6008**	0.8342**	0.3558
sozpos9	0.5522**	0.9127**	0.1538
sozpos10	0.9092	0.9880	0.1273

TABLE 2.3: Factor change in odds of non-pecuniary work motivation with hard and soft individual factors as predictors for Model 2 including education time; odds are for  $> m$  vs.  $\leq m$ . Legend: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$

The presented factor changes in odds are calculated by  $\exp(\beta_k)$  for a unit increase in  $x_k$ , respectively by  $\exp(\beta_k \cdot SD_x)$  for a standard deviation increase in  $x_k$ , where the odds  $\Omega_{> m | \leq m}(x)$  are the ratios of the probability to state a higher versus lower category of non-pecuniary work motivation.



The results presented in Table 2.3 show that the odds of *Eastern Germans* stating a higher versus lower category of non-monetary willingness to work is higher than that of *Western Germans* by a factor of 1.34. Under control of the other predictors, the *age* of a person has no significant impact. The *qualification*, measured in equivalents of *education time* (*bildzeit*) has a significant impact on non-monetary work motivation. An additional year of education time shifts the odds of a higher versus lower work motivation by a factor 1.026, holding all other predictors constant. For a one standard deviation increase in education time, specifically for an increase of 2.67 years, the odds of a higher work motivation shifts by the factor 1.07. To be unemployed (*alogem*) increases the odds of non-monetary work motivation by the factor 1.27. Roman Catholics (*konfess1*) as well as Protestants (*konfess2*) have by the factor 1.2 higher odds of non-pecuniary work motivation than the rest of the population between 15 and 64 years. People who assign themselves to a middle social position have, by factors of 0.56 (*sozpos5*) and 0.48 (*sozpos6*), lower odds of stating a higher versus lower non-monetary working commitment than people who see themselves in the lowest social position (*sozpos1*). Compared to the results of Table 2.1, not only do we see the strong significance of these remarkable results, but looking at the factor changes in Table 2.3, we can also see that the impact of self-assessed social position is astonishingly large.

Interpreting the results for a conservative role model (*rollkons*) and gender (*female*) we must take into account the interaction effect between the two variables. Non-conservative women (*female*) have odds of higher versus lower non-monetary working attitudes that exceed the odds of non-conservative *men* by a factor of 1.59. This extent of the gender effect for non-conservative people is remarkably high. In contrast, *conservative* women (*kons\_female*) have, by a factor of 0.69, lower odds than *non-conservative women*. *Men*, however, do not differ significantly between the conservative (*rollkons*) and the non-conservative group.

Table 2.4 presents the factor change in odds of non-pecuniary work motivation of Model 4, where the variable *bildzeit* for qualification is substituted by a dummy set, representing several kinds of graduation and professional education (*bbild1* to *bbild9*). Except for the variable for homemakers, who get negatively significant, the hard factors do not substantially differ, compared to the second model as shown in the previous table. Homemakers have odds of a higher versus lower category of non-pecuniary work motivation that is by the factor 0.85 lower than the odds of all other people between 15 and 64 years,

holding all other variables constant.

Except for qualification groups 3 (foreman, technicians, vocational academy) and 9 (another foreign degree), all qualification groups have significant higher odds of non-monetary work motivation than the reference group 1 (non-skilled, semi-skilled workers). This corresponds to qualification groups 2, 4, 5, 6 and 8, including the following qualification degrees: apprenticeship, vocational school (*bbild2*), polytechnic, bachelor (*bbild4*), university degree, master (*bbild5*), PhD, habilitation (*bbild6*), and other German degrees (*bbild8*)<sup>15</sup>. Except for qualification group 8, which consists of only 15 observations in the sample, the highest shift of odds is observed for people with PhD or habilitation (*bbild6*). These people of the sixth qualification group have odds of non-monetary working attitudes that are by the factor 2.16 higher than the odds of non- and semi-skilled workers (*bbild1*). For the other significant qualification groups, the factor change in odds range from 1.22 to 1.26, including people with apprenticeship or vocational school (*bbild2*: factor 1.26), people with polytechnic degree or bachelor (*bbild4*: factor 1.25), and people with master or other university degrees (*bbild5*: factor 1.22).

Altogether, we see that having any degree increases the odds of non-monetary working commitment for a huge majority of people and qualification groups. Concretely, for almost every kind of qualification, having a degree increases the odds of non-pecuniary working attitude by a factor of 1.22 to a factor of 2.16; the only exception is the qualification group 3 (foreman, technicians, vocational academy), whose odds of non-monetary work motivation do not significantly exceed the odds of non-skilled and semi-skilled workers.

As shown in Table 2.5, the *predicted probabilities* of the four categories of non-pecuniary work motivation are 7.26% for strongly disagreeing (SD), 15.25% for disagreeing (D), 37.67% for agreeing (A) and 39.82% for strongly agreeing (SA) to the statement: "I would also like to work, if I didn't need the money."

*East Germans (ost)* have a predicted probability of strongly agreeing (SA) that is 7.08 percentage points higher than West Germans have, holding all other variables constant at their means.

Starting from the mean of education time (*bildzeit*), 12.09 years, a marginal increase leads to a 0.61 percentage point higher predicted probability of strongly agreeing (SA), keeping all other variables constant at their means.

<sup>15</sup>Because the German qualification degrees cannot in every case exactly be translated into English, the German terms for the qualification groups are presented in Footnote 9.

<b>Model</b>	<b>Model 4</b>		
Individual Characteristics	Factor change in odds for a unit increase in $x$	SD increase in $x$	SD of $x$
<b>Hard factors</b>			
female	1.5959***	1.2633***	0.5000
ost	1.3289***	1.1230***	0.4079
alterz	1.0020	1.0257	12.6582
alogem	1.2479**	1.0644**	0.2819
hausmf	0.8537*	0.9522*	0.3095
konfess1	1.1914**	1.0843**	0.4621
konfess2	1.1926**	1.0817**	0.4461
bbild2	1.2588***	1.1213***	0.4975
bbild3	1.2001	1.0519	0.2771
bbild4	1.2462**	1.0584**	0.2580
bbild5	1.2244*	1.0550*	0.2645
bbild6	2.1593***	1.0775***	0.0969
bbild8	5.0958*	1.0598*	0.0357
bbild9	0.9777	0.9996	0.0166
<b>Soft factors</b>			
rollkons	0.9933	0.9971	0.4381
kons_female	0.6931***	0.8889***	0.3214
sozpos2	0.4650**	0.9118**	0.1205
sozpos3	0.4813***	0.8714***	0.1883
sozpos4	0.4974***	0.8569***	0.2212
sozpos5	0.5476***	0.7683***	0.4377
sozpos6	0.4770***	0.7401***	0.4065
sozpos7	0.5478***	0.7775***	0.4182
sozpos8	0.5964**	0.8320**	0.3558
sozpos9	0.5522**	0.9127**	0.1538
sozpos10	0.8901	0.9853	0.1273

TABLE 2.4: Factor change in odds of non-pecuniary work motivation with hard and soft individual factors as predictors for Model 4, including several kinds of graduation and professional education; odds are for  $> m$  vs.  $\leq m$ . Legend: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$

*Unemployed people's (alogem)* predicted probabilities of strongly disagreeing (SD) and disagreeing (D) are 1.46 and 2.43 percentage points lower, respectively, than the predicted probabilities of all other people. Non-worker's probability of strongly agreeing (SA) is 5.75 percentage points higher than the predicted probability of all other people. This is valid as far as all other variables are kept constant at their means.

Category	SD	D	A	SA	
<b>Predicted probability</b>	.07264829	.15251917	.3766662	.39816634	
<b>Variable</b>	<b>dy/dx</b>	<b>dy/dx</b>	<b>dy/dx</b>	<b>dy/dx</b>	<b><math>x_k</math></b>
female***	-.0313664***	-.0494572***	-.0300046***	.1108282***	.494686
ost	-.0182927***	-.0301692***	-.0223112**	.0707731***	.210804
alter (n.s.)	-.00026	-.0004133	-.0002514	.0009247	41.6206
bildzeit**	-.0017122**	-.0027218**	-.0016561**	.0060901**	12.0897
alogem	-.0146424***	-.0243255***	-.0185311**	.057499***	.08703
hausmf	.0107131	.0165052	.0085122*	-.0357305	.107283
rollkons (n.s.)	-.0001473	-.0002342	-.0001427	.0005242	.25894
kons_female***	.0287749***	.0420885***	.016198***	-.0870614***	.116934
konfess1**	-.0126261**	-.0204042**	-.0135813**	.0466116**	.308961
konfess2**	-.012287**	-.0199128**	-.0134376**	.0456374**	.274088
sozpos2	.0707619*	.0876895**	.0049124	-.1633638*	.014742
sozpos3	.0659726**	.0838093***	.0084598	-.1582417**	.036824
sozpos4	.0607764**	.07898***	.0112329	-.1509893**	.051587
sozpos5	.0442327**	.0642753***	.0249711***	-.133479**	.25823
sozpos6***	.0588925***	.0813296***	.0229974***	-.1632195***	.20888
sozpos7***	.046479***	.0665664***	.0235396***	-.1365851***	.225936
sozpos8	.0401376**	.0572235**	.0189998***	-.1163609**	.148683
sozpos9	.0510056*	.0679001*	.0123195	-.1312253*	.024242
sozpos10 (n.s.)	.0066675	.0103504	.0055539	-.0225718	.01647

TABLE 2.5: Discrete and marginal changes of non-pecuniary work motivation with hard and soft individual factors as predictors of Model 2.  $dy/dx$  is for discrete change of dummy variable and for marginal change of metric variable  $x$  is held at its mean for all variables except  $x_k$ . Legend: \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$ .

For *Catholics* (*konfess1*) and *Protestants* (*konfess2*), the predicted probabilities of strongly agreeing (SA) are about 4.6 percentage points higher than for all other people in the age of 15 to 64 years, holding all other variables constant at their means.

The predicted probability of strongly agreeing (SA) for *non-conservative women* (*female*)<sup>16</sup> is 11.08 percentage points higher than that for *non-conservative men*, holding all other variables constant at their means. For *men*, the predicted probabilities of conservative and non-conservative people hardly differ (*rollkons*); for *women*, the predicted probabilities of conservative and non-conservative people differ significantly. So conservative women have approximately 8.7 percentage points<sup>17</sup> lower predicted probability of strongly agreeing (SA) than non-conservative women have.

In a ranking of the lowest to the highest social position from 1 to 10, people with a higher social position have lower predicted probabilities of strongly agreeing (SA) and higher probabilities of disagreeing (D) and strongly disagreeing (SD), compared to the reference group of the lowest position (*sozpos1*). For example, the group corresponding to the sixth social position (*sozpos6*) has predicted probabilities of strongly agreeing (SA) that are 16.3 percentage points lower, and predicted probabilities of disagreeing (D) and strongly disagreeing (SD) that are, respectively, 8.1 and 5.9 percentage points higher than the predicted probabilities of the lowest social position (*sozpos1*). The non-pecuniary work motivation of almost all other groups (except the highest social position, *sozpos10*) is lower than of the people who see themselves at the lowest level of social position (*sozpos1*). These results concerning the non-monetary work motivation of people who assign themselves to the lowest social position are one of the most remarkable outcomes of this analysis.

After having presented the individual characteristics that have a significant impact on non-pecuniary work motivation, I want to mention which *further characteristics* were not significant, but were still analyzed. The reason for this is that having the result of a possible impact factor that it is not significant, tells us much more than having not analyzed the factor. Here are the characteristics that were *not significant*, at least *under control of the predictors presented above*.

<sup>16</sup>Because of the interaction effect (*kons\_female*) between conservative gender roles (*rollkons*) and gender (*female*), the gender-dummy female represents the gender effect of non-conservative people but not the gender effect in general.

<sup>17</sup>The *group effect* of conservative vs. non-conservative *women* of 8.6 percentage points is calculated by adding the *group effect* of conservative vs. non-conservative *men* (*rollkons*) of 0.0524 and the *gender difference in group effects* (*kons\_female*) of -0.087.

### Individual hard factors

- *Individual net income (pneink)*: The logarithm of this variable ( $pneinkl_n$ ) is only significant if the gender dummy female is dropped. Additionally, dropping the dummy for East Germany (*ost*) increases the significance. This is because of women and East Germans on average have a lower income and a significantly higher non-monetary work motivation.
- *Migration background (migra)*: This variable is only significant if the variables for qualification (*bildzeit* or the dummies *bbild*) are dropped.

### Individual soft factors

- *Religiosity (religioes)*: The outcome of the variable for religious affiliations, ordered into four categories, was generated by self-assessment of the interviewees. This variable is not significant, even when dropping the variable for religious confession; this is also the case for a binary variable for religiosity.
- *Aspiration for education (bildasp)*: The educational aspiration of parents for their children is not a significant predictor for non-monetary work motivation.

### Household characteristics

- *Household size*: number of people living in the household
- *Household income*: total net income of the household, including social payments
- *Children*: the number of children under 18, 15, 6, 3 and 2 years, respectively, living in the household
- *Partner*: existence of a partner, married or unmarried to the interviewee
- *Partner income (ppneink)*: individual net income of the partners living in the same household; partner net income was considered in several forms: exact net income, classified net income, minimum income

For all these characteristics, no significant impact on non-monetary work motivation could be found, at least under control of other variables.

## 2.6 Conclusions

The intention of my investigation is to find out whether an unconditional basic income is an economically feasible alternative to workfare and activation policy, as they are currently dominant in European social and labor market policy. Although a basic income does not undermine monetary working incentives at all, it implements an exit option out of the labor market. Hence, the labor supply reactions are the crucial to assessing whether a basic income is economically feasible. Concretely, we have to answer the question, who would use the exit option and to what extent.

Although some economic research based on simulation models and a few field experiments exists, the state of basic income research is still characterized by a lack of current data analysis, particularly focussing labor supply effects. The analysis presented in this article makes useful contributions to fill this gap based on an exceptionally suitable data set, the new German panel study “Labour Market and Social Security” (PASS), conducted annually by the Institute for Employment Research (IAB). Thus, my analysis is focussed on the residents of Germany, but it can surely give interesting insights relevant to other comparable countries.

For a deep analysis of labor supply reactions with the aim to get valid results for practical use, work motivation plays a deceive role. Therefore, I tried to identify the essential determinants of work motivation, concentrating on individual factors, but adding household characteristics, too. Because of the exit option implemented by a basic income, I focussed the first part of my investigation, presented in this article, on the non-pecuniary motivation to work. For this, I used an ordinarily scaled variable that contains four affirmation levels to the statement: “*I would also like to work, if I didn't need the money*” with the outcome categories ‘strongly disagree’ (SD), ‘disagree’ (D), ‘agree’ (A) and ‘strongly agree’ (SA).

My analysis of non-pecuniary work motivation brings to light a number of interesting insights that can stimulate the debate about activation policy as well as about the feasibility of basic income. First of all, the fear of an economic disaster, caused by the labor supply effects of a basic income, cannot be verified. To the contrary, there is a huge majority that would be willing to work, even if they do not need the money. According to the predicted probabilities presented in Table 2.5, we can expect the following labor supply effects of a



basic income: A proportion of approximately 77.5% of the persons between 15 and 64 years would rather (A: 37.67%) or most likely (SA: 39.82%) be willing to work. In contrary, a portion of 15.25% would rather not (D), and only 7.26% is most likely unwilling (SD) to work. These are the estimated results for the whole population at the age of 15 to 64. The analysis of the essential predictors of non-monetary work motivation gives further interesting insights.

Firstly looking at the hard individual factors, we see that women have a significantly higher probability of being willing to work than men, even if they do not need the money. Hence, the thesis of some skeptical feminists that a basic income will lead to a high proportion of female homemakers cannot be verified in general. However, there is a big difference between women with a conservative role model and non-conservative women. On the one hand, *non-conservative* women have a strongly higher working commitment than non-conservative men; on the other hand, *conservative* women have a significantly lower working commitment than conservative men. Thus, for conservative persons there is a higher probability of female homemakers than for the rest of the population. However, conservative women make up only 11.7% of the population and they already have a significant higher proportion of homemakers than non-conservative women. Furthermore, conservative women, who currently do paid work for more than 15 hours a week make up just 5.9% of all people working more than 15 hours a week. Hence, the possible problem of an increasing proportion of female homemakers caused by a basic income will probably not be of high magnitude. In contrast, looking on the whole population, the proportion of women within paid workers is expected to increase with a basic income.

A further interesting insight is given by the analysis of self-assessed social position, as a predictor of working attitude. It is shown that persons that see themselves in the lowest social position have a significantly higher non-monetary work motivation than almost all other persons. Only the persons with the highest social position do not differ significantly in their non-monetary working commitment, compared to the persons with the lowest social position.

Unemployed persons also have a significantly higher non-pecuniary work motivation than all other persons. The probability of being willing to work, even if the person did not need the money, for unemployed persons, thus, is significantly higher than of the rest of the population with 15 to 64 years. This is

the fact, although a higher qualification level generally leads to a higher non-monetary working commitment, and the proportion of higher educated persons within unemployed people is lower than in average.

Furthermore, the fact that Eastern Germans still have a significantly higher non-monetary working commitment can be seen as evidence that working attitudes do not rapidly change over time, even if there is a big change in social and economic circumstances.

All in all, the analysis of non-pecuniary work motivation shows no evidence for an economic disaster caused by the labor supply effects of a basic income. Quite the contrary is shown. Especially the groups of persons that are mostly expected to leave the labor market if they do not need the money, show higher working attitudes than the average of the population. This particularly concerns unemployed persons and persons who see themselves in the lowest social position.

These results show that there is no evidence that activation policy would be useful or even necessary for the majority of unemployed persons. In contrary, the insights of my analysis give many arguments that a basic income indeed is economically feasible, and hence, should be the preferred policy.

# Chapter 3

## Work Values in Germany

This chapter is based upon Tolciu and Hohenleitner (2011), "Same but Different: Work Values in Germany", in *Hamburg Review of Social Sciences*.

### 3.1 Introduction

The heated social state debate championed in Germany at the beginning of 2010 by the foreign minister, Guido Westerwelle, who deplored the 'late Roman decadence' of a society that would treat welfare beneficiaries more generously than workers, still receives special interest, both from politicians and in public opinion.

A particular focus of the discussion is on issues related to the apparently insufficient working incentives for people entitled to 'Unemployment Benefits II' (UB II), popularly known as 'Hartz-IV'-recipients<sup>1</sup>. While some see the roots of the problem in the state policy which would rather foster idleness than work, others lament the working attitudes and the supposed unwillingness of a part of the unemployed to escape unemployment.

The economic literature on the Hartz IV laws and their effects on the German labor market is comprehensive. An important attribute of the research conducted so far is that it deals extensively with *general* evaluations and discussions of

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<sup>1</sup>The Hartz IV concept is a set of recommendations that came into force on January 1, 2005. The German government aimed for an increase in employment, and particularly for a reduction in long-term unemployment. The labor market reforms relying on the 'right-and-duty' principle were meant to 'activate' recipients of social benefits. Roughly said, in Germany an unemployed person first receives 'Unemployment Benefit I', which is calculated on the basis of previous job remunerations for one year after the job loss. Afterwards, the unemployed receive 'Unemployment Benefit II' (UB II or colloquial 'Hartz IV'), which has a fixed rate independent of previous employment. UB II can also be paid partially to supplement a low work income.

the reforms (BMAS, 2005; Christensen, 2004; Giesecke and Groß, 2005; van Suntum, 2005). With few exceptions, there has been little evidence for which factors influence the individual work incentives under the current regulatory framework. The analysis which have approached this issue have concentrated exclusively on financial aspects related to the German tax and social benefits system and neglect non-pecuniary incentives, such as (work) norms and values (Boss and Elendner, 2005; Boss et al., 2005; Deutsche Bundesbank, 2004; Haan et al., 2008)<sup>2</sup>. By and large, attitudinal factors and values have not found much attention in the economic literature to date, although there is an increasing awareness that they do play an important role in determining individual behavior (Manski, 2000; Soetevent, 2004).

Work values are, amongst others, central to the understanding of human working behavior, as they exercise a lasting influence on an individual's career development. According to Alvi (1980), the achievement of a high level of job satisfaction and a sense of self-fulfillment depend largely upon the degree of compatibility between one's work values and the requirements of a given working environment.

In accordance with these considerations, the aim of the present paper is to examine if, to what extent, and in what terms different population groups value work. Moreover, based on an ordered logit analysis we explore the factors which determine an individual's motivation to work. While the occupational status of individuals plays an important role in both analysis, in line with the current social debate, a special focus is set on unemployed people and on recipients of unemployment benefits II (UB-II-recipients) or colloquially spoken 'Hartz-IV'-recipients.

Moreover, particular attention is given to differentials in work values between eastern and western federal states (*Bundesländer*). In this research, we treat the former German Democratic Republic (East Germany or the new German states) and the former Federal Republic of Germany (West Germany or the old German states) as types of state socialist and capitalist nations. Contemporary Germany, characterized by an experience of dual political economies with similar cultural roots provides a unique opportunity to examine how subjective attitudes regarding the 'meaning of work' and individual beliefs of what is desirable about

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<sup>2</sup>Assuming that the incentives to take on a job depend on the additional net income it would provide, these studies conclude that the current framework would not provide enough stimuli for the unemployed; on the contrary, it even would impair their willingness to work.

jobs may be shaped by exposure to different economic systems, divergent ideologies and everyday life conditions. Therefore, We use the regional residence of individuals (East versus West) as a proxy for measuring the remains of state socialist and capitalist economies. Though we acknowledge that regional residence embodies various meanings, we argue that this differentiation reflects a person's socialization under different social and economic frameworks.

Although twenty years have passed since reunification, we expect attitudinal gaps between people from the eastern and western federal states for essentially two reasons. The first one relates to the long-lasting difference in experiences from the period when Germany was divided. An attribute of the former GDR was the socialist ideology which emphasized work as a special esteem factor (in the sense of self-fulfillment through working) and a way of contributing to a 'better' society. Since the effects of 50 years of socialization in a centrally planned economy cannot be easily erased, we expect that certain work values conveyed during the socialist time are still vivid for people living in the East. The second reason relates to the fact that, even after two decades after reunification, structural labor market gaps between western and eastern federal states are still substantial. Thus, differentials in work values previously formed might have been maintained, and even enhanced within the actual political and economic context.

This argument is very much in line with the controversial debate referring to the framework which caused or enhanced the establishment of different value sets within the German context. On the one hand, the 'socialization hypothesis' depicts that values are acquired by means of socialization processes, and that the institutional context in which individuals live have a significant impact on this socialization; the proponents of this theory see the existing values differentials between citizens living in Eastern and Western Germany as a reflection of the long-lasting influence of the socialization in the GDR state (Kaase and Bauer-Kaase, 1998). On the other hand, the proponents of the so-called 'situation' or 'experience hypothesis' explain the existing differentials as a phenomenon which occurred during and/or followed the reunification process. Hence, the East German 'particular mentality' was built as a reaction to the painful transformation process following the fall of the wall (Pollack and Pickel, 1998).

This paper is structured as follows: the next section provides a short discussion on the main insights coming from the literature dealing with work values

and individual labor market behavior. Then, the data used for the study is presented. In Section 3.4, the pecuniary and non-pecuniary motivation to work is analyzed by region and occupational status of individuals; moreover, we closely look at the value attached to specific work values by different groups. In Section 3.5, an ordered logit analysis enriches the present research by providing insights into the factors which determine an individual's motivation to work. Finally, the last section comprises the main conclusions of the paper and related policy implications.

## 3.2 Work values: conceptualization and previous findings

Although there has been a growing interest in recent years in studying value systems in general, and work values more specifically, the concept of 'work value', however, has remained the subject of controversy in the literature. Different perspectives on the nature and meaning of work make the agreement on a single, widely approved definition and measurement method difficult (Sagy, 1997).

Broadly speaking, attitudes and values have been used to describe the 'something else' that makes a group more than simply the sum of its individual members (Durkheim, [1895] 1964), as well as the attributes that separate human society from animal groups. A definition put forth by Taris and Feij (2001, p. 3) curtails values as "*enduring beliefs that a specific mode or conduct is preferable to its opposite, thereby guiding individual's attitudes, judgments and behaviours*". Work values have been defined as the outcomes people desire and feel they should attain through work (Brief, 1998; Frieze et al., 2006). Work values shape employees' preferences in the workplace, exerting a direct influence on their attitudes and behaviors, job decisions as well as perceptions and problem-solving (Dose, 1997).

Smith, 1965 believes that work values exert a central influence in a person's life, since work occupies far more time than any other daily activity. Whereas, for some people, work may simply be a means of earning a living, to many others, it offers challenges and satisfactions, a sense of achievement, success and enrichment in life. According to Morse and Weiss (1955, p. 191), work "*gives one a feeling of being tied into the larger society, of having something to do,*

of *having a purpose in life*". In other words, people work because it is useful, because it is 'right' and/or because it 'feels right' (Shamir, 1990).

Values are generally considered to be exceedingly durable once acquired by a person (Inglehart, 1990). However, this approach can be considered as an empirical finding rather than as a defining property of values, since otherwise rapid value changes would be excluded, by definition, from research (Borg and Braun, 1996). Following the stance put forth by previous research, this paper refers to *value items* (rather than 'values'); these include those items that measure a person's assessment of an object or statement on a scale from 'not important' to 'very important' (Borg and Braun, 1996; Borg and Staufenbiel, 1993).

Drawing on the economic and sociological literature, amongst others, three key insights referring to work values can be identified. The first illustrates that distinct historical experiences produce distinct national cultures (Inglehart, 1990). The national context and the related state political ideology have often been used to explain differences in work commitment between countries. This kind of research is based on the argument following Durkheim ([1895] 1964), that people possess 'mental programs' which contain a fragment of national culture, most clearly expressed in different values.

Work values in former communist countries are assumed to differ substantially from those in western societies, due to reasons such as different school education, propaganda through state-controlled media, and individual learning of what is rewarded and what is not<sup>3</sup>. However, especially for the German case, researchers provide rather ambiguous results regarding the extent to which East Germans differ in their attitudes from their western compatriots. While according to Liebig and Verwiebe (2000), some authors describe and interpret every minor difference in attitudes as proof for the existence of an 'invisible wall' in the citizens' mentality (Kaase and Bauer-Kaase, 1998), others, though they do acknowledge some deviations in attitudes, assess the differences as less significant (Wegener and Liebig, 1998). According to the latter, values differentials do not follow regional patterns but are visible, rather, in different population groups regardless of their geographical location.

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<sup>3</sup>Yet, it is difficult to interpret such differences because of the contextual factors involved in this type of intercultural research. The problems are much reduced, however, if one compares German eastern and western federal states, since in this case, common language and heritage control a variety of 'noise factors' and, thus, allows for an assessment of differences in work values.

The second insight refers to the fact that differences in work values are not only correlated with national and/or regional environments, but also differ by individual employment status. Beechey and Perkins (1987) and Hakim (1995) reveal gaps in working attitudes between unemployed, part-time and full-time workers; furthermore, the economic literature of social interactions illustrates a relevant interdependence between the individual unemployment status and the aggregate level of work values prevalent in the reference group (Clark et al., 2008; Kolm et al., 2003; Stutzer and Lalive, 2002). The 'social norm effect of unemployment' implies that the subjective well-being of unemployed people is negatively correlated with the strength of a social work norm, i.e. it is higher in communities where there is a weaker work norm. Lindbeck et al. (1999, p. 3) explain this finding by arguing that, as the number of individuals who are unemployed increases, social pressure diminishes. Thus, for example, living on transfer payments becomes less embarrassing when more individuals are doing likewise; in addition, higher regional unemployment rates hurt the unemployed less, as their situation is more bearable if it occurs on a larger scale.

Finally, the last insight highlights that (work) values might change over time and these changes can be explained by relevant political and/or economic events (such as crises). For example, the trends for the former Czechoslovakia over the years 1984 to 1990 reflected a decline of traditional 'communist' values and a rise of 'capitalist' values (Slejska and Borg, 1991). While people found it increasingly less important for their work to contribute to society, the importance of making a lot of money went up considerably. This development is also documented for East Germany and some other eastern European countries after the fall of the Iron Curtain. Liebig and Verwiebe (2000) note that, in this context, so-called 'achievement-related attitudes' (e.g. regarding income disparities and distribution, job salaries) converge more rapidly to western, capitalist approaches since they can be more easily compared, quantified and classified on the new prevailing monetary scale divisions. Therefore, the rapid adaptation of East Germans to the western German achievement-related standards does not appear surprising, but is rather driven by a factual adjustment of the institutional monetary framework (Hauser, 1999; Liebig and Verwiebe, 2000).



### 3.3 Data

The present analysis is sustained by a rich and up-to-date data set containing both labor market related variables and information regarding work values (or attitudes) of individuals living in Germany. The ‘Labor Market and Social Security’ data set (PASS) is an annual household survey commissioned by the Institute for Employment Research (IAB).

For the first wave of the panel study (2006/2007), 18,954 people in 12,794 households were interviewed. The applied survey design is a two-stage random sample including 300 postal code areas. The survey units consist of two partial populations: people and households in receipt of ‘Unemployment Benefit II’ (UB II) and people and households registered as residents of Germany. Initially, a personal interview was carried out with the heads of all selected households; subsequently, members aged 15 or over were interviewed; people aged 65 or over were presented with an abridged questionnaire referred to as a pensioners’ questionnaire. In Table 3.1, some descriptive statistics are presented in order to get a better overview of the data.

The complete data set includes 18,954 people, who were either employed (5,488 people), unemployed (5,835 people, including 4,580 beneficiaries of UB II) or homemakers (2,092 people). Not included in the analysis are pensioners, people who were fulfilling either their military or civil service, and people who

Descriptive statistics (in %)						
	Employed	Unemployed*	Unemployed in UB II**	Young unempl. in UB II***	Home- makers	Total of the sample
<b>Region</b>						
East	23.05	39.35	40.07	45.68	14.29	72.52
West	76.95	60.65	59.93	54.32	85.71	27.48
Total (in Tsd.)	5,488	5,835	4,580	359	2,092	18,954
<b>Gender</b>						
Men	55.98	50.61	51.64	49.86	4.02	45.97
Women	44.02	49.39	48.36	50.14	95.98	54.03
Total (in Tsd.)	5,488	5,835	4,580	359	2,092	18,954
<b>Educational level</b>						
Highly educated****	35.71	16.95	16.42	6.41	18.79	24.13
Other	64.29	83.05	83.58	93.59	81.21	75.87
Total (in Tsd.)	5,488	5,835	4,580	359	2,092	18,954

TABLE 3.1: Source: PASS 2006/2007, own calculations. Notes: \* *Unemployed* refers to all unemployed people in the dataset (age group 15-64), \*\* *unemployed in UB-II-receipt* includes all unemployed people who receive UB II, \*\*\* *young unemployed in UB-II-receipt* includes all unemployed people in the age group 15-24, who receive UB II, \*\*\*\* *Highly educated* includes all people with full maturity certificates and tertiary education according to the CASMIN classification.

were undertaking vocational training or are in a school (5,539 people). The group of unemployed covers both people who were unemployed and people who were in a job creation scheme provided by the Federal Employment Agency ('Ein-Euro-Jobs').

### 3.4 Differentials in the motivation to work

One claim put forth by previous studies dealing with attitudinal gaps across countries refers to the fact that work values are appraised differently in communist and capitalist societies. Accordingly, it is expected that even twenty years after the fall of the wall, East Germans weigh work values differently from their western counterparts. Following historical considerations, particularly non-pecuniary work values might be more highly esteemed in the East than in the West. For pecuniary work values it is the other way around, due to the fact that the importance of pay was de-emphasized by socialism (Braun, 1993). However, when accounting for the common institutional framework introduced since reunification, one question which arises is whether these differentials still hold within the current context. Moreover, against the background of previous research and the ongoing social state debate, it is also relevant to explicitly analyze work attitudes by occupational status.

First insights regarding both East-West and employment status differentials are given by a distinctive examination of the pecuniary and non-pecuniary motivation to work. The motivation for working is a psychological process resulting from the interaction between the individual and the environment. Accordingly, the work motivation is largely determined, not only by financial incentives, but also by common values regarding work behaviors through their association with expected social rewards and sanctions.

The non-pecuniary motivation is captured in the present analysis by two statements revealing working attitudes disentangled from financial aspects ("*I would work even if I didn't need the money*", Table 3.2, and "*Work endows the feeling to be part of society*", Table 3.3). The results confirm previous findings from the 90s, that a non-pecuniary work motivation is more widely spread in the East than

in the West<sup>4</sup>. The differentials are relatively large: 35.30 percent of all East Germans strongly agree with the first statement (compared to 29.64 percent of all West Germans); moreover, 43.06 percent further value work because of its sense of a common bond (compared to 36.21 percent in the western part of Germany).

The analysis by occupational status reveals that the proportions of those who would work when not depending on remuneration are higher in the East for every single employment category. Particularly high is the group of (young) unemployed recipients of UB II employment benefits: 55.02 percent in the East and 44.63 percent in the West strongly agreed on the statement (see Table 3.2).

The patterns depicted by different occupational groups on the second item reflecting the non-pecuniary work motivation ("*Work endows the feeling of being a part of society*") illustrate a typical individualism-collectivism dichotomy. This dichotomy, largely discussed in the cross-cultural psychology literature (Hofstede, 1980; Triandis, 1989), essentially reflects basic work values. The core meaning of individualism is giving priority to personal goals over the goals of the group. In work values, this translates into a higher emphasis on factors such as high income, prestige, independence and leadership; conversely, collectivist cultures shape work values that serve the group through the subordination of personal goals, i.e. egalitarianism and 'work for the common good' (Triandis, 1989). While socialist countries are considered typical collectivist societies, western (capitalist) countries are assumed to usually emphasize individualist (work) values. Meulemann (1996, p. 212) points out that in the GDR, work represented an '*ideology of integration*', since through work everybody was given an equal chance to be part of the society, regardless of the work output achieved. Whereas in the western part of Germany, work and work performance was desired to build up individuality, in the eastern part it was meant to shape collectivism.

In line with these ideas, the higher figures depicted for almost every employment category in the eastern part of Germany do not appear surprising (see Table 3.3). Work as a modality to be part of the society is valued more in the East by both groups, the employed and unemployed people, whereas especially the unemployed (including UB-II-recipients) display the highest score values. This

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<sup>4</sup>A similar pattern was found in 1991 by Wilpert and Maimer in a survey based on representative samples of the total population in both parts of Germany (1447 East Germans and 1187 West Germans): 79.7 percent of all eastern respondents and 64.4 percent of all western respondents reported they would continue to work even if all financial needs were eliminated (Wilpert and Maimer, 1991).

is, however, understandable, since the sense of affiliation to a community (or to society in general) is highly correlated with the social status given by paid employment. Job loss is, therefore, associated not only with a loss of income, but also with negative psychological consequences, such as low self-esteem and feelings of being 'worth nothing', or of not belonging to the community anymore.

The pecuniary motivation to work is illustrated by means of an indicator assessing working attitudes strictly related to payment. As expected, the shares of those who see work mainly as a means to earn money are larger in the western part of Germany, though not for all occupational categories. This can be historically explained, since in the eastern part, work was espoused, for a long time, as a self-fulfilling activity and a modality of building a better society, regardless of earnings. Meulemann (1996, p. 211) states that in the GDR, working morale had priority over joy of working, and instead of earning a lot of money, one received appreciation.

By and large, regarding the patterns depicted by unemployed people in general, and UB-II-recipients in particular, one can notice that their figures are above the average when assessing the value of both the pecuniary and non-pecuniary motivations to work. This provides first indications for the fact that, contrary to the assumptions put forth in the current social state debate, unemployed people in Germany value work, on average, as much as, or even more than people in other occupational categories. This might reflect the high average value attached to work in our society and the resulting pressure put on unemployed people to conform to this norm. In other words, given a certain level of work moral in society, work might become relatively more important, especially for the unemployed, not only due to financial factors, but also due to sociological and psychological aspects.

### **Between a 'fulfilling' and a 'secure' job: which work values matter for whom?**

With regard to work values, a commonly used differentiation is between extrinsic and intrinsic work values. Extrinsic work values refer to external job outcomes and include work benefits and job security. Furthermore, they reflect preferences for income and, in general, for benefits which are unrelated to the worker's tasks, e.g. a good pension plan or generous holiday entitlements. In contrast, intrinsic work values refer to the intrinsic outcomes gained from

**Non-pecuniary motivation: I would work even if I didn't need the money**  
(Proportion of those who 'strongly agree' with this statement)

	East Germany	West Germany
<b>All</b>	<b>35.30</b>	<b>29.64</b>
Employed	44.48	37.46
Unemployed*	51.57	41.61
Unemployed in UB II**	55.02	44.63
Young unempl. in UB II***	52.03	43.51
Homemaker	41.05	39.72

TABLE 3.2: Source: PASS 2006/07, own calculations. Notes: \* *Unemployed* refers to all unemployed people in the dataset (age group 15-65), \*\* *unemployed in UB-II-receipt* includes only those unemployed who receive UB II, \*\*\* *young unemployed in UB-II-receipt* includes all the unemployed in the age group 15-24 who receive UB II.

**Non-pecuniary motivation: Work endows the feeling of being part of society**  
(Proportion of those who 'strongly agree' with this statement)

	East Germany	West Germany
<b>All</b>	<b>43.06</b>	<b>36.21</b>
Employed	57.51	46.10
Unemployed*	61.30	57.61
Unemployed in UB II**	63.41	63.31
Young unempl. in UB II***	47.38	42.54
Homemaker	43.98	48.17

TABLE 3.3: Source: PASS 2006/07, own calculations. Notes: \* *Unemployed* refers to all unemployed people in the dataset (age group 15-65), \*\* *unemployed in UB-II-receipt* includes only those unemployed who receive UB II, \*\*\* *young unemployed in UB-II-receipt* includes all the unemployed in the age group 15-24 who receive UB II.

**Pecuniary motivation: Work is only a means to earn money**  
(Proportion of those who 'strongly agree' with this statement)

	East Germany	West Germany
<b>All</b>	<b>8.57</b>	<b>11.28</b>
Employed	11.29	14.00
Unemployed*	13.13	12.63
Unemployed in UB II**	13.94	14.13
Young unempl. in UB II***	14.23	9.64
Homemaker	18.28	14.01

TABLE 3.4: Source: PASS 2006/07, own calculations. Notes: \* *Unemployed* refers to all unemployed people in the dataset (age group 15-65), \*\* *unemployed in UB-II-receipt* includes only those unemployed who receive UB II, \*\*\* *young unemployed in UB-II-receipt* includes all the unemployed in the age group 15-24 who receive UB II.

working. They include aspects such as “broadening one’s horizons and having meaningful work” (van Vianen et al., 2007, p. 19).

Borg and Braun (1996) hypothesized that while the working motivation is likely to be understood differently in the new and old federal states, specific work values might be similarly structured and appraised. An in-depth analysis of intrinsic and extrinsic work values by region and employment status reveals that, when looking at east-west patterns, the overall differences are, indeed, rather small. However, the gaps are significant when considering the occupational status of individuals (see Tables 3.5 and 3.6).

Furthermore, the results illustrate that a general assertion regarding the preponderance of neither intrinsic nor extrinsic work values can be made on a regional basis. Intrinsic work values such as ‘an enjoyable job’ are given a higher importance in the West; however, the suitability of a job to the skills ones possesses is valued more in the East. Also, while extrinsic aspects such as ‘workplace security’ are on average more valued in the East, other aspects such as ‘good career opportunities’ seem to play a higher role in the West<sup>5</sup>.

Though a job’s security appears to be by far the most valued work characteristic for people in both East and West Germany<sup>6</sup>, the slightly higher figures for the new federal states can be explained by the higher rates of unemployment which prevailed after reunification.

While unemployment is generally perceived as an individual experience, in the eastern part of Germany it was seen as a collective fate, as it affected a large section of the population overnight. As a consequence, at the aggregate level, it corresponded with the collapse of large parts of economy and, at the individual level, with a rapid loss of income and status. Moreover, the higher emphasis put on work values related to remuneration and job security is also explainable by the current economic situation, which is still marked by substantial structural differences, including average incomes in the East being markedly lower than in the West although costs are approaching similar levels.

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<sup>5</sup>A similar trend is visible in a survey conducted in 1991 by EMNID. On the basis of 14 items, respondents were asked to indicate which work related aspects they considered ‘particularly important’. ‘Workplace security’ (East: 70 percent, West: 53 percent), ‘good wage security’ (East: 68 percent, West: 62 percent), ‘generous vacation conditions’ ‘security’ (East: 13 percent, West: 20 percent) and ‘interesting work’ (East: 31 percent, West: 43 percent) were among these items.

<sup>6</sup>The gap between the ratings of extrinsic work values is quite striking: ‘job security’ is rated as very important by 63.19 percent in the East and 62.39 percent in the West compared to 23.29 percent, respectively 19.03 percent who see values such as ‘high pay’ and 19.30 percent, respectively 20.98 percent who see ‘good career prospects’ as very important.

The analysis regarding work values by occupational status highlights that the share of those who rate intrinsic values highly (i.e. 'an enjoyable job' or 'job that best suits my skills') is larger in the groups of employed people (both in the West and in the East). However, the variation across other occupational groups is rather small, particularly in the eastern part of Germany.

On the contrary, extrinsic work values are rated relatively unequally by different occupational groups. In general, compared to others, employed people regard these values as less significant. Furthermore, the importance of values such as 'good career prospects' and 'workplace security' are mostly appreciated by unemployed people in general, and especially by those who receive UB IV.

Summing up, differentials in the importance attached to specific work values

**East Germany: importance of intrinsic and extrinsic work values by occupational status** (Proportion rating the items as 'very important')

	All	Employed	Unemployed*	Unemployed in UB II**
<b>Intrinsic work values</b>				
An enjoyable job	<b>60.42</b>	77.67	77.09	72.27
A job that best suits my skills	<b>52.15</b>	68.23	65.85	65.29
<b>Extrinsic work values</b>				
Workplace security	<b>63.19</b>	81.36	86.47	85.64
High pay	<b>23.29</b>	27.17	36.56	35.04
Good career prospects	<b>19.30</b>	20.00	28.34	29.07

TABLE 3.5: Source: PASS 2006/2007, own calculations.

Notes: \* *Unemployed* refers to all unemployed people in the dataset (age group 15-65), \*\* *unemployed in UB-II-receipt* includes only those unemployed, who receive UB II

**West Germany: importance of intrinsic and extrinsic work values by occupational status** (Proportion rating the items as 'very important')

	All	Employed	Unemployed*	Unemployed in UB II**
<b>Intrinsic work values</b>				
An enjoyable job	<b>61.30</b>	78.01	72.71	72.43
A job that best suits my skills	<b>49.49</b>	63.34	60.33	61.04
<b>Extrinsic work values</b>				
Workplace security	<b>62.39</b>	80.19	80.70	82.62
High pay	<b>19.03</b>	24.63	31.38	31.00
Good career prospects	<b>20.98</b>	24.71	29.28	31.96

TABLE 3.6: Source: PASS 2006/2007, own calculations.

Notes: \* *Unemployed* refers to all unemployed people in the dataset (age group 15-65), \*\* *unemployed in UB-II-receipt* includes only those unemployed, who receive UB II

are more noticeable when taking into account the occupational status of individuals than the region they live in. This finding, which partly confirms the hypothesis of Borg and Braun (1996) can be explained by proximate differences in needs, fears and pressures on the labor market. While unemployed people, in particular, translate their job and income loss in extrinsic work values, employed people mirror their situation by attaching a comparably higher importance to intrinsic work values.

### 3.5 Determinants of pecuniary and non-pecuniary work motivation

An important research sector was concerned with identifying the emergence of individual differences in working values and motivations. The results differ in the emphasis they place on a variety of environmental, socio-cultural, personal and even genetic factors (Keller et al., 1992; Sagy, 1997). Another branch of studies reveals that the individual work motivation is also predicted by work content variables (task characteristics) such as skill variety and autonomy. When employees have larger degrees of autonomy, receive constructive feedback about their performance, and have an identifiable piece of work to do, they may experience feelings of happiness, and an increase in their motivation to keep performing well (Hackman and Oldham, 1980).

While we are able to account for socio-demographic and geographical factors in our analysis, due to data constraints, we have to omit work-related information such as job satisfaction and remuneration.

For assessing the determinants of pecuniary and non-pecuniary working motivation, we employ ordered logistic models (OLM). The dependent variables are coded in four categories (1: 'strongly disagree' (SD), 2: 'disagree (D)', 3: 'agree' (A) and 4: 'strongly agree' (SA)). For this type of data, ordered probability models are suitable tools of analysis (Long and Freese, 2006, p. 183–222). From the two alternative model types, the ordered logit and the ordered probit, we choose to present the results of the ordered logit model since the results for the ordered probit model were only slightly different. The ordered logit model is based on the following specification:

$$y_i^* = \mathbf{x}_i\boldsymbol{\beta} + \varepsilon_i$$



where  $\mathbf{x}_i$  is a set of explanatory variables and  $\varepsilon_i$  is the disturbance term. In this analysis,  $y_i^*$  is the real but not observable degree of non-pecuniary work motivation; what we observe is:

$$\begin{aligned} y_i = 1 &\Rightarrow \text{SD} && \text{if } \tau_0 = -\infty \leq y_i^* < \tau_1 \\ y_i = 2 &\Rightarrow \text{D} && \text{if } \tau_1 \leq y_i^* < \tau_2 \\ y_i = 3 &\Rightarrow \text{A} && \text{if } \tau_2 \leq y_i^* < \tau_3 \\ y_i = 4 &\Rightarrow \text{SA} && \text{if } \tau_3 \leq y_i^* < \tau_4 = +\infty \end{aligned}$$

The fitted model is:

$$\Pr(y_i = m|\mathbf{x}) = F(\tau_m - \mathbf{x}\boldsymbol{\beta}) - F(\tau_{m-1} - \mathbf{x}\boldsymbol{\beta})$$

In the ordered logit model,  $\varepsilon_i$  has a standard logistic distribution, whereas in the ordered probit specification,  $\varepsilon_i$  has a standard normal distribution. The results are presented as odds ratios, calculated as:

$$\frac{\Omega_{>m|\leq m}(\mathbf{x}, x_k + 1)}{\Omega_{>m|\leq m}(\mathbf{x}, x_k)} = \exp(\beta_k)$$

which are interpreted as factor change in odds for a unit decrease in the predictor variable  $x_k$  while holding each of the other predictors in  $\mathbf{x}$  constant. Concretely, we use the odds  $\Omega_{>m|\leq m}$ , i.e. the probability of observing a higher versus a lower outcome category.

As shown in Table 3.7, people from eastern federal states are more likely to display a higher non-pecuniary working motivation than people from western federal states (that is, by a factor of 1.343 and 1.496, as illustrated in Models 1 and 2, respectively). Remarkably, the odds of valuing work for its monetary aspects are also higher for East Germans than West Germans, but only by a factor of 1.140 and less significantly so (Model 3).

Furthermore, a gender effect can be observed. The odds of stating a higher category of working motivation if they did not need the money are higher, by a factor of 1.427 for women than men, holding all other variables constant (Model 1); conversely, women's chances of having a higher category of monetary working motivation (Model 3) are lower than men's by a factor of 0.737.

Also noticeable are the effects of education and religiosity on pecuniary working motivation. The chance of displaying a higher level of agreement with the statement “work is only a means to earn money” (Model 3) is lower by a factor of 0.808 for more educated people, and lower by a factor of 0.812 for people who consider themselves religious.

While being employed has no significant effect on the stated category of working motivation, being unemployed increases the odds of stating a higher non-pecuniary as well as pecuniary working motivation than other population groups. More precisely, unemployed people are more likely to declare a higher category of non-pecuniary motivation by a factor of 1.217 (Model 1) and 1.316 (Model 2), and to declare a higher category of pecuniary working motivation, by a factor of 1.147, than the rest of the population living in Germany, aged 15 to 64 years

Summing up, the analysis reveal similar effects of being unemployed and of living in the eastern part of Germany, insofar as both indicators increase the individual propensity of having both a higher non-pecuniary and a pecuniary working motivation. The influence is most powerful (in terms of scale and significance) when referring to the non-pecuniary motivation to work, in particular in relation to the statement “Work endows the feeling to be part of society” (Model 2).

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<sup>6</sup>The variable for education level according to the CASMIN-classification depicts the values 1 for *no degree*, 2 for *general elementary education*, 3 for *basic vocational education*, 4 for *intermediate vocational qualification*, 5 for *intermediate general qualification*, 6 for *full maturity certificate with vocational qualification*, 7 for *full maturity certificate with vocational qualification*, 8 for *lower tertiary education* and 9 for *higher tertiary education*

Determinants of non-pecuniary and pecuniary motivations to work			
	<b>M1: Non-pecuniary motivation: I would work even if I didn't need the money</b>	<b>M2: Non-pecuniary motivation Work endows the feeling to be part of society</b>	<b>M3: Pecuniary motivation: Work is only a means to earn money</b>
<b>Dep. variables</b>	Odds ratios	Odds ratios	Odds ratios
Women	1.4269***	1.0888	.7373***
Age	1.0044*	1.0296***	1.0005
East	1.3432***	1.4959***	1.1396*
Religiosity	1.0820	1.1125	.8124***
Education	1.0240*	.9329***	.8082***
Children	1.0043	.9727	1.0441*
Unemployed	1.2173**	1.3155**	1.1474*
Employed	1.0426	.9974	1.0305
<b>Pseudo R-squared</b>	.0064	.0247	.0302
<b>Sample size<sup>1</sup></b>	14,413	14,413	14,413

TABLE 3.7: Source: PASS 2006/2007, own calculations. Notes: *Women* reveals the gender of individuals (1-Women, 0-Man); *Age* is person's age (in years); *East* is a variable depicting the regional location of individuals (1-eastern federal states, 0-western federal states); *Religiosity* is a dummy with the value 1 for people who consider themselves as being (very) religious and 0 otherwise; *Education* depicts the educational level of individuals according to the CASMIN-classification<sup>7</sup>; *Children* indicates the number of own children (living in and outside the household); *Unemployed* is a dummy with the value 1 for being unemployed and 0 otherwise; *Employed* is a dummy with the value 1 being employed at least 1 hour per week and 0 otherwise. <sup>1</sup>: Sample of residential population in Germany, aged 15 to 64 years. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01

### 3.6 Conclusions

In response to the lack of empirical evidence regarding the importance attributed to work in our society, the present paper aims to fill this gap by analyzing to what extent, and in what terms, different population groups value work. Moreover, by means of an ordered logit analysis, we explore the factors which determine an individual's working motivation. In line with the current social state debate, while the main differentiation is made according to the occupational status of individuals, a special focus is set on unemployed persons.

The analysis of a recent data set in reference to insights from previous studies brings to light a number of relevant aspects concerning current work values patterns. Firstly, it reveals that while the motivation to work (or the meaning attached to work) still differs between the eastern and western parts of Germany (with a rather non-pecuniary interpretation of east Germans), there are only small differences when it comes to the structure and importance attached to certain work values. In other words, it is the meaning of work (*die Bedeutung*), not the relevance (*die Bedeutsamkeit*) of specific work values which follows a separate east-west path.

A second aspect highlighted by the present analysis is the fact that there is a significant difference between the importance attributed to intrinsic and extrinsic work values when accounting for the individuals' occupational status. Employed people place a higher value on work attributes such as 'an enjoyable job', whereas unemployed people appreciate more 'workplace security'. This is, however, explainable by the different needs and conditions the two groups face on the labor market.

Thirdly, despite certain arguments adopted in the current social state debate, unemployed people — and particularly UB-II-recipients — do not attach a lower importance to work when compared to other occupational categories. On the contrary, on average, they value both the intrinsic and the extrinsic motivation to work more highly than other groups. This is possibly due to the rather high value people in general place on work. The existential threat of losing not only the income for living costs but possibly social status also, raises the importance attached to work by unemployed people.

This mechanism is likely to be enforced when, as mentioned above, work is highly rated as a value by the whole society. Following a social multiplier effect, the high relevance of work which is found at the aggregate level might,

thus, be translated into a higher emphasis being put on work by single individuals, and vice versa. A new branch in economics which deals with the effect social interactions have on the individual's performance in the labor market suggests, however, that an individual's behavior is also significantly affected to a lesser extent by the behavior of a relevant peer group (i.e. people with whom one shares common characteristics or those who live nearby, such as neighbors, friends, parents, working colleagues). Against the results put forth by the present analysis, it means that the importance attached to work by unemployed individuals (though not only) varies according to the narrower social environment they live in. In other words, persons having a peer group in which work is highly valued might accordingly place a higher emphasis on work.

The insights referring to the importance attached to work and work values, on the one hand, and to the role played by work values on the labor market on the other hand, constitute important guiding points for policy makers and practitioners. Their benefits are related to modalities of formulating the framework more accurately, and better predicting the consequences of public policies. The existing structures and institutions in charge of the development, implementation and evaluation of labor market policies should therefore increasingly take into account not only classical cost-benefit considerations, but also non-market factors such as norms and values. In the end, policy measures are successful only if they are simultaneously implemented with compatible (changes in) core values which are shared and sustained by all occupational categories alike.



# Chapter 4

## Impact of Welfare Sanctions on (Non-)Employment

This chapter is based upon Hillmann and Hohenleitner (2015), *“Impact of Welfare Sanctions on Employment Entry and Exit from Labor Force — Evidence from German Survey Data”*, HWWI Research Paper 168, which is a revised version of Hillmann and Hohenleitner (2012), *“Impact of Benefit Sanctions on Unemployment Outflow — Evidence from German Survey Data”*, HWWI Research Paper 129.

### 4.1 Introduction

During the last two decades, many European countries went through a paradigm shift in unemployment policy from welfare towards workfare, commonly referred to as ‘activation policy’. In Germany, a comprehensive labor market reform based on the so-called ‘Hartz laws’ led to a substantial restructuring of the unemployment and social benefit system.<sup>1</sup> More than 6 million people were immediately affected by the implementation of the last reform step in January 2005; 4.5 million of them became entitled to the new unemployment benefits II (UB II), commonly known as ‘Hartz IV’. The ‘Hartz laws’ entailed an extensive monitoring and sanction system, and work requirements were strengthened radically. Under the reformed system, a person must accept any job regardless of its impact upon their occupational skills or any other external effect.<sup>2</sup>

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<sup>1</sup>The reforms are named after Peter Hartz, the chief of the commission that set up the design of the four reform laws. For a comprehensive overview of each reform step, see Ebbinghaus and Eichhorst (2006).

<sup>2</sup>Unwelcome (long-term) effects of benefit sanctions comprise unstable employment and low wages, also below the subsistence level.

The purpose of this paper, therefore, is to look beyond the imperative of getting people employed at any price. In other words, our analysis complements employment entry with labor market dropout as another probable response to welfare sanctions. Crucially, we examine the effects of German welfare sanctions — namely the ‘UB II’ sanctions — on unemployment outflow in both directions, i.e. our analysis considers job entry and labor market dropout (also called ‘non-employment’) as equally plausible and important responses to welfare sanctions.<sup>3</sup>

We aim to demonstrate a causal connection between the use of German UB II sanctions — sanctions that are meant to encourage a swifter entry into employment — and an increased likelihood of labor market exit. Due to continuous pressure on the part of jobcenters, sanctioned welfare recipients may increase their search efforts or accept jobs with poorer conditions. However, as not everyone will successfully find a job that pays enough for them to leave the welfare system,<sup>4</sup> sanctions may actually drive some of these benefit recipients to search for alternatives beyond welfare and employment. Such alternatives include living on parents’, children’s or a partner’s income, on assets, student’s assistance<sup>5</sup>, disability pension, early retirement pay — or in some cases even on illegal work, begging or criminal activity (Ames (2009), Götz et al. (2010), Machin and Marie (2004), Schreyer et al. (2012), Wolff (2014)).

By far the majority of European studies focus on the recipients of unemployment insurance benefits (UIB) who are, on average, more likely to find a job than welfare recipients. In reality, a significant proportion of welfare recipients consists of the long-term unemployed, and only-partly-employable people.<sup>6</sup> Presumably, welfare recipients are, then, more likely to end in non-employment than recipients of UIB. However, only very few of these studies on UIB recipients consider exit from labor-force as a possible consequence of benefit sanctions. The purpose of this study is to fill this gap as it provides one of the first

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<sup>3</sup>For the remainder of the paper, we use the word ‘welfare’ synonymously with the German tax-based transfer, UB II, even though welfare, technically, is a hypernym. Thus, we differentiate welfare sanctions from benefit sanctions that usually refer to the receipt of unemployment insurance (UI) payments.

<sup>4</sup>As explained in Section 4.3.1, also *employed* people can be eligible for UB II if their earned income does not cover the minimum subsistence level of their households. Hence, eligibility to the German UB II does not only depend on the absolute amount of the claimant’s earned income but also on the number of family members living in their household.

<sup>5</sup>Røed and Westlie (2012) find evidence that unemployment insurance benefit (UIB) sanctions in Norway increase the transition rate into education by about 200%.

<sup>6</sup>Only-partly-employable people comprise, for example, persons with health restrictions and persons caring for infants or for elderly and sick relatives.



European analysis of sanctions against welfare recipients, augmenting the view on exit into employment by the exit into non-employment.

An overview of the scarce European studies on welfare sanctions — only one of which takes into account the non-employment option — is given in Section 4.2. In the remainder of this section, we introduce some of the best-known European studies of unemployment insurance benefit (UIB) sanctions, comprising a few studies considering the option of leaving the labor force.<sup>7</sup> It should be noted that for both groups of studies — of UIB as well as of welfare recipients — several of them are restricted to specific districts of a country, to certain sectors of the labor market, or to particular groups of benefit recipients and, hence, are not necessarily valid for the total of a country's benefit recipients.

For instance, Abbring et al. (2005) analyze the impact of UIB sanctions on the transition rate into employment for the Dutch metal and banking sector. They estimate a positive and significant effect of sanctions on re-employment for men and women separately, whereas the effect for female unemployed with an increased transition rate of 98% for the metal industry and 85% for the banking sector turns out to be considerably higher than for males.

For Denmark, Svarer (2010) examines a large Danish register dataset to investigate the effect of sanctions on re-employment rates in the period from January 2003 to November 2005. Svarer (2010) obtains positive estimates for the sanction coefficient. The estimates of the time-varying effect of sanctions suggest a remarkably high effect for the first four weeks after a sanction has been imposed. However during the following eight weeks, the effect drops sharply and loses significance after thirteen weeks.

The study of van den Berg and Vikström (2014) analyzes the monitoring and sanction regime of the Swedish unemployment insurance (UI) system on re-employment durations and ensuing job quality. Using combined register data sets covering the (un-)employment history of the Swedish population over 1999 to 2004, and applying an extended Timing-of-Events (ToE) approach, they find a significant positive effect of sanctions on re-employment, but an adverse effect on job quality. Whereas job exit rates increase by 23%, wages decrease by 4% and the probability of moving from part-time to full-time employment falls by 15%.

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<sup>7</sup>Other European studies on UIB sanctions are provided, for example, by Cockx et al. (2011) for Belgium and Røed and Westlie (2012) for Norway.

The two following studies are based on unemployment insurance (UI) register data for certain Swiss cantons. The data records the date of sanction warnings and imposition, allowing an analysis of ex-ante and ex-post effects. Firstly, Lalive et al. (2005) find that both warnings and enforced sanctions affect the unemployment exit rate positively. Their model reveals a 28% shift in the unemployment exit rate after a warning. The transition out of unemployment increases again by 23% after a sanction was enforced. Compared to the effect of sanction enforcement, the results already indicate that the warning exhibits a quantitatively similar effect. Using the same administrative data source, Arni et al. (2013) employ a multivariate mixed proportional hazard model for competing risk to examine the impact of warnings, and how the imposition of sanctions affect the unemployment exit hazard to either regular employment or non-employment (i.e., out of labor force) as the two competing risks. This elaborate analysis shows a positive impact of warnings and sanction enforcements on unemployment exit rates to either of the two competing risks, whereas the announcement of a sanction increases the risk of exit to non-employment considerably. Beyond examining the unemployment exit hazard, Arni et al. (2013) amplify their approach by including an analysis of the post-unemployment employment periods with respect to job stability and earnings. They find significant evidence that a sanction during a period of unemployment reduces the duration of the first employment and non-employment period. Regarding wages, sanction warnings and impositions significantly lower post-unemployment earnings.

Similar to other European countries, in Germany the initial studies on benefit sanctions have focused on UIB receipt. Müller and Steiner (2008) explore the ex-post effect of unemployment benefit sanctions on unemployment-to-employment transitions between 2001 and 2004 separately for West and East Germany. They restrict the sample to unemployment insurance benefits (UIB) and unemployment assistance (UA) inflow cohorts in the years 2001 and 2002 at the beginning of the unemployment spell.<sup>8</sup> Combining Propensity Score Matching (PSM) with a Mixed Proportional Hazard (MPH) model for discrete-time hazard rates, Müller and Steiner (2008) find robust positive effects of benefit sanctions for men and women in East and West Germany. The effect decreases with elapsed unemployment duration until the sanction is imposed.

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<sup>8</sup>In contrast to unemployment insurance benefits (UIB), unemployment assistance (UA) was tax based. UA existed only until the end of 2004. Since 2005 the unemployment benefit system has changed substantially. Further information is given in Section 4.3.

Hofmann (2012) investigates the ex-post effect of sanctions on an individual's likelihood of gaining regular employment, holding an irregular job, or leaving the labor force. A dynamic matching approach is applied to a sample of individuals that entered UIB receipt between April 2000 and March 2001 in West Germany. The results reveal rather ambiguous effects: while the Average Treatment effect on the Treated (ATT) for the outcome of entry to regular employment turns out to be positive and mainly driven by young UIB recipients, the ATT for the probability to hold an irregular job is positive for women but negative for men. The positive effect for women is driven by the older subgroup and the negative effect for men is found to be stronger in regions with higher unemployment rates. Regarding the outcome of leaving the labor market, benefit sanctions lead to a higher drop off within the group of older women. Also, sanctioned men have a higher probability of withdrawing from the labor market when compared to non-sanctioned men.<sup>9</sup>

However, given the considerably higher proportion of welfare recipients compared to UIB recipients, the extensive monitoring and sanction regime introduced under 'Hartz IV', and the fact that these strengthened regulations primarily target UB II recipients, we have chosen to put the focus on unemployed welfare recipients. We provide the first analysis of the causal ex-post effects of German welfare sanctions — namely UB II sanctions — on the hazard rates to both employment and non-employment. We examine the effects on unemployment duration after the imposition of benefit sanctions, referred to as ex-post effects, and abstract from ex-ante effects, caused by implementing and tightening up the monitoring and sanction regime, or by possible warnings before imposing a sanction.

In contrast to previous studies of benefit sanctions, we estimate the effect on all employable household members, and not just on the recipient of the sanction, as UB II applies to households.<sup>10</sup> As a consequence, we also treat the other household members as affected. We exploit data from a novel German panel survey, especially designed for research on employable welfare recipients and their household members. It provides detailed information about individuals'

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<sup>9</sup>This result is especially found for men who have been sanctioned during the 2nd or 3rd stratum, i.e. during the 3rd until 6th month of UIB receipt.

<sup>10</sup>Unlike the individually-granted unemployment insurance benefit (UIB), since 2005 called UB I, the means-tested social benefit UB II applies to an entire household as a so-called 'need unit', i.e. to all related members of a household. More detailed information on the institutional framework is given in Section 4.3.

(un-)employment histories, including information on UB II sanctions and periods of non-employment. Employing a Timing-of-Events (ToE) approach, we estimate a discrete multivariate Mixed Proportional Hazard (MPH) model to the survey data that covers the first three years after the implementation of 'Hartz IV', from 2005 to 2007.

The remainder of the paper is organized as follows: Section 4.2 briefly summarizes research on the effects of sanctions upon welfare recipients in Europe, and Section 4.3 outlines the institutional structure of the German unemployment benefit and sanction scheme implemented by the 'Hartz IV' law. A detailed description of the data, in particular of the group differences between sanctioned and non-sanctioned unemployed in UB II receipt, is provided in Section 4.4. Section 4.5 introduces the econometric model, whereas the results are presented and discussed in Section 4.6, followed by a conclusion in Section 4.7.

## **4.2 Previous studies on European welfare sanctions**

To date, the study of welfare sanctions in European countries have been very limited. So far, there have been two studies focusing on welfare recipients in Rotterdam (Netherlands), one recent study in Finland, and three studies in Germany.

An early Dutch study on welfare sanctions is provided by van den Berg et al. (2004). They use a Mixed Proportional Hazard (MPH) model and find sanctions to have a significantly positive effect on the unemployment-to-employment hazard of welfare recipients in Rotterdam. In figures, a sanction raises transition rates to work by 140%. Moreover, they find a substantially negative effect on the probability an individual becomes long-term unemployed if the sanction is imposed at a relatively early stage. The more recent Dutch analysis for the same municipality by van der Klaauw and van Ours (2013) investigates the effects of re-employment bonuses and benefit sanctions on the re-employment probability of welfare recipients and find that benefit sanctions exhibit positive effects on employment probability, whereas re-employment bonuses are not verified as an effective policy instrument.

A very recent study by Busk (2014) compares the effects of unemployment insurance benefit (UIB) and welfare sanctions in Finland with respect to the outcomes employment, participation in a program of the Active Labor Market Policy (ALMP), and exit from labor force. Using the Timing-of-Events (ToE) approach, Busk (2014) finds evidence for a positive effect of ongoing sanctions upon UIB and welfare recipients on taking up employment as well as for completed sanctions upon welfare recipients.<sup>11</sup> However, she found no effect of completed UIB sanctions on transition rates into employment. Regarding participation in programs of the ALMP, sanctions have a slight positive effect on welfare recipients, but no effect on UIB recipients. Finally, she found exit from labor force positively affected by both UIB and welfare sanctions. This study for Finland — together with our German study — are the first European analysis of welfare sanctions considering the non-employment option.<sup>12</sup>

The majority of the earlier German studies on benefit sanctions focused on recipients of unemployment insurance benefits (UIB). However, since the 'Hartz IV' law came into force in January 2005, employable welfare recipients — namely UB II recipients — have come increasingly into the focus of political discussion and, with it, also into the focus of scientific research. But still, research on the effects of German welfare sanctions is very limited, and none of the previous studies take into account the non-employment option.

A very early and comprehensive research on German UB II recipients provided by Schneider (2008, 2010), analyzes the effect of UB II sanctions on reservation wages, job search effort, and employment outcome using the German cross-sectional survey of unemployed UB II recipients in January 2005. Adopting a Propensity Score Matching (PSM) approach, Schneider (2008, 2010) finds only the effect on unsubsidized employment as partially significant and positive; the remaining effects on reservation wages, job search effort, and subsidized

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<sup>11</sup>Unlike other studies, Busk (2014) distinguishes between effects during the periods of benefit cut (ongoing sanctions), and the effects after the benefit cuts (completed sanctions).

<sup>12</sup>The Finish welfare recipients in this study differ quite a lot from the German UB II recipients as membership in a Finish unemployment insurance (UI) fund is voluntary and, hence, the proportion of welfare recipients in Finland with good labor market prospects can be expected to be considerably higher than in Germany.

employment proof neither statistically nor economically significant.<sup>13</sup> The positive impact on unsubsidized employment turns out to be larger if the sanction is imposed earlier within the period of benefit receipt.

Using an uniquely combined data set of German administrative and survey data for unemployed in UB II receipt between 2006 and 2007, Boockmann et al. (2014) estimate the effect of benefit sanctions on the transition from welfare receipt to unsubsidized employment. Assessing the potential bias due to sanction endogeneity, Boockmann et al. (2014) employ an instrumental variable regression (with both the reported sanction strategy and the sanction frequency rates of 154 German welfare agencies as instruments) to measure the effectiveness of an intensified sanction regime by means of the Local Average Treatment Effect (LATE). Boockmann et al. (2014) find evidence that benefit sanctions increase the probability to leave UB II receipt for employment within six months after the benefit cut by about 58 percentage points. Based on the results, they support a tighter use of benefit sanctions as it is supposed to increase the probability of leaving welfare dependency towards unsubsidized employment.

A recent study by van den Berg et al. (2014) focuses on the effect of mild and strong sanctions, applied to unemployed young male UB II recipients in Western Germany from the time they first received welfare payments until they took up unsubsidized employment.<sup>14</sup> The data set is limited to an inflow sample into unemployed UB II receipt of 'young adult' men, aged 18 to 24 years, during January 2007 and March 2008.<sup>15</sup> Van den Berg et al. (2014) apply a Timing-of-Events (ToE) approach with two dynamic treatments (mild and strong sanctions); the results indicate that strong (mild) sanctions increase the transition rate from welfare without employment to unsubsidized work by 120% (37%).

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<sup>13</sup>In the studies by Schneider (2008, 2010), unsubsidized employment means jobs with an income that is high enough to leave UB II receipt. As it is not restricted on hours worked, it includes also part-time employment. In contrast, subsidized employment includes regular jobs with supplementary UB II receipt. This implies, also a regular (full-time) job with a low income that not sufficiently covers the minimum subsistence level of the employed and related household members, is defined as subsidized employment.

<sup>14</sup>Contrary to the studies by Schneider (2008, 2010) who define "unsubsidized employment" as a job, which is paid well enough to leave (supplementary) benefit receipt, "regular employment" in the study of van den Berg et al. (2014) does not exclude receiving top-up benefits.

<sup>15</sup>On average, the group of 'young adults' are sanctioned more often and more tightly than older UB II recipients, see Section 4.3.2.

### 4.3 The German welfare regime for employable people and its sanction scheme

Before 2005, the structure of the German unemployment benefit system comprised three main elements: unemployment benefits (UB), also referred to as unemployment insurance benefits (UIB), unemployment aid (UA), also called unemployment assistance (UA), and social assistance (SA). The former (UB/UIB) were not means-tested, the latter two (UA, SA) were both tax-based and means-tested. The 'Hartz IV' law merged the former unemployment aid (UA) and social assistance (SA) for employable people into the unemployment benefit II (UB II). Besides, the former unemployment benefits (UB) became UB I, but with stronger eligibility conditions.<sup>16</sup>

#### 4.3.1 The means-tested unemployment benefit system under Hartz IV

The means-tested UB II provides basic social security for 'needy job-seekers' and their related household members. Technically, every person, who lives in Germany and is between the employable ages of 15 to 64 years and is able to work at least three hours per day, but is not able to cover the substantial needs of their household, satisfies the eligibility criteria for UB II.<sup>17</sup> As UB II is means-tested, recipients and their household members are classified as 'needy' but do not necessarily have to be unemployed.

In contrast to insurance benefit UB I, which is granted individually, the means-tested UB II applies to households, or the so-called 'need units'.<sup>18</sup> A 'need unit', also referred to as a 'need community' (*Bedarfgemeinschaft*), consists of at least one person capable of working. The partner, regardless of their marital status and any children younger than 25 years belong to the 'need unit', as long as they share the same household.<sup>19</sup>

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<sup>16</sup>Social assistance (SA) is still left for needy persons who are neither eligible to UB I nor to UB II.

<sup>17</sup>The eligibility requirements of UB II are codified in the Social Code Book II (SCB II).

<sup>18</sup>Henceforth, 'household' and the official term 'need unit' are used interchangeably.

<sup>19</sup>Persons who live together as a merely flat-sharing community do not belong to the same household in the sense of the SCB II.

The heterogenous group of UB II recipients includes people who are unemployed but not entitled to insurance benefit UB I, or whose UB I or earned income is below the household's subsistence level. Normally, individuals end up in UB II receipt after they have exceeded their maximum period of UB I receipt (in most cases, 6–12 months), and most of them are henceforth classified as long-term unemployed.<sup>20</sup> Another group of UB II recipients is represented by people who did not pay (sufficient) contributions to unemployment insurance (UI), such as former pupils, students, self-employed persons or employees who worked for less than 12 months within the eligibility period of three years (before 2007) or two years (since January 2007), respectively.

In comparison to the former UA, UB II is granted under tightened acceptance regulations. Whereas UA provided protection against loss of job quality and income to a certain extent, UB II recipients are obliged to accept or hold any jobs they are physically, intellectually, and mentally able to. In other words, this ignores their professional experience while also affecting the possibility of future skilled employment.<sup>21</sup>

Key tools of the comprehensive monitoring scheme in Germany are the 'integration contract' (*'Eingliederungsvereinbarung'*) and the appointments of 'personal case managers'. Explicitly, the integration contract specifies the duties of clients with respect to job search activities. It can determine further obligations, e.g., more or less specified commitments to participate in a program of Active Labor Market Policy (ALMP).

### 4.3.2 Welfare sanctions under Hartz IV

In consequence of the paradigm shift towards 'activation policy', with the 'Hartz IV' law a comprehensive monitoring and sanction scheme has been established.<sup>22</sup> Additionally, case managers are encouraged to strictly apply UB II sanctions. While the number of UB II recipients in the last years have decreased from

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<sup>20</sup>As defined in the German Social Code Book III (SCB III), long-term unemployed are persons registered as unemployed at least for one year.

<sup>21</sup>Even employed persons, receiving supplementary UB II (the so-called *'Aufstocker'*) are strictly encouraged to search for additional or better paid jobs in order to reduce their means dependent benefits.

<sup>22</sup>The legal basis of the UB II sanction scheme is regulated in §§31, 31a, 31b, and 32 SCB II.



around 5.3 million people in 2007 to 4.4 million in 2014,<sup>23</sup> the number of imposed sanctions per year increased — after fluctuating around 750,000 from 2007 to 2009, it finally exceeded one million in 2012, where it has remained quite stable until 2014.<sup>24</sup> Apparently, sanctions in form of temporarily benefit cuts — principally lasting three months — have become a crucial instrument in the German welfare policy. This is all the more weighty, as repeated sanctions can swiftly lead to a total loss of UB II, including accomodation benefits.<sup>25</sup>

Recipients of UB II are exposed to sanctions for a broad range of reasons such as insufficient job search effort, refusing to sign an 'integration contract',<sup>26</sup> non-acceptance of job offers or an offer for an integration measure, resigning a job contract, or provoking a dismissal from a job or an integration measure. These failures are considered as *major* 'breaches of duty' and cause a 30% reduction of the base benefit in the first step. Repeated major failures within one year increase the penalty: the second failure is sanctioned with a 60% cut, the third with a total cut of UB II, including housing benefits. Further justifications for sanctions are missing appointments with case managers, or missing medical or psychological treatments. Initaly, these types of non-compliant behavior, classified as *minor* 'breaches of duty', reduce base benefit by 10%, followed by an increase of 10% points for each recurrence. Young UB II recipients, between the ages of 15 to 24 years, are sanctioned even harder. Apart from minor mistakes (missed appointments), already the first failure entails an immediate 100% cut of the base benefit, the second yields a total cut of UB II, including housing benefits.

In fact, unemployed in the last sanction step face the very real risk of homelessness. Hence, it can be expected that such a sanction scheme increases compliance and concessions on the expected job quality, particularly of unemployed who already experienced a sanction.

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<sup>23</sup>These numbers represent the annual average of the monthly stock of employable welfare recipients, namely UB II recipients.

<sup>24</sup>Source: publicly available statistics of the German Federal Employment Agency (FEA).

<sup>25</sup>UB II consists of the base benefit, housing or accomodation costs, and social security contributions.

<sup>26</sup>While refusing to sign an 'integration contract' is no longer a legal justification for imposing a sanction, this was not the case during our observation period (2007 to 2010).

## 4.4 Data

Our analysis is based on a novel German panel survey ‘Labour Market and Social Security’ (PASS).<sup>27</sup> It is an annual household survey in the field of German labor market and welfare state research, conducted at the request of the Institute for Employment Research (IAB), and provided by the Research Data Centre (FDZ) of the IAB.<sup>28</sup> The PASS survey is especially developed and provided for (internal and external) research on UB II and for comparisons between benefit recipients and the total population.

The PASS survey enables us to contribute to previous research on welfare sanctions in the following two points: First, we analyze the effects of sanctions on the transition rates not only to employment but also to non-employment, and second, we consider the impact of being indirectly affected by sanctions caused by other members of the ‘need unit’.<sup>29</sup> Because of these merits of the PASS, we accept the drawbacks of a complex survey design and the associated typically human recall errors, yielding under-reported sanction events.<sup>30</sup>

### 4.4.1 The PASS survey

The PASS study consists of annual panel data on individual and household level as well as several datasets describing the entire employment history of individuals and the episodes of households’ UB II receipt since January 2005. We exploit the first two waves of the survey.<sup>31</sup> For the first wave about 18,954 individuals, belonging to 12,794 households, were interviewed between December 2006 and July 2007. The second wave, conducted between December 2007 and

<sup>27</sup>The abbreviation is based upon the German survey title, *Panel Arbeitsmarkt und Soziale Sicherung* (PASS).

<sup>28</sup>The FDZ (*Forschungsdatenzentrum*) of the IAB provides researchers access to micro data for non-commercial empirical research in the fields of social security and employment.

<sup>29</sup>Concretely, we assume and treat all employable household members as affected by a sanction. However, the low number of exclusively indirectly sanctioned individuals in our sample does not support a proper application of distinct estimations for direct and indirect sanctions separately. The task of disentangling the effects of direct sanctions (applied to the person itself) from the indirect ones (applied to another household member) should be the focus of further research on welfare sanctions.

<sup>30</sup>Furthermore, the conceivable alternative for us as researchers outside of the IAB, to use a Scientific Use File (SUF) that is a 2% random sample of administrative data (the so-called *Sample of Integrated Labour Market Biographies*, or SIAB for short), lacks information on exact sanction periods and the household context. However, this information is crucial for our analysis, and hence, the SIAB is not a suitable alternative for our research target.

<sup>31</sup>An extensive documentation on the first two waves of PASS is provided by Christoph et al. (2008) and Gebhardt et al. (2009).

July 2008, covers 12,487 persons in 8,429 households. Summing up, there are over 10,000 employable individuals in the age of 15 to 64, living in more than 7,300 households, who had been interviewed in both waves.

As the PASS is targeted towards low-income households and unemployed, the survey is structured as follows: There are two sub-samples, the *FEA-sample* which covers households and individuals entitled to UB II, and the *Microm-sample* that covers households and individuals registered as German residents. The latter is a stratified sample where the probability of a low-income (medium-income) household to be interviewed is 4 times (2 times) the probability of a high-income household. Consequently, UB II recipients and low-income earners are disproportionately represented. This is one of the PASS study's great advantages, as this segment of the population is more difficult to reach and follow up over time, and hence normally under-represented in surveys.

Besides unemployment spells, the survey comprises employment spells and — in comparison to administrative data — highly beneficial 'gap spells', recording the periods out of labor force explicitly. The detailed information in the various spell datasets enables us to track households' UB II receipt and individuals' transitions out of unemployment. Both unemployment and employment episodes are reported on a monthly frequency since January 2005. The UB II spells, reported on household level, cover detailed information on imposed sanctions, such as the type of accused violation, the date of the sanction enforcement and its duration. The survey set further comprises information on socio-demographic characteristics like individuals' household structure, labor market status, earned income, and households' net income including any kind of social benefits. Moreover, there are several subjective indicators like employment orientation and experienced social status.

#### 4.4.2 Sample selection

Our analysis covers the calendar years of 2005 to 2007. We select all individuals between 15 and 64 years that were interviewed in both of the first two waves that entered unemployed UB II receipt within the observation period.

As the spells of UB II receipt are recorded on household level, the information on imposed sanctions is also reported on household level. Even though it is possible to attribute sanctions to household members who cause it, we consider all household members as affected by sanctions. Hence, from the moment the

first sanction is imposed, we classify all employable household members as sanctioned. This appears reasonable, as UB II receipt applies to households, and thus, the entire household is exposed to the budget cut.

### 4.4.3 Descriptive statistics

Our final sample consists of 3,996 unemployment spells, whereas 742 end with a transition into employment, 601 with a transition out of labor force, and 2,653 are right censored, i.e. the persons remained unemployed until December 2007. The final sample records 3,599 unemployed persons from 15 to 64 years, who had received UB II at least for one month in the respective period from January 2005 to December 2007. 391 of them (that is 10.86%) had been sanctioned.

TABLE 4.1: Sanction rates of selected PASS data (2005–2007)

Sex/Age group	Individuals	Sanction rate <sup>1</sup>
All	3, 599	10.86
Men	1, 533	11.29
Women	2, 066	10.55
15–24 years	605	12.56
25–49 years	2, 067	11.66
50–64 years	927	7.98

Source: Own calculations based on selected data of the PASS survey. <sup>1</sup>Percentage sanction rates, calculated as share of sanctioned unemployed UB II recipients in the period between January 2005 and December 2007.

Table 4.1 depicts the ratios of sanctioned unemployed UB II recipients who had been affected by at least one sanction between January 2005 and December 2007 in relation to all unemployed people who received UB II at least for one month within this period.<sup>32</sup>

The sanction rate of ‘young adults’ (15–24 years) is with 12.56% considerably higher, whereas the sanction rate of persons above 50 years is with 7.98% considerably lower than the total sample average of 10.86%.

Table 4.2 provides summary statistics of the basic explanatory variables of our final sample, differentiated according to persons with or without a sanction,

<sup>32</sup>The sanction rates depicted in our study are different from others, especially from administrative data. First, they depend on the observation period: the longer considered unemployment episodes last, the longer unemployed are at risk to be sanctioned, and hence are more likely to be sanctioned within the observation period. Second, the official sanction quotas, reported by the FEA, are based on the share of *currently* sanctioned persons within a month. In contrast, we consider a person as sanctioned *beyond* the sanction period.

drawn from individual data (PANEL) and spell properties (SPELL). As the survey starts in 2005, it lacks sufficient information on previous employment states. Therefore, we decide to refrain from capturing state dependence by explicit control variables but approach capturing by means of unobserved heterogeneity terms.

At first glance, the mean values in Table 4.2 reveal a fairly homogenous picture between sanctioned and non-sanctioned unemployed. In both groups, men and women are equally represented. Negligibly but still significant more non-sanctioned UB II recipients live in eastern Germany. From the continuous variable *age* we derive three age-group dummies, whereby *age24-* contains all unemployed individuals with an age between 15 and 24 years. Correspondingly, *age50+* takes the value one for unemployed that are between 50 and 64 years old. To non-sanctioned unemployed, UB II recipients with a sanction are, on average, with 38 years about 2 years younger, have with 20.1% (19.9%) a higher (lower) proportion of individuals younger (older) than 25 (49) years and rather live without a partner in the same household. The share of the two age cohorts (*age24-* and *age50+*) in either group reflects legal regulations and common practice of sanction enforcement: Case managers are explicitly obliged to sanction young adults below 25 years more strongly, whereas persons above 50 years are treated less strictly, yielding a share of elder UB II recipients (29.3% for *age50+*) that exceeds the share of the younger (15.2% for *age24-*).

Households with children below the age of six (*child6*) account for a similar part of around 20% in both groups. With respect to the (vocational) qualification level, we differentiate between three skill groups. The levels *high skilled* refers to unemployed holding a university degree, *med skilled* comprises individuals with a secondary or high school certification or any type of successfully accomplished apprenticeship. The remaining fraction of unemployed without any degree serves as a reference (*low-skilled*). The dummy variable *migrated* indicates whether or not UB II recipients have an immigrant background, meaning that they either migrated themselves (first generation), or they have at least one parent who migrated (second generation).

The PASS survey, furthermore, provides information about general attitudes to work. The dummies *non-monetary*, *monetary* and *social* indicate, whether a specific motivation is crucial for the person. The answers are not mutually exclusive, and individuals may report more than one (or none) of the three inquired working motives as important. On average, the share of UB II recipients that

TABLE 4.2: Summary statistics of selected variables<sup>1</sup>

Variable	Non-sanctioned	Sanctioned
PANEL DATA		
woman	0.576	0.564
east***	0.399	0.364
age***	40.28 (0.032)	37.91 (0.088)
age24-**	0.152	0.201
age50+***	0.293	0.199
couple***	0.311	0.262
child6	0.188	0.201
med skilled	0.595	0.561
high skilled	0.081	0.084
migrated*	0.267	0.226
non-monetary	0.800	0.816
monetary	0.534	0.511
social**	0.887	0.869
SPELL DATA		
exit to employment**	0.109	0.130
exit to non-employment	0.098	0.094
d4-6***	0.117	0.111
d7-12***	0.210	0.208
d13-36***	0.546	0.565

Source: Own calculations based on selected data of the PASS survey. <sup>1</sup>Means are calculated over 93913 person months of unemployed UB II receipt within January 2005 and December 2007, comprising 3996 UB II spells, 3586 non-sanctioned and 410 sanctioned persons. Standard deviations are given in parantheses. Two-sided mean comparison tests (t-tests) give significance levels of \*10%, \*\* 5%, \*\*\*1%. Current unemployment durations (measured in months) are represented by the dummies *d4-6*, *d7-12*, and *d13-36*.

evaluate working as important in order to participate in society (*social*) is with 86.9% about 1.8% points significantly lower for sanctioned than non-sanctioned UB II recipients.

SPELL data provide a first impression about the probable effect of benefit sanctions on employment and leaving the labor market. Apparently, a higher share (13.0%) of sanctioned unemployed exit to employment compared to the non-sanctioned group (10.9%). Concerning unemployment duration, half of the UB II recipients in both groups come up with a duration of more than a year. In general, the share increases with duration and remains insignificantly different in means between the two groups.

## 4.5 Estimation approach

In this paper, we examine the effects of sanctions on the transition rates of unemployed UB II recipients into employment or non-employment. In particular, we focus on the effect after the imposition of a benefit sanction, commonly referred to as ex-post effect.<sup>33</sup> For our analysis, we set up a model that accounts for individual's unemployment duration dependence. From the beginning of each unemployment spell, the individuals are at risk to switch to one of the two probable states in time  $T$ : become employed ( $e$ ) or exit the labor market and enter non-employment ( $ne$ ). If neither occurs, the individual remains unemployed and the respective spell is classified as censored ( $c = 0$ ). Let  $t_e$  be the corresponding duration until exiting unemployment for a job, and  $t_{ne}$  be the time until the unemployed leaves the labor market.

For each period of unemployment, we observe the point in time,  $T_s$ , of a sanction enforcement and the respective time,  $t_s$ , until the individual experiences their first sanction.<sup>34</sup> Even though our final sample is already restricted to unemployed UB II recipients, there are still numerous observed and unobserved components, causing a non-negligible correlation between the probability of a sanction and unemployment duration. As a consequence, we cannot treat the effect of a sanction and, in particular, the time until a sanction  $t_s$  as exogenous.

In order to disentangle the effects of an unemployment benefit sanction from other observable or unobservable factors influencing the exit from unemployment, Abbring and van den Berg (2003a,b) developed the Timing-of-Events (ToE) approach, enabling a causal identification of dynamic treatment effects of imposed sanctions on the exit hazard of unemployed. The elaborate technique reveals the causal from the selective effect of an imposed benefit sanction on unemployment duration.

To analyze the duration  $t_o$  with  $o \in \{e, ne\}$  until the point of transition in  $T_o$ , we employ a discrete Mixed Proportional Hazard (MPH) framework. The exit rate to either destinations  $o \in \{e, ne\}$ , conditioned on the months elapsed until the

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<sup>33</sup>After a sanction is imposed, a mixture of ex-ante and ex-post effects occur. As people are both backward-looking and forward-looking, ex-ante effects caused by the threat of recurrent sanctions affect the outflow behavior of UB II recipients. Sticking to terms, the effect after a sanction is labeled as ex-post effect in the literature, see Lalive et al. (2005) and Arni et al. (2013).

<sup>34</sup>It is a common approach in the literature to evaluate the effect of the first sanction solely, see van den Berg et al. (2004), Abbring et al. (2005), Lalive et al. (2005) and Svarer (2010).

sanction enforcement  $t_s$ , is given by:

$$\theta_o(t_o|x, v_o, t_s) = \lambda_o(t_o) \exp[x' \beta_o + \delta I(t_s < t_o) + v_o], \quad (4.1)$$

where  $\lambda_o(t)$  represents the baseline hazard (duration  $t$  until exit to state  $o$ ).  $x$  is a vector of observables, describing individual characteristics and controlling for local labor market conditions. The dummy variable  $I(t_s < t)$  indicates whether a sanction has been enforced during the unemployment spell. Hence,  $I(\cdot)$  takes the value one if the time interval until a sanction has been imposed  $t_s$  is shorter than the interval until exit  $t_o$  or shorter than the entire unemployment spell in case of a censored record.  $v$  is a random term, controlling for the unobserved components presumably affecting the hazard rates. The corresponding conditional density function of  $\theta_o(t_o|x, v_o, t_s)$  is

$$f_o(t_o|x, v_o, t_s) = \theta_o(t_o|x, v_o, t_s) \exp\left(-\int_0^{t_o} \lambda_o(\tau|x, v_o, t_s) d\tau\right). \quad (4.2)$$

As unemployment duration is measured in months, we specify a discrete MPH for both probable states  $o \in \{e, ne\}$  and adopt the common flexible piecewise-constant step function approximating the duration dependence of the baseline hazard

$$\lambda_o(t_o) = \exp\left[\sum_k \lambda_{o,k} D_k(t_o)\right] \quad (4.3)$$

for  $k = 1, \dots, 4$  fixed time intervals.  $D_k(t_o)$  denotes time-varying dummy variables equal to one in the corresponding interval and  $\lambda_{o,k}$  the estimated parameters for the specific interval  $k$ . According to the distribution of the unemployment duration, we define the following intervals (in months): [0–3]; (3–6]; (6–12]; (12–36]. We set  $\lambda_{o,1} = 0$  for the first time dummy ( $k = 1$ ) to avoid collinearity in an estimation with a constant term.

Again, the probability of a sanction during the receipt of UB II is likely to be endogenous. Unemployed that do not comply with entitlement requirements or do not behave according to compliance commitments are at risk to experience a sanction. Here we can expect that this type of behavior, in turn, affects the unemployment duration of the individuals, entailing a correlation between the unobserved components of the two processes. Hence, both the hazard of being sanctioned and the hazard of exiting unemployment to one of the two states



$e, ne$  must be estimated jointly.<sup>35</sup>

Similar to the unemployment exit hazard, also the hazard rate of being sanctioned  $\theta_s(t|x, v)$  is assumed to follow a MPH specification

$$\theta_s(t_s|x, v_s) = \lambda_s(t_s) \exp[x' \beta_s + v_s], \quad (4.4)$$

with  $\lambda_s(t_s)$  as duration dependence. For a parsimonious but flexible estimation, we specify  $\lambda_s(t_s)$  as a quadratic function of log-time. The respective conditional density of  $t_s|x, v_s$  is

$$f_s(t_s|x, v_s) = \lambda_s(t_s|x, v_s) \exp\left(-\int_0^{t_s} \lambda_s(\tau|x, v_s) d\tau\right). \quad (4.5)$$

Based on the modeling framework so far, the joint distribution of the processes  $t_o|t_s, x, v_o$  and  $t_s|x, v_s$  can be fully described by the proposed Mixed Proportional Hazard (MPH) specification. Thus, the hazard of the latent failure (either unemployment exit or the hazard being sanctioned) depends on the duration  $t_o, t_s$  until this event occurs in  $T_o, T_s$ , on the observable characteristics comprised by  $x$ , and the unobservable components in  $v_o, v_s$  capturing the unobserved heterogeneity that is assumed to be gamma distributed. The MPH model allows for the simultaneous modeling of the two failures  $T_o, T_s$ . To ensure that the MPH framework is applied appropriately, we verify that the following assumptions have been met. Controlling for  $x$  and  $v$ , we ensure that the shape of the hazard of an unemployment exit  $\theta_o$  is not influenced by the hazard of a sanction unless a sanction occurs in  $T_s$  implying  $\theta_o|t_s, x, v_o$  for  $t_o > t_s$ .

Unemployed in Germany are warned about the possibility of sanctions in case of non-compliant behavior, immediately after they have entered unemployment. These instructions about legal consequences are constantly repeated with every official letter that includes any request or invitation to the benefit recipient. Such permanent warnings, as well as explicit warnings of case managers who assess non-compliant behavior, can already cause so-called ex-ante effects.<sup>36</sup>

<sup>35</sup>Here, one may argue that a MPH analysis with the exit to employment and non-employment as two competing risk should have been applied instead of treating the two processes independently. However, due to the limited number of surveyed individuals in our data, we run into convergence problems of the likelihood function.

<sup>36</sup>The effects of (explicit) warnings are commonly referred to as ex-ante effects in the literature, see Lalive et al. (2005) and Arni et al. (2013). As outlined in Section 4.1, there are less than a handful of empirical studies analyzing the ex-ante effects of explicit warnings — they do indeed provide significant evidence of these effects.

But our study focusses on the ex-post effects of sanctions. Nevertheless, we might expect a moderate change in behavior, immediately before a sanction is imposed, as the unemployed could expect that a sanction is going to be applied if she or he does not behave according to the compliance commitments. However, whether sanctions indeed are enforced, depends primarily on the case managers and how strict they follow the sanction regulations and whether they are willing to accept possible reasons that could justify the seemingly non-compliant behavior. Boockmann et al. (2014) find that the probability to be sanctioned varies considerably across welfare agencies, according to their sanction policies which depend on the region, the entire economic situation that makes it either more or less difficult to find a job, regardless of the search intensity and the willingness to accept worse job conditions, and probably on the attitudes of the chief officers. Altogether, it is very difficult for the unemployed to assess whether they will be sanctioned, and additionally, they do not know the exact point in time,  $T_s$ , at which a possible sanction will be imposed. Following the argumentation of Abbring and van den Berg (2003a,b), we assume that the so-called no-anticipation assumption is satisfied. This assumption is important for our analysis in order to guarantee that individuals do not change their behavior before the treatment occurs.

Moreover, it is assumed that the unobserved heterogeneity is independent from the time-varying covariates in  $x$ . The independency and no-anticipation assumption ensures that the causal effect of a specific treatment on the hazard of exiting unemployment is identified by a MPH framework, hence conditional on the observed explanatory variables in  $x$  and the unobserved heterogeneity  $v_o$  and  $v_s$ . Therefore, selectivity is captured by the correlation between those two unobserved heterogeneity components  $v_o$  and  $v_s$ .

## 4.6 Results

For the analysis, we focus on two main hazard specifications: one for the exit to employment  $\theta_e$ , the other for the exit to non-employment  $\theta_{ne}$ . To avoid bias potentially arising from endogeneity of the sanction treatment, we model the duration until the sanction imposition as endogenous. All models are specified

as discrete MPH models,<sup>37</sup> where hazards for both  $\theta_e$  and  $\theta_{ne}$  are estimated simultaneously.<sup>38</sup>

For our baseline models (Specification I) in Subsection 4.6.1, we assume the effect of a sanction as constant across the sample population. The impact of a sanction enters the unemployment hazard equation as a time-varying dummy variable  $\delta$ , being 1 in  $t$  if a sanction already has been imposed, and zero otherwise. Besides  $\delta$ , all models include a basic set of explanatory variables reflecting individual socio-economic characteristics, working motives and, to approximately capture general labor market conditions, a set of dummy variables for each federal state and the respective unemployment rate ( $uq$ ). For the sensitivity analysis in Subsection 4.6.2, we allow the effect of a sanction to vary across the sample population. Hence, the expanded models (Specification II) let  $\delta$  interact with selected explanatory variables used before, and outlined in Table 4.2 of Section 4.4.

Finally, Submodels (a) and (b) differ with respect to the specification of the baseline hazard. Submodel (a) assume a log-linear combined with a log-quadratic impact of unemployment duration on the unemployment exit hazard ( $\theta_e, \theta_{ne}$ ).<sup>39</sup> In contrast, Submodel (b) impose a piecewise-constant duration dependence as a more flexible approach in explaining how different unemployment periods might affect the exit to employment or non-employment.

### 4.6.1 Baseline models

The results in Table 4.3 provide significant evidence of a positive impact ( $\delta$ ) of benefit sanctions on employment entry for Submodels (a) and (b). We find that sanctions enhance the transition to employment by 70% for the log-quadratic

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<sup>37</sup>The episodes of (un-)employment are reported on a monthly frequency on a short observation period, so we use discrete MPH models.

<sup>38</sup>As mentioned in Section 4.5, we estimate the two processes as independent due to convergence problems of the likelihood function in a competing risk specification that otherwise would have been preferable.

<sup>39</sup>Although, the model is applied to discrete data, we estimate the parameters for a constant log-linear and log-quadratic impact of unemployment duration on the outflow hazard.

baseline hazard (a), and by 68% for the flexible piecewise-constant duration dependence (b).<sup>40</sup> Our results for the employment hazard are in line with the majority of previous German and other European studies that predominantly find positive effects of benefit sanctions on employment entry for UIB and welfare recipients.<sup>41</sup>

It is worth emphasizing that the recent studies by far do not reveal the entire picture of the impact of, in particular, welfare sanctions as most studies focus on unemployment insurance benefit (UIB) sanctions. One potentially adverse effect of sanctions upon an increase in the exit rates from labor force is empirically found and presented in Table 4.3. We obtain strongly positive and significant evidence of benefit sanctions on the hazard out of labor force. Sanctions increase the transition rate to non-employment by 60% for the log-quadratic specification (Submodel (a)) and by considerable 79% for the piecewise-constant specification (Submodel (b)) of the baseline hazard. Hence, the estimated effects of benefit sanctions on exit from labor force, which, for UIB recipients in Switzerland and Germany were found by Arni et al. (2013) and Hofmann (2012), respectively, are also confirmed by this study for employable welfare recipients (i.e. UB II recipients) in Germany.

Apparently, there are two groups of UB II recipients which respond to sanctions differently: after a benefit sanction, one group reacts with a successful job search, to some extent by accepting worse employment conditions or/and by increasing the general search effort for jobs, whereas the other group becomes increasingly prone to exit the labor force, possibly driven by an increased search effort for alternatives to welfare receipt and employment<sup>42</sup>

The negative log-quadratic term of unemployment duration in the Model Ia in Table 4.3 reveal a non-linear relation between unemployment duration and the

<sup>40</sup>For the estimation procedure we use the program *Sabre*. Besides others, *Sabre* has been developed for estimation of multivariate generalized linear mixed models, especially applied to discrete data and small data samples. One shortcoming is that the procedure does not report the estimated mass points for unobserved heterogeneity.

<sup>41</sup>Well-known recent European studies on UIB recipients — mainly finding positive effects of benefit sanctions on employment entry — are provided by van den Berg and Vikström (2014) for Sweden, by Busk (2014) for Finland, by Arni et al. (2013) for Switzerland, and by Hofmann (2012) for Germany (see Section 4.1). Most recent studies on welfare recipients — also finding positive effects of sanctions on employment entry — are provided by van der Klaauw and van Ours (2013) for the Netherlands, by Busk (2014) for Finland, and by Boockmann et al. (2014) and van den Berg et al. (2014) for Germany (see Section 4.2).

<sup>42</sup>As mentioned in Section 4.1, such alternatives include: living on the income of relatives and/or friends, student's assistance, disability pension, early retirement pay, illegal work or even criminal activity (Ames (2009), Götz et al. (2010), Machin and Marie (2004), Røed and Westlie (2012), Schreyer et al. (2012), and Wolff (2014)).

TABLE 4.3: Baseline models: exit equations ( $\theta_e$  and  $\theta_{ne}$ )

Variable	Employment $\theta_e$				Non-employment $\theta_{ne}$			
	Model Ia		Model Ib		Model Ia		Model Ib	
	coef	z-stat	coef	z-stat	coef	z-stat	coef	z-stat
$\delta$	0.528	2.45	0.520	3.75	0.469	2.04	0.583	3.44
lnt	0.285	1.69			0.972	4.01		
lnt <sup>2</sup>	-0.121	-2.81			-0.237	-3.91		
d4–6			3.755	25.96			4.122	20.76
d7–12			2.692	19.74			3.438	19.75
d13–36			1.396	12.25			1.978	12.54
women	-0.591	-5.43	-0.454	-5.59	0.164	1.56	0.196	2.04
med skilled	0.613	4.65	0.394	3.65	0.341	2.66	0.050	0.45
high skilled	0.794	4.31	0.471	3.04	0.186	0.85	-0.175	-0.87
age24–	-0.540	-2.93	-0.698	-4.33	1.462	6.73	0.988	7.75
age50+	-1.168	-7.47	-0.751	-6.44	-0.031	-0.25	0.318	2.71
couple	-0.039	-0.36	-0.139	-1.48	0.840	6.08	0.603	5.88
child6	-0.338	-2.60	-0.186	-1.71	-0.262	-1.97	-0.093	-0.76
migrated	-0.083	-0.72	-0.024	-0.23	-0.220	-1.72	-0.158	-1.38
uq	-0.193	-6.69	-0.096	-5.06	-0.147	-5.00	-0.073	-3.23
non-monetary	0.366	2.70	0.280	2.39	-0.213	-1.65	-0.232	-1.98
monetary	-0.122	-1.31	-0.055	-0.67	-0.094	-0.91	-0.037	-0.40
social	0.021	0.14	0.089	0.68	0.257	1.51	0.272	1.76
regional dummies	yes		yes		yes		yes	
unobs. heterogen. <sup>1</sup>	yes		yes		yes		yes	

<sup>1</sup>Mass points for the terms of unobserved heterogeneity are estimated but not reported by *Sabre*, the program we used for the estimation procedure.

hazard to leave UB II receipt for employment  $\theta_e$  respective non-employment  $\theta_{ne}$ . Putting it differently, after a certain spell length, the probability of finding a job or leaving the labor market declines with ongoing UB II receipt.

Imposing the unemployment duration dependence as a flexible piecewise constant baseline function (Model Ib) in terms of four intervals ( $[0 - 4)$ ;  $[4 - 7)$ ;  $[7 - 13)$ ;  $[13 - 37)$ , in months) brings up positive and significant estimates for all three intervals (given  $[0 - 4)$ -interval as reference group). This holds for both hazards  $\theta_e$  and  $\theta_{ne}$ . The estimated coefficients are positive and significant but decline in the magnitude of their impact conditional on unemployment duration. In light of the inverse u-shaped duration dependence in the Model Ia for  $\theta_e$  and  $\theta_{ne}$ , the

impact is supposed to turn negative for shorter interval setting in the end.<sup>43</sup>

TABLE 4.4: Baseline models: sanction equations ( $\theta_s$ )

Variable	Employment ( $e$ )				Non-employment ( $ne$ )			
	Model Ia		Model Ib		Model Ia		Model Ib	
	coef	z-stat	coef	z-stat	coef	z-stat	coef	z-stat
lnt	-0.351	-1.58	-0.350	-1.57	-0.350	-1.58	-0.334	-1.49
lnt <sup>2</sup>	0.063	1.10	0.062	1.07	0.062	1.07	0.063	1.08
woman	-0.195	-1.63	-0.190	-1.60	-0.190	-1.59	-0.195	-1.64
med skilled	0.165	1.17	0.170	1.21	0.171	1.21	0.175	1.24
high skilled	0.051	0.20	0.076	0.31	0.077	0.31	0.090	0.36
age24–	0.253	1.37	0.271	1.48	0.272	1.48	0.278	1.52
age50+	-0.493	-3.16	-0.493	-3.18	-0.493	-3.18	-0.502	-3.24
couple	-0.009	-0.07	-0.014	-0.10	-0.013	-0.09	-0.008	-0.06
child6	-0.088	-0.59	-0.083	-0.55	-0.083	-0.55	-0.079	-0.53
migrated	-0.246	-1.65	-0.252	-1.69	-0.252	-1.69	-0.262	-1.77
uq	-0.076	-2.22	-0.076	-2.22	-0.076	-2.22	-0.071	-2.22
Log-Lik	-5551		-5221		-4828		-4519	
cases	3239		3239		3239		3239	
N	150204		150204		150204		150204	

A quick glance through Models Ia and Ib in Table 4.3 reveals the typical impacts of the explanatory variables on unemployment-to-employment hazard  $\theta_e$ . Apart from the direction of the impact, almost all become statistically significant with some variations in the size of the coefficients between Submodels (a) and (b). The variables *migrated* and *couple*, and two variables of work motivation (*monetary* and *social*) turn out to be insignificant. Female, younger and elder UB II recipients, and unemployed UB II recipients in households with children below six years are less likely to enter employment. High- and medium-skilled unemployed, and unemployed reporting they are also motivated to work if they do not require the money (*'non-monetary working motivation'*) have a higher likelihood to leave unemployment for employment. Apart from the sanction coefficient, also the significance of the explanatory variables is robust against continuous and discrete specification of duration dependence.

Considering Models Ia and Ib for the exit hazard to non-employment  $\theta_{ne}$ , the estimated coefficients form a slightly different picture. Compared to the unemployment-to-employment hazard,  $\theta_e$ , the impact of living with a partner in the same

<sup>43</sup>Due to the small sample size, we choose the parameters to estimate parsimoniously.

household (*couple*), being younger than 25 (*age24-*) and older than 49 (*age50+*) for Model Ib positively affects the hazard to non-employment. In other words, younger and elder (for Model Ib) unemployed UB II recipients are more likely to exit the labor market. With respect to duration dependence, we find the similar inverse u-shaped impact as for the exit hazard to employment, implying an increasing probability to remain unemployed after a certain length of the unemployment spell.

Surprisingly, unemployment duration exhibits no significant effect on sanction probability (see Table 4.4). Moreover, people over 50 years of age (*age50+*) and *migrated* persons are less likely to be sanctioned, whereas the remaining factors turn out to be insignificant. Finally, the probability of experience a sanction increases with a declining regional unemployment rate  $uq$ , supporting common practice that job centers pursue a stricter sanction policy in regions with better economic conditions and a lower share of UB II recipients.

#### 4.6.2 Sensitivity analysis

We modify the baseline specification with selected interaction terms to analyze whether sanction effects with respect to age and education vary across different subgroups of the sample population. First, we let the dummy for being sanctioned  $\delta$  interact with either age groups (*24-* and *50+*), and second with two qualification levels (*medium* and *high skilled*).

As shown in Table 4.5, we find strong evidence for a positive sanction effect on the exit hazard to employment  $\theta_e$ . Considering interaction terms for the age groups, we find the transition to employment to be positively influenced by sanctions for either age cohorts. Apparently, younger than 25 or elder than 49 years old UB II recipients, affected by a sanction are more likely to enter employment, whereas in general these age groups are associated with a lower transition probability.

The interaction with qualification levels in the Models IIa and IIb reveal a slightly changed picture as the impact of sanctions on the age group of ‘young adults’ (*24-*) becomes insignificant. For older unemployed UB II recipients, the transition rate to employment remains positively affected by sanction enforcements. The general positive effect of education on the transition probability to employment becomes insignificant for high-skilled and for medium-skilled unemployed in Model IIb. On average, the transition probability of high-skilled

TABLE 4.5: Exit to employment ( $e$ ): exit equation ( $\theta_e$ )

Variable	2 Interaction terms				4 Interaction terms			
	Model IIa		Model IIb		Model IIa		Model IIb	
	coef	z-stat	coef	z-stat	coef	z-stat	coef	z-stat
$\delta^*$ med					0.396	1.97	0.296	1.63
$\delta^*$ high					-0.105	-0.20	0.285	0.60
$\delta^*$ age24–	0.834	1.79	1.097	2.51	0.733	1.56	1.010	2.29
$\delta^*$ age50+	1.114	2.91	0.957	2.72	0.852	2.04	0.716	1.89
lnt	0.284	1.69			0.280	1.67		
lnt <sup>2</sup>	-0.123	-2.87			-0.123	-2.88		
d4–6			3.747	25.91			3.754	25.93
d7–12			2.696	19.76			2.696	19.74
d13–36			1.396	12.25			1.394	12.23
women	-0.586	-5.56	-0.468	-5.77	-0.578	-5.49	-0.458	-5.63
med skilled	0.608	4.77	0.412	3.82	0.576	4.51	0.385	3.52
high skilled	0.794	4.45	0.486	3.14	0.798	4.40	0.465	2.93
age24–	-0.596	-3.16	-0.789	-4.65	-0.586	-3.12	-0.782	-4.60
age50+	-1.229	-7.94	-0.826	-6.82	-1.207	-7.80	-0.807	-6.62
couple	-0.041	-0.39	-0.144	-1.54	-0.037	-0.36	-0.140	-1.48
child6	-0.329	-2.59	-0.190	-1.74	-0.324	-2.55	-0.187	-1.72
migrated	-0.084	-0.75	-0.023	-0.23	-0.081	-0.72	-0.026	-0.25
uq	-0.193	-6.82	-0.100	-5.29	-0.191	-6.76	-0.098	-5.17
non-monetary	0.365	2.74	0.293	2.50	0.356	2.68	0.285	2.42
monetary	-0.118	-1.30	-0.055	-0.67	-0.120	-1.31	-0.053	-0.65
social	-0.001	0.00	0.070	0.53	0.005	0.03	0.079	0.60
regional dummies	yes		yes		yes		yes	
unobs. heterogen. <sup>1</sup>	yes		yes		yes		yes	

<sup>1</sup>Mass points for the terms of unobserved heterogeneity are estimated but not reported by the program *Sabre*.

unemployed seems to be unaffected by sanctions. To sum up, sanction effects do vary in its impact across different age cohorts of the sample population.

Focusing on sanctioned unemployed UB II recipients with regard to their qualification level, the model does not indicate any significant impact of sanctions on high skilled unemployed. For medium qualified persons, Model IIa with the log-quadratic specification (Submodel (a)) indicates a significantly positive effect of sanctions on the transition to employment.



TABLE 4.6: Exit to employment ( $e$ ): sanction equation ( $\theta_s$ )

Variable	2 Interaction terms				4 Interaction terms			
	Model IIa		Model IIb		Model IIa		Model IIb	
	coef	z-stat	coef	z-stat	coef	z-stat	coef	z-stat
lnt	-0.350	-1.57	-0.350	-1.57	-0.350	-1.57	-0.350	-1.57
lnt <sup>2</sup>	0.062	1.07	0.062	1.07	0.062	1.07	0.062	1.07
woman	-0.190	-1.60	-0.190	-1.60	-0.190	-1.60	-0.190	-1.60
med skilled	0.170	1.21	0.170	1.21	0.170	1.21	0.170	1.21
high skilled	0.076	0.31	0.076	0.31	0.076	0.31	0.076	0.31
age24–	0.271	1.48	0.271	1.48	0.271	1.48	0.271	1.48
age50+	-0.493	-3.18	-0.493	-3.18	-0.493	-3.18	-0.493	-3.18
couple	-0.014	-0.10	-0.014	-0.10	-0.014	-0.10	-0.014	-0.10
child6	-0.083	-0.55	-0.083	-0.55	-0.083	-0.55	-0.083	-0.55
migrated	-0.252	-1.69	-0.252	-1.69	-0.252	-1.69	-0.252	-1.69
uq	-0.076	-2.22	-0.076	-2.22	-0.076	-2.22	-0.076	-2.22
regional dummies	yes		yes		yes		yes	
Log-Lik	-5553		-5222		-5551		-5221	
cases	3239		3239		3239		3239	
N	150204		150204		150204		150204	

Concerning the hazard to non-employment in Table 4.7, the results for the medium skilled sanctioned appear robust against the two different baseline hazards. Here, sanctions on medium-skilled unemployed robustly facilitates the transition to non-employment, whereas the insignificant effect of sanctions on high-skilled unemployed resembles the results found for the hazard to employment in Table 4.5.

In summary, sanction effects do not only vary across different age cohorts but also across different qualification levels. The results of a general positive impact of sanctions on transition out of unemployment, as obtained by the baseline models presented in Table 4.3, are only partially verified by the models, controlling for interaction effects. Put differently, even if benefit sanctions on average facilitate the flow out of UB II receipt across the estimation sample, the impact on the behavior within distinct sub-groups may be ambiguous. So far, we find no evidence for a contradicting effect, for example that sanctions on young UB II recipients exhibit a positive impact of the transition to employment, whereas the effect upon older UB II recipients turns out to be negative. In particular, the transition of unemployment to employment or out of the labor

TABLE 4.7: Exit to non-employment ( $ne$ ): exit equation ( $\theta_{ne}$ )

Variable	2 Interaction terms				4 Interaction terms			
	Model IIa		Model IIb		Model IIa		Model IIb	
	coef	z-stat	coef	z-stat	coef	z-stat	coef	z-stat
$\delta^*med$					0.498	1.90	0.526	2.20
$\delta^*high$					-1.161	-1.09	-0.175	-0.17
$\delta^*age24-$	0.445	1.23	0.766	2.39	0.349	0.97	0.654	2.01
$\delta^*age50+$	1.171	3.37	1.037	3.39	0.905	2.24	0.687	1.93
lnt	0.974	4.02			0.968	4.00		
lnt <sup>2</sup>	-0.237	-3.92			-0.238	-3.97		
d4-6			4.119	20.72			4.126	20.73
d7-12			3.442	19.77			3.442	19.75
d13-36			1.975	12.52			1.973	12.49
women	0.162	1.52	0.188	1.95	0.168	1.60	0.201	2.08
med skilled	0.346	2.69	0.055	0.49	0.306	2.39	0.009	0.08
high skilled	0.195	0.89	-0.169	-0.84	0.252	1.15	-0.174	-0.85
age24-	1.442	6.76	0.922	7.04	1.439	7.01	0.931	7.09
age50+	-0.113	-0.87	0.237	1.97	-0.093	-0.72	0.266	2.18
couple	0.847	6.15	0.604	5.89	0.845	6.32	0.608	5.91
child6	-0.267	-1.99	-0.099	-0.81	-0.262	-1.97	-0.090	-0.73
migrated	-0.227	-1.77	-0.150	-1.30	-0.220	-1.73	-0.148	-1.28
uq	-0.150	-5.08	-0.076	-3.39	-0.147	-5.06	-0.074	-3.30
non-monetary	-0.217	-1.68	-0.219	-1.86	-0.226	-1.76	-0.229	-1.95
monetary	-0.090	-0.87	-0.036	-0.38	-0.093	-0.91	-0.034	-0.36
social	0.253	1.48	0.265	1.70	0.254	1.50	0.277	1.78
unobs. heterogen. <sup>1</sup>	yes		yes		yes		yes	

<sup>1</sup>Mass points for the terms of unobserved heterogeneity are estimated but not indicated by the program *Sabre*.

force within different sub-samples of welfare recipients entails different, and probably inconsistent, sanction effects. Unfortunately, the small sample size does not allow a more differentiated analysis.

TABLE 4.8: Exit to non-employment (*ne*): sanction equation ( $\theta_s$ )

Variable	2 Interaction terms				4 Interaction terms			
	Model IIa		Model IIb		Model IIa		Model IIb	
	coef	z-stat	coef	z-stat	coef	z-stat	coef	z-stat
lnt	-0.350	-1.57	-0.350	-1.57	-0.350	-1.57	-0.350	-1.57
lnt <sup>2</sup>	0.062	1.07	0.062	1.07	0.062	1.07	0.062	1.07
woman	-0.190	-1.60	-0.190	-1.60	-0.190	-1.60	-0.190	-1.60
med skilled	0.170	1.21	0.170	1.21	0.170	1.21	0.170	1.21
high skilled	0.076	0.31	0.076	0.31	0.076	0.31	0.076	0.31
age24–	0.271	1.48	0.271	1.48	0.271	1.48	0.271	1.48
age50+	-0.493	-3.18	-0.493	-3.18	-0.493	-3.18	-0.493	-3.18
couple	-0.014	-0.10	-0.014	-0.10	-0.014	-0.10	-0.014	-0.10
child6	-0.083	-0.55	-0.083	-0.55	-0.083	-0.55	-0.083	-0.55
migrated	-0.252	-1.69	-0.252	-1.69	-0.252	-1.69	-0.252	-1.69
uq	-0.076	-2.22	-0.076	-2.22	-0.076	-2.22	-0.076	-2.22
regional dummies	yes		yes		yes		yes	
Log-Lik	-4825		-4516		-4822		-4514	
cases	3239		3239		3239		3239	
N	150204		150204		150204		150204	

## 4.7 Conclusion

In this paper, we have analyzed the impact of benefit sanctions on transition rates from unemployment into two distinct outcomes: employment and non-employment. In contrast to the majority of European studies on benefit sanctions, we focused on employable welfare recipients, in Germany recipients of the UB II, instead of recipients of unemployment insurance benefits. Unlike previous studies — and due to the regulations that UB II is not granted individually but paid to the entire household — we assumed and treated all employable household members of a so-called ‘need unit’ as affected. On average, the labor market perspectives of welfare recipients are worse than for UI recipients, so that leaving benefit receipt for non-employment appears as a more appealing option for them than for UI recipients.

Based on a Mixed Proportional Hazard (MPH) model which treats sanctions as endogenous, we actually identified two distinct effects: unemployed UB II recipients that become affected by a sanction are more likely to enter employment, but are also more likely to leave the labor market, at least temporarily.

With our analysis we provide causal evidence that the positive effect of benefit sanctions on employment entry of welfare recipients is at expense of a likewise increased probability to get them off the labor market. In other words, there are two groups of unemployed welfare recipients that respond to benefit sanctions differently. Whereas one group of sanctioned individuals on average exhibit increasing transition rates to employment, the other group becomes more likely to leave the entire labor force. According to job search theory, the positive effect of benefit sanctions on the transition to employment is supposed to arise from enhancing job search efforts and from accepting worse job conditions. Thus, the increased transition rate to employment might be at expense of job quality in terms of lower wages and lower job stability (Arni et al. (2013)). On the other hand, the increased probability for an exit from labor force is likely driven by an intensified search for alternatives to welfare receipt and employment.

At first glance, the findings of an increased impact on transition out of unemployment coincides with the policy intentions — at least the short-term ones — that predominantly aim to reduce the duration and amount spent on welfare in order to lower both unemployment rates and fiscal costs. Here, welfare policy that aims to push people into employment at any price might be accompanied by a downgrade in occupational skills, unstable employment and low wages, even below the subsistence level. In the long run, the latter potentially leads to the opposite of the policy's intended outcomes — increased durations of (supplementary) welfare receipt for more and more individuals, and hence increased expenditures for welfare payments.

In the end, future research should target the examination of such likely negative effects to obtain a comprehensive evaluation of the impact of benefit sanctions that goes beyond public labor market policy that merely aims to bring people as quickly as possible from benefit receipt into employment.

# Chapter 5

## Welfare Sanctions: Impact on Employment and Benefit Receipt

This chapter is based upon Hohenleitner and Hillmann (2019a), *“Impact of Welfare Sanctions on Employment and Benefit Receipt — Considering Top-Up Benefits and Indirect Sanctions”*, HWWI Research Paper 189.

### 5.1 Introduction

Most of the European countries have restructured their social security system towards shorter periods of eligibility in the unemployment insurance system. As a result, more unemployed people and their families have to rely on means-tested tax-based welfare payments for low-income earners and so-called ‘needy job-seekers’. Despite the fact that an increasing number of people in Europe are either directly affected by the new structured welfare system at some point in their lives, or are at least indirectly affected by the side-effects on the labor market, scientific literature about the effects of the monitoring and sanction systems in the welfare policy of European countries is still scarce and rather selective. With this comprehensive study on the ex-post effects of sanctions against UB-II-recipients in Germany we want to contribute an important step towards filling this gap.

In Germany, the restructuring of the unemployment benefit and welfare system reached its peak in January 2005 with the implementation of the ‘unemployment benefits II’ (UB II, colloquially known as the ‘Hartz IV’ laws) which established the means-tested welfare payments for needy employable people and their related household members, and which brought a huge number of people from unemployment insurance receipt to welfare receipt — during the

implementation of 'Hartz IV' and thereafter. And as the means tested welfare payments are defined to merely cover the minimum existence level, sanctions in the form of punitive benefit cuts, mostly lasting several months, have been criticized more and more often, or at least critically questioned for various reasons and regarding different aspects even, to some extent, in the economic literature and scientific policy advice.<sup>1</sup>

Yet despite the increasing public awareness of the potentially adverse effects of welfare sanctions, the majority of European studies on unemployment benefit sanctions still focus on unemployment insurance (UI) recipients who are usually closer to the labor market, and thus are more likely to take up unsubsidized employment, than employable welfare recipients. The latter are often either long term unemployed, low-income workers or have other restrictions which prevent them from taking up the kind of employment that would bring them out of benefit receipt, for example, caring for children, or for elderly and sick family members. Others are job starters who have just finished school or university; still others may want to re-enter the labor market after a longer period of exclusive family work or a long-lasting disease. So, even if a non-negligible part of them are well-educated, such as recent university graduates, the group of welfare recipients is much more heterogeneous than UI recipients are; and the majority of welfare recipients face stronger obstacles to attaining employment well-paid enough to cover their household's minimum subsistence level than recipients of unemployment insurance benefits (UIB) do, who are predominantly unemployed for less than one year, and usually have worked in regular employment for a longer period beforehand. Therefore it is more than questionable, whether the findings on UIB sanctions, predominantly revealing positive effects on entering employment, are transferable to people receiving welfare benefits.

Meanwhile, a couple of studies on welfare sanctions in Europe have been conducted, often restricted to either small regional entities or specific subgroups, like the Dutch studies on the municipality of Rotterdam (van den Berg et al. (2004), van der Klaauw and van Ours (2013)), and the studies on young male welfare recipients in Western Germany (van den Berg et al. (2014, 2015)). Similar to the literature on UI sanctions, most of the studies on welfare sanctions are focused merely on the transition from unemployment to employment (Boockmann et al. (2014), van den Berg et al. (2014)), although others also consider

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<sup>1</sup>See for example Ames (2009), Kumpmann (2009), Götz et al. (2010), Ehrentraut et al. (2014), Wolff (2014), van den Berg et al. (2015), and van den Berg et al. (2017).

the option to leave the labor market, namely the non-employment option (Busk (2014) for Finland, Hillmann and Hohenleitner (2015) for Germany, and van den Berg et al. (2015) for young men in Western Germany).

More extensive studies on welfare sanctions, each for whole countries, are provided by Schneider (2008, 2010) for Germany and Busk (2014) for Finland. As described, in more detail, in the following Section 5.2, Busk (2014) compares the effects of UI benefit and welfare sanctions in Finland, indeed finding differences in the effects; and Schneider (2008, 2010) provides a very early study on German welfare recipients using survey data, conducted shortly after the implementation of UB II. Although quite comprehensive, distinguishing exits to unsubsidized and subsidized work, and additionally regarding reservation wages and job search effort, Schneider (2008, 2010) finds only the effects on unsubsidized work to be partially positive significant. A reason for the mostly insignificant effect estimations of the remaining outcomes may be the fact that the data survey was conducted in the very early stages of implementing the new welfare system under 'Hartz IV', which took close to a full year to be nearly working as planned. Thus, the survey data was conducted during a time when the monitoring and sanction regime was still under construction and not fully effective.

Therefore, a more recent comprehensive analysis of the impact of welfare sanctions in Germany is still needed. With the present work, we intend to take a crucial step ahead in filling this gap. We use a data set specially designed for this research project by the Research Data Centre (FDZ) of the Institute for Employment Research (IAB), based on a 2% sample of administrative data of the German Federal Employment Agency (FEA), and comprising the years 2004 until 2010. In contrast to most other European studies on benefit and welfare sanctions who, by and large, apply the timing-of-events (ToE) approach, and similar to Schneider (2008, 2010) for welfare sanctions and Hofmann (2012) for UI benefit sanctions, we conduct propensity score matching (PSM) for our analysis. This non-parametric approach, needing no assumptions for a functional form like is necessary for ToE models, seems to be even more appropriate for the purpose of analyzing an extremely heterogeneous population group such as welfare recipients.

Our study is the only that conducts all analysis for the whole population of employable welfare recipients, as well as for various subgroups using distinct categories, e.g. for age, gender, region, and education, the latter indirectly

comprised in a special variable for the individual ‘labor market access’ (LMA). Unique features, compared to other European studies on welfare sanctions, are that our analysis not only comprise unemployed but also employed welfare recipients, in Germany colloquially called ‘*Aufstocker*’, and additionally, employable people indirectly affected by sanctions against their family members, a scenario that we briefly refer to as ‘indirect sanctions’.

Depending on the initial sample, we differentiate between multiple exit events: exit to mere employment versus employment with supplementary welfare receipt, which is defined similarly to unsubsidized versus subsidized employment like Schneider (2008, 2010) differentiates it, and the exit event of leaving welfare receipt, all for initially *unemployed* welfare recipients. The non-employment option of leaving the labor force is indirectly considered by interpreting the divergence between entering mere employment and exiting welfare. For *employed* people receiving top-up benefits — a group of welfare recipients almost neglected by scientific literature — we additionally consider the option of quitting employment for mere welfare receipt. And finally, our analysis of employable people who are *indirectly* affected by sanctions upon their family members also distinguish between initially unemployed and employed welfare recipients with the same corresponding exit events we analyze for the directly sanctioned.

In order to clearly draw the line between what we provide with our study and what are the limitations, the following aspects should be mentioned: sanctions in the form of temporary benefit cuts do not only affect the sanctioned individuals after the sanctions (ex-post effects), they also affect non-treated people receiving welfare or UI benefits, as they are threatened by potential sanctions, which may affect their behavior already before the imposition of a sanction. These effects, still at the individual level, are referred to as ex-ante effects. Furthermore, the perception of the sanction regime of the welfare and benefit system by society as a whole may even cause ex-ante effects at the general level, like possible effects on the market wages of the labor market. The analysis of ex-ante effects on the market as well as on the individual level exceeds our research subject. We limit our research to the individual ex-post effects of welfare sanctions from an economic perspective, with a focus on labor market outcomes. We also refrain from considering other individual ex-post effects like effects on an individual’s income, economic wealth, personal well-being, health state, and many other individual aspects possibly affected by welfare sanctions.



Moreover, we restrict our analysis to the first sanction an individual experiences and do not consider repeated sanctions. This is the common approach, applied by the overwhelming majority of studies on benefit and welfare sanctions; an exception is the study on sanctions against young welfare recipients in Germany by van den Berg et al. (2015) who explicitly disentangle ex-post effects of first and second sanctions. Also as is common in previous studies, we apply binary treatment variables and do not distinguish between different durations or extents of benefit cuts; an exception here is an earlier study on young welfare recipients in Germany by van den Berg et al. (2014) who distinguish between two categories of sanctions: mild and strong.

The remainder of the paper is organized as follows: Section 5.2 gives a brief overview of empirical literature on benefit sanctions in European UIB and welfare systems. Section 5.3 introduces the data sample and describes the treatment, outcome, and control variables. In Section 5.4 we explain the methodological approach. A detailed presentation of our numerical and graphical results is provided in Section 5.5. We critically discuss and assess these results against the background of previous studies considering methodological aspects in Section 5.6. And finally, we conclude our results in Section 5.7.

## 5.2 Literature

We provide a detailed overview of the well-known European empirical literature on sanctions against recipients of unemployment insurance benefits (UIB), *UII sanctions* for short, in our previous paper on benefit sanctions, Hillmann and Hohenleitner (2015). Thus, here, we just mention and briefly summarize these studies. The large majority of empirical studies merely analyze the ex-post effects of sanctions; only a hand full of studies also consider ex-ante effects of sanctions, like Lalive et al. (2005) and Arni et al. (2013), who both use data from several Swiss cantons which enable the authors to distinguish between the effects of warnings and imposed sanctions in order to disentangle ex-ante and ex-post effects of benefit sanctions. Hofmann (2012) and Arni et al. (2013) also give brief overviews of the European literature on benefit sanctions, where Hofmann (2012) explicitly mentions the quasi-experimental and laboratory experimental studies which also consider ex-ante effects. Other European studies on benefit sanctions include Abbring et al. (2005) and Svarer (2010) which use data sets from the Netherlands, and van den Berg and Vikström (2014) which

uses Swedish data, all analyzing the effects of UI sanctions on the transition rate from unemployment to employment. The vast majority of these studies apply the timing-of-events (ToE) approach, mostly using mixed proportional hazard (MPH) models, except from Hofmann (2012) who applies propensity score matching (PSM).

All these European studies on UI sanctions, referred to as benefit sanctions, find more or less positive effects of sanctions on taking up employment. Those who disentangle ex-ante and ex-post effects by analyzing warnings and imposed sanctions separately, such as Lalive et al. (2005) and Arni et al. (2013), find, in addition to positive effects of sanctions, positive effects of warnings upon the transition from unemployment to employment. If taking up regular employment or other employment, like subsidized work, is distinguished, as e.g. by Hofmann (2012), the positive effects on entering regular employment are stronger. And generally, earlier sanctions seem to be more effective than sanctions imposed later in the unemployment period (see for instance van den Berg et al. (2004) and Hofmann (2012)). Moreover, Hofmann (2012) analyzes diverse subgroups of UIB recipients and reveals that the positive sanction effects on taking up regular employment are mainly driven by younger unemployed. Nevertheless, as the findings of sanction effects on recipients of UIB are not necessarily transferable to welfare recipients, we mainly focus on literature about sanctions against welfare recipients.

Empirical literature about the effects of *welfare sanctions* in Europe is still scarce, although since the implementation of unemployment benefits II in 2005, the effects of welfare sanctions against UB-II-recipients in Germany have come more and more into the focus of policy advice and science. In our previous study on welfare sanctions, Hillmann and Hohenleitner (2015), we provide an overview of German and other European studies on welfare sanctions until mid-2015, which we thus mention here only briefly. Another overview, including qualitative surveys on German welfare sanctions, is provided by van den Berg et al. (2014, 2015), both focusing on young welfare recipients in Germany.

Two studies on welfare sanctions in the Netherlands use data about welfare recipients in the municipality of Rotterdam, and both find positive treatment effects on the transition from unemployment to work. The early Dutch study by van den Berg et al. (2004) additionally finds that sanctions at an early stage reduce the probability of long-term unemployment. The more recent Dutch study by van der Klaauw and van Ours (2013) additionally reveals re-employment

bonuses to be an ineffective policy instrument. Both studies apply the ToE approach using mixed proportional hazard (MPH) models.

A more recent study from Finland by Busk (2014) analyzes the effects of unemployment insurance benefit (UIB) and welfare sanctions on employment, on participating in a program of the Active Labor Market Policy (ALMP), and on exiting from labor force. Busk (2014) estimates positive treatment effects of ongoing, i.e. currently executed, sanctions upon UIB and welfare recipients, and of completed sanctions upon welfare recipients on their probability to take up employment. The sanction effects on participating in a measure of the ALMP are slightly positive for welfare recipients, and not significant for UIP recipients. Finally, she finds the exit from labor force positively affected by both UIB and welfare sanctions.

The first study about welfare sanctions in Germany after the implementation of unemployment benefits II is provided by Schneider (2008, 2010) who uses cross-sectional survey data, conducted shortly after the implementation of UB II. Applying propensity score matching (PSM), she analyzes the effects of sanctions against unemployed UB-II-recipients on employment, reservation wages, and job search effort. She finds merely unsubsidized<sup>2</sup> employment as partially positive affected, while the other outcomes turn out to be not significantly affected. She also finds earlier sanctions to be more effective in terms of unsubsidized employment than sanctions later in the unemployment spell.

More current German studies on welfare sanctions are provided by Boockmann et al. (2014), Hillmann and Hohenleitner (2015), and van den Berg et al. (2014, 2015). Boockmann et al. (2014) apply an instrumental variable (IV) regression in order to estimate the effect of welfare sanctions on the transition from unemployed UB-II-receipt to unsubsidized employment. Using a unique combined data set of German administrative and survey data, they find positive effects of welfare sanctions on taking up employment without supplementary welfare receipt. Another German study, provided by Hillmann and Hohenleitner (2015), uses a rich panel survey employing the timing-of-events (ToE) approach with mixed proportional hazard (MPH) models; it reveals positive effects of German welfare sanctions on employment entry as well as on leaving the labor force.

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<sup>2</sup>Schneider (2008, 2010) defines 'unsubsidized employment' as jobs with an income that is high enough to leave UB II receipt; it may include also part-time employment. In contrast, 'subsidized employment' may include regular jobs with supplementary UB II receipt.

The two German studies by van den Berg et al. (2014, 2015) focus on the special situation of young UB-II-recipients who are sanctioned more severely and more frequently. Both investigations apply the ToE approach and use administrative data, analyzing the effects of sanctions against male unemployed, aged under 25 years, and living in Western Germany. The restriction to this sub-group of young welfare recipients is chosen in order to get a preferably homogeneous group for the analysis. While the study of van den Berg et al. (2014) is the first which distinguishes between mild and strong sanctions,<sup>3</sup> van den Berg et al. (2015) are the first who do not only analyze the effects of first sanctions, but also of second sanctions, considering only strong sanctions. Van den Berg et al. (2014) find a positive impact of mild and strong sanctions against young welfare recipients on their hazard rate to unsubsidized work,<sup>4</sup> whereby the effect is larger for strong sanctions. They further reveal that part of the sanction effect is caused by the expectation of intensified monitoring. In contrast to previous findings, mostly on UI sanctions, which identify earlier sanctions as more effective, the authors of this study do not find sanction effects dependent on the moment of imposition during the welfare spell.

The later study of the authors, van den Berg et al. (2015), analyzing first and second sanctions against young male welfare recipients in Western Germany, additionally considers the non-employment option, namely the possibility of leaving the labor market. Furthermore separate models are estimated for people living alone and people living in multi-person households, as the latter ones may rely on other household member's income and thus might react less sensitive on sanctions. The authors find the employment effect of first sanctions most effective for single persons but still strongly effective for young men in multi-person households. Also second sanctions raise the exit rates into employment for young men in single households. For those living in multi-person households the second sanction was not significantly affecting the employment entry. Concerning the other exit-option, out of the labor force, van den Berg et al. (2015) find strong effects of the first and second sanction against young male

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<sup>3</sup>Mild sanctions are imposed for missing an appointment and amount to a 10% benefit cut for three months in the first instance; strong sanctions are imposed for all other breaches of duty and, for welfare recipients younger than 25 years, result in a 100% cut of the base benefit from the very first failure. More detailed information of the sanction regime applied to German UB-II-recipients are given, for example by Hillmann and Hohenleitner (2015) and van den Berg et al. (2014, 2015).

<sup>4</sup>Unlike Schneider (2008, 2010) who defines unsubsidized employment as jobs which pay enough to leave (supplementary) welfare receipt, in the study of van den Berg et al. (2014) 'unsubsidized employment' does not exclude receiving top-up benefits.

unemployed living alone, but no significant effects on those living in multi-person households. Moreover, van den Berg et al. (2015) use the initial daily wage as an indicator of job match quality in order to identify possible adverse post-unemployment effects. And indeed, they find the positive employment effects accompanied by reduced wages. This implies that sanctions in the form of benefit cuts reduce the reservation wages of the treated.

### **5.3 Data**

The data set we use for our analysis is based on an extract of the “Sample of Integrated Labour Market Biographies” (SIAB) supplemented by selected information from administrative data of the German Federal Employment Agency (FEA). The SIAB is a 2% random sample drawn from the “Integrated Employment Biographies” (IEB) of the IAB; the IEB comprises all individuals in Germany who are either employed or benefit recipients according to the German Social Code III or II (SC III since 1975, SC II since 2005) and who are officially registered as job-seekers with the German FEA or participants in programs of active labor market policies (ALMP) (in the data since 2000) at least once during the observation period. These data, which come from different sources, are merged in the IEB, where the labor market status is given on a daily base.<sup>5</sup>

Our data set is assembled and prepared by the Research Data Centre (FDZ) of the Institute for Employment Research (IAB) at the German FEA especially for this research project. It is based on selected variables of the SIAB over the complete years from 2004 to 2010, supplemented by selected information about sanctions and household members obtained from process-produced data of the FEA’s administrative sources. This combined data set, exclusively prepared and provided to us for this research project, comprises 978,459 observations in the form of ‘spell data’ (episodes of several employment status) for 223,725 individuals each having received Unemployment Benefits II (UB II) at least once in the observation period.

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<sup>5</sup>See vom Berge et al. (2013).

### 5.3.1 Samples and subsamples

For our analysis, we use two annual inflow cohorts — i.e. cohorts who come into UB II receipt in 2007 and 2008, each restricted to *employable* people in the age of 15 to 56 years. As we have a huge amount of results to present and as the findings of both years do not substantially differ, we present the results of the more current inflow cohort of 2008 in Section 5.5 and use the findings of the inflow sample of 2007 just as a kind of robustness check. The inflow samples are divided into two kinds of inflow status: *employed* people receiving supplementary UB II and *unemployed* UB-II-recipients. In doing so, the employment status on the day of entering welfare receipt is clearly defined. Moreover, we conduct all analysis separately for men and women.

In addition to the two main groups of unemployed and employed welfare recipients, we analyze several subgroups. Firstly, we differentiate between the following age groups: people aged between 15 and 25 years, because people younger than 25 (*under 25: u25*) face stricter monitoring and sanction conditions, and people aged 25 to 56 (*over 25: o25*). Secondly, we distinguish different places of residence: people from a federal state belonging to the former *Western Germany* (WG) or to the former *Eastern Germany* (EG). And finally, we analyze people with different levels of labor market access: *low*, *middle*, and *high*. This variable is based on the classification of the German FEA and the Jobcenters dividing their clients into so-called “market clients” (*Marktkunden*) with a *high* level of labor market access, “counseling clients” or “advisory clients” (*Beratungskunden*) with a *medium* level of access to the labor market, and “guided clients” (*Betreuungskunden*) with a *low* level of market access who are supposed to need support or even guidance in order to be able to take up employment. As this variable of the clients’ classification is often missing in the original data set, we estimate the missing values using further characteristics correlating with labor market access like (school and occupational) education and previous periods of (un-)employment.

### 5.3.2 Treatment variables

We carry out the whole analysis with two kinds of treatment variables: direct and indirect sanctions which indicate punitive cuts of unemployment benefits II that are imposed either directly against the UB-II-recipient or indirectly against

a related household member. Such benefit reductions start at 10%-cuts for minor failures (being late or missing an appointment) and 30%-cuts for major failures (all other state of affairs causing a sanction), each calculated as percentage points of the base benefit, increasing for repeated failures of the same kind until 100% of the UB II — including costs for accommodation and health insurance — is cut, with such cuts typically lasting for three months. In order to exclude statistical outliers with very short benefit cuts, and following Hofmann (2012), we ignore benefit cuts that last for seven days or less. As we neither have information about the amount of benefit cut nor about the reason for the sanction, we cannot distinguish between *minor* and *major* “breaches of duty” which cause different percentage points of benefit reductions.<sup>6</sup> Following the common practice of the vast majority of studies on the effects of benefit sanctions, we only consider the first sanction but not repeated sanctions.

We regard people as directly sanctioned from the beginning of the first punitive benefit cut that is imposed on them directly. We consider an individual to be indirectly sanctioned if they are not punished, themselves, but are indirectly affected by sanctions imposed upon one of their related household members. We regard them as indirectly sanctioned from the beginning of the first sanction against a household member on, and as long as the individual does not face a direct sanction. The moment that an indirectly sanctioned individual also faces a direct sanction, they are removed from the sample; he or she can neither be used in either of the two treatment groups nor, of course, in the control group. The reason for this is that we only use the first sanctions — be it either a direct or an indirect sanction — and thus we need people without previous sanctions for the treatment groups. Nevertheless, we do not totally disentangle direct and indirect sanctions, as in the group of direct sanctioned there are concluded also later direct and indirect sanctions. This goes along with previous studies on benefit sanctions who also only consider the first sanction and define a person as sanctioned from the beginning of the first sanction on. But in contrast to the direct sanctioned, the group of indirect sanctioned contains only persons who are *exclusively* indirectly sanctioned.

As controls, self-evidently we can only use non-sanctioned people. Each individual that has been sanctioned as a UB-II-recipient since the implementation of unemployment benefits II in January 2005 is defined as (pre-)sanctioned until

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<sup>6</sup>Further details about different kinds of “breaches of duty” and the amount of benefit reduction they cause we provide in our previous paper about the effects of benefit sanctions against German UB-II-recipients; see Hillmann and Hohenleitner (2015).

the end of the observation period, that is the end of December 2010, and hence cannot be used in the control group at all.

With regards to the stratification, as explained below in Section 5.4.4, the assignment to either a treatment or a control group works as follows: if a person is sanctioned within the stratum but is non-sanctioned before, the person enters the treatment group of this stratum. Hence, treated people are only placed into one treatment group, namely the treatment group of the current stratum when they face their first sanction; but they can neither be in the treatment group of a stratum before nor in the following strata. A disadvantage of the stratification is that the exact starting time of the sanction within the stratum is no longer considered. This loss of information resulting from stratification may cause a bias that we carefully discuss in Section 5.4.4.3.

### **5.3.3 Outcome variables**

We use two different kinds of outcome variables for our analysis: continuous metric variables measuring the durations until the exit events (“duration outcomes”), and binary variables indicating the current (monthly) employment state (“probability outcomes”). Because of the necessity of stratification, which we explain in Section 5.4 and Subsection 5.4.4, we measure all outcome variables from the start of the stratum on, and not from the beginning of the treatment, namely from the imposition of the sanction on. This holds for duration outcomes as well as for probability outcomes. As mentioned above, the loss of information about the exact starting time of the treatment due to stratification can cause a bias that we discuss in detail in Section 5.4.4.3.

The duration outcomes are continuous variables measuring the duration from the start of the stratum until an exit event, i.e. employment entry, welfare exit, or exit into mere welfare receipt, the latter for the subgroup of employed people with supplementary welfare receipt. The binary outcomes, indicating the current employment state, concretely indicates whether the individual’s initial employment state remains unchanged (value 0), or whether the employment state has changed (value 1) towards either employment entry, welfare exit, or employment exit, meaning welfare receipt alone as a possible outcome for people formerly employed with supplementary benefit receipt. These kinds of outcome variables show the shares of people with (value 1) or without (value 0) an exit event within the monthly prolonged observation periods lasting from



the start of the stratum until the end of the consecutive final months. As the share of people with exit events in relation to the whole (sub-)sample reveals the probability of the exit event, we refer to these kind of outcome variables as “probability outcomes”.

Furthermore, it must be stressed that we face two different samples for the two kinds of outcome variables: the samples for the metric duration outcomes excludes right-censored spells because durations can only be determined for people who experience an exit event until the end of the observation period, actually until the end of December 2010; in contrast, the samples for the monthly binary outcomes include right-censored spells because even if there is no exit event within the observation period, the outcome status can be defined, concretely as value zero indicating no change in labor market status. Hence, the analysis of probability outcomes comprise the full (sub-)samples, while the analysis of the duration outcomes are based on samples reduced by the right-censored spells.

### 5.3.4 Control variables

Applying Propensity Score Matching (PSM), as described in Section 5.4, it is essential to include as many covariates as available that are supposed to affect both the assignment to the treatment and the dependent variables.<sup>7</sup> This holds independently of whether or not the control variables *significantly* influence the treatment and the outcome. For our analysis of the effects of welfare sanctions, we use the explanatory variables presented in the following tables, distinguishing between binary (Table 5.1) and metric (Table 5.2) control variables; specifically, we use them for the propensity score estimation of the selection into the treatment of either direct or indirect sanctions.

When implementing propensity score matching (PSM) with dichotomous treatments applying either logit or probit estimation models, only binary or metric variables shall serve as controls; nominally or ordinally scaled variables ought to be transferred into binary variables.<sup>8</sup> Table 5.1 contains the correspondingly built dummies along with the binary variables used as controls.

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<sup>7</sup>See, for example, Ho et al. (2007).

<sup>8</sup>An advantage of such a transformation is that several tests in order to assess the matching quality can be conducted more easily; see, for example, Müller (2012).

The binary variables *child-u3* and *couple* indicate whether or not an individual has a child younger than 3 years of age, or a partner living in the same household. The dummy for professional education, *pquali*, denotes whether or not the individual has successfully completed a vocational training, including polytechnic or university degrees. The variable for school education, *squali*, is divided into three dummies: graduation from main school (*low*) (*Hauptschule*), secondary school (*middle*) (*Realschule*), and high school (*high*) (*Gymnasium*), where the reference category is having no graduation (*none*). The dummy variables for *age groups*, *nationality*, and *bula*, representing 15 of the 16 federal states (*German Bundesländer*), should be self-explanatory. The dummies for the quarterly inflow cohorts, (*qinfl*), indicate whether an individual entered welfare receipt in the 1st, 2nd, 3rd, or 4th quarter of the year 2008.<sup>9</sup> And finally, the dummies for the quarterly duration of the employment states, *employed* (*emp*), *unemployed* (*ue*), and *employed with supplementary* (*supp*) welfare receipt, indicate whether people have experienced either no (0), between zero and three, inclusive (3), between three and six, inclusive (6), up to nine (9), or up to twelve

TABLE 5.1: Explanatory variables — binary controls

Denotation	Dummy variables	Reference category
Age groups	age: 15-17, 18-24, 35-44, 45-56	age: 25-34
Child under 3 years	child-u3	
Partner in the household	couple	
Nationality	German, non-EU-foreigner	EU-foreigner
School education	squali: low, middle, high	squali: none
Professional education	pquali	
Federal state	bula: 15 of 16 federal states	bula: Bavaria
Quarterly inflow cohort	qinfl: 0208, 0308, 0408	qinfl: 0108
Duration of previous emp	employed: 0, 3, 6, 9 months	emp: 12 mon.
Duration of previous ue	unemployed: 0, 3, 6, 9 months	ue: 12 mon.
Duration of previous supp	supplementary: 0, 3, 6, 9 months	supp: 12 mon.

<sup>9</sup>For the inflow cohort of 2007, used as a sensitivity check, we apply corresponding dummies.

(12) months of the specific employment status during the year previous to the welfare receipt.

The first four of the metric control variables listed in Table 5.2 are taken from the labor market statistics of the German Federal Employment Agency (FEA); the statistics are provided to the public, and are available via the FEA's website. We merged the monthly data, each depicted separately for the 16 federal states, of the following four rates, reflecting the labor market situation of the federal state in the current month: The sanction rate (*sancrate*) of the FEA may be a bit misleading as it only denotes the share of the *currently* sanctioned people of the total of UB-II-recipients, and does not depict a person as sanctioned after the sanction period anymore. Thus, the current sanction rates of the FEA are much lower than in our data, as we also consider a person to be sanctioned after the end of the period of punitive benefit cut. The unemployment rate (*uerate*), the vacancy rate (*vacrate*), and the share of employable UB-II-recipients (*elbrate*) (*ELB*: "erwerbsfähige Leistungsberechtigte") in relation to the whole workforce in Germany are also publicly provided by the FEA.

Information about the wages of previous jobs and incomes gained from employment during the year previous to the welfare receipt was obtained from our main dataset based on the SIAB, described above at the beginning of Section 5.3. We differentiate between the wages and the yearly income of the main employment on the one hand, and the average wage and the sum of yearly incomes over all jobs, on the other hand. Finally, we use the metric variable with the information about the duration of the three mentioned employment states *employed* (*emp*), *unemployed* (*ue*), and *employed with supplementary* (*supp*) welfare receipt that reveal the summarized duration of these states during the year

TABLE 5.2: Explanatory variables — metric controls

Denotation	Metric variables	Varying by
Sanction rate	sancrate	month, federal state
Unemployment rate	uerate	month, federal state
Vacancy rate	vacrate	month, federal state
Employable UB-II-recipients	elbrate	month, federal state
Previous wage	daily wage	main job, all jobs
Previous income	yearly income	main job, all jobs
Previous empl. states	duration in days of previous year	status: emp, ue, supp

previous to entering the current period of welfare receipt.<sup>10</sup>

## 5.4 Methodological approach

We use a dataset based on administrative data, and hence the assignment to the treatment is not random like in case of experimental data. Thus, we have to account for the selectivity of the treatment process.<sup>11</sup> And as the probability of being treated is most likely influenced by unobserved characteristics that also affect the outcome, we have to solve the problem of endogenous treatment and confounding factors<sup>12</sup>.

Furthermore, we examine different exit events which can be treated as so-called “competing risks” (CR) if they are mutually exclusive, as transition into mere employment (*O*) versus into employment with supplementary welfare receipt (*S*), or as transition into employment versus into non-employment would be. But we also examine exit events that are not mutually exclusive but overlapping, such as exit into mere employment (*O*) versus exit from welfare (*ExWel*), as the latter one comprises exits into non-employment as well as into mere employment.<sup>13</sup>

### 5.4.1 Choice of method

A common method to identify the effect of an endogenous treatment on the probability of a subsequent exit event is the *timing-of-events* (ToE) approach. Originally designed for endogenous treatments on a *single risk*, it is common to

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<sup>10</sup>The ‘current period of welfare receipt’ refers to those periods of the inflow cohort from 2008 that were actually analyzed.

<sup>11</sup>A crucial advantage of using random samples of administrative data, however, is that they constitute a representative selection of individuals faced with “real world” conditions, and thus the external validity of analysis based on “real world” data is much higher than experimental data would be.

<sup>12</sup>We give a short definition of confounders and further information on how to deal with possibly unobserved confounding factors in Section 5.5.4.2 in the context of sensitivity analysis for checking the robustness of our estimations.

<sup>13</sup>For more details about the diverse exit events we examine, see the beginning of Section 5.5.

combine ToE models with another type of popular multivariate duration models, namely *mixed proportional hazard* (MPH) models for *competing risks* (CR).<sup>14</sup> As a crucial presupposition is often violated in practice, specifically, that the requirement that the competing risks have to be independent conditional on the covariates and the treatment, Drepper and Effraimidis (2016) developed an advanced combination of MPH competing risks and ToE models that allow for multiple CR which can be “dependent by way of unobserved characteristics”, but where the competing risks still have to be mutually exclusive.<sup>15</sup>

Nevertheless, when applying the ToE approach, the specification of the model is still crucial; in particular, if the restrictions imposed on the heterogeneity distribution are not justified, a significant bias can result.<sup>16</sup> Additionally, MPH specifications impose restrictions on the functional form of the outcome equations that can severely distort parameter estimates.<sup>17</sup> Such kinds of restrictions on the functional form can be avoided using matching techniques such as *propensity score matching* (PSM).<sup>18</sup> And “model dependence”, as it usually occurs “in parametric causal inference”, can be reduced by applying “non-parametric matching procedures” like PSM, which offer “causal inference with fewer assumptions”.<sup>19</sup>

Although a parametric regression model is used to estimate the propensity score (PS), *propensity score analysis* (PSA) with its two-step procedure is considered non-parametric,<sup>20</sup> because nonparametric density estimators, such as kernel functions, are used. And it is valued as a powerful matching technique which, properly applied, is able to balance the covariates in such a way that “the causal effect inference from observational data” becomes “as reliable as possible”.<sup>21</sup> For these reasons, non-parametric matching methods like PSM are being applied to an increasing number of empirical studies in several disciplines, such

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<sup>14</sup>Such timing-of-event (ToE) models, whether joint or not with MPH models for competing risks (CR), have been used for many empirical studies to evaluate the effect of active labor market programs or benefit sanctions on the probability of unemployed to enter employment, like van den Berg et al. (2004), Abbring et al. (2005), Lalive et al. (2005), and Rosholm and Svarer (2008).

<sup>15</sup>See Drepper and Effraimidis (2016).

<sup>16</sup>See Heckman et al. (1999), Gaure et al. (2007), and Hofmann (2012).

<sup>17</sup>See inter alia Hofmann (2012), Drepper and Effraimidis (2016), and Zhang (2017).

<sup>18</sup>See inter alia Hofmann (2012) and Zhang (2017).

<sup>19</sup>See Ho et al. (2007).

<sup>20</sup>See inter alia Gangl and DiPrete (2004), Reinkowski (2006), Ho et al. (2007) Urkaregi et al. (2014), and Zhang (2017)

<sup>21</sup>See Zhang (2017).

as epidemiology and medicine, as well as social sciences and economics.<sup>22</sup>

Moreover, non-parametric approaches to solving the selection problem are extraordinarily beneficial in cases of possibly heterogeneous treatment effects, because they lead to consistent results even if the treatment affects diverse subgroups or individuals within the surveyed population in different ways.<sup>23</sup> This is a strong argument for us to use PSM, as the ex-post effects of benefit sanctions can be expected to be quite heterogeneous between different individuals and groups of welfare recipients. And indeed, the results of our analysis, presented in Section 5.5, confirm this supposition.

Despite the advantages of using non-parametric matching techniques, the vast majority of previous studies on the effect of benefit sanctions apply other methods, predominantly ToE and MPH approaches. One reason for this might be the complexity of propensity score analysis and the huge effort implementing PSA hence entails.<sup>24</sup> From our point of view, however, the advantages exceed the drawbacks, even more so as we analyze overlapping exit events and not just mutually exclusive ones, which would be necessary in order to apply MPH with competing risks.

Furthermore, a large and rich dataset is necessary, or at least conducive, when performing a reliable and robust PSA. Our dataset generally fulfills these requirements for the main part of our analysis. More precisely, the data set is sufficiently huge and extensive to apply PSM for our main topics and for most of the population groups under study.<sup>25</sup> Similar to Hofmann (2012), who uses a dynamic matching approach for her study on the ex-post effects of unemployment insurance (UI) sanctions in West Germany which takes the timing of the treatment into account, we also implement a *dynamic* approach of propensity score matching (PSM), applying stratification to deal with the flexible timing of the treatment and the missing start date for the untreated.

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<sup>22</sup>See inter alia Stürmer et al. (2006), Ho et al. (2007), Caliendo and Kopeinig (2008), Urkaregi et al. (2014), and Zhang (2017).

<sup>23</sup>See Reinkowski (2006).

<sup>24</sup>See Müller (2012).

<sup>25</sup>Nevertheless, because of the necessary stratification, there is a loss of observations per analyzed stratum which, in a couple of cases with small subgroups, can lead to convergence problems caused by too few exit events in the treatment group.

### 5.4.2 The matching approach

The general questions we want to answer with matching procedures are whether a specific treatment is causal for the outcome of the observed entities, and how distinct and strong such a possible impact on the outcome would be. The fundamental problem we face is that we can observe only the factual but not the counterfactual state for the same statistical unit under otherwise completely identical— i.e. apart from the treatment — conditions.<sup>26</sup> Instead of measuring, we have to estimate the counterfactual state. One possible way to put this into practice is to use comparable groups of *treated* ( $D_i = 1$ ) and *untreated* ( $D_i = 0$ ) individuals, where  $D_i$  is a dichotomous treatment indicator for individual  $i$ , with value 1 for the status “treated” and 0 otherwise, and  $i = 1, \dots, N$ , with  $N$  denoting the number of observed individuals in the entire surveyed population.<sup>27</sup>

The outcomes then are defined by  $Y_i(D_i)$  for each individual,  $i$ , and the individual treatment effect (TE) is defined as

$$(\textit{individual TE}) \quad \tau_i = Y_i(1) - Y_i(0). \quad (5.1)$$

But the individual treatment effect,  $\tau_i$ , cannot be measured, as we can observe only one of the two outcomes per individual  $i$ , because a person can either be treated or not at one specific point of time. Therefore *individual* treatment effects cannot be estimated, and hence, analysis in general have to focus on estimating *average* treatment effects of the investigated group.<sup>28</sup>

The evaluation of treatment effects can generally be based on various measures like the “*average treatment effect*” (ATE), which refers to the impact of the treatment on the *entire group* of population under study, and the “*average treatment effect on the treated*” (ATT), referring to the *treated share* of the examined group.

<sup>26</sup>See inter alia Roy, 1951, Rubin, 1974, Heckman and Smith, 1995, Rubin, 2004, and Gangl and DiPrete (2004).

<sup>27</sup>This applies to binary treatments; see Caliendo and Kopeinig (2008), Heinrich et al. (2010), and Müller (2012) who provide helpful practical guidance on implementing PSM.

<sup>28</sup>See Caliendo and Kopeinig (2008). A potential way to determine the individual treatment effect, ( $\tau_i$ ), could be to measure the outcome before and after the treatment, where the outcome before the treatment would serve as the counterfactual state. This holds only if all other factors beyond the treatment which affect the outcome are stable between the two measurement points, but this is often not fulfilled in practice; see Reinkowski (2006) and Müller (2012). Furthermore not every kind of outcome can be measured before and after the treatment like it is the case with duration outcomes and exit events during an unemployment episode which ends when the event takes place.

As the ATE would possibly also include individuals who are not targeted by the treatment, in the practice of evaluating treatment effects of political measures and other targeted interventions, the ATT is the predominantly used parameter.<sup>29</sup> For our study of the ex-post effects of benefit sanctions we follow this reasoning and the common approach to use an ATT estimator to analyze treatment effects.

The *true value* of the average treatment effect on the treated (ATT) is given by

$$(\text{true ATT}) \quad \tau_{ATT} = E[\tau|D = 1] = E[Y(1)|D = 1] - E[Y(0)|D = 1], \quad (5.2)$$

where  $E[Y(1)|D = 1]$  is the expected value of the outcome  $Y(1)$  for the treated ( $D = 1$ ) in case of being treated,  $E[Y(0)|D = 1]$  is the expected value of the outcome  $Y(0)$  for the treated in the hypothetical case of not being treated — referred to as the counterfactual state — and  $E[\tau|D = 1]$  is the difference between the two, which is the expected value of the treatment effect on the treated. But as the average outcome in the counterfactual case,  $E[Y(0)|D = 1]$ , is not observable, we have to build an artificial simulacrum of the counterfactual state. A common approach to implement such an artificial counterfactual is to use control groups of untreated individuals in order to compare their outcomes with the outcomes of the treated. The estimation of treatment effects is then conducted by measuring the average outcome differences between the groups — properly weighted if required, depending on the matching technique. *Naively*, the ATT could be estimated as

$$\widehat{\tau}_{ATT} = E[\tau|D = 1] \approx E[Y(1)|D = 1] - E[Y(0)|D = 0]. \quad (5.3)$$

But as people differ in their properties,  $E[Y(0)|D = 0] \neq E[Y(0)|D = 1]$  regularly holds, and thus the estimation of the counterfactual state with Equation (5.3) cannot be error-free. Specifically, if potential differences in the average properties of the treatment and control group also affect the outcome variable, the estimated treatment effects are distorted by so-called “confounding factors”.<sup>30</sup> The estimation error caused by such confounders based on different characteristics between treatment and control group is referred to as “*selection*”

<sup>29</sup>See Heckman, 1997 and Müller (2012).

<sup>30</sup>See Caliendo and Kopeinig (2008) and Müller (2012).



bias" (SB)<sup>31</sup> and can be formalized as

$$(\textit{selection bias}) \quad SB = E[Y(0)|D = 1] - E[Y(0)|D = 0]. \quad (5.4)$$

Because of this potential selection bias of unknown quantity, Equation (5.3) must be supplemented by Equation (5.4) in order to get a *proper estimation parameter* of the ATT:

$$\widehat{\tau}_{ATT} = E[\tau|D = 1] = E[Y(1)|D = 1] - E[Y(0)|D = 0] + SB. \quad (5.5)$$

It has to be stressed that differences in the average characteristics of the treatment and control groups are only problematic if they also affect the outcome. Otherwise they don't distort the estimation of the ATT.<sup>32</sup> The precondition which requires that the differences between the outcomes of the treatment and control groups must be independent of the selection process into the treatment, and thus are caused exclusively by the treatment itself, is called "*conditional independence assumption*" (CIA)<sup>33</sup> or "*unconfoundedness*"<sup>34</sup>. It can be formalized as

$$(\textit{CIA/unconfoundedness given } X) \quad Y(0), Y(1) \perp\!\!\!\perp D|X, \quad \forall X, \quad (5.6)$$

where  $X$  represents the vector of covariates and  $\perp\!\!\!\perp$  stands for stochastic independence.<sup>35</sup> This strong assumption requires that all confounding factors must be eliminated or at least held constant, and hence they ought to be included in the estimation procedure as control variables.<sup>36</sup> If the CIA is not satisfied, the estimated ATT is distorted by unobserved confounders.<sup>37</sup> Therefore sensitivity analysis need to be conducted in order to check the robustness of the results against possibly unobserved confounding factors.<sup>38</sup> In Section 5.5.4.2, we give more detailed information about robustness checks and the kinds of sensitivity analysis we carried out, as well as about their results.

Another assumption that has to be satisfied in order to properly apply matching

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<sup>31</sup>See Heckman et al., 1998.

<sup>32</sup>See Müller (2012).

<sup>33</sup>See Rosenbaum and Rubin (1983) and D'Orazio et al. (2006).

<sup>34</sup>See Lechner (1999).

<sup>35</sup>See Reinkowski (2006) and Caliendo and Kopeinig (2008), whereby Reinkowski (2006) uses the symbol  $\perp$  for stochastic independence.

<sup>36</sup>See Müller (2012).

<sup>37</sup>See Reinkowski (2006) and Caliendo and Kopeinig (2008).

<sup>38</sup>See Caliendo and Kopeinig (2008), Heinrich et al. (2010), and Müller (2012).

procedures is the so-called “*stable-unit-treatment-value-assumption*” (SUTVA). This precondition asserts that the treatment effect on one individual must not be influenced by the treatment of another entity, and thus the stability of the causal effect should be given.<sup>39</sup> In our analysis of benefit sanctions, we can presume that the SUTVA is satisfied. On the one hand, because of the huge number of people in the investigated group of UB-II-recipients in Germany, the sanction of people living in other households and families generally does not affect the labor market outcome of another sanctioned individual. And on the other hand, we disentangle the effect of sanctions against more than one member of a household by distinguishing between direct and indirect sanctions.<sup>40</sup>

If the preconditions are satisfied, and ideally a large and rich dataset is available with which to investigate the research question, matching techniques are a powerful and reliable method to solve the selection problem. As mentioned above, the treatment and control groups are not usually identical in their characteristics, and possible confounding factors must be considered and dealt with. The matching approach then tries to solve the selection problem by constructing an artificial simulacrum of the counterfactual of each treated individual using a properly generated sub-control-group of untreated.<sup>41</sup> In order to minimize the selection bias caused by the distorting effects of possible confounders, the matching procedure should lead to an artificially generated control group of untreated which is, on average, as similar as possible to the treatment group, for a properly defined set of covariates.<sup>42</sup> Even though there are several matching procedures available, because of its advantages we use the now common technique of *propensity score matching* (PSM) for our analysis.

### 5.4.3 Matching on propensity score

Matching on propensity score is an elegant and powerful technique for balancing the covariates of the treatment and control group. It simplifies the matching procedure by using a one-dimensional parameter instead of searching for so-called “statistical twins”<sup>43</sup>.

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<sup>39</sup>See Gangl and DiPrete (2004) and Müller (2012).

<sup>40</sup>For further details see Section 5.3.2.

<sup>41</sup>See Reinkowski, 2006.

<sup>42</sup>See Müller (2012).

<sup>43</sup>“Statistical twins are cases that resemble their statistical siblings in selected variables”, see Bacher (2002); strictly defined, statistical twins are meant to be identical for the whole set of selected variables.

With an increasing number of covariates, we get an exponentially growing number of possible matches. Hence, the more covariates are included into the vector  $X$ , the more severe the problem of emerging dimensionality will be if we try to find a statistical twin as a substitute for the counterfactual case.<sup>44</sup> In order to solve such kinds of dimensionality problems, Rosenbaum and Rubin (1983) suggest to apply a so-called “balancing score” (BS). Balancing scores are defined as functions  $b(X)$  of the relevant covariates  $X$  which holds

$$(\text{balancing score}) \quad X \perp\!\!\!\perp D | b(X), \quad \forall X. \quad (5.7)$$

Formula 5.7 means that, given the balancing score  $b(X)$ , the *conditional distribution* of the set of control variables  $X$  is independent of the treatment, and thus  $X$  is, on average, identical for the treatment and the artificially built control group after the matching procedure.<sup>45</sup> It is crucial to distinguish between the initial group of the non-treated whose members are at available to serve as counterfactuals, on the one hand, and the artificially built simulacrum generated by applying one of diverse applicable matching techniques, on the other hand. Those matching procedures use and generally weight the non-treated people — concretely their values of covariates and outcomes — in order to build a control group that is a reflection of the treatment group which should preferably be as alike as possible, regarding a set of well-defined control variables  $X$ . It also has to be stressed that not the matched individuals themselves necessarily need to be as similar as possible, like the ideal-typical case of “statistical twins” would claim. In contrary, because of the weighting and potential re-using<sup>46</sup> of proper untreated matches, the similarity of treated and matched controls is not on the individual level but just on the group level. More precisely, both groups — treated and matched controls — are just *averagely* similar in the set of their control variables. And balancing scores, properly used for the matching procedure, do have the so-called “balancing property” which means that they lead to such balanced groups of treated and matched controls.<sup>47</sup>

According to Rosenbaum and Rubin (1983), balancing scores in large samples

<sup>44</sup>See Reinkowski, 2006 and Caliendo and Kopeinig (2008).

<sup>45</sup>See Rosenbaum and Rubin (1983) and Reinkowski, 2006; note that we use a notation similar to Caliendo and Kopeinig (2008), where  $\perp\!\!\!\perp$  is used to symbolize stochastic independence; Reinkowski, 2006 uses the symbol  $\perp$  for statistical independence.

<sup>46</sup>Matching procedures as a rule use sampling with replacement. Thus, controls can be re-used as proper matches for several treated individuals.

<sup>47</sup>See Rosenbaum and Rubin (1983).

enable the assignment to treatment and control group without using the outcome variable.<sup>48</sup> In other words, balancing scores allow one to predict whether an individual is treated or not using only the information given by the vector  $X$  of control variables.<sup>49</sup>

This also holds for the propensity score (PS), the most frequently used balancing score in empirical studies, which is defined as

$$(\textit{propensity score}) \quad PS = P(X) = Pr(D = 1|X). \quad (5.8)$$

The *propensity score* (PS) is a one-dimensional matching parameter with balancing properties that can be interpreted as the predicted probability ( $Pr$ ) of being treated ( $D = 1$ ), given the vector of observed covariates ( $X$ ).<sup>50</sup>

Rosenbaum and Rubin (1983) show that if the conditional independence assumption (CIA) — also referred to as unconfoundedness — depends upon the covariates  $X$ , it is also conditional on the balancing score. The CIA based on the PS can thus be formalized as

$$(\textit{CIA/unconfoundedness given the PS}) \quad Y(0), Y(1) \perp\!\!\!\perp D | P(X), \quad \forall X \quad (5.9)$$

which implies that given the propensity score — determined by the observable covariates  $X$  which are not affected by the treatment — the outcomes are independent of the assignment into the treatment.<sup>51</sup>

A further assumption that has to be satisfied in order to properly implement PSM is the so-called ‘common support condition’, also referred to as the ‘overlap condition’. The common support (CS) is met if

$$(\textit{common support}) \quad 0 < P(D = 1|X) < 1, \quad \forall X \quad (5.10)$$

holds. The CS condition ensures that for all observed sets of  $X$ , and thus for all estimated  $P(X)$ , there is a positive probability of being treated as well as of staying untreated.<sup>52</sup> This excludes that there are values or value ranges of  $X$  or

<sup>48</sup>See Rosenbaum and Rubin (1983), cited by Reinkowski, 2006.

<sup>49</sup>This clearly relates to the *initial* group of the untreated, whose members are just *potentially* used as controls, in contrast to the *matched* untreated which *actually* are used as controls after the matching.

<sup>50</sup>See Reinkowski, 2006 and Caliendo and Kopeinig (2008).

<sup>51</sup>See Caliendo and Kopeinig, 2008.

<sup>52</sup>See Heckman et al. (1999), cited by Caliendo and Kopeinig (2008).

$P(X)$  that allow for cases of so-called “perfect predictability” of the treatment  $D$  given  $X$  or  $P(X)$ .

According to Rosenbaum and Rubin (1983)<sup>53</sup>, if both preconditions are satisfied — i.e. the conditional independence assumption (CIA) and the common support (CS) condition — then the condition of ‘strongly ignorable treatment assignment’ (SITA), or ‘strong ignorability’ for short, is fulfilled. And given strong ignorability, the *PSM estimator of the ATT* can be written as

$$\tau_{ATT}^{PSM} = E_{P(X)|D=1} \{E[Y(1)|D=1, P(X)] - E[Y(0)|D=0, P(X)]\} \quad (5.11)$$

which implies that the estimated average treatment effect on the treated (ATT) using PSM is calculated as the difference of mean outcomes between treated and matched untreated over the range of common support, where the outcome values of the control group are “appropriately weighted by the propensity score distribution” of the treated.<sup>54</sup>

#### 5.4.3.1 Implementation steps

As mentioned above, PSM is quite a complex kind of analysis to carry it out in practice, consisting of the following *implementation steps*<sup>55</sup>:

1. *propensity score estimation*
2. *choice of matching algorithm*
3. *check of common support*
4. *matching quality and effect estimation*
5. *sensitivity analysis*

where (1) *estimating the propensity score* and (4) performing the chosen *matching* procedure with subsequent *effect estimation* are the two main steps in estimating the ATT applying propensity score analysis (PSA). Nevertheless, it is also essential to check the common support, the matching quality, and finally the robustness in order to evaluate the reliability of the results.

<sup>53</sup>See Rosenbaum and Rubin (1983), cited by Reinkowski, 2006 and Caliendo and Kopeinig (2008).

<sup>54</sup>See Caliendo and Kopeinig (2008).

<sup>55</sup>See Caliendo and Kopeinig (2008), slightly modified by Müller (2012).

In cases of binary treatment variables, discrete decision models, like logit or probit models, are usually applied in order to *estimate the propensity score*.<sup>56</sup> As both models lead to quite similar results, the choice between them is not crucial.<sup>57</sup> For our analysis of the effects of benefit sanctions, we use a probit model to estimate the propensity score according to the standard setting of the Stata module “*psmatch2*”, developed and documented by Leuven and Sianesi (2014).<sup>58</sup>

A more important decision when performing PSA is the *choice of the matching algorithm*. An overview of diverse matching techniques and deeper insights into their advantages and drawbacks are given by various specialist literature<sup>59</sup> and shall not be discussed in-depth here.

For our analysis we predominantly conduct *kernel matching* (KM), as it applies a non-parametric matching estimator that is generated by using “weighted averages of all individuals in the control group to construct the counterfactual outcome”.<sup>60</sup> And thus KM leads to lower variance, because more information of the group of untreated is used, compared to another common matching technique called *nearest neighbor matching* (NNM).<sup>61</sup> We apply the two different variants of the latter (NNM) as a kind of robustness check.<sup>62</sup>

After the matching algorithm is chosen, the matching procedure is conducted under the restriction of common support. Subsequently, the ATT is estimated according to the above derived Formula 5.11 (*PSM estimator of the ATT*), provided that the matching quality is adequately fulfilled and thus the balancing property of the PSM is given.<sup>63</sup> And finally sensitivity analysis have to be conducted in order to check the robustness of the estimation results.<sup>64</sup>

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<sup>56</sup>See Müller (2012).

<sup>57</sup>See Caliendo and Kopeinig (2008).

<sup>58</sup>See also Sianesi (2001) for a short overview of how to implement PSM estimators with Stata.

<sup>59</sup>See for example Guo and Fraser (2015), cited by Müller (2012); and see also Caliendo and Kopeinig (2008) and Müller (2012) for a brief overview and discussion.

<sup>60</sup>See Caliendo and Kopeinig (2008).

<sup>61</sup>See Caliendo and Kopeinig (2008).

<sup>62</sup>See Section 5.4.3.2 for further details of the matching approaches we use and Section 5.5.4.2 for further details of the robustness checks we conduct by comparing the results of different matching algorithms.

<sup>63</sup>See Section 5.5.4.1 for more details about our checks of matching quality.

<sup>64</sup>For more details about the different kinds of robustness checks we conducted see Section 5.5.4.2.

### 5.4.3.2 Details of the matching approach

For our analysis of the ex-post effects of benefit sanctions against employable welfare recipients in Germany, we apply two general kinds of matching procedures: “nearest neighbor matching” (NNM) and “kernel matching” (KM). We carry out the whole analysis separately for women and men, as well as for two distinct samples of inflow into welfare receipt — specifically, for the inflow samples of the years 2007 and 2008 — using results from 2008 for our main evaluation, as presented in Section 5.5, and the results from 2007 in one of our sensitivity analysis.

After checking out several variations of nearest neighbor matching (NNM) with  $k=3$  (3-NNM) and  $k=5$  (5-NNM) nearest neighbors, varying the caliper, as well as the kernel matching (KM) kernel and bandwidth, we decided to apply both 5-NNM with a caliper of 0.01 and KM using an *Epanechnikov kernel* (EKM) with a bandwidth of 0.06, with both the bandwidth of KM and the caliper of NNM chosen to avoid bad matches. The common support is a priori fulfilled for all of our estimations of ATT, as we use the appropriate option of the Stata module *psmatch2* that we apply for all of our PSM estimations. As is usual in PSM, we apply sampling with replacement also for the NNM, because it is beneficial for the matching quality to re-use good matches.

As we explain in Section 5.4.4, we implement a dynamic matching approach with stratification in order to solve the missing start point problem for the untreated which arises from the flexible timing of the treatment. Additionally, we carry out the whole analysis for two kinds of outcome variables: firstly, for duration outcomes and secondly, for probability outcomes.<sup>65</sup> In the case of probability outcomes, we calculate monthly updated ATT for overlapping periods for each stratum of every group and subgroup of welfare recipients under study, as well as for direct and indirect sanctions. Regarding direct sanctions, we estimate monthly updated ATT over 24 months for each quarterly strata; concerning indirect sanctions, we estimate the ATT over 18 months for each half-yearly strata.<sup>66</sup> Hence we have a tremendous number of estimations to assess and interpret which we present in a cumulative form in tables, as well as in a more detailed form via a choice of graphs which show the development of accumulated ATT over time.

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<sup>65</sup>For more information about the two kinds of outcome variables see Section 5.3.3.

<sup>66</sup>Detailed information about the stratification we provide in Section 5.4.4.

For our main evaluation, we use kernel matching (KM), while nearest neighbor matching (NNM) serves as one of our robustness checks. KM was chosen because of its advantages when compared to the also popular NNM. First of all, KM uses not only a few nearest neighbors like NNM, but the majority of the observations in the control group given the common support condition, whereby they are properly weighted conditionally on their similarity of propensity scores to the individuals in the treatment group who they are matched with. Secondly, bootstrapped standard errors can be computed by applying KM with Stata. However, as bootstrapping is extremely time-consuming and requires a large amount of computing capacity, especially when faced with a huge number of estimations, we mainly use the common calculation of standard errors as implemented by Leuven and Sianesi (2014) in their Stata module *psmatch2* which “does not take into account that the propensity score is estimated”. In order to check the reliability of these results, we additionally carry out spot checks with bootstrapped standard errors.<sup>67</sup>

#### 5.4.4 Stratification

Fredriksson and Johansson (2008) point out that in cases of flexible timing of treatments, it “makes more sense to think of the assignment to treatment as a dynamic process, where the start of treatment is the outcome of a stochastic process”, instead of dealing with treatment assignment as a “static problem” whereby “the information contained in the timing of treatment is typically ignored”. We implement a dynamic approach of propensity score matching (PSM), applying stratification in order to take the timing of the treatment at least roughly into account, and to solve the problem of missing start dates for the untreated.

Estimating the ex-post effects of sanctions on subsequent duration outcomes like unemployment duration or subsequent binary outcomes like the labor market status, we face an initially undetermined timing of treatment which can start at any point in time; thus the starting point to measure the subsequent outcomes for the untreated is also undetermined. For this so-called ‘missing start date problem’, Lechner (1999) provides several solutions in the context of matching approaches. Another prominent alternative to deal with flexible starting points of treatments would be the timing of events (ToE) approach,

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<sup>67</sup>As mentioned in Section 5.5.4.2, the results obtained with bootstrapped standard errors widely confirm the results of the commonly calculated estimations of the ATT.



discussed in Section 5.4.1. Weighing up the advantages and disadvantages, we follow Sianesi (2004), Fitzenberger and Speckesser (2007), and Hofmann (2012) applying the practicable and feasible method of stratification. One of the advantages of using a stratified matching approach is that it “allows for heterogeneous treatment effects for the different duration intervals considered”,<sup>68</sup> namely for the different strata of welfare duration.

We extend the above introduced static PSM with binary treatments to a dynamic setting. This dynamic PSM approach divides the duration of welfare receipt into a set of intervals, the so-called ‘strata’. Based on these strata, a treatment group for every stratum is defined, and finally each treatment group is matched with a selected group of completely untreated, the matched control group.

#### **5.4.4.1 Details of the stratification**

The main decision when applying stratification is to find a proper length of the duration intervals. On the one hand, short intervals are preferable: “first, a relatively short observation window reduces the potential bias due to conditioning on future outcomes described in Fredriksson and Johansson (2008). Second, the shorter the duration intervals are defined, the better effect heterogeneity between the duration intervals can be controlled for.”<sup>69</sup> On the other hand, the shorter the chosen observation window, the fewer treatments are observed. Too few cases in the treatment group can lead to problems in the estimation process and prevent one from getting reliable and statistically significant results, even if the effect, in reality, is strong.

In our analysis of the ex-post effects of welfare sanctions, we apply quarterly strata of welfare duration in the case of direct sanctions and half-yearly strata in the case of indirect sanctions.<sup>70</sup> The latter choice is due to a lack of merely indirect sanctioned people.

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<sup>68</sup>See Hofmann (2012).

<sup>69</sup>See Hofmann (2012).

<sup>70</sup>Hofmann (2012) uses duration intervals of two months in her analysis of ex-post effects of unemployment insurance (UI) sanctions, based on administrative data of the Federal Employment Agency (FEA). We do not have access to complete administrative data sets of the German FEA, but use a data set based on the “Sample of Integrated Labour Market Biographies” (SIAB) which is a 2% sample of administrative data, provided by the Research Data Centre (FDZ), which prepares administrative data of the FEA in order to provide them for internal and external scientific research. Because we are not using a full data set but a 2% sample, two-monthly strata would be too short to guarantee a sufficient number of observations in the treatment group for several subgroups of welfare recipients under study.

Another choice when applying stratification is the number of observation intervals considered. As with increasing duration of welfare receipt, the number of sanctions per month decreases (i.a. because of an increasing number of exit events over time and hence a decreasing number of people at risk of being sanctioned), we restrict our analysis to sanctions within the first year of welfare receipt. In the case of *direct sanctions*, this means we apply *four quarterly strata*  $S_i$  with  $i = 1, \dots, 4$ , and in the case of *indirect sanctions* we apply *two half-yearly strata*  $S_i$  of welfare duration with  $i = 1, 2$ .

A further decision when implementing a stratified matching approach concerns the point in time when the measurement of the outcome variable begins. We could potentially start to measure from the beginning of the stratum, or from the end of the stratum on. The latter option would have the disadvantage that the short-term effects within the stratum would be excluded and thus underestimated; using the beginning of the stratum as starting point has the advantage that the information on events during the stratum is included, but it generates a potential bias caused by late treatments<sup>71</sup> within the stratum, which also leads to an underestimation of the treatment effect. Hence, the potentially conceivable option of choosing the starting point for measuring the outcome somewhere within the stratum, e.g. in the middle of the stratum, would not solve the problem of potentially biased effect estimations, as both distortions work in the same direction and, thus, would not cancel out the bias. In order to avoid losing information on events during the stratum, we follow the usual approach in the literature,<sup>72</sup> and apply stratification with measuring the outcomes from the beginning of the strata.

#### 5.4.4.2 Formalized PSM with stratification

Detailed formalized descriptions of the causal inference problem with endogenous treatments and the evaluation approach applying propensity score matching (PSM) in a stratified manner are provided by Sianesi (2004), Fitzenberger and Speckesser (2007), and Hofmann (2012), where Hofmann (2012) applies a more complex stratification using substrata.<sup>73</sup>

<sup>71</sup>Further information on what is meant by “late treatments” and how they can cause a bias to the detriment of the treated is given below in Section 5.4.4.3.

<sup>72</sup>For example see Fitzenberger and Speckesser (2007) and Hofmann (2012).

<sup>73</sup>As mentioned in Section 5.4.4.3 and Section 5.5.4.2 and following Hofmann (2012), we developed a procedure to deal with the similar problem of distortions caused by stratification

For reasons of understandability by a more general public, we use our own notation when presenting the results in Section 5.5 that should be intuitively understandable, even for readers who might not be that familiar with a high degree of formalization. As our notation in Section 5.5 diverges from Sianesi (2004), Fitzenberger and Speckesser (2007), and Hofmann (2012), we give, here, a brief overview of the formalized PSM with stratification adapted to our notation, and for more detailed information we refer the reader to the aforementioned literature.

Similar to Fitzenberger and Speckesser (2007) who follow Sianesi (2004), we apply “the standard static binary treatment approach recursively depending on the elapsed [...] duration” of welfare receipt. For this purpose, “the standard binary treatment approach” has to be extended “to a dynamic setting”. Hence, the estimated ATT calculated with Formula 5.11 for the common approach with static treatments, presented in Section 5.4.3, “has to be interpreted in a dynamic context. We analyze treatment conditional upon the [...] [welfare] spell lasting at least until the start of the treatment and this being the first [...] treatment during the [...] [welfare] spell considered.”<sup>74</sup> Hence, the *PSM estimator of the ATT for stratified matching*, taking into account dynamic treatments, can be formalized as

$$\begin{aligned} \tau_{ATT}^{PSM}(S_i, m_j) = & E_{P(X)|D_i=1} \{E[Y_j(1)|D_i = 1, W \geq m_0, D_0 = \dots = D_{i-1} = 0, P(X)] \\ & - E[Y_j(0)|D_i = 0, W \geq m_0, D_0 = \dots = D_{i-1} = 0, P(X)]\} \end{aligned} \quad (5.12)$$

where  $D_i$  is the treatment dummy for sanctions starting in the  $i$ th interval of welfare duration  $W$ , namely in stratum  $S_i$ ;  $m_0$  is the month directly before the beginning of the stratum  $S_i$ ;  $D_0$  is the potential observation time of people at risk (i.e. in welfare receipt) previous to the considered welfare spell; equation  $D_0 = \dots = D_{i-1} = 0$  requires that no treatment has occurred before the beginning of stratum  $S_i$ , which for the treated means that  $D_i = 1$  (imposed in stratum  $S_i$ ) is the first treatment they experience;  $P(X)$  is the propensity score (PS) (see Formula 5.8) given the vector of control variables  $X$  with (at least during the welfare spell) time-invariant characteristics; the term  $Y_j(1)|D_i = 1$  is the treatment outcome of the treated for period  $P_j \equiv [m_1, m_2, \dots, m_j]$ , starting

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that we call the ‘adjustment procedure’, and which we use for spot checks to reveal biased outcomes.

<sup>74</sup>See Fitzenberger and Speckesser (2007); we refer to “welfare” spells where they refer to “unemployment” spells; moreover they investigate the effects of a specific training program for unemployed as the treatment, while we investigate the ex-post effects of welfare sanctions.

with the first month  $m_1$  after the beginning of stratum  $S_i$  and ending with final month  $m_j$ , where  $j$  counts the months after the start of stratum  $S_i$ ; and the term  $Y_j(0)|D_i = 0$  is the non-treatment outcome of the matched untreated for period  $P_j$  with final month  $m_j$ . Formula 5.12 is based on the PSM estimator for ATT in case of static treatments, according to Caliendo and Kopeinig (2008) (see Formula 5.11), supplemented and advanced according to the estimated treatment parameter for a stratified matching approach presented by Fitzenberger and Speckesser (2007), and finally adjusted to the notation we use for an illustrative way of presenting our results in Section 5.5.

It may catch an attentive reader's eye that the second part of the equation denotes the non-treatment outcomes according to Caliendo and Kopeinig (2008) as  $Y_j(0)|D_i = 0$ , instead of  $Y_j(0)|D_i = 1$  according to Fitzenberger and Speckesser (2007); additionally, the latter ones do not include  $P(X)$  in their formula.<sup>75</sup> The reason for these differences is that Fitzenberger and Speckesser (2007) present an estimated treatment parameter in an earlier state of the matching approach which represents the *true ATT* (according to Formula 5.2 in Section 5.4.2) with  $Y_j(0)|D_i = 1$  as the non-observable counterfactual outcomes of the treated. But Formula 5.12 does not depict the true ATT but the *estimator of the ATT*, using the non-treatment outcomes of the matched untreated  $Y_j(0)|D_i = 0$ , appropriately weighted by the propensity score distribution  $P(X)$  of the treated, as the counterfactual case.

We estimate the average treatment effect on the treated (ATT) for two kinds of outcome variables: metric duration outcomes and monthly updated binary outcomes indicating the labor market status over time. Formula 5.12 is designed for the more complex case of monthly updated binary outcomes, but can easily be adapted to the case of metric duration outcomes. The latter ones do not require monthly updates, and hence the index  $j$ , counting the months after the beginning of the stratum  $S_i$ , as well as the final month  $m_j$  are omitted for the estimation of duration outcomes.

As mentioned above and outlined in Section 5.4.1, a huge advantage of using propensity score matching (PSM) with stratification, instead of ToE models with a defined and hence inflexible distribution function, is that the PSM estimator in Formula 5.12 allows for "heterogeneity in the individual treatment effects and for an interaction of the individual treatment effects with dynamic sorting taking place", provided that the "dynamic version of the conditional

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<sup>75</sup>See Formula 2 in Fitzenberger and Speckesser (2007).

mean independence assumption (DCIA)'' holds.<sup>76</sup> Concretely in our case, implementing PSM in a stratified manner allows for heterogeneous treatment effects for different intervals (strata  $S_i$ ) of welfare duration  $W$ . Thus, our estimations for divers strata can reveal varying ex-post effects of sanctions depending on the previously elapsed duration of welfare receipt.

#### 5.4.4.3 Bias caused by stratification

There are two kinds of distortions caused by not considering the exact time of the treatment when applying PSM in a stratified manner. The first one is the core of the bias problem as a result of stratification. Thus we call it 'the core bias'. Not only can it weaken the estimated treatment effect, but may even turn it into the opposite direction; if this kind of bias occurs, it affects the estimated ATT on probabilities as well as on durations.<sup>77</sup> Even if the core bias can decrease over time, it does not fully vanish. The second kind of bias we call the 'time lag bias', as it is caused by a time lag in the measurement of the treatment effect. It leads to a reduced effect estimation for probability outcomes as long as the treatment effect has not fully expired, namely if there is still a noticeable monthly effect. The time lag bias is less problematic than the core bias as it can merely weaken the estimated effect but cannot turn it into the opposite direction and the distortion diminishes over time and vanishes when the treatment effect is expired. Hence, the time lag bias does not affect the ATT on probabilities in the long run and, moreover, it does not affect the ATT on durations at all. Therefore, we place the main focus in this section and in the discussion of the results in Sections 5.5 and 5.6 on the core bias. In the following sections, however, we do not distinguish between different kinds of biases, but usually refer to the entire distortion that we call "the bias", for short.

As we analyze the ATT based on two different kinds of outcome variables, it is important to take into account that the bias works in opposite directions depending on the type of outcome. In general, and if we do not explicitly mention the kind of outcome variables, we refer to probability outcomes. Concretely, a bias "to the detriment of the treated", or more precisely "to the detriment of the

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<sup>76</sup>See Fitzenberger and Speckesser (2007) also for further details on the DCIA, the dynamic version of the CIA.

<sup>77</sup>As explained in Section Section 5.3.3, we carry out the whole analysis for two kinds of outcome variables: metric duration outcomes and monthly updated binary outcomes, indicating an exit event changing the labor market status; hence we get estimations of the ATT on durations as well as on probabilities.

treatment effect”, connotes a negative bias of the ATT on probabilities. And as lower probabilities correlate with higher durations, the bias of the ATT on durations would be accordingly positive; conversely, in case of a positively biased ATT on probabilities, the reverse is valid and the ATT on durations would be biased negatively. All this holds for both kinds of distortions, the core bias and the time lag bias, as well to the bias in total.

In the remainder of this section we highlight different aspects of the distortions and analyze the effects of the core bias in the context of different kinds of exit events.

### **Origins and effects of the distortions**

The origin of both kinds of distortions is that the outcome is measured from the start of the stratum on, while the treatment can occur at any time within the stratum. The observations with treatments after the beginning of the stratum, at first, cause a temporal shift in measuring the treatment effects on outcomes due to the *time lag between beginning of the stratum and treatment* which can distort ATT estimations on probabilities for a limited period of time. Secondly, and much more important, the time lag enables observations with *exit events before the treatment*. These events previous to the treatment are the origin of the more problematic core bias. The distortion due to *previous events in the control group* is the *typical case* of the bias arising from matching in a stratified manner that is regularly discussed in the literature. Furthermore, analyzing ex-post effects of benefit sanctions, the standard cases, predominantly regarded in the literature, are exit events that lead out of risk to be treated: getting employed without receiving top-up benefits and exiting from benefit receipt comprising the non-employment option. Hence, exit events before the treatment in these standard cases can only occur in the group of non-treated. However, we additionally analyze exit events that do not transition from being at risk to getting sanctioned — i.e. taking up employment with supplementary welfare benefits and the reverse case of leaving employment with top-up benefits for mere welfare receipt. Both examples enable observations with events previous to the treatment, including those outside of the control group; they additionally allow for *previous events in the treatment group*. The latter one can generate *reverse causality*, and thus yield a further problem adding to the core bias and affecting it.

**Time lag bias:** The time lag bias results from the delay between the beginning of the stratum which is the starting point for measuring the outcome variable, and the average beginning of the treatment. Thus, a potential treatment effect occurs with a temporal shift according to the average time lag, while we implicitly take the start of the stratum to be the beginning of the treatments. This time lag weakens the effect estimations only if the ATT is measured on a specific reference date which is before the treatment effect ends. Therefore it arises in cases of monthly updated ATT on probabilities as long as the impact of the treatment is still effective; after the treatment effect has expired in the case of ATT on durations, the time lag bias does not become effective. The reason why the time delay does not affect the ATT on durations is that it *extends the durations* until the treatment, measured in absolute values, *equally for both groups*: treated and untreated. Thus, the difference between the durations of treatment and control group remains unaffected by the time lag, and hence, so does the estimated ATT based on durations. In the case of ATT on probabilities, however, and before the impact of the treatment is fully expired, the ATT is tendentially underestimated. The time lag bias is larger the later in the stratum the treatments averagely begin. It has merely a weakening effect, which is limited in time, but it can never turn the ATT into its opposite direction.

**Core bias:** The core bias arises from exit events prior to the treatment. Because we analyze an assumed causality between the treatment as the cause and the probability or the timing of the event as the effect, the cases with reverse order, namely with *exit events before the treatment*, are generally problematic. At best, reverse cases might cause a measured individual treatment effect of zero. This holds true if the probability of the exit events before the treatment would be identically distributed for the treated and the matched control group. Then, the estimated ATT is merely weakened by the observations with a measured 'zero effect'. The core bias, which in this case causes only a 'weakening effect', is higher the larger the share of observations with reverse order, and thus, the more often observations with a measured individual effect of zero occur. Only in the special case where there is actually no treatment effect, the distortion caused by the zero effect of the pre-treatment events would be zero. Otherwise, namely if there is a positive or negative treatment effect, the estimated ATT is weakened by the bias; but in this case the core bias cannot turn the effect into the opposite direction. In this particular constellation, facing identically distributed pre-treatment events in the treatment and in the matched control

group, the core bias is least problematic. In contrast, much more problematic is the typical case most often discussed in the literature, where exit events lead 'out of risk' to be treated. In our study, this concretely refers to exit events that lead out of welfare receipt and, thus, out of risk to be sanctioned. This kind of exit event results in a probability of zero for the treatment group that the event occurs before the treatment. Hence, the higher the probability of exit events before the treatment of the matching partner is for the people in the control group, the stronger the core bias to the detriment of the treated, which causes a negative bias of the ATT on probabilities, and an accordingly positive bias of the ATT on durations. Thus, a negative treatment effect on probabilities would be strengthened by the negative core bias, and a positive treatment effect would be reduced or the estimations of the ATT would even turn into the opposite direction.

As we see already in this first glance at the core bias under different conditions, its effects are strongly determined by the kind of exit events under study, and by other specific circumstances. Therefore, in the following, we provide more detailed information about the core bias under various conditions and in the context of certain kinds of exit events which we analyze in our study:

### **Exit events out of risk: exit from welfare and to mere employment**

Examples of exit events leading out of risk to be sanctioned are exit to mere employment ("*only job*") (*O*), and exit from welfare (*ExWel*) which also comprises the non-employment option of leaving the labor market. As mentioned above, under these circumstances, the probability of exit events before the treatment is zero in the treatment group, while the probability of exit events prior to the treatment of their matching partner in the control group is usually non-zero. This causes a negative core bias, which weakens a potential positive treatment effect or may even turn the estimations of the ATT into the opposite direction, namely into negative values of the ATT on probabilities. In contrast, the negative core bias would strengthen a potential negative treatment effect, and thus result in even stronger negative values of the ATT. The more right-skewed the distribution of treatments within the stratum is, and the more likely early exit events in the group of non-treated are, the more severe the expected negative core bias will be.



**Exit event staying at risk: exit to employment with top-up benefits**

The exit event into employment with supplementary welfare receipt ( $S$ ) induces a change of the labor market status accompanied with staying at risk to be sanctioned even after the event takes place. For this kind of transition from unemployed to employed welfare receipt, there are different constellations depending on the probability of pre-treatment events of the treatment group compared to the group of non-treated. The average difference between the pre-treatment outcomes of the two groups, in turn, depends on the probabilities of treatments in the two labor market states: before and after the event happens. Concretely, the average outcome difference before the treatment depends on whether unemployed compared to employed welfare recipients are more, less, or equally likely to be sanctioned and how large the potential individual differences averagely are.

**Reverse causality (indirect):** If the average probabilities of treatments in the pre- and post-event status are different, we are confronted with the problem of reverse causality: then the likelihood to be treated depends on the exit event. That is the reverse causality of what we initially intended to analyze. Even if the event — taking up employment with supplementary benefit receipt — generally does not directly cause a sanction, there may be an indirect causality, mediated by a potentially unequal probability to be sanctioned in the pre- and post-event status, concretely in the status of unemployed compared to employed welfare receipt.

**Equal probability of pre-treatment events for treated and non-treated:** As mentioned above, giving an example of the effects of the core bias under various circumstances, this constellation arises under the precondition of identically distributed exit events before the treatment for the treated and the matched control group. Under this precondition, the probabilities of pre-treatment events for treated and non-treated are equal and, hence, the maximal effect of the core bias is reduced to a distortion which can only weaken a potential positive or negative treatment effect but cannot turn it into the opposite direction. The reason for this is that the exit events prior to the treatment induce a measured individual treatment effect of zero, caused by the identical probability distribution of pre-treatment events for both groups: treated and matched controls.

The larger the share of observations with reverse order, and thus with a measured individual treatment effect of zero, the stronger the weakening effect of the bias. This constellation with equal probabilities of pre-treatment events for both groups can only occur if the exit event does not affect the likelihood of the treatment. Concretely, in order to make this constellation feasible for exit into supplementary welfare receipt, the probabilities of being sanctioned must be equal for unemployed and employed welfare recipients.

**Lower probability of pre-treatment events for the treated:** Under these conditions, a bias to the detriment of the treated, or more precisely to the detriment of the treatment effect, occurs. Hence, the core bias negatively distorts the ATT on probabilities. If there is a huge number of pre-treatment events in the control group and only a few of these events in the treatment group, the negative bias can be strong enough to not only weaken a potential positive treatment effect, but to even turn it into negative effect estimations. An already negative treatment effect, however, would lead to even stronger negative values of the estimated ATT. This goes along with the statements to the core bias in case of an exit event out of risk causing a probability of pre-treatment events for the treated of zero, which is the extreme case — or technically speaking, the marginal case — of this category of circumstances causing a core bias.

**Higher probability of pre-treatment events for the treated:** In this case, a bias in favor of the treated, or more precisely in favor of the treatment effect, occurs. Hence, the core bias positively distorts the ATT on probabilities. In extreme cases with many pre-treatment events in the treatment group and considerably less of such events in the group of matched non-treated, the positive bias may be strong enough to not only reduce a potentially negative effect but to even turn it into a positive estimation of the ATT. In contrast, an already positive sanction effect would lead to even stronger positive estimations, and hence a positive ATT would be overestimated.

#### **Exit event staying at risk: exit from job to mere welfare**

Another exit event not leading out of risk, apart from exiting to supplementary welfare receipt, is the converse case of exiting employment with top-up benefits to mere welfare receipt (*ExJob*). Theoretically, the same constellations with

similar consequences are possible, like the previously described cases for exit to supplementary welfare receipt. In practice, however, exiting employment for mere welfare receipt is an event with a high probability of reverse causality directly caused by the exit event which tendentially leads to a strong positive core bias.

**Reverse causality (direct):** As ‘culpably’ losing a job is a compelling reason to impose sanctions against welfare recipients, the likelihood that, in the case of reverse order of treatment and exit event, the event of exiting employment is the reason for the sanction, and thus the direct cause of the treatment. Such cases of reverse causality lead to a higher probability of pre-treatment events for the treatment group compared to the matched controls. This causes a positive bias in the ATT on probabilities which might not just weaken a potential negative effect, but even turn it into positive effect estimations, while an already positive ATT would be strengthened and thus be overestimated. This constellation is similar to the above described case of higher probability of pre-treatment events for the treated compared to the non-treated for exits into supplementary welfare receipt. The main difference with exit into mere welfare is that the latter one is most probably distorted considerably more severely into the positive direction than in the case of exits to supplementary welfare receipt.

### **Further thoughts to exit events staying at risk**

After the previous overview on diverse constellations of the two analyzed exit events staying at risk — exit to supplementary, and exit to mere welfare receipt — we reflect about the factors that determines which kind of constellation occurs.

If the event, or more precisely the change of the labor market status caused by the event, does not affect the likelihood to be treated, the probability of the pre-treatment event should be equal for treatment and non-treatment group. The reason for this is that the matching procedure per definition targets at equal non-treatment outcomes for both groups: treatment and matched controls. In this case, the core bias is minimal and has only a weakening effect.

In contrast, if the probability of being sanctioned is affected by the exit event, the probabilities of pre-treatment events for treatment and control group are not equal. If the treatment is less likely after the event, pre-treatment events for the

treatment group would be less likely than for the control group. The extreme case would be a probability of pre-treatment events of zero for the treatment group which leads to a maximal negative potential core bias if the probability of pre-treatment events is more than zero.

In the opposite case, if the treatment is more likely after the event, pre-treatment events for the treatment group would be more likely than for the matched non-treated. Thus the potential core bias would work in the positive direction. This is even more the case when the exit event directly causes the treatment which is quite probably the case for at least some of the observations of exiting employment before getting sanctioned. Then the probability of pre-treatment events for the treatment group tends to be much higher than for the controls. This causes a strongly positive core bias. The more cases with reverse causality, the stronger the positive core bias.

We have to take into account the effects of the aforementioned bias when we interpret the results in Chapter 5.5.

## 5.5 Results

In this section, we present the estimation results of propensity score matching (PSM), applying kernel-based matching for the inflow sample into welfare 2008.<sup>78</sup> Specifically, we report the estimated average treatment effects on the treated (ATT) of welfare sanctions on the probability of different outcome events, and on the duration until these events occur. For *unemployed* benefit recipients (Section 5.5.1), we analyze the sanction effect on taking up employment — differentiating between mere employment, and employment while receiving supplementary UB II, i.e. top-up welfare benefits — and on completely leaving welfare receipt.<sup>79</sup> For *employed* benefit recipients (Section 5.5.2), we carry out basically the same analysis, but restrict the employment options to exit into mere employment, and consider the additional option of exiting employment with

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<sup>78</sup>Further details of the matching approach, i.e. the matching algorithms and the inflow samples we apply, are given in Section 5.4.3.2; our findings from robustness checks based on comparing the results of different matching algorithms and inflow samples are addressed in Section 5.5.4.

<sup>79</sup>The difference between ‘taking up mere employment’ and ‘exit from welfare receipt’ is that the latter one is more comprehensive as it additionally comprises the non-employment option (see Sections 5.5.1.2 and 5.5.2.2).

top-up benefits for mere welfare receipt. In Section 5.5.3, we present the results of PSM analysis for the case of being affected merely by *indirect* sanctions, i.e. by sanctions against related household members. And finally, in Section 5.5.4, we give information on our checks of matching quality and sensitivity analysis and the findings we get from these.

In addition to the two main groups of unemployed and employed welfare recipients, we separately analyze the effects of welfare sanctions on several sub-groups: first, we define three age groupings: people *under 25 years* (*u25*), who face stricter monitoring and sanction conditions than the older ones, people aged *25 years and over* (*o25*), and the whole range of ages (*all*) comprising *u25* and *o25*; second, we differentiate between people who live in a federal state belonging to (the former) *West* or *East* Germany; and third, we differentiate between people of three different levels of labor market access: *low*, *middle*, *high*.<sup>80</sup>

As outlined in Section 5.3.3, our analysis are based on two distinct kinds of outcome variables: metric duration outcomes and monthly updated binary outcomes. The *metric* outcomes, measured in days from the beginning of each stratum ( $S_i$ ) of welfare duration<sup>81</sup>, represent the *durations* until a specific event takes place, e.g. employment entrance or exit from welfare. Thus the estimated ATT, calculated as the difference between mean outcomes for the treatment group and the matched control group, reveals the average number of days a sanctioned person is expected to stay longer (*positive ATT on durations*) or shorter (*negative ATT on durations*) in welfare receipt until the event happens, compared to the case of not having been sanctioned. This illustrative interpretation of the ATT, revealing the sanction effect as the difference of average welfare duration until a specific event occurs, is an advantage of the approach of metric outcomes. A disadvantage of using durations as outcome variables is that right-censored spells, i.e. cases without the specific (exit) event taking place in the observation period, drop out of the analysis of metric outcomes.

The *monthly updated binary* outcome variables, by contrast, indicate whether a specific event occurs within a defined time period  $P_j$ , beginning at the start of stratum  $S_i$  and ending on the last day of the final month  $m_j$ . Hence, the estimated ATT represents the difference in average *probabilities* that the event

<sup>80</sup>Further information on the variable ‘labor market access’ are given in Section 5.3.1.

<sup>81</sup>In the case of direct sanctions,  $S_i$  are quarterly strata of welfare duration with  $i=1-4$ ; in the case of indirect sanctions,  $S_i$  are half-yearly strata with  $i=1-2$ .

takes place within the time period  $P_j$ , for the sanctioned, and matched non-sanctioned groups. One advantage of this approach is that it shows the development of the cumulative ATT over time, revealing the effects of welfare sanctions in the short, medium and long run,<sup>82</sup> which we exemplarily illustrate by a wide selection of *plotted ATT on probabilities* and their 90% confidence intervals<sup>83</sup>, shown in the appendix, Figures A.1–A.37. In the case of *direct* sanctions, we estimate the ATT over two years time — that is for final months  $m_j$  with  $j=1-24$ , and in the case of *indirect* sanctions over a period of one and a half year — that is for final months  $m_j$  with  $j=1-18$ .

But even the *tables* with condensed information on the estimation results of *binary* outcomes provide additional information compared to the ATT on durations. These summaries expose the *number of* (overlapping) periods with *significant* (positive/negative) ATT on probabilities, summarized over a certain number of overlapping periods,  $P_j$ , which we have restricted to periods with final months  $m_j$  subsequent to the end of stratum  $S_i$ .<sup>84</sup> Beyond the different definition and interpretation of binary outcome variables, and thus of their derived ATT, a critical reason for the increase of informative value is that we need not restrict the analysis to uncensored spells, which is another major advantage of using binary outcomes.

Let us illustrate this by an example: in the case of the exit event ‘employment entrance’, welfare spells of people who don’t find a job within the observation period cannot be a part of the analysis of metric outcomes, as their welfare durations until employment entry are unknown. However, these so-called ‘right-censored’ spells are included in the analysis based on binary outcomes. This might lead to significant sanction effects on the probability to take up employment, even if the ATT on the duration until employment entrance is insignificant.

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<sup>82</sup>In this paper *short-term* effects refer to a period of three months maximum, *medium-term* effects to more than three until twelve months time, and *long-term* effects to periods of more than twelve months.

<sup>83</sup>We illustrate the ATT plots with confidence intervals (ci) of 90% corresponding to the significance level of  $\alpha=0.1$  used for the tables with binary outcomes.

<sup>84</sup>We initially calculate the monthly updated ATT of binary outcomes for periods,  $P_j$ , starting with final month,  $m_j$ , after the beginning of the stratum, i.e. starting with  $m_j=m_1$ . Due to the bias during the strata caused by not taking into account the exact time of treatment within the stratum (see Section 5.4.4.3), however, we restrict the evaluation of our results to the time periods  $P_j$  with final months  $m_j$  subsequent to the end of strata. Specifically, we consider final months  $m_j$  with  $j=4-24$  following the end of quarterly strata in case of direct sanctions; in the case of indirect sanctions, we consider final months  $m_j$  with  $j=7-18$  following the end of half-yearly strata.

A further reason why using binary outcomes could reveal significant effects, even if the analysis of durations does not, is that the extent of the treatment effect as well as its direction can vary over time in a way that leads to such seemingly contradictory results. This could be the case if the sanction effect on the probability of a specific event turns out to be, for example, positive in the first few months and changes direction in the following months, and thus the later monthly effect (at least partly) neutralizes the earlier one, which might lead to an insignificant effect on the duration as a whole.

It is important to recognize that, in contrast to a *positive ATT of metric outcomes* which, for sanctioned people implies a politically *unintended increase in average duration* until a specific event takes place (e.g. employment entry or exit from welfare), a *positive ATT of binary outcomes* indicates an *increase in average probability* that the specific event takes place within a defined time period and, thus, works in the politically *intended* direction; for *negative ATT*, of course, the reverse is valid.

As explained in Section 5.4, we have to consider that the matching procedure combined with stratification may cause a bias to the disadvantage of the treated, which could lead to negatively biased estimations of the ATT on probabilities and positively biased ATT on durations. Even if the bias arises from exit events during the stratum, the estimations for final months  $m_j$  after the end of stratum (i.e. with  $j > 3$  in case of direct sanctions, respectively with  $j > 6$  in case of indirect sanctions) can also be negatively affected, as — calculating the ATT on probabilities for overlapping periods  $P_j$  — the exit events within the stratum are still contained in  $P_j$ . However, the later the final month occurs, the higher the number of cases with exit events after the end of stratum is; and therefore, the share of possibly biased cases diminishes, concurrently the entire potential bias shrinks.

As we cannot verify or quantify the potential bias, we initially present the pure numeric results as a first step, then relativize the results below by discussing the insights that we gain from the plotted ATT regarding the possible bias as a second step; finally, we discuss our findings more comprehensively, reflecting on the plausibility in general, and comparing them with previous studies, in Section 5.6.

### 5.5.1 Unemployed welfare recipients

Initially, we present the estimation results for unemployed welfare recipients who are the main target group of activation policy in the German welfare system under 'Hartz IV'. Unemployed people, in particular, are at the focus of the monitoring and sanction regime, with the goal of speeding up their employment entrance, and exiting their receipt of welfare benefits.

#### 5.5.1.1 Transition from unemployment to employment

Investigating the sanction effects on transition from unemployment to employment, we distinguish between three exit events: first, exit to *mere employment* which means *without welfare receipt* ( $O$ : 'job only'); second, exit to *employment with top-up benefits* ( $S$ : 'supplementary'); and third, exit to *employment in general* ( $G$ : 'job in general') which consists of 'job only' ( $O$ ) and job with additional welfare receipt ( $S$ ) without any distinction.

Table 5.3 shows the estimation results of *metric* outcomes, namely the average treatment effects on the treated (ATT) of benefit sanctions on welfare *duration* until employment entrance, for the first two quarterly strata for unemployed men who entered welfare (UB II) in 2008. For women, the estimated ATT of the metric outcomes turned out to be either not significant or not sufficiently reliable — the latter one dominating because of too few observations in the treatment group, which is defined in cases of less than 50 treated persons.

Within the reliable results (black figures) in Table 5.3 for men, the ATT of just the exit into mere employment case for the second stratum ( $S_2$ ) turned out to show significant sanction effects. Concerning the whole range of ages from 15 to 56 years (*all*), those men who are sanctioned during the second stratum of their welfare receipt need, on average, 70.14 days longer to find a job with sufficient earnings to bring them out of welfare receipt than the unsanctioned control group. For men aged 25 years and over ( $o25$ ), the sanction effect on the duration until leaving welfare for mere employment is a bit smaller: more precisely, the sanctioned need, on average, 57.02 days longer than if they would not have been sanctioned. Also, for the sub-groups of unemployed men in Western Germany, and male unemployed with mid-level labor market access, we get significant positive effects on welfare durations in the case where sanctions were imposed during the second stratum ( $S_2$ ). These results, being counter to



the political intention of speeding up employment entrance, could — at least partly — be caused by the above mentioned potential bias to the detriment of the treated, which we briefly elucidate at the end of this section and discuss more detailed at the end of Section 5.5.1.2 and in Section 5.6.

TABLE 5.3: Unemployment duration until employment entrance — men 2008

Age	Region	Market access	Exit to job (G/O/S) <sup>2</sup>	ATT <sup>1</sup>	
				S <sub>1</sub>	S <sub>2</sub>
all			G	3.49	21.30
all			O	-18.47	70.14***
all			S	20.41	4.71
all	West		G	0.05	7.55
all	West		O	-25.35	65.06**
all	West		S	20.96	6.01
all		middle	G	-7.92	13.70
all		middle	O	-19.61	84.37***
all		middle	S	1.88	-4.32
all		high	G	13.03	177.93*
all		high	O	4.95	240.59***
all		high	S	38.44	202.91*
u25			G	0.81	29.25
u25			O	12.51	92.33**
u25			S	6.07	1.03
u25	West		G	-1.69	28.40
u25	West		O	-16.81	139.99**
u25	West		S	-5.61	-69.42
o25			G	8.72	10.64
o25			O	-22.15	57.02**
o25			S	25.97	7.44
o25	West		G	7.42	1.82
o25	West		O	-33.78	40.14
o25	West		S	34.21	10.69

<sup>1</sup>ATT of *metric outcomes*: difference of mean durations until employment entrance, measured in days from the start of quarterly stratum of welfare duration (S<sub>i</sub>).

<sup>2</sup>Events: exit to: job only (O), i.e. without welfare receipt, job with supplementary welfare receipt (S), job in general (G), i.e. comprising (O) and (S).

Subgroups: age-group in years: all=15–56, u25=15–24, o25=25–56; region: West/East German states; labor market access: low, middle, high.

Significance levels:  $\alpha=0.1^*$ ,  $\alpha=0.05^{**}$ ,  $\alpha=0.01^{***}$ .

Gray figures: not reliable results (<50 treated cases).

TABLE 5.4: Unemployed's employment entrance — men 2008

Age	Region	Market access	Exit to job (G/O/S) <sup>2</sup>	Number of periods with sign. ATT <sup>1</sup>			
				S <sub>1</sub>	S <sub>2</sub>	S <sub>3</sub>	S <sub>4</sub>
all			G	n.s.	n.s.	5(+)	n.s.
all			O	n.s.	21(-)	20(-)	2(-)
all			S	n.s.	3(+)	15(+)	n.s.
all	West		G	n.s.	n.s.	9(+)	3(-)
all	West		O	n.s.	16(-)	17(-)	n.s.
all	West		S	n.s.	15(+)	16(+)	n.s.
all	East		G	3(-)	2(-)		
all	East		O	1(+)	14(-)		
all	East		S	13(-)	n.s.		
all		low	G	2(-)			
all		low	O	17(-)			
all		low	S	n.s.			
all		middle	G	n.s.	n.s.	3(+)	2(-)
all		middle	O	n.s.	18(-)	20(-)	2(-)
all		middle	S	1(-)	3(+)	4(+)	n.s.
all		high	G	n.s.			
all		high	O	1(+)			
all		high	S	1(-)			
u25			G	n.s.	1(-)	n.s.	
u25			O	n.s.	9(-)	7(-)	
u25			S	n.s.	n.s.	8(+)	
u25	West		G	n.s.	n.s.	2(+)	
u25	West		O	6(-)	1(-)	n.s.	
u25	West		S	3(-)	1(-)	1(-)	
o25			G	n.s.	n.s.	2(+)	3(-)
o25			O	n.s.	21(-)	20(-)	13(-)
o25			S	7(-)	4(+)	n.s.	1(-)
o25	West		G	n.s.	n.s.	2(+)	7(-)
o25	West		O	n.s.	13(-)	15(-)	14(-)
o25	West		S	n.s.	17(+)	3(+)	5(-)
o25	East		G	3(-)	2(-)		
o25	East		O	1(+)	9(-)		
o25	East		S	4(-)	1(-)		

<sup>1</sup> ATT of *binary outcomes*: difference of mean probabilities of employment entrance until the end of month  $m_j$  after start of quarterly stratum of welfare duration ( $S_i$ ); number of positive (+) and/or negative (-) significant ATT, summarized over periods  $P_j$  with final months  $m_j$ , considering  $j=4-24$ , or no significant (n.s.) ATT in any of these periods, on a significance level of  $\alpha=0.1$ .

<sup>2</sup> *Events*: exit to: job only (O), i.e. without welfare receipt, job with supplementary welfare receipt (S), job in general (G), i.e. comprising (O) and (S).

*Subgroups*: age-group in years: all=15-56, u25=15-24, o25=25-56; region: West/East German states; labor market access: low, middle, high.

As explained above, there are substantial merits to extend the analysis of durations by additionally calculating the *ATT on probabilities* using (monthly updated) *binary* outcomes which indicate different labor market states: firstly, right-censored observations with unknown duration until employment entrance are also included in the analysis, which is expected to reduce problems caused by too few observations in the treatment group. Secondly, the monthly updated binary outcomes, at least for several periods  $P_j$ , can show significant effects on the probability of getting a job, even if the effect on the duration until employment entrance is insignificant. Indeed the monthly updated ATT on probabilities based on binary outcomes, presented in Table 5.4 (men) and Table 5.5 (women), yield significant results in many more cases than the ATT based on durations.

On the one hand, the significant *positive* ATT on *durations* for men, presented in Table 5.3, are confirmed by the corresponding significant *negative* ATT on *probabilities* in Table 5.4, which indicates lower mean probabilities of getting a job for sanctioned men compared to the fictive case of not having been sanctioned. According to this, the ATT of binary outcomes for exit to 'job only' ( $O$ ) show significantly *negative* effects of sanctions imposed during the second stratum ( $S_2$ ) for all 21 considered periods  $P_j$  with final months  $m_j$  after end of stratum  $S_i$  (i.e. for  $j = 4, \dots, 24$ ) for the following two groups: unemployed men of the whole range of ages (*all*), that is 15–56 years, and unemployed men aged 25 years and above (*o25*). These significantly negative ATT of binary outcomes imply that the average probability of sanctioned people leaving welfare receipt for *mere employment* ( $O$ ) before the end of the final month  $m_j$  is significantly lower than for the unsanctioned control-group. Hence, the negative ATT of binary outcomes confirm the economically and politically unintended direction of sanction effects on taking up employment without benefit receipt for unemployed men, which we likewise get from the metric outcomes.

On the other hand, the binary outcomes reveal a much more differentiated picture than the metric outcomes, showing plenty of periods  $P_j$  with significant negative as well as positive ATT, even if the durations do not yield reliable and significant estimates: in contrast to duration outcomes, binary outcomes for unemployed men lead to significant ATT not only for sanctions in the second stratum of welfare receipt ( $S_2$ ), but — depending on the subgroup — for at least a few periods,  $P_j$ , of each of the first four quarterly strata ( $S_1, \dots, S_4$ ). Moreover, they yield significant treatment effects for further subgroups of unemployed men (e.g. living in East Germany, having a low level of labor market

access), as well as for unemployed women. Furthermore, significant effects of welfare sanctions are not only shown for the exit to ‘job only’ ( $O$ ), but also to employment with supplementary benefit receipt ( $S$ ) and employment in general ( $G$ ).

Considering only cases — where ‘cases’ here refers to (sub)groups combined with strata  $S_i$  — with at least 14 of the regarded 21 periods  $P_j$  with significant ATT in Table 5.4 for unemployed men, reveals *negative* sanction effects only for the probability to enter *mere employment* ( $O$ ), whereas *positive* effects appear only for entering *employment with top-up benefits* ( $S$ ). In other words, regarding

TABLE 5.5: Unemployed’s employment entrance — women 2008

Age	Region	Market access	Exit to job (G/O/S) <sup>2</sup>	Number of periods with sign. ATT <sup>1</sup>		
				S <sub>1</sub>	S <sub>2</sub>	S <sub>3</sub>
all			G	n.s.	n.s.	19(+)
all			O	1(-)	7(-)	n.s.
all			S	n.s.	n.s.	21(+)
all	West		G	1(+)	n.s.	21(+)
all	West		O	n.s.	3(-)	1(-)
all	West		S	n.s.	n.s.	21(+)
all		middle	G	n.s.	n.s.	21(+)
all		middle	O	2(-)	n.s.	n.s.
all		middle	S	1(-)	n.s.	21(+)
u25			G	n.s.		
u25			O	4(-)		
u25			S	n.s.		
u25	West		G	21(+)		
u25	West		O	n.s.		
u25	West		S	n.s.		
o25			G	2(-)	n.s.	21(+)
o25			O	n.s.	n.s.	1(+)
o25			S	10(-)	n.s.	21(+)
o25	West		G	1(-)		
o25	West		O	n.s.		
o25	West		S	4(-)		

<sup>1</sup> ATT of *binary outcomes*: difference of mean probabilities of employment entrance until the end of month  $m_j$  after start of quarterly stratum of welfare duration ( $S_i$ ); number of positive (+) and/or negative (-) significant ATT, summarized over periods  $P_j$  with final months  $m_j$ , considering  $j=4-24$ , or no significant (n.s.) ATT in any of these periods, on a significance level of  $\alpha=0.1$ .

<sup>2</sup> *Events*: exit to: job only (O), i.e. without welfare receipt, job with supplementary welfare receipt (S), job in general (G), i.e. comprising (O) and (S).

*Subgroups*: age-group in years: all=15–56, u25=15–24, o25=25–56; region: West/East German states; labor market access: low, middle, high.

cases with a high persistence of significant ATT, sanctions against unemployed men on average lower their chance of finding a job which is well-paid enough to bring them out of benefit receipt, but raise the probability of getting a job needing supplementary welfare benefits.<sup>85</sup>

A high persistence of *positive* sanction effects for *male* unemployed occurs only in a few cases. To be precise, cases with at least 14 of 21 periods  $P_j$  of positively significant ATT show up only for the transition into *job with top-up benefits* ( $S$ ), only for sanctions imposed during the second and/or third stratum ( $S_2$ ,  $S_3$ ) of their welfare receipt, and only for the following three groups: the total of unemployed men (*all*), male unemployed living in Western Germany (*West*), and those who live in West Germany and are 25 years and older (*o25/West*).

For *female* unemployed (Table 5.5), in contrast, *positive* sanction effects dominate the results. Focusing on the cases with more than 10 periods  $P_j$  with significant ATT, positive sanction effects on the probability of getting a job were observed in all cases, for at least 19 of the 21 periods considered. All these cases with a high persistence of positive ATT occur for exit into *employment with top-up benefits* ( $S$ ) and for exit into *job in general* ( $G$ ). Similar to male unemployed, persistent *positive* sanction effects do *not* appear for women taking up *mere employment* ( $O$ ). It is striking that, with one exception, unemployed women show more than 10, namely 19 or all 21 of the regarded periods with significant ATT only for the *third* stratum ( $S_3$ ) — i.e. for sanctions imposed during the 7th to the 9th month of their welfare receipt. The sole exception are female unemployed under 25 years living in Western Germany (*u25/West*) who show all 21 considered periods  $P_j$  with significant positive effects on taking up employment with supplementary welfare benefits ( $S$ ) for sanctions imposed during the *first* stratum ( $S_1$ ).

Rounding out the numerical results for the probability of transition from unemployment to employment presented in Table 5.4 and 5.5, we find distinct and persistent *positive* effects of welfare sanctions only for the probability of taking up employment with *supplementary welfare receipt* ( $S$ ). However, the probability of entering *mere employment* ( $O$ ), i.e. without needing top-up benefits, is either *not affected* significantly and persistently, or is mainly affected *negatively*. Whereas persistent and significant *positive* sanction effects on the probability

<sup>85</sup>The extent to which these results could be caused by a relatively high negative bias for the exit into job only ( $O$ ) and a lower (or even no) bias for the exit into employment with supplementary welfare receipt ( $S$ ) is discussed in Section 5.5.1.2 and Section 5.6, using the findings from the graphs with plotted ATT, which give us deeper insights.

to enter employment show up primarily for unemployed *women*, concerning exit into employment with top-up benefits (*S*), distinct *negative* sanction effects appear almost exclusively for unemployed *men*, concerning exit into mere employment (*O*). As mentioned above, these results may be caused by a relatively high negative bias for the exit into job only (*O*) and a lower (or even no) bias for the exit into employment with supplementary welfare receipt (*S*). Methodological reasons for a tendentially lower negative bias in the case of exits into employment with supplementary welfare receipt (*S*) compared to entering mere employment (*O*) are outlined in Section 5.4.4.3; detailed discussions about the bias and the plausibility of these results are provided in Section 5.5.1.2 and Section 5.6, using the findings from the graphs with plotted ATT presented below.

Deeper insights into the results based on binary outcomes, namely the ATT on probabilities, are obtained from the graphs depicting the monthly updated ATT of welfare sanctions and its 90% confidence interval, which are included as figures in the appendix for selected groups and subgroups as well as for different exit events. Figures A.1 to A.15 show the monthly updated ATT (and its 90% confidence intervals) of benefit sanctions against unemployed UB-II-recipients on their probability to enter employment, separately for women (red) and men (blue), for the various subgroups, and for different categories of employment as exit events.<sup>86</sup> These graphs illustrate the development of the cumulative sanction effect on the probability to take up a job for overlapping periods,  $P_j$ , each starting with the beginning of the quarterly stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of consecutive final months  $m_j$ , representing the months after the beginning of stratum (with  $j=1-24$ ).

According to the summarized information in Tables 5.4 and 5.5, Figure A.1 for the third stratum ( $S_3$ ) of *men* shows 20 significantly *negative* ATT for overlapping periods  $P_j$  with final months  $m_j$  after the stratum, that is for  $P_4-P_{24}$  with  $m_4-m_{24}$ , regarding exit to *mere employment* (*O*), which means without receiving additional welfare benefits. At the same time, Figure A.2 shows a significantly *positive* effect for *women* sanctioned in their third quart of welfare receipt ( $S_3$ ) for all 21 periods  $P_j$  with final months after the stratum ( $P_4-P_{24}$ ).<sup>87</sup> An ATT of around 0.15, for example, as is depicted in Figure A.2 for stratum  $S_3$  of women, reveals a 15 percentage points higher average probability of taking up employment with supplementary UB-II-receipt (*S*) for unemployed who are

<sup>86</sup>An overview of the figures with plotted ATT in the appendix is given in Table A.1.

<sup>87</sup>Cases with confidence intervals (CI) that overlap the zero line do *not* reveal a *significant* treatment effect (ATT) on the significance level of  $\alpha = 0.1$ .

sanctioned in the third quarter of welfare receipt compared to the case of not having been sanctioned. As shown, the plotted ATT, of course, contains the same information, just in another form compared to the tables with condensed results. But the graphs additionally depict the exact values of the ATT and — at least as importantly — show the development of the ATT over time for overlapping periods  $P_j$ , which could give us further insights, possibly also about the potential bias.

Indeed, it is salient that most of the cases with *persistent* negative significant ATT show strongly negative significant ATT already during the stratum, that is, for periods  $P_1$ – $P_3$  with final months  $m_1$ – $m_3$ . This can be seen as a strong hint that, at least in several cases, we are confronted with a considerable bias to the detriment of the treated that might be high enough to possibly turn positive effects into negative results. Because we can see such cases with presumably strong negative distortions even more distinctly for the transition from unemployment to exiting welfare, we discuss this phenomenon in some detail in Section 5.5.1.2, and more comprehensively in Section 5.6.

As we calculate the ATT for *overlapping periods*  $P_j$ , the plots of the ATT for final months  $m_1$ – $m_{24}$  reveal the development of the *cumulative sanction effect over time*, specifically over two years after the beginning of the stratum. In the case of *increasing* (*decreasing*) ATT over time, the *monthly* sanction effect turns out to be *positive* (*negative*). Hence, a nearly *monotonic negative slope* of the plotted ATT exposes that individuals with later exit events (after the stratum) are on average negatively affected by sanctions, independently of whether the total effect might be positive because of a possibly high enough positive effect shortly after the sanction. Put differently, even if the sanction effect is initially positive for people with a relatively high probability of getting a job, people with worse chances on the labor market are on average even less likely to become employed if they are sanctioned than if they would not have been sanctioned. In these cases with nearly monotonic negative slopes of cumulated ATT over a longer period, the *monthly* sanction effect in the medium and long run is negative, regardless of a possibly positive effect in the short run, or even in total.

Indeed, we find such shapes of *decreasing ATT* over a longer time frame (of at least more than one or even two years) for a number of cases, for example, for strata three and four ( $S_3$ ,  $S_4$ ) of unemployed men's exit into mere employment (Figure A.1) as well as for strata  $S_3$  and  $S_4$  of the following subgroups: male unemployed living in Western Germany (*West*) (Figure A.4), men with

medium-level market access (*middle*) (Figure A.7), and men aged over 25 years (*o25*) (Figure A.11), each in terms of exit into job without needing top-up benefits (*'job only'* (*O*)). These graphs with almost monotonous negative slopes imply that the individual's chances of finding a job that brings them out of benefit receipt are, on average, negatively affected by sanctions, at least if they do not belong to the possibly positively affected people with exit events immediately, or short after the sanction.<sup>88</sup> Hence, no matter what the extent and direction of the short term effects, in these cases the *monthly* sanction effects are negative in the medium and long run.

Ordinary economic thinking, however, would lead us to expect a cumulated ATT that is positive and *monotonously increasing* with declining slope. Simplifying and neglecting other aspects, it could be assumed that sanctions must have a distinct positive effect on taking up employment as more pressure on unemployed people would increase their search effort, lower their reservation wages, and let them accept worse job conditions; all of this should lead to a higher probability of taking up a job. This assumed clearly positive effect of sanctions would be expected to be high at the beginning and should decrease with the passing of time, but never become less than zero; hence, the cumulated ATT should increase, or possibly stagnate after a while, but never decrease or even become negative.

Our results, indeed, show a few examples of graphs with plotted ATT which reveal nearly this kind of shape: starting with a positive ATT that is almost monotonously increasing with declining slope. This applies to the third stratum ( $S_3$ ) of men regarding exit to employment with top-up benefits (*S*) (Figure A.2), as well as for men in Western Germany (*West*) under the same conditions (Figure A.5).

But more often, we find shapes with initially increasing, and later slightly decreasing, alternating, or stagnating ATT, which are still not too far away from the above mentioned ideal-typically expected form. Such patterns of plotted ATT for overlapping periods we find, for example, for stratum  $S_3$  of unemployed women (*all*) (Figures A.2 and A.3), of female unemployed living in Western Germany (*West*) (Figures A.5 and A.6), of women with medium-level access

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<sup>88</sup>Even if our numeric results and the plots of ATT display negative ATT of sanctions for the main part of the overlapping periods  $P_j$  — as is the case for many of the previously mentioned subgroups of unemployed men regarding exit into mere employment — we cannot exclude the possibility that the total sanction effect is nevertheless positive, because of a (possibly high enough) bias to the detriment of the treated that we cannot quantify.



to the labor market (*middle*) (Figures A.8 and A.9), and of women aged over 25 years (*o25*) (Figures A.12 and A.13), all in terms of exit to work with supplementary benefits (*S*) and to job in general (*G*). These cases are examples of unambiguously positive sanction effects already within the strata, despite the most likely considerable negative distortions during the stratum. In most of these cases, there is a noticeable shift upwards of around 10 percentage points immediately after the stratum, which is a strong hint of a negative bias of about 0.1 (see stratum  $S_3$  of Figures A.2, A.3, A.5, A.6, A.8, and A.9). Hence, for example, unemployed women living in Western Germany who are sanctioned in their third quarter of welfare receipt are 35 percentage points more likely to take up employment with top-up benefits than if they would not have been sanctioned (see stratum  $S_3$  of Figure A.5).

We have shown that on the one hand, a number of (sub)groups and strata reveal shapes of cumulated ATT over time that are at least close to the shape that is expected by classical economic theory; on the other hand, there is, nevertheless, also a huge bundle of different patterns of plotted ATT that are partly far away from the expected one: there are n-shaped plots, as well as u-shaped, zigzag- or s-shaped plots with nearly monotonic negative slope as mentioned above. Obviously, there is a vast variety of shapes of *cumulated* ATT over time for diverse groups and different strata of welfare receipt, which implies that the *monthly* effects of welfare sanctions on the probability of entering employment are neither clearly positive, nor distinctly negative, but depend on the individuals and their situations concerning their labor market access, how long their welfare receipt already lasts, the timing of the sanction, and many other factors.

### 5.5.1.2 Transition from unemployment to welfare exit

Beyond the aim to expedite the unemployed's taking up employment, another purpose of German welfare policy under 'Hartz IV' — mainly targeting fiscal objectives — is to speed up the unemployed's exit from welfare receipt (*ExWel: 'exit from welfare'*), here namely from UB-II-receipt. The main difference between transition to mere employment and transition to welfare exit is that the latter one also includes the option to leave the labor market, the so-called non-employment option.<sup>89</sup>

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<sup>89</sup>For more details about the non-employment option, which means neither being (self-)employed nor receiving unemployment benefits, see Section 5.1 and Hillmann and Hohenleitner (2015).

The effects of sanctions on the transition from unemployment to welfare exit (*ExWel*), measured in days of welfare duration, and presented in Table 5.6 for unemployed men who entered UB-II-receipt in 2008, go widely along with the results for transition to mere employment (*O*) shown in Table 5.3. The same subgroups of unemployed men who are sanctioned in their second stratum ( $S_2$ ) of welfare receipt and show significant positive ATT on durations for exit into ‘job only’ (*O*) (Table 5.3) reveal significant positive ATT also in terms of duration until exiting welfare (Table 5.6). That means, sanctioned unemployed men need averagely more time than the matched unsanctioned until they get a job which is well-paid enough to bring them out of benefit receipt, or until they leave UB-II-receipt including the non-employment option. For example, male unemployed with medium level of labor market access (*middle*) need on average 108.64 days longer to leave welfare receipt if they are sanctioned in the second stratum than without being sanctioned. For women there is no significant ATT on durations within the reliable results for any of the four strata, as it is also the case for exit to employment (see Section 5.5.1.1).

TABLE 5.6: Unemployed’s welfare duration — men 2008

Age	Region	Market access	ATT <sup>1</sup>			
			S <sub>1</sub>	S <sub>2</sub>	S <sub>3</sub>	S <sub>4</sub>
all			29.61	94.71***	126.59***	69.45**
all	West		43.34*	81.79***	151.65***	98.17**
all	East		-54.76	147.21**	59.26	103.04
all		low	114.98**	13.91	22.38	177.87*
all		middle	24.15	108.64***	138.65***	45.51
all		high	-42.25	128.16	165.23	n.r.
u25			87.02**	118.65**	132.95**	66.62
u25	West		112.89**	80.38	78.41	-1.58
o25			16.91	89.19***	139.58***	83.43**
o25	West		27.97	83.33**	157.07***	110.13***
o25		low	96.04*	15.51	25.98	345.31***
o25		middle	12.88	110.25***	154.79***	57.46
o25		high	-74.35*	99.07	146.07	n.r.

<sup>1</sup> ATT of *metric outcomes*: difference of mean durations until exit from welfare receipt, measured in days from the beginning of quarterly stratum of welfare duration ( $S_i$ ).

*Subgroups*: age-group in years: all=15–56, u25=15–24, o25=25–56; region: West/East German states; labor market access: low, middle, high.

*Significance levels*:  $\alpha=0.1^*$ ,  $\alpha=0.05^{**}$ ,  $\alpha=0.01^{***}$ .

*Gray figures*: not reliable results (<50 treated cases); n.r.: no results.

It is remarkable that for each of these cases with significant positive ATT on durations in Table 5.3 and Table 5.6, the ATT for exiting welfare (*ExWel*) is larger than for entering mere employment (*O*). So the whole range of ages (15–56 years) (*all*) of unemployed men who are sanctioned in their second quarter of UB-II-receipt ( $S_2$ ) need, on average, 94.71 days longer to leave welfare receipt (Table 5.6), but just 70.14 days longer to enter mere employment (Table 5.3), each compared to the matched unsanctioned control group. Hence, according to these numeric results, welfare sanctions seem to not only lower male unemployed's chances of entering mere employment, but also their probability of leaving welfare receipt via the non-employment option. However, it has to be emphasized that these results are most likely distorted by a bias to the detriment of the sanctioned, which tends to be relatively high for exit events which exclude continued welfare receipt, i.e. exit to mere employment (*O*) and exit from welfare (*ExWel*), as explained in Section 5.4.4.3, discussed in more detail, based on the plotted ATT, later in this section, and finally examined from a more comprehensive point of view in Section 5.6.

Analyzing unemployed's transition to welfare exit reveals more significant results than transitions into mere employment. A simple reason is that examining 'exit from welfare' (*ExWel*) produces (at least slightly) less right-censored spells than examining 'exit to job only' (*O*), because exiting welfare includes the non-employment option, and hence includes additional exit events. Thus, we get significant positive ATT for exit from UB-II-receipt (*ExWel*) not only for the second stratum ( $S_2$ ) as for entering 'job only' (*O*) in Table 5.3, but for quite a few cases for each of the four examined strata ( $S_1$ – $S_4$ ).

One prominent result of Table 5.6 is the significant negative ATT for unemployed men aged over 25 years with a high degree of labor market access (*o25/high*), which is the only ATT on durations that we have got in the politically intended direction. People of this group who are sanctioned in their first quarter of welfare duration ( $S_1$ ) leave UB-II-receipt on average 74.35 days earlier than members of the matched control group. For this subgroup of men with good chances on the labor market, and who sanctioned in an early state of their unemployment, an outstanding effect in the intended direction to shorten welfare receipt seems quite plausible.

The results for the binary outcomes with predominantly significant *negative* ATT on probabilities presented in Table 5.7 for men largely confirm the significant *positive* ATT on durations in Table 5.6. But there are also a few cases with

TABLE 5.7: Unemployed's exit from welfare — 2008

Age	Region	Market access	Men				Women
			Number of periods with sign. ATT <sup>1</sup>				S <sub>1</sub>
			S <sub>1</sub>	S <sub>2</sub>	S <sub>3</sub>	S <sub>4</sub>	S <sub>1</sub>
all			2(+)/3(-)	20(-)	21(-)	6(-)	n.s.
all	West		5(-)	12(-)	21(-)	15(-)	n.s.
all	East		5(+)	9(-)			10(-)
all		low	12(-)	n.s.			3(-)
all		middle	2(+)	20(-)	21(-)	1(-)	n.s.
all		high	4(+)	2(-)			3(+)
u25			16(-)	14(-)	10(-)		n.s.
u25	West		18(-)	2(-)			n.s.
o25			3(+)/2(-)	13(-)	21(-)	17(-)	n.s.
o25	West		3(+)/2(-)	5(-)	21(-)	16(-)	n.s.
o25	East		9(+)	1(-)			
o25		low	7(-)	n.s.			
o25		middle	3(+)	11(-)	21(-)	6(-)	2(-)
o25		high	8(+)				

<sup>1</sup> ATT of *binary outcomes*: difference of mean probabilities of exit from welfare receipt until the end of month  $m_j$  after start of quarterly stratum of welfare duration ( $S_i$ ); number of positive (+) and/or negative (-) significant ATT, summarized over periods  $P_j$  with final months  $m_j$ , considering  $j=4-24$ , or no significant (n.s.) ATT in any of these periods, on a significance level of  $\alpha=0.1$ .

*Subgroups*: age-group in years: all=15-56, u25=15-24, o25=25-56; region: West/East German states; labor market access: low, middle, high.

significant *positive* ATT on probabilities in Table 5.7, mainly for men and with at least medium or even high level of labor market access (*middle, high*), and for men living in Eastern Germany (*East*), in all cases exclusively for sanctionees in the first stratum ( $S_1$ ). For women, the results were predominantly not significant, except for a few cases in the first stratum. Whereas women living in Eastern Germany (*East*) sanctioned in their first quarter of receiving welfare benefits ( $S_1$ ) show relatively persistent *negative* ATT on probabilities,<sup>90</sup> women with early sanctions ( $S_1$ ) and a high degree of labor market access show just a few overlapping periods  $P_j$  with *positive* ATT on probabilities.

In order to get deeper insights into the results for the binary outcomes, we focus

<sup>90</sup> As further discussed in Section 5.6, the relatively persistent negative ATT for the first stratum ( $S_1$ ) of women living in Eastern Germany could be caused by a relatively high bias to the detriment of the treated because of a higher propensity to work of women in the Eastern part of Germany, and thus a higher probability for early exit events of the untreated.

on the plotted ATT shown in Figures A.16 to A.27 of the appendix for the transition from unemployment to exit from welfare (*ExWel*).<sup>91</sup> As seen above for the transition into employment presented in Section 5.5.1.1, the graphs for exiting welfare also reveal a huge variety of diverse shapes of plotted ATT. And also similar to the results for employment entrance, there are several cases with indications of high negative distortions which emerged during the stratum, at least if the results are persistently negatively significant. Such patterns in the graphs we find mainly for transition into mere employment (*O*) and exiting welfare (*ExWel*). However, they occur more often and intensely for exit from welfare, as for example, in the second and third stratum ( $S_2, S_3$ ) of men, presented in Figures A.16, A.17, A.20, A.22, A.24, and A.26 which, for male unemployed and several subgroups, show predominantly persistent negative significant (cumulated) ATT over the full considered period of two years from the beginning of the strata.

Nevertheless, there are also cases without stable negative significant results, which show similar patterns with clearly negative deviations during the stratum compared to the following periods  $P_j$  with final months  $m_j$  (immediately) after the stratum (i.e. with  $j > 4$ ). Such kinds of patterns occur for men as well as for women, often with no or just a few significant results for periods with final months after the stratum. So the second stratum ( $S_2$ ) of unemployed women (Figure A.16), the second until fourth stratum ( $S_2-S_4$ ) of female unemployed in Western Germany (*West*) (Figure A.17), and the second stratum ( $S_2$ ) of women with medium-level labor market access (*middle*) (Figure A.20), of women aged 25 years or older and living in Western Germany (*o25/West*) (Figure A.25), and of women over 25 years with medium-level market access (*o25/middle*) (Figure A.26) have notably lower ATT during the stratum compared to the following periods  $P_j$  with  $j > 4$  which mainly show insignificant results.

These graphs revealing strongly negative average treatment effects during the stratum — in most cases *significantly* negative with an ATT (on probabilities) of at least around minus 10 percentage points — provide severe indications of a negative bias, especially if the following ATT with final months immediately after the stratum are distinctively higher. Hence, such patterns of plotted ATT apparently confirm the suspicion, mentioned in Section 5.5.1.1 for exit to mere employment, that there are cases with negative distortions high enough to turn possibly positive effects into insignificant or even negative results. To put it

<sup>91</sup>An overview of the figures with monthly updated ATT on probabilities in the appendix is given in Table A.1.

differently, even if Table 5.7 shows mainly negative or insignificant results, this could be caused by a negative bias that emerged during the strata, which possibly turns strong positive effects to insignificant results, and weak positive or insignificant effects to negative results.

But even if the total effect of welfare sanctions should mainly be positive or at least insignificant, there are also people being confronted with a negative impact of sanctions. Again similarly to Section 5.5.1.1, there are several graphs of plotted ATT over time that show remarkably strong negative slopes for periods  $P_j$  with final months  $m_j$  after the stratum (i.e. for  $j > 4$ ), which indicates notably negative *monthly* sanction effects for people with later exit events. These people can be assumed to have tendentially worse chances on the labor market, or have other reasons for being prone to a later exit from welfare. And obviously those people are (additionally) negatively affected by welfare sanctions, even if the majority would be positively affected to an extent that the total effect might be positive.

Examples of initially strongly increasing and partly even significant positive treatment effects (mainly within the stratum) and subsequently strongly decreasing cumulated ATT are the first stratum ( $S_1$ ) of the following subgroups of male unemployed: men living in Eastern Germany (*East*) (Figure A.18), men with mid- or even high-level of labor market access (*middle, high*) (Figures A.20 and A.21), male unemployed aged 25 or over (*o25*) (Figure A.24), men over 25 years and living in West Germany (*o25/West*) (Figure A.25), and men over 25 with medium- or high-level labor market access (*o25/middle, o25/high*) (Figures A.26 and A.27). On this, the shapes of plotted ATT for the first stratum ( $S_1$ ) of men with mid- or even high level of labor market access (*middle, high*) are particularly striking with a very abrupt turn from a steep positive slope within the stratum to a steep negative slope afterwards. Hence, there are subgroups of unemployed people which are quite heterogeneous concerning their reaction to sanctions. In these cases, people with already high chances for an early exit event get pushed by sanctions and are more likely to leave welfare receipt. But people with tendentially later exit events get even worse chances for an early exit from welfare if they are sanctioned. Put differently, these are examples of strongly positive short-term effects of benefit sanctions on the one hand, and strongly negative sanction effects in the medium and long run, on the other hand.

Analogically to the graphs of cumulated ATT over time for employment entrance presented in Section 5.5.1.1, the shapes of plotted ATT for exiting welfare are likewise very heterogeneous. We again find, for example, u-, n-, s- and zigzag-shaped graphs. So we cannot make one clear statement about the impact of welfare sanctions on leaving UB-II-receipt for different groups and subgroups of unemployed. Instead, the results show us how diverse people react on sanctions, which can lead them to earlier or later exits from welfare, depending on the specific conditions and circumstances.

## 5.5.2 Employed people with supplementary welfare receipt

Despite the term '*unemployment benefit II*', it is not just unemployed, but also *employed* people who are eligible for the German UB II, if their household's income doesn't meet the legally defined minimum subsistence level. The share of employed people receiving supplementary UB II — the so-called '*Aufstocker*' — grew from about 23% of the employable UB-II-recipients in 2007, to around 30% in 2012 this number stabilized, and even decreased slightly from around 1.3 to 1.2 million people between 2007 and 2012 and still remains significantly above one million people. Thus, this is a notable and important group that cannot be neglected in a comprehensive analysis of welfare sanctions.

For employed welfare recipients, we consider the following exit events: entering mere employment (*O: 'job only'*), i.e. without receiving top-up benefits, presented in Section 5.5.2.1; and exiting the receipt of welfare benefits (*ExWel: 'exit from welfare'*), comprising the alternative to leave the labor force, shown in Section 5.5.2.2, and additionally, the option of exiting employment with top-up benefits for mere welfare receipt (*ExJob: 'exit from job'*) discussed in Section 5.5.2.3.

### 5.5.2.1 Transition from supplementary welfare receipt to mere employment

The aim of German welfare policy regarding employed people needing top-up benefits to cover their household's subsistence minimum is to increase the so-called *Aufstocker's* family income in order to totally leave welfare receipt. Beyond possibly increasing the income of a related household member, the employed welfare recipients themselves could either try to increase their working hours, negotiate with the current employer to raise their salary, or find a better

paid job — each in order to enhance the household’s income sufficiently to leave welfare receipt. Even if sanctions against employed welfare recipients may not be primarily intended to foster this aim, it could nevertheless be seen as a welcome side-effect of the sanction policy of German Jobcenters. And based on the previous studies of benefit and welfare sanctions which found such kinds of ‘pushing effects’ for sanctioned unemployed, it seems natural to suppose similar impacts for employed welfare recipients.

Our estimations of the average treatment effects on the treated (ATT) of welfare sanctions on the probabilities of transitions from employment with supplementary welfare receipt to mere employment presented in Table 5.8, however, show either no significant ATT or — mostly even persistent — negative sanction effects, especially for the first two strata ( $S_1$ ,  $S_2$ ) of employed men.<sup>92</sup> Additionally, taking into account the plotted ATT over two years time, as shown in Figure A.28 until Figure A.30, we get deeper information about the possible bias to the detriment of the treated; furthermore, we see the development of the effects of benefit sanctions against employed welfare recipients, revealing long-term effects.

For example, the first and second stratum ( $S_1$ ,  $S_2$ ) of *all* employed men receiving

TABLE 5.8: Top-up benefits to mere employment — 2008

Age	Region	Market access	Men		Women
			Number of periods with sign. ATT <sup>1</sup>		
			$S_1$	$S_2$	$S_1$
all			21(-)	21(-)	14(-)
all	West		21(-)	21(-)	
all		middle	21(-)	19(-)	
all		high	8(-)		
o25			21(-)	21(-)	
o25	West		20(-)		

<sup>1</sup> ATT of *binary outcomes*: difference of mean probabilities of exit to mere job until the end of month  $m_j$  after start of quarterly stratum of welfare duration ( $S_i$ ); number of positive (+) and/or negative (-) significant ATT, summarized over periods  $P_j$  with final months  $m_j$ , considering  $j=4-24$ , or no significant (n.s.) ATT in any of these periods, on a significance level of  $\alpha=0.1$ .

*Subgroups*: age-group in years: all=15–56, o25=25–56; region: West/East German states; labor market access: low, middle, high.

<sup>92</sup>Our estimations of the ATT on *durations* until the exit event, for the transition from employment with receiving top-up benefits (*‘supplementary’*) ( $S$ ) to mere employment (*‘job only’*) ( $O$ ), yield no reliable results because of too few exit events in the treatment group.



top-up benefits (Figure A.28), of employed men living in *Western* Germany (Figure A.29), and of male employed with *medium*-level labor market access (Figure A.30) reveal negative ATT on probabilities of around -0.05 to -0.10 within the strata, and steeply downwards pointing shapes of monthly updated ATT for at least one year after the strata. On the one hand, the already considerable negative ATT within the strata can be seen as a strong indication of a negative bias; on the other hand, the ongoing strong negative progression of the *cumulated* ATT reveals a remarkable negative *monthly* ATT during about one year after the sanction. This means that even if the possible distortion during the strata is high enough to turn insignificant or positive short-term effects into negative ATT, there are considerable negative effects of welfare sanctions in the medium and long run — at least in some cases, and for individuals with tentatively later exit events, and thus with already worse chances of obtaining a job which gets them out of needing supplementary welfare benefits.

Altogether, the plots for *employed* welfare recipients' exit to employment without benefit receipt are much more homogeneous than the very heterogeneous shapes of ATT plots for *unemployed* people's exit to mere employment. On the whole, the plots of employed's cumulated ATT are mostly downwards heading and show either insignificant or significant negative effects of benefit sanctions against female and male employed welfare recipients. Thus, even if the short term effects might be non-negative, the monthly effects in the medium and long run tend to be clearly negative, in most cases for at least one year after the strata.

### 5.5.2.2 Transition from supplementary welfare receipt to welfare exit

As already mentioned in Section 5.5.1.2, the exit event 'leaving welfare receipt' (*ExWel*) comprises 'taking up mere employment' ('*job only*') (*O*) as well as the possibility of leaving the labor force, the so-called 'non-employment option'. Consequently, there are more exit events in the analysis than if we only consider entering mere employment, which is an advantage of estimating the ATT of duration outcomes. In contrast to the analysis of the durations until entering mere employment presented in Section 5.5.2.1, where we have too few cases with exit events in the treatment group in order to get reliable results, the estimations for exiting welfare (*ExWel*) yield several reliable, and even a few significant results also for duration outcomes which we present in Table 5.9 for male employed receiving supplementary welfare benefits.

According to the results in Table 5.9, employed men receiving top-up benefits who are sanctioned in their first quarter of welfare receipt need more than two months (namely 67.62 days) longer until they leave welfare receipt than if they would not have been sanctioned. These results, being in contrast to the expected direction of sanction effects, may at least be partly due to a potential negative bias to the detriment of the treated which we cannot quantify, as mentioned on several occasions before. Hence, we focus the main part of our interpretation on the probability outcomes, from which we expect to derive further insights.

Table 5.10 shows a few estimations with positive ATT on probabilities, almost exclusively for the first stratum ( $S_1$ ) of employed men living in Western Germany and aged over 25 years (*o25/West*). The second strata ( $S_2$ ) of several subgroups, such as men over 25 years (*o25*) or men with medium-level labor

TABLE 5.9: Duration of top-up benefits until exit from welfare — men 2008

Age	Region	Market access	ATT <sup>1</sup>	
			$S_1$	$S_2$
all			67.62*	102.75**
all	West		43.17	151.95**
all		middle	92.44*	114.95*
o25			32.00	158.64**
o25	West		7.93	182.02*

<sup>1</sup> ATT of *metric outcomes*: difference of mean durations of (supplementary) welfare receipt until welfare exit, measured in days from the start of quarterly stratum of welfare duration ( $S_i$ ).  
*Subgroups*: age-group in years: all=15–56, u25=15–24, o25=25–56; region: West/East German states; labor market access: low, middle, high.  
*Significance levels*:  $\alpha=0.1^*$ ,  $\alpha=0.05^{**}$ ,  $\alpha=0.01^{***}$ .  
*Gray figures*: not reliable results (<50 treated cases).

TABLE 5.10: Employed's exit from welfare — men 2008

Age	Region	Market access	Number of periods with sign. ATT <sup>1</sup>		
			$S_1$	$S_2$	$S_3$
all			n.s.	17(-)	3(-)
all	West		1(+)		3(-)
all		middle	1(-)	20(-)	
o25			n.s.	21(-)	
o25	West		8(+)	21(-)	

<sup>1</sup> ATT of *binary outcomes*: difference of mean probabilities of exit from welfare receipt until the end of month  $m_j$  after start of quarterly stratum of welfare duration ( $S_i$ ); number of positive (+) and/or negative (-) significant ATT, summarized over periods  $P_j$  with final months  $m_j$ , considering  $j=4-24$ , or no significant (n.s.) ATT in any of these periods, on a significance level of  $\alpha=0.1$ .  
*Subgroups*: age-group in years: all=15–56, o25=25–56; region: West/ East German states; labor market access: low, middle, high.

market access (*middle*), are dominated by negative ATT. Clues as to the extent to which these persistent negative results could be caused by a bias to the detriment of the sanctioned were obtained from the plotted ATT. Indeed, Figures A.31 and A.32 reveal strongly negative ATT — in many cases as early as the second and third strata ( $S_2, S_3$ ) — of about 10 until 20 percentage points, while in a few cases an upwards shift shortly after the strata is an additional indication of a probable bias of around -0.1 to -0.2.

The fact that we do not get reliable, or even significant results for other subgroups of employed men with top-up benefits, like men under 25 years (*u25*) or men living in Eastern Germany (*East*), is most probably caused by a lack of exit events in the treatment group, especially as these subgroups in general consist of notably fewer individuals. For women, a lower sanction rate can be an additional important reason for the scarcity of reliable results, at least if there is a low number of observations anyway, as is the case for transition from supplementary welfare receipt.

Comparing Tables 5.8 and 5.10, it is salient that exiting welfare (Table 5.10) leads to less significant negative results and even a few significant positive ATT, while entering mere employment (Table 5.8) shows just negatively significant ATT. Such a discrepancy between the two exit events must be due to the non-employment option which obviously tends to be affected positively by welfare sanctions. In other words, benefit sanctions against employed welfare recipients tendentially raise their probability to leave the labor market. That is valid at least for the subgroups and strata which show the mentioned discrepancy between the events ‘welfare exit’ with less negative or even positive ATT, and ‘exit to mere employment’ with clearly negative estimations of the sanction effect.

### 5.5.2.3 Transition from supplementary to mere welfare receipt

Examining the effects of benefit sanctions on transitions from employment with supplementary welfare receipt, there is one further possible event that has to be taken into account: the option to exit employment (*ExJob*) in order to live on welfare benefits exclusively.

Looking at the purely numerical results of our analysis of sanction effects on duration outcomes presented in Table 5.11, we see a strongly significant negative effect on durations until exiting employment for the first stratum ( $S_1$ ) of

men receiving top-up benefits. More precisely, male employed receiving supplementary unemployment benefits II (UB II), who are sanctioned in their first quarter of welfare receipt leave their job on average nearly three months (exactly 88.13 days) earlier than if they would not have been sanctioned. However, in contrast to previously discussed exit events, for exiting employment (*ExJob*) we face a potential bias not to the detriment, but in *favor* of the treated — in terms of a higher probability of the exit event, and thus of a shorter duration until the event occurs. The monthly updated ATT on probabilities, presented in Table 5.12 and plotted in Figures A.33 to A.35 shall give us further hints on whether, and to what extent this could be the case.

We see in Table 5.12 that the first stratum ( $S_1$ ) of employed women and men, as

TABLE 5.11: Duration of top-up benefits until mere welfare receipt — men 2008

Age	Region	Market access	ATT <sup>1</sup> S <sub>1</sub>
all			-88.13***
all	West		-90.42***
all		middle	-66.13*
o25			-100.23***
o25	West		-107.08***

<sup>1</sup> ATT of *metric outcomes*: difference of mean durations of supplementary until mere welfare receipt, measured in days from the beginning of quarterly stratum of welfare duration ( $S_i$ ).

*Subgroups*: age-group in years: all=15–56, o25=25–56; region: West/East German states; labor market access: low, middle, high.

*Significance levels*:  $\alpha=0.1^*$ ,  $\alpha=0.05^{**}$ ,  $\alpha=0.01^{***}$ .

*Gray figures*: not reliable results (<50 treated cases).

TABLE 5.12: Employed's exit to mere welfare — 2008

Age	Region	Market access	Men	Women
			Number of periods with sign. ATT <sup>1</sup> S <sub>1</sub>	S <sub>1</sub>
all			21(+)	21(+)
all	West		21(+)	
all		middle	21(+)	
all		high	10(+)	
o25			21(+)	
o25	West		21(+)	

<sup>1</sup> ATT of *binary outcomes*: difference of mean probabilities of exit to mere welfare receipt until the end of month  $m_j$  after start of quarterly stratum of welfare duration ( $S_i$ ); number of positive (+) and/or negative (-) significant ATT, summarized over periods  $P_j$  with final months  $m_j$ , considering  $j=4-24$ , or no significant (n.s.) ATT in any of these periods, on a significance level of  $\alpha=0.1$ .

*Subgroups*: age-group in years: all=15–56, o25=25–56; region: West/East German states; labor market access: low, middle, high.

well as of various subgroups of employed men, reveals persistent significant positive sanction effects on the probability of leaving employment for mere welfare receipt (*ExJob*). Even the relatively small subgroup of employed men with a high level of labor market access (*high*) shows a quite large number of overlapping periods  $P_j$  with positive ATT on the probability of quitting their job and living only on welfare. Nevertheless, as outlined in Section 5.4.4.3, the possibility of a positive bias must be taken into account.

Indeed, there are a few shapes of plotted ATT over time for certain strata and (sub-)groups, as shown in Figures A.33 through A.35, which provide indications of positive distortions, at least in some cases. For example, the first stratum ( $S_1$ ) of employed women depicted in Figure A.33 shows a steeply upwards pointing shape of monthly updated *cumulated* ATT during the stratum that is staying quite constant around approximately 15 percentage points afterwards. This is a strong indication of a positive bias of around +0.15, because there is no plausible reason for a factual, strong positive *monthly* effect during the stratum which ends abruptly directly afterwards — not least because sanctions are usually spread randomly within the strata and are not imposed only at the beginning and middle of the strata.<sup>93</sup>

Similar to the women, employed men (Figure A.33), men with a high level of labor market access (*high*) (Figure A.34), and men over 25 years living in Western Germany (*o25/West*) (Figure A.35) exhibit relatively high positive ATT during their first stratum ( $S_1$ ) which are mainly upwards heading quite steeply. Even though the initial trend continues pointing strongly upwards for three further months subsequent to the stratum, lasting until period  $P_6$  with final month  $m_6$ , the plotted ATT subsequently turns direction and tends to head downwards. The interpretation of such shape of cumulated ATT — in contrast to the straightforward example of female employed — is rather ambiguous.

The monthly updated cumulated ATT for the mentioned groups of men is already quite high in the first month after the beginning of stratum  $S_1$ , that is, in period  $P_1$  with final month  $m_1$ . They start with an ATT of more than 20 (Figure A.33), more than 10 (Figure A.34), and more than 15 percentage points

<sup>93</sup>Although the distribution of welfare sanctions in the first stratum ( $S_1$ ) often tends to be skewed to the left and thus is less uniform than within later strata, the hypothetical possibility that sanctions could be imposed exclusively at the beginning and middle of the first stratum is neither likely nor plausible. Otherwise it could have been an explanation for such a pattern of plotted ATT on condition that the welfare sanctions have only very short-term effects not lasting longer than one month maximum which is also very unlikely, given that sanctions regularly last three months.

(Figure A.35), respectively. Additionally, the ATT mostly increases distinctly in the following two periods ( $P_2$ ,  $P_3$ ) with final months  $m_2$  and  $m_3$  within the stratum. Such kinds of patterns during the stratum could give a good reason for suspecting a positive bias, at least if the ATT would stay constant or point downwards directly after the end of the stratum.

However, the upwards pointing trend of the cumulated ATT exceeding the duration of the stratum for about three further months implies that the *monthly* ATT is still positive during months  $m_4$  to  $m_6$ . Such positive monthly effects after the stratum, though, cannot be caused by the potential positive bias which could emerge exclusively during the strata.<sup>94</sup> Accordingly, we have an indication of factual positive effects of welfare sanctions against the mentioned groups of employed men — male employed of any age (*all*: 15–56 years), men with a high level of labor market access (*high*), and men over 25 years living in Western Germany (*o25/West*) — sanctioned in their first quarter of welfare receipt ( $S_1$ ) which increases their probability to give up employment for mere welfare receipt (*ExJob*). Later on — after about six months of increasing ATT — the curves change direction, mainly heading downwards. This implies negative *monthly* effects of welfare sanctions in the medium and long run for the aforementioned groups of employed men.

Altogether, such kinds of cumulated ATT — increasing for the first six months and decreasing afterwards — that we found for the first stratum of several groups, imply that employed welfare recipients who tend to quit or lose their job and live just on welfare already during their first half-year of benefit receipt would be more likely to experience this if they are sanctioned. In contrast, people who tend to exit their jobs later than half a year after starting to get top-up benefits are even less likely to exit from employment if they are sanctioned.

However, these effects predominantly tend to be not significant, at least with our data set based on a 2% sample of administrative data which, in cases of small treatment groups, tend to suffer from a scarcity of rare exit events. Taking a markedly larger sample, or using the administrative data as a whole, could lead to more significant results also for rare exit events and small treatment

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<sup>94</sup>As a consequence of how the treatment and the outcome variables are generated (see Sections 5.3.2 and 5.3.3), and as explained in Section 5.4.4.3, in contrast to the *negative* potential bias on probability outcomes of other exit events, the *positive* potential bias for transitions from supplementary to mere welfare receipt can emerge only *during* the strata, and not also shortly afterwards.

groups, as is the case with transitions from employment with supplementary welfare receipt into mere welfare receipt.

### 5.5.3 Indirect sanctions

In a comprehensive analysis of benefit sanctions' impact on employment and welfare receipt, the effect on indirect sanctioned — that is, people who are affected by sanctions against related household members — is a further important research topic. This is particularly the case if families or households are entitled to the benefits rather than individuals, which in many European countries applies for tax-based welfare payments in contrast to unemployment insurance payments. Likewise, the tax-funded welfare benefits for employable people in Germany, named unemployment benefits II (UB II), are granted to needy workers and job-seekers along with their related household members; in contrast, the insurance based unemployment benefits I (UB I) are granted on an individual basis and independently of need.

Although a vast majority of unemployed people in Germany do not receive unemployment insurance benefits (UB I) but welfare payments (UB II), and although a big share of the employable welfare recipients do not live alone but together with related household members, the question of how sanctions influence the employment-related behavior and labor force decisions of indirectly affected household members is an issue almost entirely neglected by previous economic research.<sup>95</sup> In order to account for the importance of this topic, we carried out the whole analysis for *direct sanctions*, referring to sanctions against the employable individuals themselves, as well as for *indirect sanctions*, referring to sanctions against household members of the employable welfare recipients.

Nonetheless, the outcome of the analysis based on indirect sanctions is relatively scarce, primarily due to a lack of cases in the treatment group. The main reason for this shortage is that we have to impose strong restrictions in order to identify individuals who are suitable for the treatment group of the indirectly sanctioned. On the assumption that the effect of direct sanctions exceeds the effect of indirect sanctions, we can only use people for the treatment group of

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<sup>95</sup>As mentioned in the introduction, Section 5.1, we already considered indirect sanctions in our previous study on the impact of welfare sanctions on labor market decisions based on survey data (Hillmann and Hohenleitner (2015)), but we could not disentangle the effects of direct and indirect sanctions because of far too few cases of indirect sanctioned in the survey data.

indirect sanctioned if they have never been directly sanctioned either before, or after the indirect sanction until the exit event occurs.<sup>96</sup> Moreover, we had to broaden the strata to six months in order to get a notable amount of treatments per stratum. This, however, entails other drawbacks like a longer period that cannot properly be used to reveal short-term effects. Notwithstanding the difficulties, we got some reliable and significant results that we present and discuss in Section 5.5.3.1 for unemployed, and in Section 5.5.3.2 for employed welfare recipients who are affected by sanctions exclusively against related household members.

### 5.5.3.1 Impact of indirect sanctions on transition from unemployment

Estimating the effects of indirect sanctions on the transition from unemployment, we got reliable and significant results only for women and their probabilities to enter employment; for men, for exits from welfare (*ExWel*) comprising the non-employment option, and for duration outcomes, we did not get reliable and significant results.

The estimation results presented in Table 5.13 reveal 10 of 12 considered overlapping periods  $P_j$  with significant negative ATT for entering mere employment (*O*: 'only job'), and 7 of 12 considered periods with significant negative

TABLE 5.13: Unemployed to employment — indirect sanctions

Women 2008			
Age	Region	Exit to job (G/O/S) <sup>2</sup>	Number of periods with sign. ATT <sup>1</sup> S <sub>1</sub>
all	West	G	7(-)
all	West	O	10(-)
all	West	S	n.s.

<sup>1</sup> ATT of *binary outcomes*: difference of mean probabilities of employment entrance until the end of month  $m_j$  after start of half-yearly stratum of welfare duration ( $S_i$ ); number of positive (+) and/or negative (-) significant ATT, summarized over periods  $P_j$  with final months  $m_j$ , considering  $j=7-18$ , or no significant (n.s.) ATT in any of these periods, on a significance level of  $\alpha=0.1$ .

<sup>2</sup> *Events*: exit to: job only (O), i.e. without welfare receipt, job with supplementary welfare receipt (S), job in general (G), i.e. comprising (O) and (S).

<sup>96</sup>See Section 5.3.2. For individuals who are directly sanctioned, in contrast, later indirect sanctions or repeated direct sanctions are no obstacles to stay in the treatment group — we just have to make sure that we only use the first sanction of an individual for reasons of comparability.



ATT for taking up any kind of employment ( $G$ : 'job in general').<sup>97</sup> This applies for unemployed women living in Western Germany (*West*), sanctioned in their first half-yearly stratum of welfare receipt ( $S_1$ ). By contrast, unemployed men do not reveal significant effects of welfare sanctions against related household members. The plotted ATT, exemplarily presented in Figure A.36 for transition into mere employment ( $O$ ) and Figure A.37 for entering employment in general ( $G$ ), each for unemployed living in Western Germany (*West*), confirm that only women react to family member's sanctions. That especially women in Western Germany react significantly to other household member's sanctions might be due to a historically conditioned and still existing lower propensity to participate in the labor market when compared to men, and to women in Eastern Germany.

The negative direction of the effect, however, seems less plausible and may be caused by a negative distortion to the detriment of the treated. On the one hand, the graphs for entering mere employment (Figure A.36) show downwards heading cumulated ATT for women, wherein the first few months tend to be increasingly significant also because of very narrow confidence intervals, later changing abruptly to wider confidence intervals which let the ATT initially become insignificant. This pattern of plotted ATT and its confidence intervals speaks for a possibly negative bias. However, the ATT in all cases and strata, for women and men, starts close to zero in the first month. Nevertheless, for women the cumulated ATT is slowly but noticeably downwards heading during the first few overlapping periods, predominantly for exit into mere employment ( $O$ ). These facts together can be seen as indications of a notable but small negative bias in the first few months.

On the other hand, the downwards trend of the cumulated ATT continues for the whole observation period of 18 months for the first stratum of women in Western Germany (*West*) and their exit into employment without receiving top-up benefits ( $O$ ) (Figure A.36). Even if the emergence of a negative bias exceeds the duration of the stratum, this cannot explain the clearly long-term downwards trend of the cumulated ATT, as the emergence of the bias after the stratum is diminishing with progressing final months. Hence, there indeed seems

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<sup>97</sup>For indirect sanctions, we carried out the analysis for two half-yearly strata ( $S_1, S_2$ ), each for 18 overlapping periods  $P_j$  with final months  $m_j$  with  $j = 1, 2, \dots, 18$ . Because of a potential bias emerging predominantly during the strata, we consider only the periods  $P_j$  with final months  $m_j$  after the end of the half-yearly strata, i.e. with  $j = 7, 8, \dots, 18$ .

to be a negative *monthly* effect of indirect sanctions against women in Western Germany, at least in the medium and long run.

Yet, at first sight, a negative effect of indirect sanctions seems less plausible if we assume that the partner of the woman is sanctioned. A sanction against the partner might be more likely a reason for increasing the effort to find a job in order to compensate for the financial loss. But a high proportion of women receiving UB II are single mothers living with their children. And as people aged under 25 years generally are not allowed to leave their parents' household if they cannot fully live on their own income<sup>98</sup> — which in Germany is still more likely for children whose parents are already depending on welfare benefits — it is plausible that a considerable share of sanctioned household members of women are not their partners but their children. This holds even more as young people under 25 years are sanctioned considerably more frequently than people aged 25 years or older.

If the negative effect of indirect sanctions on women's probability to take up mere employment in the medium and long run is mainly due to sanctioned children, the negative direction of the impact seems more plausible than in the case of sanctioned partners. A reason for this is that people under 25 are not only sanctioned more frequently, but also more severely. If their fault is not just having been unpunctual or having missed an appointment which is punished with a 10% cut of the base benefit, their so-called 'major breach of duty' is initially sanctioned with a 100% cut of the base benefit. And in the first case of recurrence they already get a 100% cut of the whole UB II including accommodation costs, where each sanction generally lasts three months, regardless of the amount of benefit cut.<sup>99</sup> As a consequence, mothers — and single mothers even more — may have less time and energy to find a new job if they have to struggle with their sanctioned children and with the huge loss of household's income over three months. Furthermore, it also seems plausible that the negative effects last considerably longer than just the three months time during the sanction, because compensating a 100% benefit cut in the household may cause debts of a size that must be repaid over many months, given that the family is

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<sup>98</sup>'Own income', in this case, includes all income or assets that brings them out of welfare receipt in terms of UB II. Hence, receiving students assistance wouldn't be an obstacle to move out of the parents' household.

<sup>99</sup>People aged 25 years or over are sanctioned for the same 'major faults' with a 30% cut of the base benefit initially, and in case of recurrence with a 60% cut of the base benefit in the second step, and with a 100% cut including costs for accommodation and mostly even health insurance only in the third step.

living at the bare subsistence level anyway, even without being sanctioned or repaying debts.

All in all, the result that women's probability of finding a job which brings them out of benefit receipt is affected negatively even in the long run, seems more plausible if they are affected by sanctioned children than by sanctioned partners. Firstly the loss of household's income is more severe, secondly the threat of repeated sanctions is larger, and finally they might feel more responsible for their children's future than for a partner's behavior and thus — instead of increasing the effort to find a job for themselves — possibly increase the effort to support their children to get a job or an apprenticeship.

As we can not differentiate between sanctions against partners, children, or even parents, because of too few cases of merely indirect sanctioned in general, we just give a first glimpse showing that there is much more to explore in this research field. But our data set, based on a 2% sample of administrative data, is too limited for more detailed research on this topic.

### 5.5.3.2 Impact of indirect sanctions on transition from supplementary welfare receipt

Analyzing the effects of indirect sanctions on the transition from employment with supplementary benefit receipt, we got reliable and significant results only for women and their probability of leaving welfare receipt (*ExWel*). For men, for exits to employment (*G/O/S*), for exits from employment to mere welfare receipt (*ExJob*), and for duration outcomes, we did not get reliable and significant results.

TABLE 5.14: Supplementary welfare receipt — indirect sanctions

		Women 2008
Age	Exit from/to	Number of periods with sign. ATT <sup>1</sup> S <sub>1</sub>
all	from welfare ( <i>ExWel</i> )	2(-)
all	to job only ( <i>O</i> )	n.s.
all	to welfare only ( <i>ExJob</i> )	n.s.

<sup>1</sup>ATT of *binary outcomes*: difference of mean probabilities of the exit event until the end of month  $m_j$  after start of half-yearly stratum of welfare duration ( $S_j$ ); number of positive (+) and/or negative (-) significant ATT, summarized over periods  $P_j$  with final months  $m_j$ , considering  $j=7-18$ , or no significant (n.s.) ATT in any of these periods, on a significance level of  $\alpha=0.1$ .

The estimation results presented in Table 5.14 show significant negative ATT on the probabilities of exiting welfare (*ExWel*), only for two overlapping periods  $P_j$  with final months  $m_j$  after the end of the first half-yearly stratum ( $S_1$ ), i.e. for  $P_j$  with  $j > 6$ . This applies for employed women receiving top-up benefits who are affected by sanctions exclusively against related household members. As discussed in detail in Section 5.5.3.1 above, it is more likely that negative effects of indirect sanctions are caused by sanctioned children than by sanctioned partners. It seems plausible to transfer those findings about unemployed women to employed women with supplementary welfare receipt as well— at least for their entry to mere employment which is also a part of exiting welfare.

It is striking that we get insignificant ATT for taking up mere employment (*O*) despite the negative significant ATT on the probabilities of exiting welfare (*ExWel*). Even if the insignificant results for entering employment are caused by too few exit events in the treatment group, the number of exit events for leaving welfare is obviously high enough to obtain at least a few significant results. As the only difference between entering mere employment and leaving welfare is the non-employment option, the significant negative ATT must be caused by the additional exit events of leaving the labor market. Hence, employed women with top-up welfare benefits are less likely to leave the labor market if they are affected by a sanction against a related household member in their first half-yearly stratum of benefit receipt ( $S_1$ ). This seems plausible independent of whether the sanctioned family member is a child or a partner or even a parent, as the family's income is reduced by the sanction, making it less affordable to leave the labor force.

As mentioned above, for the analysis of the effects of indirect sanctions, a much larger sample than our data set based on a 2% sample of the administrative data is necessary in order to get more detailed results in this research field.

#### **5.5.4 Matching quality and robustness**

In order to evaluate the reliability of our results, we checked the matching quality and the robustness of our estimations in several kinds of ways.

#### 5.5.4.1 Matching quality

After the matching process, the quality of the matching has to be examined. Specifically, we have to figure out whether the estimated propensity score is appropriate to balance the covariates between treatment and control group. If that is the case, the so-called 'balancing property' is nearly satisfied. This means that individuals with the same propensity score have almost identical distributions of observed and unobserved characteristics, independently of being a member of the treatment or control group; in other words, the selection to treatment or control group can be assumed to be random, and hence treated and untreated on an average can be assumed as nearly identical.<sup>100</sup>

As suggested by Müller (2012), we explored the matching quality in two ways. Firstly, we checked whether significant differences in the covariates between the treated and the untreated are still significant after the matching procedure. Following Rosenbaum and Rubin (1985), we applied two-sided *t*-tests to ensure that no significant mean differences between the treatment and control groups occurred after the matching procedure for all covariates.

Following Sianesi (2004) and Müller (2012), we additionally checked the pseudo *R*-squared (pseudo  $R^2$ ), and applied the likelihood ratio chi-square test (LR- $\chi^2$ -test), both before and after the matching procedure to ensure a high matching quality. As the pseudo  $R^2$  is a measure of the heterogeneity between treatment and control group, it should be very low, preferably close to zero, after the matching and clearly lower than before the matching. Conversely, the LR- $\chi^2$ -test which checks whether at least one single covariate has a significant impact on the probability to be treated, must not be significant *after* the matching procedure in order to guarantee a good balance between matching and control group. To put it differently, the null hypothesis ( $H_0$ ) that the common effect of the covariates on the treatment is zero must not be rejected after the matching, i.e. at least  $p > 0.1$  must be satisfied; for a high matching quality, however, the *p*-value should be close to 1. If these requirements are met, the 'balancing property' is approximately satisfied.<sup>101</sup>

In the vast majority of our estimations, the balancing property was clearly satisfied. For samples with a lack of observations in the treatment group, however, there were estimations with worse matching quality which we excluded from

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<sup>100</sup>See Becker and Ichino (2002) and Müller (2012).

<sup>101</sup>See Müller (2012) and Heinrich et al. (2010).

the further analysis. Concretely, we checked for pseudo  $R^2 < 0.1$ ,  $p$ -value of LR- $\chi^2$  test close to 1, i.e. LR- $\chi^2$ -test must be (highly) insignificant, and finally, for all covariates, the mean differences between treatment and control group must not be significant *after* matching. Additionally, as a rule we rejected estimations with less than 50 treated cases, and in just a few exceptional cases we used estimations with less than 50 but at least 25 treated individuals if the balancing property was clearly satisfied.<sup>102</sup>

#### 5.5.4.2 Robustness

In order to check the estimated ATT for its sensitivity to the matching algorithm, we carried out the entire analysis with two different matching procedures: firstly, ‘nearest neighbor matching’ (NNM) with  $k=5$  nearest neighbors (5-NNM) and a caliper of 0.01 in order to exclude bad matches, and secondly, ‘kernel matching’ (KM), specifically kernel matching using an ‘Epanechnikov kernel’ (EKM) with a bandwidth of 0.06, with and without bootstrapped standard errors.<sup>103</sup> Additionally, we checked out several variations of both of these algorithms. For example we also tried out nearest neighbor matching with  $k=3$  nearest neighbors (3-NNM) and varied the caliper of NNM and the bandwidth of EKM. The results are very robust against these variations. Amongst 3-NNM and 5-NNM, there were nearly no differences, and between 5-NNM and EKM there were only small differences which, in the overwhelming majority of cases didn’t change the significance levels of the ATT. Furthermore, we made spot checks with a special self-written adjustment procedure which is only feasible for NNM that excludes the problem of the bias caused by the stratification, but yields other drawbacks which prevented us from using it more widely. Even those checks generally brought out astonishingly low differences in the results

<sup>102</sup>It has to be stressed that even in cases with just a few observations in the treatment group ( $25 \leq n_t < 50$ ), there are still more than 1,000 individuals in the control group, because benefit sanctions are rare events from a statistical point of view. Hence, there is still a large number of untreated people available for the matching procedure which can yield reliable results with proper matching quality even if the number of treated is scarce.

<sup>103</sup>In Section 5.4.3, we explain, in more details, the decision process of choosing the matching algorithms and their concretizations — as bootstrapping is extremely time-consuming and needs tremendous computing capacity, we carried out just spot-checks with bootstrapped standard errors in order to compare the significance levels with the results gained from the calculation of standard errors as it is implemented by Leuven and Sianesi (2014) in their Stata module ‘*psmatch2*’ which “does not take into account that the propensity score is estimated”. The spot-checks with bootstrapped standard errors widely confirm our results gained from the standard calculation implemented by *psmatch2*.

of using 5-NNM with and without adjustment. Hence, on the whole, our findings seem to be very robust against variations of the matching algorithm.

Additionally, we did the whole analysis — including the variants of the matching algorithms — for two different inflow samples: people who started to receive welfare benefits in 2007, or in 2008. We analyzed the inflow samples of 2007 and 2008 separately to figure out the dependency of our findings on the year of entering welfare receipt. In spite of self-evidently occurring variations in the concrete values of the ATT and standard deviations at large, the two inflow samples don't reveal fundamentally different results. Hence, our findings are not strongly dependent on the year that the observation starts. This holds at least for the period of 2007 and 2008.<sup>104</sup>

Another kind of sensitivity analysis investigates unobserved confounding factors. These so-called confounders are variables that affect the treatment variable — more precisely, the likelihood of being treated — as well as the outcome variables, and thus they can cause a hidden selection bias which distorts the estimations of the treatment effects.<sup>105</sup>

As explained in Section 5.4.3, the 'conditional independence assumption' (CIA), also called 'unconfoundedness', has to be satisfied in order to obtain robust results. The CIA claims that differences in the outcome between treatment and control group must be independent of the selection process, and thus be caused only by the treatment.<sup>106</sup> To satisfy this, it is favorable to use as many potential confounders as possible as control variables. Nevertheless, investigating humans and their behavior, we generally have to reckon with unobserved confounders, and hence we have to deal with possible unobserved heterogeneity. One proper way to assess the quality of the estimated treatment effects, even for the case that the CIA is not fully satisfied, are the sensitivity analysis based on the so-called 'Rosenbaum bounds' and the 'Hodges-Lehmann point estimators', suggested by Rosenbaum (1993).<sup>107</sup> Following Liu et al. (2013) and Müller (2012), we carried out spot-checks based on Rosenbaum bounds

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<sup>104</sup>Variations in the quarter of the year in which the individual starts to receive welfare do most probably influence the results because of seasonal effects. Hence, in our estimations we control for the inflow quarters within the inflow years; see Section 5.3.4.

<sup>105</sup>A good introduction to sensitivity analysis for unobserved confounding is given by Liu et al. (2013).

<sup>106</sup>See Müller (2012), Rosenbaum and Rubin (1983), and D'Orazio et al. (2006).

<sup>107</sup>See Rosenbaum (2002), DiPrete and Gangl (2004), and Müller (2012). Rosenbaum's approaches are the most frequently used method to deal with unobserved heterogeneity associated with matching methods like propensity score matching (PSM); see Liu et al. (2013).

and Hodges-Lehmann point estimators in order to explore the impact of potential unobserved confounding factors on our estimations of the treatment effects (ATT). All in all, we find the estimation results of the probability outcomes mainly robust against potential unobserved heterogeneity caused by unobserved confounders. The duration outcomes, however, reveal even more estimations whose unconfoundedness could not be affirmed.<sup>108</sup> These results of the robustness checks go often, but not always, along with high levels of significance or insignificance of the estimated ATT.

## 5.6 Discussion and assessment

The results of our comprehensive analysis presented in Section 5.5 agree, in part, with previous studies on benefit and welfare sanctions in Europe, differ, in places, from other studies' findings and, for certain aspects, no comparison is currently possible, due to the uniqueness of our analysis. At a minimum, those parts of our findings which contradict corresponding studies need to be discussed and evaluated. Furthermore we have to take into account the potential bias arising from stratification, introduced with its possible variations on a theoretical base in Section 5.4.4.3; in Section 5.5, we mention it on various occasions, presenting our numerical results and partly discussing and assessing the bias in greater detail while interpreting our graphical results. In this section, we discuss and evaluate our results in the context of related studies and the potential bias.

The unique aspects of our analysis are, first, that we consider not only unemployed but also employed welfare recipients receiving supplementary welfare benefits and, second, that we do not only consider sanctions against employable welfare recipients themselves (direct sanctions) but also analyze the effect of imposed sanctions upon other members of the employable individuals' households (indirect sanctions). All other well-known European studies on welfare sanctions are restricted to *unemployed* welfare recipients and to sanctions *directly* imposed upon them. Hence, our results for *employed* welfare recipients, in Germany colloquially called '*Aufstocker*' presented in Section 5.5.2, and our

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<sup>108</sup>The null hypothesis supposing that the estimated effects are exclusively due to confounding factors, can be rejected up to the maximum value of  $\Gamma_{max}$  with a  $p$  value of  $p < 0.1$ ; evaluating the results of the sensitivity analysis with Rosenbaum bounds, we follow Aakvik (2001) who suggests that a  $\Gamma$  of 2.0 with still significant treatment effects can be seen as a 'very large' robustness.



results for welfare recipients *indirectly* affected by sanctions upon their household members, presented in Section 5.5.3, cannot be benchmarked against other studies, however we still considering them due to the potential bias.

But let us first focus on the bulk of our results, presented in Section 5.5.1, which reveal how unemployed welfare recipients respond to sanctions imposed upon them directly. In this field, there already exists some partly corresponding studies which can be used to validate, as well critical question our results. The most striking fact when comparing our findings with the results of corresponding studies is that not only we do estimate significant positive effects of welfare sanctions on entering employment and leaving welfare receipt, but we also obtain a lot of cases with significant negative estimations of the average treatment effect on the treated (ATT).

From the perspective of the sanctioned individuals who may have experienced not only the pushing effects of sanctions, but also their paralyzing and debilitating impacts, this might not be surprising. Also, several studies on the impact of welfare sanctions, mostly qualitative surveys or paraphrasing them, are consistent with that or give at least indications of adverse individual effects that can be detrimental to taking up employment (see e.g. Ames (2009), Götz et al. (2010), Ehrentraut et al. (2014), Wolff (2014), and van den Berg et al. (2015, 2017)). Such detrimental conditions could adversely affect the physical as well as the mental condition of the sanctioned as a result of financial and existential pressure, examples of which include: an increasing burden of debt; being threatened with, or being disconnected from electricity and heating services; and losing, or being in danger of losing one's home. These existential threats can easily be seen as factors that are not only detrimental to the individual's well-being, but also to the probability of achieving employment.

Nevertheless, the overwhelming majority of other studies' results — not only about UIB sanctions but also about welfare sanctions — reveal positive effects of sanctions on the transition rate from unemployment to employment. At first glance, the following reasons may be responsible for this seemingly contradiction between qualitative and quantitative surveys: either the mentioned conditions, detrimental for the individual, are less effective than the pushing effects, at least for the majority of sanctioned people, such that the effect of individuals who react positively to sanctions, in terms of taking up employment, dominates the effect of individuals who are impeded or even prevented from getting employed by (possibly severe) adverse individual sanction effects. Or the

vast majority of estimated positive sanction effects on entering employment can be caused by time-related factors: it seems plausible that negative effects like rising debt and health problems can accumulate and increase over time and, hence, be more effective in the medium and long run, while the pushing effects of sanctions work primarily in the shorter run. And if individuals who predominantly respond positively to pressure are in the majority — at least in the time horizon of the survey — the impact on people with predominantly adverse effects is outweighed.

Now let us turn the focus from those general reflections back to the information we gain from our data analysis considering the potential bias, and the findings of corresponding studies in order to assess and finally conclude our results.

### 5.6.1 Unemployed welfare recipients

Although we cannot quantify the negative bias, presumably distorting our results for unemployed welfare recipients in terms of entering employment and leaving welfare, we can assess our findings by means of the curve shape of the plotted ATT, and against the backdrop of related results from previous studies. As already partly discussed and evaluated in Section 5.5.1, there are hints as to the magnitudes of such negative distortions. Here we refer to the outcomes which measure the probabilities of exit events.<sup>109</sup> Hence, the ATT could range from  $-1.0$  to  $+1.0$  which implies that the mean difference of the probability of experiencing the exit event between sanctioned and matched non-sanctioned can theoretically range from a minimum of  $-100$  to a maximum  $+100$  percentage points. The hints that we get from the graphs with the plotted ATT show that, for the cases of the most extreme and persistent negative ATT of men, we get negative values in the range of  $-0.15$  to  $-0.25$  during the strata and shortly afterwards, which is the period during which the bias arises. This means that the estimated sanction effect shows the sanctioned to have a 15 to 25 percentage points lower probability of experiencing the exit event than the non-sanctioned control group. And if, additionally, the curve of plotted ATT shortly after the stratum skips or steeply slopes upwards, it is very likely that the initially negative values are strongly biased. Nevertheless, we cannot limit the bias to those

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<sup>109</sup>The other class of outcome variables we use for our analysis measures the durations until the exit events occur. As outlined in Section 5.3.3 and explained in detail at the beginning of Section 5.5, the findings for these kinds of outcomes can be interpreted mostly in accordance with the outcomes of probabilities, but because of the reversed signs they must be interpreted in the opposite direction.

maximal negative values during the strata, as the factual treatment effect could be positive, and according to previous studies, should be estimated positively. Hence, the bias may, in some cases, even exceed the 15 to 25 percentage points mentioned above.

All in all, we must evaluate the absolute values especially of our negative outcomes very critically, as they are most likely caused by a considerable negative bias as a result of the stratification explained in Section 5.4.4. The extent of the bias, however, depends strongly on different circumstances, as explained in detail in Section 5.4.4.3. Amongst other things, it strongly depends upon the kind of exit events, the distribution of the treatments within the strata, and the likelihood and distribution of the exit events of the matched control group during the strata. Hence, the bias can vary strongly between different exit events, and it can vary still noticeably also between different groups and sub-groups and between the strata.

In spite of these difficulties in interpreting and assessing the absolute values of our results, the shape of the ATT's curve, which reveals the development of the sanction effects over 24 months, can be interpreted independently of a potential or actual bias. As already explained while presenting the results in Section 5.5, we show the *cumulated* ATT over time and, thus, the differences of estimated ATT between the overlapping periods with consecutive final months reveal the *monthly* effects. Hence downward slopes after the stratum reveal negative monthly effects, while upward slopes reveal positive monthly effects. This is valid independently of the bias which can arise only within the strata, or possibly short afterwards. As we thus cannot reliably interpret the slope within the strata, and possibly shortly afterwards, we refrain from interpreting these very early periods with the help of slopes. But we can decently interpret the slope after the strata — at least within a short distance of the strata — in order to get information about the monthly effects in the medium and long term.

To summarize: even if the absolute values of our results are contestable, the development of the ATT over time still provides reliable insights. Taking all of this into account, and reviewing the plausibility while also being aware of the findings of previous studies, we come to the following conclusions by assessing and evaluating our results.

### 5.6.1.1 Divergences between different strata

It is evident that the ATT for the first strata ( $S_1$ ) tend to differ, and are often less negative or more positive than for later strata ( $S_2$  to  $S_4$ ). This could be due to two reasons: firstly, early sanctions could be more effective in the positive sense, namely that they increase the transition rate into employment and out of welfare receipt. This would go along with previous studies, mainly on UI sanctions, but also partly on welfare sanctions. secondly, the discrepancies between the first and the later strata also give a strong hint that the bias arising in the first strata could be lower than in the later ones. This also seems plausible, as there are several factors that account for a tendentially more left skewed distribution of sanctions within the first stratum compared to later strata. And as explained in detail in Section 5.4.4.3, a left skewed distribution of sanctions within the stratum reduces the potential bias as it lowers the probability of exit events of the matched non-treated before the sanctions of their matching partners occur.

One of the factors which may cause a left skewed distribution of sanctions is that a substantial proportion of people entering UB-II-receipt are initially also receiving unemployment insurance benefits (UIB), which is called unemployment benefits I (UB I) in Germany. If the amount of monthly UB-I payments does not cover the minimum subsistence level of their families, they are eligible for supplementary UB II. This can either be the case because UB I, depending on the previous income, is too low, or because the individuals are sanctioned within the UIB system, and thus become eligible to apply for UB II. If people are sanctioned in the UB I system, their breach of duty automatically causes a sanction within the UB II receipt, too. Furthermore, sanctions at the beginning of UB-I-receipt are disproportionately more likely, as part of the people do not fulfill, or even know their obligation to start job search three months before the end of their previous employment.

Moreover, the case workers at the German Jobcenters responsible for UB II are generally required to make initial offers to the clients in order to check their willingness to work, even at the beginning of UB-II-receipt. Because of a lack of proper jobs, such offers are mostly not job offers but 'offers' to participate in a measure of the ALMP which, at least for UB-II-recipients, is often some standardized training on how to prepare job applications that does not necessarily fit the clients' requirements. Refusing or culpably dropping out of such

measures results in strong sanctions lasting three months.<sup>110</sup> Thus, the sanction probability at the start of UB-II-receipt, independently of also getting UB-I-payments, is disproportionately high.

All these factors count for early sanctions within the first stratum causing a left skewed sanction distribution, and thus lead to less negatively biased ATT. Nevertheless, we cannot identify, for certain, whether the more positive ATT in the first strata ( $S_1$ ) come from the lower negative bias, or from the possibility that early sanctions might be more effective than later ones. But as the progression of the later strata (from  $S_2$  to  $S_4$ ) do not give clear hints supporting the hypothesis that earlier sanctions are more effective, it seems more likely that the outlying results for the first strata ( $S_1$ ) are mainly caused by a lower bias.

### **5.6.1.2 Divergences between men and women regarding employment entry**

Comparing the estimated treatment effects of men and women upon the rate of entry into different types of employment, it is striking that negative ATT occurs more often for men with respect to mere employment, and positive ATT occur more often for women with respect to employment with supplementary welfare receipt. Besides, all other outcomes tend to be mostly insignificant. Consistent with that, for the concise exit event of 'entering employment in general', which comprises mere employment as well as employment with top-up benefits, men tend to show negative significant ATT, and women tend to show positive significant treatment effects.

This all together points to the fact that the estimated negative ATT for men concerning employment in general are mainly driven by the ATT on entering mere employment, while the positive effects for women regarding employment in general are mainly driven by the ATT on entering subsidized work, concretely employment with top-up welfare benefits. Additionally, these results seem to imply that women's responses to welfare sanctions are stronger towards employment with supplementary benefit receipt, while men tend to respond more strongly with regard to mere employment. That seems plausible as still more women than men work in lower paid or part-time jobs, especially if they raise children.

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<sup>110</sup>More detailed information about the sanction regime under UB II are provided by Hillmann and Hohenleitner (2015) and van den Berg et al. (2014, 2015).

However, the fact that the estimated ATT for men in terms of entering mere employment show mainly negative results, and the estimations for women in terms of entering subsidized work are mainly positive, does not seem to be plausible at the first sight. Indeed this is a strong hint for biased results. Recalling the explanations about the bias in various constellations in Section 5.4.4.3, the potential negative bias works strongest for exit events out of risk — specifically, out of welfare receipt — and tends to be weaker for exit events staying at risk, like taking up employment while receiving top-up welfare benefits. As there seems to be no other plausible explanation for the divergence between men and women in terms of negative respectively positive treatment effects, it seems most likely, that the negative estimations for men are indeed caused by a substantial negative bias.

Hence, the absolute values of the estimations for exits into mere employment must be severely questioned. As other studies which distinguish between exits into subsidized versus unsubsidized work tend to find stronger positive sanction effects for unsubsidized work than for subsidized work, it can also be asked why for women, we estimate positive effects in terms of entering employment with supplementary welfare receipt, but mainly insignificant effects in terms of entering mere employment.

Taking all of this into account, it seems most likely that the effects for women on taking up mere employment are, in fact positive, as well as for men, and that the effects for men are even stronger. The reason for our diverging results in terms of mere employment in this scenario would be that the estimations are most probably strongly negatively biased, for men even more than for women. This assessment seems to be plausible, as men in terms of taking up mere employment presumably tend to even more early exit events than women do. And hence, the share of non-treated controls with exit events occurring before the sanction of their matching partners, and thus the negative bias, for men is tentatively higher than for women.

Nevertheless, it is not possible to assess, with any certainty, whether the sanction effects are stronger for regular or subsidized employment. Practical plausibility considerations can point in both directions. On the one hand, sanctioned people who want to escape from the pressure of future repeated sanctions and intensified monitoring by the Jobcenters, can be assumed to have stronger incentives to find a way out of welfare receipt, be it taking up employment or leaving the labor force. It seems plausible that entering mere employment

would be preferred to starting a job needing top-up welfare benefits, because the latter option would not get them out of risk to be sanctioned again. At least people with good chances on the labor market, who do have such kind of choices, may prefer taking up mere employment to subsidized work.

On the other hand, the share of people with worse chances on the labor market within the quite heterogeneous group of welfare recipients is considerably higher than in the group of UIB recipients. Thus, it might be that even if the majority of sanctioned people would prefer to get out of welfare receipt, they cannot find a job which pays enough to cover their and their family's (if any) minimum subsistence level. These people may respond to sanctions with an increased probability of taking up employment even if this does not lead them out of welfare receipt. Although they are still at risk of being sanctioned because of the continued welfare receipt, they could expect to be less severely monitored if they start a new job, as they have demonstrated their willingness to work.

Hence, in practice, it is ambiguous whether a majority of sanctioned would respond with a higher probability either to take up mere employment, or to start a job with top-up welfare receipt. Referring to corresponding studies like, e.g. Schneider (2008, 2010), which tendentially find unsubsidized work positively, and more strongly affected than subsidized work, previous findings point more in the direction of stronger positive sanction effects resulting in the take up of mere employment than for supplementary welfare receipt. Following these findings, it can be suspected that the negative bias for men in our results regarding taking up mere employment would be even higher than assessed above; it would not just move an insignificant effect in the negative direction, but turn even a significant positive effect into negative effect estimations. This scenario cannot be ruled out — rather, it must be seen as a not unlikely possibility. If this is the case, the stronger negative ATT estimations for men just reflect a higher probability of early exit events in the group of non-treated which causes a bias high enough to not only outweigh, but even dominate the actual positive sanction effect. Following these thoughts, women obviously have lower probabilities of early exits into mere employment, and thus show a lower negative bias. In terms of employment with top-up benefits, however, sanctioned women are more likely to respond positively than sanctioned men. But as mentioned above, the bias for this exit event which does not lead out of risk is not as strong or, at the best, is even negligible.

Following the scenario of the previous paragraph, and in accordance with corresponding studies, we can assess and summarize our results as follows: the absolute values of our results concerning exit to employment are, most likely, negatively biased in a substantial extent, at least regarding entrance to mere employment. It can be assumed that the total effects of sanctions in terms of taking up mere employment are actually positive for men, possibly a bit stronger than for women, as men are generally more likely to experience early exit events to mere employment and, thus, they may be more responsive with regards to this exit event. Accordingly, their negative bias is stronger which turns their positive effects into negative estimations of the ATT. The lower negative bias of women turns their real, probably also positive, effects into insignificant estimations concerning exits into mere employment.

As the bias for exits into employment with supplementary welfare receipt seems to be considerably lower, it can be assumed that the stronger estimated effects for women reflect the real effects, considering the fact that women respond more positively to sanctions in terms of taking up subsidized work than men. Additionally, it can be presumed that a possibly still working negative bias is much weaker than for exit to mere employment. Hence, the factual treatment effects may even be positive for men, while a weak negative bias turns them to insignificant results. Whereas, for women, the effect is clearly positive and, given a still existing but small bias, presumably to a bit larger extent than the estimated ATT depict.

### **5.6.1.3 Divergences between entering mere employment and exiting welfare**

On the whole, the estimated effects of sanctions on exiting welfare are stronger than on entering mere employment. As mentioned above in Section 5.5, the difference between them is that exiting welfare also includes the option of leaving the labor force, namely the non-employment option. Although in this study, we do not explicitly analyze the exit event of leaving the labor force — we have done this already in our previous study on welfare sanctions (Hillmann and Hohenleitner (2015)) — the findings of these analysis also provide implicit insights into the non-employment option. As the estimated treatment effects on exiting welfare point in the same direction as on entering mere employment, but are stronger, we can conclude that exiting the labor market is affected by



sanctions in a similar way as entering mere employment. This does not necessarily mean to a similar *extent*, but in a similar direction. That conclusion goes along with our previous study analyzing the exit from labor force (Hillmann and Hohenleitner (2015)) and with other related studies on welfare sanctions which also consider the non-employment option (Busk (2014) for Finland and van den Berg et al. (2015) for young welfare recipients in Germany).<sup>111</sup>

According to the line of arguments and the final conclusive assessment for exit into mere employment above, the absolute values of the estimated ATT for exit from welfare must likewise be interpreted under the assumption of being negatively biased in a substantial, presumably even stronger, extent. The reason for this is that exit from welfare leads out of risk, just as exit to mere employment does, but to a stronger extent as it additionally comprises exit from labor force which also leads out of risk to be sanctioned. And as explained above in this section, and in more detail in Section 5.4.4.3, exits out of risk imply the highest potential negative bias. Hence, according to the above concluding assessment concerning exit to unsubsidized work, we can assume that the actual effect of welfare sanctions on exiting welfare — as well as the effect on both of its components, entering mere employment and leaving the labor force — is positive.

#### **5.6.1.4 Highlights of the subgroups**

As the sanction effects on various subgroups are described in detail in Section 5.5, here we shall highlight just a few apparent patterns. There are indications that people living in Western Germany tend to have stronger positive sanction effects in terms of taking up employment in general while, in terms of leaving welfare receipt, people in Eastern Germany tend to reveal stronger positive sanction effects. The latter may be driven by a higher proportion of people leaving the labor market. These findings hold more strongly for men than for women.

People with a high level of labor market access respond to welfare sanctions with a stronger enhanced transition rate out of welfare receipt than people with a medium level of labor market access, while the lowest increase of transition rates out of welfare reveals sanctioned with a low level of labor market access. Whether these findings are mainly driven by entering mere employment or

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<sup>111</sup>Even the negative results do not contradict that, as they are most likely caused by a partly strongly negative bias, which likewise distorts the outcome for leaving the labor market.

leaving the labor market can not clearly be assessed by our estimations because the subgroups with low and high levels of labor market access are too small in order to get enough exit events in the treatment group to obtain more reliable results.<sup>112</sup>

#### 5.6.1.5 Development over time

Even if the absolute values of our results are strongly biased in some cases and under certain circumstances, the curve progressions of the plotted ATT reveal insights that are valid independently of the bias which almost exclusively arises during the strata. Although the *absolute* values are also biased after the strata as depicted by the *cumulated* ATT, the *slope* after the strata is no longer affected by the bias, at least more than a short distance from the end of the strata. The progression of the slope reveals the development of the *monthly* treatment effects over time. As mentioned above in the introductory part of this subsection (Section 5.6.1), downward slopes after the stratum reveal negative monthly effects, while upward slopes reveal positive monthly effects, that are both stronger, the steeper the curve runs.

In our presentation of the plotted ATT in Section 5.5, we have seen very diverse curve shapes. A substantial proportion of these plots of cumulated ATT, though, depict either a quite continuous downwards trend in the curve, or at least a downwards trend for a considerable time before the end of the observation period. In these cases, we can suspect that the downwards slope would continue for a while longer if the observation period is extended. But also, if this latter possibility for prolonged observation periods might not hold, the visible long periods of downwards slope do reveal considerable negative monthly sanction effects in the medium and long run.

To reiterate, this is not a general assertion, as there are also various other shapes of the cumulated ATT, but it holds for the described negative curve progression, of which there are numerous examples. Nevertheless, we emphasize especially these kinds of shapes with longer periods of negative slope, and thus negative monthly effects, as they are the problematic ones in practice.

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<sup>112</sup>As our data set is based on the SIAB data, a 2% sample of the IEB data set (see Section 5.3); in case of small subsamples, we occasionally suffer a lack of treated cases and, thus, a lack of exit events in the treatment group. This could be improved by using a larger sample, or even the full IEB data set combined with administrative data.

Long-term or steeply downward heading slopes of cumulated ATT can lead to negative sanction effects as a whole, even if the initial and short-term effects are strongly positive. Whether, for some cases, negative long-term effects dominate positive short-term effects depends, amongst other factors, vastly on the time horizon of the analysis. But even if such negative effects do not fully outweigh the positive ones, including in the long run, it is a fact that there are considerable numbers of sanctioned people whose probabilities of entering employment or leaving welfare are clearly negatively affected by welfare sanctions. And even if the negative impact on them would never outweigh the positive impact on others, they cannot be neglected.

## **5.6.2 Employed people with supplementary welfare receipt**

Concerning the effects of sanctions on employed welfare recipients, we cannot compare our results with other studies as this is still a relatively new field of research, and we did not find any corresponding studies. Scientific literature on employed welfare recipients in Europe is scarce, and the effects of sanctions against this group are still almost unexplored.

As in Section 5.5.2, we have already discussed our findings for employed welfare recipients in quite some detail; also, regarding the bias and the plotted ATT, we mainly summarize our findings here in brief, and partly complement them.

### **5.6.2.1 Entering mere employment**

In contrast to unemployed welfare recipients, there are no systematic differences evident between the results of the first strata ( $S_1$ ) and the following strata ( $S_2$  until  $S_4$ ). This can be an indication that the distribution of the sanctions within the first stratum of employed welfare recipients is neither left skewed nor systematically differently distributed compared to later strata.

Similar to the results for unemployed welfare recipients discussed above, we must also reckon with a probably substantial, negative bias for the employed welfare recipients in terms of entering mere employment. Thus, we cannot assess with any certainty, whether the absolute values of treatment effects are indeed negative, especially in the short run.

Nevertheless, it stands out that the slope of the cumulated ATT is strongly and quite persistently negative. Hence, the above findings and assessments for unemployed welfare recipients concerning the development of the monthly treatment effect over time can be transferred to the group of employed people. Also, in the medium and longer terms, the probability that possible initially positive effects are outweighed, or finally dominated by negative effects in the mid and long run is seemingly even higher than for unemployed people.

Employed people receiving top-up welfare benefits with a higher level of labor market access seem to be less negatively affected by sanctions than those with medium level market access.

### **5.6.2.2 Leaving welfare receipt**

Compared to entering mere employment, the effects of sanctions upon employed people on exiting welfare are considerably less negative, or even positive. This rather strong discrepancy between both the exit events can either be caused by a lower negative bias concerning leaving welfare, or by a strong positive effect of sanctions on leaving the labor market.

On the one hand, the first strata differ from later strata, especially from the second strata, and are tendentially insignificant. Thus, they seem to be less negatively biased. In contrast, the second strata seem to be most severely negatively biased, more so than the estimations for the third and fourth strata. Nevertheless, the strong downward trends that we see for entering mere employment cannot be observed for leaving welfare.

All together, the less negative ATT for the first strata regarding exiting welfare compared to entering employment seems to be mainly caused a lower negative bias. However, the curve progressions of the later strata point to an increased probability of leaving the labor market. Should this assessment hold true, later sanctions on employed people would have stronger adverse impacts, namely increasing the probability of leaving the labor market, than sanctions in the first quarter of welfare receipt.

Nevertheless, the evaluation of the results concerning the outcomes for employed welfare recipients with respect to exit from welfare is quite complicated to interpret, and thus rather speculative at this point. Further research with a larger sample is necessary to get more detailed and reliable results in order to properly evaluate these outcomes.

### **5.6.2.3 Leaving employment for mere welfare receipt**

Regarding the exit event of quitting employment in order to merely live on welfare receipt, we face the phenomenon of positively biased ATT, which we explained in detail in Section 5.4.4.3, and already referred to and discussed in the presentation of our results in Section 5.5.2.3. Concluding these explanations, we can detect that the overwhelming positive estimated treatment effects are mainly due to a strong positive bias; as a positive bias can only arise during the strata, the cases with upwards heading slopes during a few months after the strata reveal an actual positive monthly effect of welfare sanctions against employed people in terms of quitting their job in order to live merely on welfare payments. This kind of response to sanctions is clearly politically unintended, and can be seen as an adverse effect.

Nevertheless, the vast majority of the curve progressions are downwards heading (or at least not upwards heading), even if the total effect is estimated to be positive. This implies that even if the actual effect minus the positive bias is still positive, the monthly effects tend to reduce this initially adverse effect. But as the downwards slope is mostly not very steep, a potential actual positive effect in the short run may most probably not be outweighed in the medium or even long run.

However, whether the initial effect in the short run is indeed positive, can hardly be assessed with an adequate certainty. So also in this case, further analysis with a larger sample are necessary to be able to evaluate these outcomes properly.

### **5.6.3 Indirect sanctions**

Although we have done all the analysis for indirect sanctions as comprehensively and as well as for direct sanctions, reliable results for indirect sanctions are very scarce. This seems to be not mainly a result of factual scarce effects, but seems predominantly caused by a lack of cases of merely indirectly sanctioned welfare recipients and, as a consequence, a lack of exit events in the treatment group. An analysis of the impact of indirect sanctions requires a data set with many more cases of merely indirect sanctioned people — ideally, a full dataset based on administrative data which would provide an optimal data base.

Notwithstanding the difficulties due to a lack of cases, we get at least interesting hints as to which direction the results of an analysis with a proper data set could point. We clearly see that men do not respond significantly to indirect sanctions in our data set, but women seem to respond tendentially negatively. This holds for the transition from unemployment to mere employment, as well as for the transition from employment with top-up benefits to exit from welfare.

As these assessments are not only based on the absolute values which can be negatively distorted, but are also and mainly based on the negatively sloped curve progressions for women, this can be seen as a first glance showing that it should be worth the effort to undertake further research on this topic.

## 5.7 Conclusion

For our comprehensive analysis on welfare sanctions in Germany, we use the inflow samples into welfare for the years 2007 and 2008 from an exclusively prepared rich data set, based on a 2% sample of administrative data of the German Federal Employment Agency (FEA), and provided by the Research Data Centre (FDZ) of the Institute for Employment Research (IAB), covering the years 2004 until 2010. We present and discuss our results based on the inflow sample 2008 and use the results for 2007 as one of several kinds of robustness checks.

We conduct our analysis for diverse outcomes according to the main initial samples of unemployed and employed welfare recipients. The latter are colloquially called '*Aufstocker*' in Germany, which roughly means 'top-up recipients'. The outcomes are mainly the probabilities of various exit events changing the individual labor market status (entering/exiting employment, leaving welfare), and secondarily the corresponding durations until these events occur. For reasons of simplicity and clarity, in the summarizing presentation of our results, we only refer to the outcomes with respect to probabilities.

In addition to analyzing the sanction effects for the initial samples as a whole, we also investigate various subsamples categorized by age, gender, education (which is indirectly contained in a variable comprising the individual 'labor market access', or LMA), and finally by regional differences (living in Eastern or Western Germany). Furthermore, we carry out our whole analysis of the main and the subsamples, not only for people who are directly treated by sanctions imposed against themselves (direct sanctions), but also for employable people

who are merely indirectly affected by sanctions upon their related household members (indirect sanctions).

Employing a dynamic approach of propensity score matching (PSM) with stratification is, on the one hand, appropriate and especially favorable for analyzing an extremely heterogeneous population such as employable welfare recipients. On the other hand, we have to deal with biased effect estimations which must be considered when interpreting the absolute values of our numerical and graphical results. Nevertheless, we get a huge amount of results by analyzing the monthly updated average treatment effects on the treated (ATT) over 24 months after the sanction, which reveal the development of the cumulated, as well as of the monthly ATT over two years time. — More precisely, we use quarterly strata of the individuals' welfare durations in order to cover the dynamic setting of the treatment in the case of direct sanctions. In contrast, regarding people merely indirectly affected by sanctioned household members, our time horizon is 18 months, using half-yearly strata. — The development of the ATT over time, in fact, gives insights that are valid, independent of a possible bias of the absolute values.

The findings we get from our analysis of direct sanctions against *unemployed* welfare recipients are the most extensive ones, as this group is by far the largest of the main groups of directly and indirectly sanctioned people, and thus we have a large number of exit events, not only in the control but also in the treatment group. Hence, we also get reliable and exploitable estimations for many of the smaller subgroups, which is often not the case for smaller main samples and, thus even smaller subsamples. For these results alone, there are partly corresponding studies that we can use in order to reflect our findings against the background of previous research. This is because previous studies on welfare sanctions, as a rule, only consider direct sanctions against unemployed welfare recipients.

Though receiving several negative treatment effects, especially in terms of exit to mere employment (unsubsidized work), and primarily for men, while predominantly getting insignificant ATT for women in terms of mere employment, we assess these absolute values of our numerical results as negatively biased to a substantial extent. Taking this into account, we assume tendentially positive sanction effects on employment, at least in the short run. The same is true, but even stronger for exiting welfare, and thus, this holds also for exiting the labor

force which implicitly is revealed by the divergence between the ATT for mere employment and exiting welfare. Men are more responsive to welfare sanctions in terms of taking up unsubsidized work while, in terms of entering employment with top-up benefits (subsidized work), women were found to be more responsive than men.

Still with respect to unemployed welfare recipients, we get additional findings by analyzing various subsamples. For instance, we find indications of the following differences, at least as slight tendencies: in terms of entering employment in general — comprising subsidized as well as unsubsidized work — people living in Western Germany tend to show stronger positive sanction effects, at least in the short run. In contrast, regarding exit from welfare, people in Eastern Germany show tendentially stronger positive effects in the short run. The latter may be driven mainly by a higher share of people who leave the labor force. These findings are tendentially stronger for men than for women.

Concerning the timing of the sanctions, we see differences that account for time dependence, however, we cannot identify clear patterns that are independent of the individual factors. On the whole, we find hints that early sanctions — that is those that occur within the first quarter of welfare receipt — cause rather positive effects than later sanctions. But we cannot verify this with sufficient certainty.

An outstanding and unique achievement of our analysis is that we reveal the monthly progression of the various effects of welfare sanctions over two years time. We find the *development of sanction effects over time* for diverse exit events as extremely versatile, depending on the analyzed groups and subgroups, their individual characteristics, regional differences, and the timing of the sanction.

There are some upwards heading cumulated effects, mostly with decreasing slope, which imply positive but decreasing monthly effects. Such patterns of development of the sanction effects with mainly non-negative slopes are unproblematic in terms of the considered exit events like entering employment or leaving welfare receipt. But they are, by far, not the majority. The persistent and steeply downwards trends in the progression of the cumulated effects, however, are quite problematic in practice, and thus need special attention.

Indeed, there are a considerable number of cases with long periods of downwards heading cumulated ATT, often with steep slopes, which reveal considerable negative monthly sanction effects in the medium and long run. Whether



these scenarios actually outweigh positive sanction effects in the short run, we cannot assess for certain. But also if the negative effects in the medium and long run do not dominate the initially positive effects, we clearly find negative effects for a considerable number of the sanctioned people that cannot be neglected. The fact that the adverse effects harm people who already have lower probabilities of early exit events (and thus, worse chances on the labor market), while the positive sanction effects predominantly work for people with higher probabilities of early exit events (and thus, with initially better chances on the labor market) is especially problematic.

Regarding direct sanctions against *employed* people receiving supplementary welfare benefits, the so-called '*Aufstocker*', we find patterns of sanction effects on taking up employment and on leaving welfare receipt that are quite similar to the effects for unemployed people. However, we have no corresponding studies to compare our results and assess their absolute values, which most likely are negatively biased to a certain extent. Altogether, it is quite difficult and rather speculative to evaluate to what extent a likely negative bias distorts the absolute values of the estimated ATT. Therefore, we cannot assess whether the effects on employed people are higher or lower than on unemployed. Also we do not know whether the estimated negative treatment effects are mainly due to a strong negative bias, or rather reveal factually negative sanction effects.

However, the development of the ATT over time, which we can interpret independently of a possible bias, shows strongly and even steeper downwards heading slopes compared to the unemployed. This implies that — even if the initial and maybe also the total effects are positive — there is a considerable number of employed people who are severely negatively affected by sanctions in terms of taking up mere employment (unsubsidized work) and leaving welfare receipt in general.

A totally unexplored research question is whether, and if so, to what extent, sanctions against employed welfare recipients affect their probabilities of quitting the job and merely living on welfare. Regarding this only for *employed* welfare recipients possible exit event, we get positive sanction effects. This implies that sanctions raise the probability of quitting the job and living solely on welfare receipt, which can be judged as a politically undesirable adverse effect. It has to be taken into account that, for this kind of exit event, the bias works in

the opposite direction, that is, towards positive distortions. Hence, these findings are most likely distorted by a positive bias. And as mentioned, we cannot assess the extent of the bias, in particular not for *employed* people. Thus, the absolute values of these results are contestable.

Nevertheless, we can adequately interpret the slopes of the plotted ATT which reveal the development of the monthly sanction effects over time. Concerning the exit event 'quitting employment for mere welfare receipt', the slopes are mainly slightly negative, and only in a few cases — mostly for a short time — we find positive slopes, which are the problematic ones. The time periods with negative slopes of the cumulated ATT for this exit event can be judged as desirable, because they lower a possibly enhanced transition rate from subsidized employment to unemployment, whereby the negative slopes are rather flat and thus the diminishing effect is rather small. The positive slopes for this exit event are the problematic ones, as they reveal a factual positive effect independently of a possible bias. This implies that, for a part of the employed welfare recipients, sanctions indeed enhance the probability of leaving employment in order to merely live on welfare payments. But such upwards heading curves of cumulated ATT, revealing undesirable effects, are rather rare and occur only during short periods of about three months after the strata — i.e. after the quarter during which the sanction was imposed.

Although we carry out the whole analysis for *indirect sanctions* as well as for direct sanctions, we only got very few reliable results for indirectly sanctioned people. This is mainly due to a lack of cases of merely indirectly sanctioned welfare recipients and, as a consequence, a lack of exit events in the treatment group. In order to get more reliable estimations, further research with a data set including many more cases of merely indirectly sanctioned people is necessary.

Nevertheless, we get at least a few hints as to which direction the results of an analysis with a larger and proper data set would probably point. We find men not significantly responding to sanctions against their household members; but women seem to respond tendentially negatively. This holds for *unemployed* women in terms of entering mere employment (unsubsidized work) as well as for *employed* women in terms of exiting welfare receipt.

In conclusion, we find *highly heterogeneous effects* of welfare sanctions in terms

of total effects, as well as in their progression over time. The initial effects and their development over time depend on several conditions, specifically on individual factors like age, gender, and education, on regional differences (between Eastern and Western Germany), and on the timing of the sanction.

Generally, the negative effects tend to work stronger in the medium and long term, and the positive effects tend to work stronger in the short term. Hence, the shorter the *time horizons* of studies on welfare sanctions are, the more the positive effects are overrated systematically. Especially the frequently occurring cases with strongly negative slopes of cumulated ATT indicate that the early positive effects, mainly driven by people with good labor market chances, are at the expense of people with strongly negative sanction effects, even in the long run. These detrimental sanction effects are supposed to be driven mainly by people with worse labor market perspectives.

Therefore, the observation periods for studies on the effects of welfare sanctions should be *as long term as possible*, or at least as long as any notable effects can be measured. But nevertheless, it has to be considered that — estimating the average treatment effect on the treated (ATT) — positive sanction effects even in the long run are still only *average* effects. As long as the distribution of the sanction effects is not known, we must reckon with a (possibly wide) range of sanction effects for different people, depending on their labor market chances. Thus, even in case of positive sanction effects in total, these effects might be accompanied by negative sanction effects for (possibly a minority of) sanctioned people which are detrimentally affected by sanctions. In order to clearly identify (or exclude) negative sanction effects on various (sub-)groups, *further research* is necessary, especially with focus on people with worse labor market chances.

To investigate the effects of sanctions against employed welfare recipients more deeply, and to better explore the effects also on employable people who are indirectly affected by sanctions against their household members, *further research* is also necessary for those groups. We have done a first step to also investigate these two, as yet, almost unexplored groups of welfare recipients affected by sanctions. However, for these rather small groups of welfare recipients, our 2% sample of administrative data does not provide a sufficient number of observations with exit events in the treatment group. Using a much larger sample of administrative data should lead to a larger number of reliable and exploitable results, for *employed* welfare recipients as well as for *indirectly sanctioned*.

For further research, it is desirable to avoid the problem with biased estimations

as a result of the stratification. In order to solve this problem, we developed a procedure to employ nearest neighbor matching in a way that excludes observations with exit events occurring before the treatment of the matching partner. This program, which we call the 'adjustment procedure', avoids the bias due to stratification, however, it requires a higher computer capacity than we had access to. And because of additionally strong time limitations concerning the data access, we could only use this most time-consuming procedure for spot-checks to get an approximate idea of the size of the bias problem. Nevertheless, computer routines to avoid the bias due to stratification are generally feasible, and hence should be employed whenever possible.

With our comprehensive study on the impact of welfare sanctions, we provide a huge range of results giving important new insights across the spectrum of ex-post effects on the individual labor market status. The limitations, however, should encourage further research on a still quite selectively explored research field.

# Chapter 6

## Impact of Welfare Sanctions on Employment Quality

This chapter is based upon Hohenleitner and Hillmann (2019b), *“Impact of Welfare Sanctions on the Quality of Subsequent Employment — Wages, Incomes, and Employment Stability”*, HWWI Research Paper 190.

### 6.1 Introduction

Since the vast restructurings of the unemployment insurance and welfare systems in many European countries over the last two decades, an increasing number of people are affected by the extensive monitoring and sanction systems implemented for employable welfare recipients. Under the paradigm shift from welfare to workfare, commonly referred to as ‘activation policy’, the work requirements, especially for employable welfare recipients, were strengthened, and compliance with them is kept under strong monitoring.

In Germany, under the new unemployment benefits II (UB II) system implemented in 2005 for needy job-seekers and their families, all employable family members are, in general, legally obliged to accept any job offers regardless of their occupational skills. In conjunction with the comprehensive monitoring and strict sanction regime, this can apply strong pressure to accept job offers that are detrimental to the individual’s occupational development, and hence, can also be detrimental from a public welfare point of view.

The few existing studies on ex-post effects of benefit sanctions which look beyond employment entrance and (also) regard the *quality* of the ensuing employment find obvious adverse effects. This very young field of research is by far not sufficiently investigated, especially when it comes to welfare sanctions.

This article provides the first comprehensive study about the ex-post effects of welfare sanctions in Germany, that is the UB-II-sanctions, on the quality of subsequent employment, including aspects of employment stability. Applying propensity score matching (PSM), we estimate the average treatment effects on the treated (ATT) of sanctions during a former welfare spell on several aspects of subsequent employment's quality: initial daily wage, cumulative yearly incomes over two years, and cumulative yearly durations of three mutually exclusive employment states over two years. The latter outcome variables, in particular, mainly intended to reflect the stability of subsequent employment, reveal interesting and surprising insights which may severely affect the occupational development of the previously sanctioned, even in the long run.

In contrast to previous studies which exclusively refer to *unemployed* sanctioned, we consider *employed* welfare recipients as well, in Germany referred to as the so-called "*Aufstocker*" which means "top-up benefit recipients". Furthermore, this is the first study of sanction effects on post-unemployment — respectively post-welfare — employment quality that analyzes the sanction effects not only on the sanctioned individuals but also on their employable household members. Concretely, we distinguish between *direct* and *indirect sanctions*, whereby the latter refers to people who are indirectly affected by sanctions against one of their related household members.

We address two main research questions accordingly: Firstly, to what extent do welfare sanctions which are *directly* imposed against the concerned individuals affect the quality of the subsequent employment? And secondly, do sanctions imposed on a related household member affect the quality of the subsequent employment of the *indirectly* concerned individuals? And, if yes, to what extent? These questions shall be answered for initially *unemployed* as well as *employed* welfare recipients.

The vast majority of the few previous European studies about benefit sanctions' impact on ensuing job quality look at *unemployment insurance (UI) sanctions*. Two early comprehensive studies are provided by Arni et al. (2013) for Switzerland, and van den Berg and Vikström (2014) for Sweden. Arni et al. (2013) present one of the first empirical studies on benefit sanctions that go

beyond analyzing unemployment exits and also explores the impact on post-unemployment job quality. In order to explore ex-ante effects, they separately analyze the impact of explicit warnings prior to the sanction, which in some Swiss cantons are recorded by the employment agencies. They find subsequent earnings reduced by warnings and sanctions. Employment stability, covered by subsequent employment duration, is not affected by warnings but negatively affected by imposed sanctions. The net effect on income over two subsequent years is also negatively affected by sanctions.

Another early and comprehensive study on post-UI-sanction employment quality by van den Berg and Vikström (2014) analyzes the effect of the Swedish UI monitoring and sanction scheme on job offer decisions and employment quality. Concretely, they distinguish between effects on wages and on weekly working hours, specifically, full-time versus part-time employment. They find ex-post effects of sanctions lowering the hourly wage and the weekly working hours, even in the long run. Sanctioned people accept job offers with lower occupational level, which are “to some extent irreversible” and thus lead to “a permanent human capital loss”. Concluding, they state that monitoring the rejections of job offers is less effective than monitoring the search effort.

A German study on job match quality by Caliendo et al. (2013) provides corresponding findings although not explicitly analyzing the effects of sanctions but the effects of the ‘generosity’ of the UI benefit system. They find that, despite the disincentives of a generous UI benefit system on unemployment duration, the lower time pressure to find employment leads to better job match quality. In contrast, people who are under pressure to quickly find employment because their UI benefits are exhausted take up employment with lower wages and they are more likely to exit those employments.

Another study of the German UI system’s impact on job match quality provided by van den Berg et al. (2016) finds that job offers by the employment agencies, accompanied with the threat of being sanctioned in the case of a refusal, as well as imposed sanctions, have adverse effects on the job match quality. Concretely, they find official job offers and sanctions to increase the transition rate into work at the expense of lower wages and less stable subsequent employment spells.

Besides the very early analysis of Schneider (2008, 2010) which provide the first comprehensive studies of the effects of *welfare sanctions* in Germany, regarding the effects on reservation wages, only van den Berg et al. (2015) have explicitly analyzed the effect of German welfare sanctions on the quality of subsequent

employment. Schneider (2008, 2010) found no significant effects of sanctions on reservation wages. This might be due to the fact that she used survey data which were conducted shortly after the implementation of the new German unemployment benefit system in 2005, and that the implementation during the first year was accompanied by many difficulties involving the efficiency of the monitoring and sanction system. Van den Berg et al. (2015), with their study on young welfare recipients in Germany, found adverse effects of welfare sanctions on the job match quality which they captured by estimating the initial daily wages of the subsequent employment.

Despite the interesting and important findings of previous studies, predominantly on UI sanctions, a comprehensive study about the impact of *welfare sanctions* on subsequent employment's quality is still pending. With this study about the ex-post effects of welfare sanctions in Germany on the quality of subsequent employment, comprising different aspects of quality, including employment stability, we contribute to filling this gap.

## 6.2 Sample, variables, and methods

In this section, we give a brief overview of the data sample we use, the specification of the treatment, outcome and independent variables, and of our methodological approach.

### 6.2.1 Sample

We use a combined data set based on the 'Sample of Integrated Labour Market Biographies' (SIAB), a 2% sample derived from administrative data covering the years 2004 to 2010, merged with information on individuals' welfare sanctions (namely UB-II-sanctions) and on related household members. The latter are obtained from process-produced data of the German Federal Employment Agency (FEA). The combined data set is assembled and prepared by the Research Data Centre (FDZ) of the Institute for Employment Research (IAB) at the German FEA, and was provided to us exclusively for a comprehensive research project. This project consists of two parts: the first looks at the impact of welfare sanctions on probabilities of entering or leaving employment and welfare



receipt; the other looks at the effects of welfare sanctions on the characteristics of subsequent employment, which is the focus of this article.

We use two yearly inflow samples of employable people into welfare receipt from 2007 and 2008. As there are no substantial differences, we present the results of the more current sample from 2008 and use the results of 2007 as a kind of robustness check. We analyze men and women separately, as well as the inflow cohorts of unemployed and employed welfare recipients.

### 6.2.2 Variables

We differentiate two kinds of binary *treatment variables*: direct and indirect sanctions. Direct sanctions refer to the condition where people are affected by sanctions imposed upon them directly. By ‘indirect sanctions’, we refer to the situation where people are only indirectly affected by welfare sanctions imposed upon one of their related household members.

In order to cover the quality of employment subsequent to the initial welfare spell in which the sanction of the treated was imposed, we generate the following kinds of *outcome variables*: daily wages and yearly incomes on the one hand, and cumulative durations of distinct employment states on the other hand. Daily wages refer to the initial wage of the first employment the individual takes up after the initial welfare spell. If people should take up multiple jobs, the main employment is reported. We generate the cumulative variables of the yearly incomes by summarizing the earned income during the first year and the second year (each separately) after the initial welfare spell. The cumulative durations of employment states during the first and second year after the initial welfare spell can be divided into three mutually exclusive employment states: employed, unemployed, and ‘supplementary’ which means being employed while receiving supplementary welfare benefits. Hence, by ‘employed’, we refer to people who are merely employed — that is not receiving top-up welfare payments, and by ‘unemployed’, we refer to employable people without earned income receiving either unemployment insurance (UI) or welfare benefits, in Germany, unemployment benefits I (UB I) and unemployment benefits II (UB II), respectively.

As it is beneficial for our methodological approach to use only binary or metric variables as explanatory variables, we convert nominally and ordinally scaled

*independent variables* into dummy variables. We classify and specify our independent variables as follows:

**(a) Individual characteristics (binary):**

*Age groups:* 15–17, 18–24, 25–34 (reference category), 35–44, 45–56.

*Child under 3 years in the household:* 0 = no (reference category), 1 = yes.

*Partner living in the household:* 0 = no (reference category), 1 = yes.

*Nationality:* 1 = German, 2 = EU-foreigner (reference category), 3 = non-EU-foreigner.

*School education:* 0 = no graduation (reference category), 1 = low (lower/general secondary school), 2 = middle (intermediate secondary school), 3 = high (high school).

*Vocational degree:* 0 = no (reference category), 1 = yes.

*Federal state:* registered in one of the 16 federal states in Germany (reference category: Bavaria).

**(b) Inflow quarter and previous employment states (binary):**

*Quarterly inflow cohort (into welfare receipt):* 01/2008 (reference category), 02/2008, 03/2008, 04/2008.

*Duration of previous employment:* up to 0, 3, 6, 9, 12 months (reference category),

*duration of previous unemployem:* up to 0, 3, 6, 9, 12 months (reference category),

*duration of previous 'supplementary' states:* up to 0, 3, 6, 9, 12 months (reference category),

(all in months of the year previous to welfare receipt).

**(c) Previous employment states and characteristics of previous employment (metric):**

*Duration of previous employment:* during the year previous to welfare receipt (in days).

*Duration of previous unemployment:* during the year previous to welfare receipt (in days).

*Duration of previous 'supplementary' states :* during the year previous to welfare receipt (in days).

*Daily wage of previous employment:* (in Euros).

*Cumulated income during the year previous to welfare receipt:* (in Euros).

**(d) Labor market indicators (metric):**

*Sanction rate:* (according to: month, federal state),

*unemployment rate:* (according to: month, federal state),

*vacancy rate:* (according to: month, federal state),

*rate of employable UB-II-recipients:* in relation to the whole workforce in Germany (according to: month, federal state),

(all external variables published by the FEA).

**6.2.3 Methods**

As the assignment to the treatment is not random, we have to account for the selectivity of the treatment process. In order to estimate the average treatment effect on the treated (ATT) of welfare sanctions, we employ propensity score matching (PSM). Using the matching approach based on propensity score is especially favorable for analyzing an extremely heterogeneous population like employable welfare recipients. To ensure that the use of PSM is appropriate, it should be based on a rich data set, such as the one we had access to for this analysis. In contrast to the timing of events (ToE) approach which is more popular

in this research field, and which can result in significantly biased effect estimations if restrictions imposed on the heterogeneity distribution are not justified, such assumptions are not required by the matching approach.

Despite the dynamic setting of the treatment, we do not need to apply dynamic matching approaches, like stratifying the spells of welfare duration, in order to deal with the missing start date problem in case of outcomes based on the spell duration. Such stratification, which can cause a significant bias, is not necessary for the analysis of post-spell outcomes like those we present in this article.

The results presented here are based on kernel matching (KM), while we use nearest neighbor matching (NNM) as one of several kinds of robustness checks. Concretely, we carry out NNM with  $k = 5$  nearest neighbors and a caliper of 0.01, and kernel matching using an *Epanechnikov kernel* (EKM) with a bandwidth of 0.06 in order to optimize the matching quality which we checked beforehand for several variations of these kinds of matching procedures. For further details on our methodological approach, as well as on the checks of matching quality and robustness we conducted, we refer to our comprehensive article (see Hohenleitner and Hillmann (2019a)) which focuses on the effects of welfare sanctions on spell-duration based outcomes, such as the probabilities of entering or leaving employment and welfare receipt.

## 6.3 Results

In this section, we present the estimations of the average treatment effects on the treated (ATT) of our PSM analysis based on kernel matching, for the inflow cohort 2008. The outcome variables, regarding wage, income, and durations of employment states shall indicate different aspects of the quality of employment following welfare sanctions.

### 6.3.1 Wage and cumulative income

The effect of welfare sanctions on the daily wage of the first employment subsequent to the initial welfare spell can be interpreted as an effect on the reservation wage of the sanctioned; in case of multiple jobs, the main employment is reported. The yearly income is cumulated over the first and the second year

TABLE 6.1: Effects of direct sanctions on wage and income — 2008

Outcome variables	Unemployed		Employed	
	Men	Women	Men	Women
Daily wage	-7.16***	-6.88***	-5.88***	-4.99***
Yearly income (y1)	-2064.57***	-1587.89***	-3792.73***	-3149.49***
Yearly income (y2)	-949.39***	-874.19***	-2292.53***	-1696.11***

ATT of direct sanctions on *daily wage* of the first main employment after the initial spell of UB-II-receipt and on the cumulated *yearly income* of the first (y1) and second (y2) year after the initial UB-II-spell of former *unemployed* and *employed* UB-II-recipients (in Euros); significance levels:  $\alpha=0.1^*$ ,  $\alpha=0.05^{**}$ ,  $\alpha=0.01^{***}$ .

(each separately) after the initial spell and, if necessary, summarized over multiple jobs.

### 6.3.1.1 Direct sanctions

Table 6.1 shows the treatment effect of welfare sanctions imposed directly on the affected individuals upon the subsequent daily wage and yearly income.

All estimated treatment effects are highly negatively significant. The decrease of the absolute values of the daily wage induced by former welfare sanctions for men is higher than for women, and it is higher for initially unemployed than for initially employed (former) welfare recipients. These reductions of daily wages go along with the results of previous studies which, in the vast majority, found sanctions to clearly lower ensuing wages, which is generally interpreted as revealing reduced reservation wages. The fact that women's loss of the post-unemployment daily wage is lower than men's is presumably due to women's initially lower reservation wages.

The assessment that people with initially lower (reservation) wages have a lower scope to further reduce their (reservation) wages, and that this results in smaller negative effects of welfare sanctions on the reservation wages is supported by van den Berg et al. (2015). In their study of young welfare recipients in Germany, they find second sanctions caused little to no reductions in the reservation wages (compared to the first) and account for this by the already low reservation wages caused by the first sanction.

An initial loss of the daily wage may be less problematic if the formerly sanctioned were able to close this gap after a while. Therefore it is important to not merely rely on initial wage losses, but to focus on the development of income in the medium and long run. We here regard the effects on cumulated yearly

incomes of the first year after the initial welfare spell as reflecting the development in the short and medium run, while the impact on the second year's income we regard as covering developments already in the long run.

Interpreting and comparing the results for wages and income, it has to be taken into account that changes in income are not necessarily due to changes in wages; they can also be caused by changes in the weekly working hours. Hence, sanction effects on income need not necessarily correspond with the effects on the daily wage.

Concerning the impact of welfare sanctions on yearly incomes subsequent to the initial welfare spell, we find, again, that men are stronger affected than women, in terms of absolute values. However, in contrast to the daily wages, initially employed welfare recipients face a distinctively higher loss of subsequent yearly incomes than unemployed. This may also be due to the on average higher earning potentials of the already employed compared to the unemployed, which increases the scope for possible decreases.

Furthermore, we see that the loss of income for the first year is noticeably reduced in the second year. Nevertheless, the losses are still severe, regarding the on average already considerably smaller income prospectives of (former) welfare recipients compared to the working population as a whole. But what seems even more striking is that employed men, in particular, who face the largest decreases in income due to former sanctions, are the slowest to make up for this loss in the second year.

### 6.3.1.2 Indirect sanctions

Table 6.2 shows the treatment effect of welfare sanctions imposed against a related household member of the (indirectly) affected employable individuals, on the subsequent daily wage and yearly income.

We see a strong, but only slightly significant reduction in the average daily wage of unemployed men. All other estimated effects for *unemployed* people are strong and highly significant, where men face higher absolute losses of income than women, though the reduction in daily wages is slightly larger and much stronger significant for women. Similar to the results for direct sanctioned, indirect sanctioned only partly make up for the reduction of wages and earned incomes in the second year. So still severe income losses remain during the second year after the initial welfare spell.

TABLE 6.2: Effects of indirect sanctions on wage and income — 2008

Outcome variables	Unemployed		Employed	
	Men	Women	Men	Women
Daily wage	-9.49*	-10.36***	-0.45	+0.24
Yearly income (y1)	-3953.78***	-2740.21***	-1217.61	-677.53
Yearly income (y2)	-2085.87***	-1648.49***	-382.25	-1094.40

ATT of indirect sanctions on *daily wage* of the first main employment after the initial spell of UB-II-receipt and on the cumulated *yearly income* of the first (y1) and second (y2) year after the initial UB-II-spell of former *unemployed* and *employed* UB-II-recipients (in Euros); significance levels:  $\alpha=0.1^*$ ,  $\alpha=0.05^{**}$ ,  $\alpha=0.01^{***}$ .

Initial *employed* welfare recipients, in contrast, do not show any significant response to indirect sanctions, regarding their wages and incomes.

It is striking that, for unemployed people, the negative effects of indirect sanctions on their wages and incomes are even stronger than the effects of direct sanctions in absolute values. But these groups of unemployed are not identical, and thus not directly comparable, because the indirect sanctioned are restricted to households with more than one person ('multi-person households'), while direct sanctioned also include single households.

As the study of van den Berg et al. (2015) finds that sanction effects are lower for young welfare recipients in multi-person households than in single-person households, the conclusion seems evident that the discrepancy between the effects of direct and indirect sanctions might be driven by a possibly substantially higher responsiveness of parents in response to their sanctioned children.

Another reason for the seemingly higher responsiveness of the indirect sanctioned might be that people living with their partners may have a higher combined earned income at their disposal. Specifically, if the employed partner of an unemployed person is sanctioned, the willingness and ability to make concessions on the expected income of the unemployed might be higher than in case of singles.

### 6.3.2 Cumulated durations of employment states

The cumulative durations of employment states are intended to reflect, in a manner, employment stability, which is regarded as one aspect of employment quality. However, these variables do not necessarily reflect the stability of continuous spells, which might be more important for the affected individuals; instead, these outcome variables cover a more superordinate notion of stability that focuses on the cumulated time periods an individual spends in the distinct

employment states subsequent to welfare sanctions. This might be of greater relevance from a public welfare point of view.

### 6.3.2.1 Direct sanctions

Table 6.3 shows the treatment effect of welfare sanctions which are directly imposed against the affected individuals, on the cumulated durations of the subsequent employment states: employment, unemployment and 'supplementary', that is employment with top-up welfare benefits.

The initially *unemployed* welfare recipients show a clear and unambiguous pattern: the total duration of employment is strongly and significantly negatively affected, and the cumulative duration of unemployment is strongly and significantly positively affected by former welfare sanctions; this holds for at least two years after the initial welfare spell. The extent of the effects are roughly in the range of 10 to 20 days within the first two years after the initial spell. Hence, welfare sanctions against unemployed clearly affect their future employment states in the direction of shorter (cumulative) periods of employment and longer periods of unemployment.

The effect of former sanctions on the status of being employed with supplementary welfare benefits, however, is not statistically significant to any appreciable extent. That is, former welfare sanctions provoke a shift from employment predominantly towards unemployment, and to a negligible extent (if at all), towards employment with supplementary welfare receipt. The reduction of days in employment in favor of days in unemployment decreases from the first to the second year after the initial welfare spell, but still remains more than 50% of the shift in the first year.

Although, in the first year women are only slightly worse off than men, they make up for the loss of employment duration in the second year to a much lesser extent. While men's reduction of days in employment in the second year is around 55% of the first year, women's decrease of days in employment in the second year are still more than 70% of the first year. Regarding days in unemployment, of the increase of 18.27 days for women in the first year, more than 80% remains during the second year.

Compared to the unemployed, initially *employed* welfare recipients show a quite similar pattern in the first year, but a more different pattern in the second year after the initial welfare spell in which the sanction took place. In the first year,



TABLE 6.3: Effects of direct sanctions on employment states — 2008

Outcome variables	Unemployed		Employed	
	Men	Women	Men	Women
Employment (y1)	-22.38***	-21.66***	-29.51***	-19.08*
Employment (y2)	-12.27***	-15.16***	-14.58*	-16.88*
Unemployment (y1)	+18.23***	+18.27***	+13.80**	+16.41**
Unemployment (y2)	+10.36***	+15.46***	+4.84	+11.78
Supplementary (y1)	-1.46	-0.94	-1.04	-14.83**
Supplementary (y2)	+1.07	-0.70	+3.64	-6.09

ATT of direct sanctions on the cumulated duration (in days) of unsubsidized *employment*, *unemployment*, and employment with *supplementary* UB-II-receipt during the first (y1) and second (y2) year after the initial UB-II-spell of former *unemployed* and *employed* UB-II-recipients; significance levels:  $\alpha=0.1^*$ ,  $\alpha=0.05^{**}$ ,  $\alpha=0.01^{***}$ .

the highly significant loss of 29.51 employment days for men is considerably larger than for women with an average decrease of 19.08 employment days, which additionally is only weakly significant. But men make up for the loss by even a bit more than 50%, while women's loss of employment days in the second year is still considerably more than 80% of the first year's reduction.

In contrast to initially unemployed, employed people experiencing a welfare sanction do not just shift from employment towards unemployment. Some of them seem to shift from mere employment, i.e. without top-up benefits, to employment with supplementary benefit receipt, but obviously not to a considerable extent, as we get no significant positive results for the employment state 'supplementary'. Hence, the striking differences between higher employment decreases compared to lower unemployment increases might be explained by exits from labor force, as this so-called 'non-employment option' is the only option not covered by the mutually exclusive employment states. The 14.83 lost days in employment with supplementary welfare receipt for the first year of women also tend to be a shift towards exiting the labor market.

### 6.3.2.2 Indirect sanctions

Table 6.4 shows the treatment effect of welfare sanctions imposed against a related household member of the (indirectly) affected employable individuals, on the cumulated durations of their subsequent employment states.

It is striking that we get large and highly significant negative effects on the (cumulative) periods in employment for initially *unemployed* welfare recipients. Additionally striking is that these high losses of around 60 employment days in

TABLE 6.4: Effects of indirect sanctions on employment states — 2008

Outcome variables	Unemployed		Employed	
	Men	Women	Men	Women
Employment (y1)	-58.18***	-61.19***	-36.47	-13.35
Employment (y2)	-45.86***	-46.69***	-19.41	-8.97
Unemployment (y1)	+5.90	+7.64	-8.85	-4.39
Unemployment (y2)	-22.53	+9.54	-10.08	+2.90
Supplementary (y1)	-2.17	3.65	+19.58	-1.31
Supplementary (y2)	-5.51	+1.14	-11.99	-17.93**

ATT of indirect sanctions on the cumulated duration (in days) of unsubsidized *employment*, *unemployment*, and employment with *supplementary* UB-II-receipt during the first (y1) and second (y2) year after the initial UB-II-spell of former *unemployed* and *employed* UB-II-recipients; significance levels:  $\alpha=0.1^*$ ,  $\alpha=0.05^{**}$ ,  $\alpha=0.01^{***}$ .

the first year and around 45 days in the second year are not reflected by corresponding increases of periods in unemployment or 'supplementary'. This provides strong evidence for a substantial shift from unemployment to leaving the labor force. A similar deduction holds for initially *employed* women receiving top-up benefits, who also show a significant loss of days in 'supplementary' in the second year which is not reflected in any other employment state, and thus also provides strong evidence for a shift of employment states towards leaving the labor market.

These outstanding and surprising results about the effects of indirect sanctions may be caused by the following feasible constellations: as people within a welfare receiving household cannot leave the labor market separately from the remaining household members, two main possibilities come into question. Firstly, another household member may have increased the household's income to a sufficient extent to bring them out of welfare receipt; the previously indirectly sanctioned unemployed household members thus would be regarded as out of the labor force if they neither take up employment nor are registered as unemployed seeking employment.

Secondly, people leaving the labor market can do so by leaving the household and living on the income of other people, possibly a new partner. As outlined in our previous article on welfare sanctions, Hillmann and Hohenleitner (2015), focusing on the non-employment option, there are a bunch of other possibilities to live out of labor force. Besides living on partner's income, such alternatives could be living on parents' income, on assets, student's assistance (Røed and Westlie (2012)) or even on illegal work, begging or criminal activities (Machin and Marie (2004), Ames (2009), Götz et al. (2010), Schreyer et al. (2012), Wolff (2014), van den Berg et al. (2015)).

## 6.4 Summary and conclusion

In this evaluation of the ex-post effects of German welfare sanctions on subsequent employment quality based on administrative data, we have addressed two main questions: firstly, to what extent do sanctions which are *directly* imposed on the concerned individuals affect the quality of the subsequent employment? And secondly, do sanctions, imposed on a related household member affect the quality of the subsequent employment of the *indirectly* concerned individuals? And if yes, to what extent? Applying propensity score matching (PSM), separately conducted for initially *unemployed* and *employed* welfare recipients, as well as for men and women, each divided into direct and indirect sanctions as treatments, we find evidence for the following results.

Welfare recipients who experience sanctions in the form of temporary benefit cuts imposed against them (*direct sanctions*) show strong and highly significantly negative effects on the *daily wage* of the subsequent employment and on the *yearly incomes* during the two years after the initial welfare spell. This holds true for men and women, both for initially unemployed as well as employed welfare recipients. Our findings go along with previous studies on *unemployed* people, although most of these studies focus on unemployment insurance sanctions. The results of our study expand previous research by also considering sanctions on *employed* welfare recipients.

Although employed people's subsequent *wages* are slightly less negatively affected than the wages of formerly unemployed, in terms of *yearly incomes*, however, *employed* people are considerably more negatively affected. Formerly employed face a reduction of their yearly income of around 3150 Euros (women) and 3790 Euros (men) in the first year after the initial welfare spell. Although the reduction of yearly income decreases in the second year, the catch-up process for the employed, however is, on average, slower than for the unemployed.

Our analysis of the effects of direct sanctions on the yearly cumulative durations of subsequent employment states, meant to reflect a kind of *employment stability*, show that those who are sanctioned have significantly shorter durations of employment and are therefore unemployed for longer. This holds true for the initially unemployed with high significance, and with lower significance for the initially employed. Unemployed men and women lose around 22 days in employment in the first year and around 12 (men) and 15 (women) days in the second year after the initial welfare spell. Employed men in the first year,

however are worse off and lose almost 30 employment days, but make up for this loss quicker than others in the second year, with only about 15 days of lost employment.

Our findings that (direct) sanctions have adverse effects on subsequent employment stability go along with previous studies on sanctions against (*unemployed*) *UII recipients*. Further analysis, namely of the sanction effects for *employed welfare recipients* (the so-called "*Aufstocker*"), provided for the first time by this study, show that the (employed) "top-up benefit recipients" also face significant adverse effects on their subsequent employment durations, as well as on their yearly incomes.

Another contribution to the body of investigations of sanction effects is that we also regard employable people, who are indirectly affected by sanctions against their household members (*indirect sanctions*). Carrying out the same analysis for indirect sanctioned reveals interesting and surprising insights.

In terms of *wages and income*, indirect sanctions affect unemployed people surprisingly strongly, and generally with high significance. In the first year after the initial welfare spell, the unemployed lose around 3950 Euros (men) and 2740 Euros (women) of earned income. In the second year, the loss is still strongly significant and considerably high — around 2085 Euros (men) and 1650 Euros (women). Employed people, in contrast, show no significant effects of indirect sanctions on their subsequent wages and incomes.

Concerning the effects of indirect sanctions on *employment stability*, we also find strong significant adverse effects for initially unemployed and, with one exception, virtually no significant effects for the employed. The negative impact of indirect sanctions on the duration of employment for previously unemployed is surprisingly high, with roughly 60 days in the first year and around 46 days in the second year, for women slightly more than for men. The most striking and surprising result, however, is that these losses in employment duration neither reflect the increased durations of unemployment, nor employment with top-up benefit receipt ('supplementary'). Thus, we must conclude that the loss of duration in employment is in favor of additional time out of the labor market ('non-employment' state). The mainly insignificant results for employed people show only one exception: employed women who are indirectly affected by a household member's sanction show around 18 days less in supplementary welfare receipt in the second year. This loss is evidently in favor of the non-employed state.

In conclusion, it is worth looking beyond welfare exit and employment entrance, and also considering the quality of employment subsequent to sanctions. Our analysis provide strong evidence that the findings of previous studies — mostly about sanctions against (unemployed) UI recipients — which reveal adverse effects of sanctions on employment quality in terms of wages, incomes and employment stability, also hold true for the unemployed as well as employed welfare recipients. Furthermore, our results show that even household members of the sanctioned who are employable but currently unemployed suffer surprisingly huge and significant adverse effects of sanctions against their family members. For employed household members the effects are predominantly statistically insignificant.

Altogether, our results show strong evidence that the averagely increased transition rates into employment found by most studies on sanction effects are at the expense of employment quality, and caused by higher willingness to make concessions on the quality of employment. As with previous studies which found those adverse effects continued in the long run, our findings show that, on the one hand, there is a notable catch-up process, but, on the other hand, this catch-up process is by far not strong enough to approach the outcomes of the non-sanctioned, even within a period of two years.

For political implications of these findings, the following should be considered. Generally, there is a trade-off between avoiding long-term unemployment and pushing unemployed towards sub-optimal employment which is detrimental to their occupational development. The trade-off can be seen from the individual's welfare point of view, as well as from a public welfare point of view. The empirical findings on short- and long-term effects of employment caused by enhanced pressure, however, provide strong evidence that in practice, the net effect of pushing people into detrimental employment by far exceeds the advantages of avoiding longer periods of unemployment. This holds true for diverse factors which increase the pressure to take up sub-optimal employment, such as exhausted benefit payments (Caliendo et al. (2013)), being legally obliged to accept job offers with lower occupational level (van den Berg and Vikström (2014) and van den Berg et al. (2016)), the ex-ante effects of the threat to be sanctioned (Arni et al. (2013)), and the ex-post effects of imposed benefit and welfare sanctions.

Altogether, there are strong indications which give rise to concern that sanctioned welfare recipients and their unemployed household members accept

jobs with bad conditions and lower occupational levels, which is to some extent irreversible, and thus leads to a permanent loss of earned income, employment stability, and human capital.

# Appendix A

## Appendix to Chapter 5

### Figures

Table A.1 gives an overview of the following figures with plotted ATT, referred to in Section 5.5 for different groups and subgroups of welfare recipients. The first two parts (Figure A.1 until Figure A.15 and Figure A.16 until Figure A.27), described in Section 5.5.1, give an overview of the graphs for *unemployed* welfare recipients' (*UE*) transition to exiting welfare (*ExWel*) and to employment, distinguishing transition into job only (*O*), job with supplementary welfare receipt (*S*), and job in general (*G*). The following three parts (Figure A.28 until Figure A.35), described in Section 5.5.2, give an overview of the graphs for transition from employment with supplementary welfare receipt to entering mere employment (*O*), to exiting welfare (*ExWel*), and to exiting Job (*ExJob*), all for *employed* welfare recipients (*Emp*). And finally, the last part (Figure A.4 until Figure A.6), gives an overview of the graphs for people, effected by *indirect* (*ind*) sanctions, i.e. caused by a sanctioned household member, described in Section 5.5.3. The graphs show the plots of monthly updated cumulated ATT for four quarterly strata ( $S_1$ – $S_4$ ) in case of *direct* sanctions and for two half-yearly strata ( $S_1$ – $S_2$ ) in case of *indirect* sanctions.<sup>1</sup>

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<sup>1</sup>Some graphs depict less strata due to convergence problems, caused by too few cases in the treatment group.

TABLE A.1: Overview of the figures with plotted ATT<sup>1</sup>

Figure	Table	Section	Sanction (dir/ind)	Status (UE/Emp)	Exit to job (O/S/G) / from welfare (ExWel) / from job (ExJob)	Group / Subgroup
1–3	5.4–5.5	5.5.1.1	dir	UE	O/S/G	all
4–6	5.4–5.5	5.5.1.1	dir	UE	O/S/G	West
7–9	5.4–5.5	5.5.1.1	dir	UE	O/S/G	medium
10	5.4–5.5	5.5.1.1	dir	UE	G	u25/West
11–13	5.4–5.5	5.5.1.1	dir	UE	O/S/G	o25
14–15	5.4–5.5	5.5.1.1	dir	UE	O/S	o25/West
16	5.7	5.5.1.2	dir	UE	ExWel	all
17	5.7	5.5.1.2	dir	UE	ExWel	West
18	5.7	5.5.1.2	dir	UE	ExWel	Ost
19	5.7	5.5.1.2	dir	UE	ExWel	low
20	5.7	5.5.1.2	dir	UE	ExWel	medium
21	5.7	5.5.1.2	dir	UE	ExWel	high
22	5.7	5.5.1.2	dir	UE	ExWel	u25
23	5.7	5.5.1.2	dir	UE	ExWel	u25/West
24	5.7	5.5.1.2	dir	UE	ExWel	o25
25	5.7	5.5.1.2	dir	UE	ExWel	o25/West
26	5.7	5.5.1.2	dir	UE	ExWel	o25/medium
27	5.7	5.5.1.2	dir	UE	ExWel	o25/high
28	5.8	5.5.2.1	dir	Emp	O	all
29	5.8	5.5.2.1	dir	Emp	O	West
30	5.8	5.5.2.1	dir	Emp	O	medium
31	5.10	5.5.2.2	dir	Emp	ExWel	all
32	5.10	5.5.2.2	dir	Emp	ExWel	medium
33	5.12	5.5.2.3	dir	Emp	ExJob	all
34	5.12	5.5.2.3	dir	Emp	ExJob	high
35	5.12	5.5.2.3	dir	Emp	ExJob	o25/West
36–37	5.13	5.5.3	ind	UE	O/G	West

<sup>1</sup>Figures: Plotted ATT on probabilities for transition from un-/employment to different exit events, corresponding to the tables with condensed results based on binary outcomes in Section 5.5.

<sup>2</sup>Exit events: exit to: job only (O), job with supplementary welfare receipt (S), job in general (G); exit from: welfare (ExWel), Job (ExJob).

Subgroups: age-group in years: all=15–56, u25=15–24, o25=25–56; region: West/East German states; level of labor market access (LMA): low, medium, high.



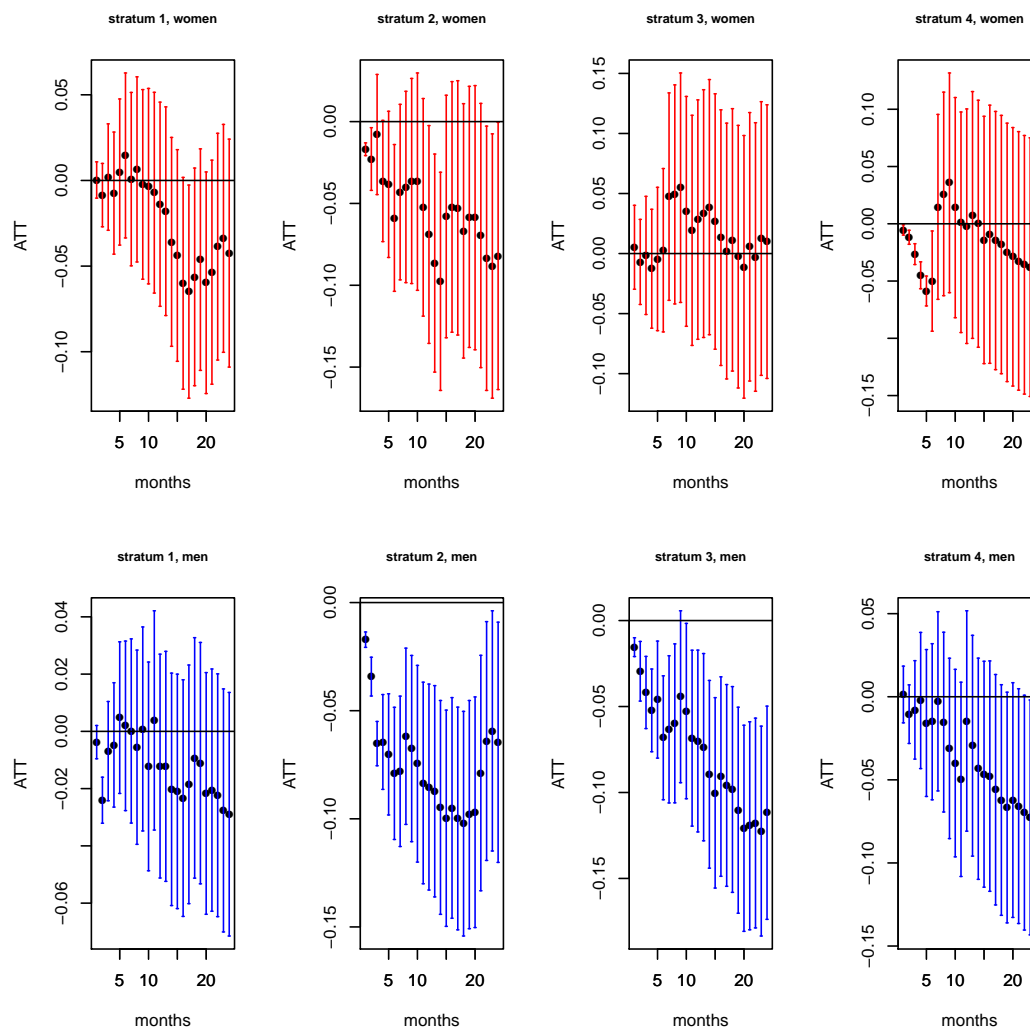


FIGURE A.1: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*”) (O) and its 90% confidence interval of *direct sanctions* for *unemployed (UE)* welfare recipients of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

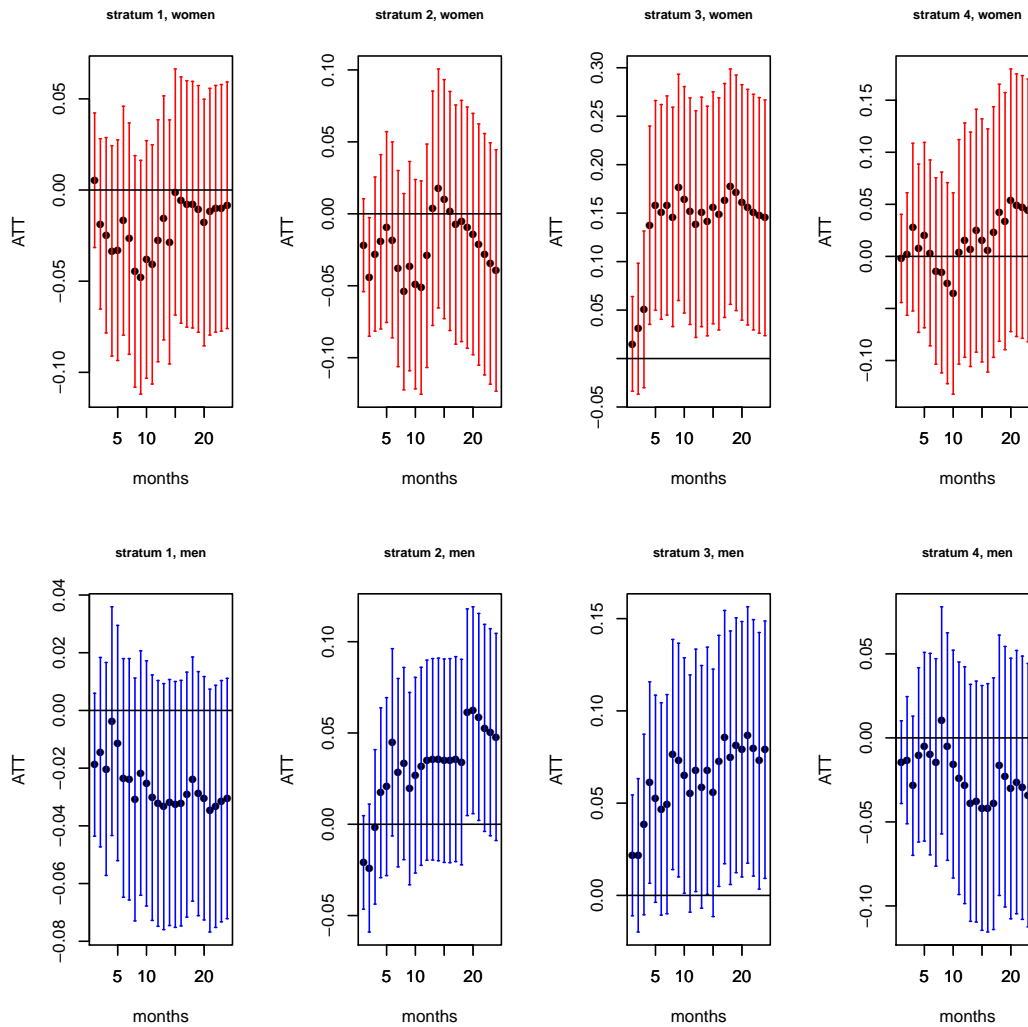


FIGURE A.2: The plots show the monthly updated ATT on the probability of exit into *employment with top-up benefits* (“*supplementary*” ( $S$ )) and its 90% confidence interval of *direct sanctions for unemployed* ( $UE$ ) welfare recipients of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

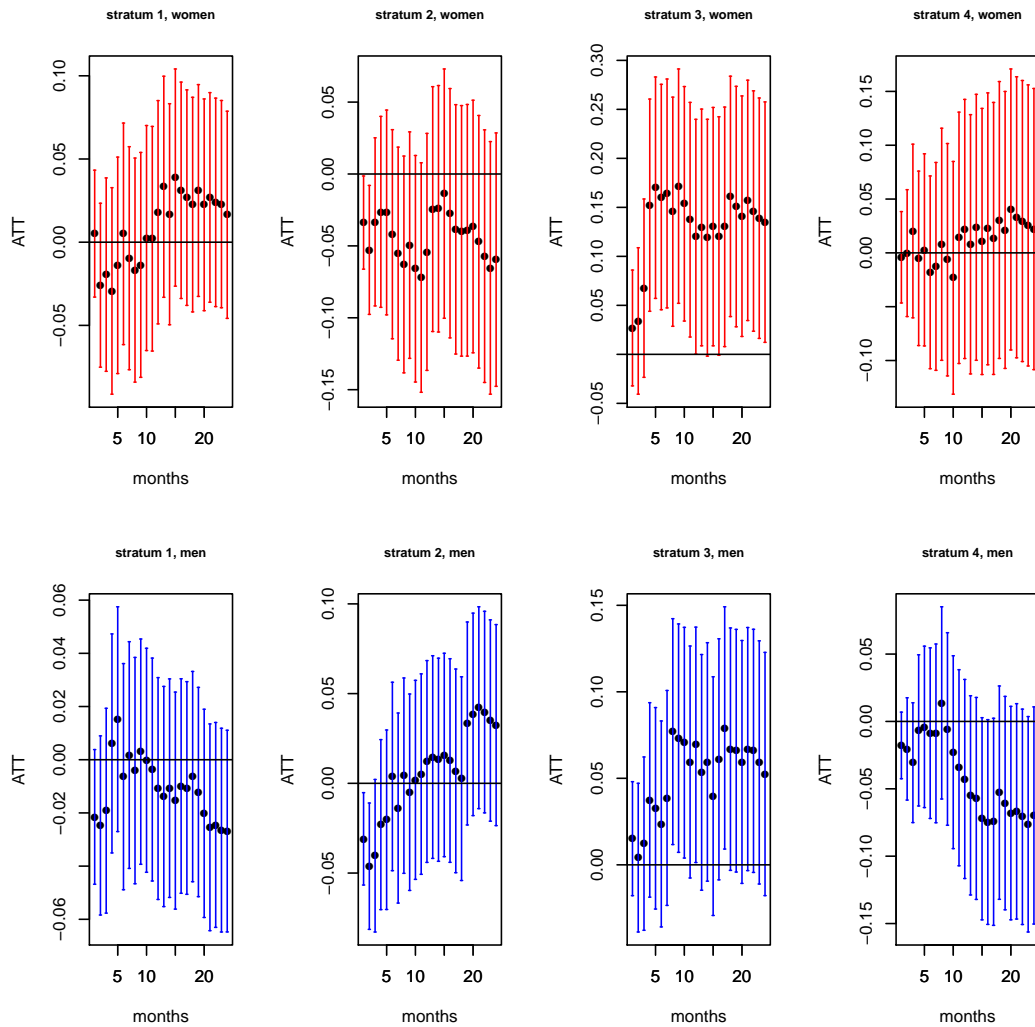


FIGURE A.3: The plots show the monthly updated ATT on the probability of exit into *employment* (“*job in general*” (G)) and its 90% confidence interval of *direct* sanctions for *unemployed* (UE) welfare recipients of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

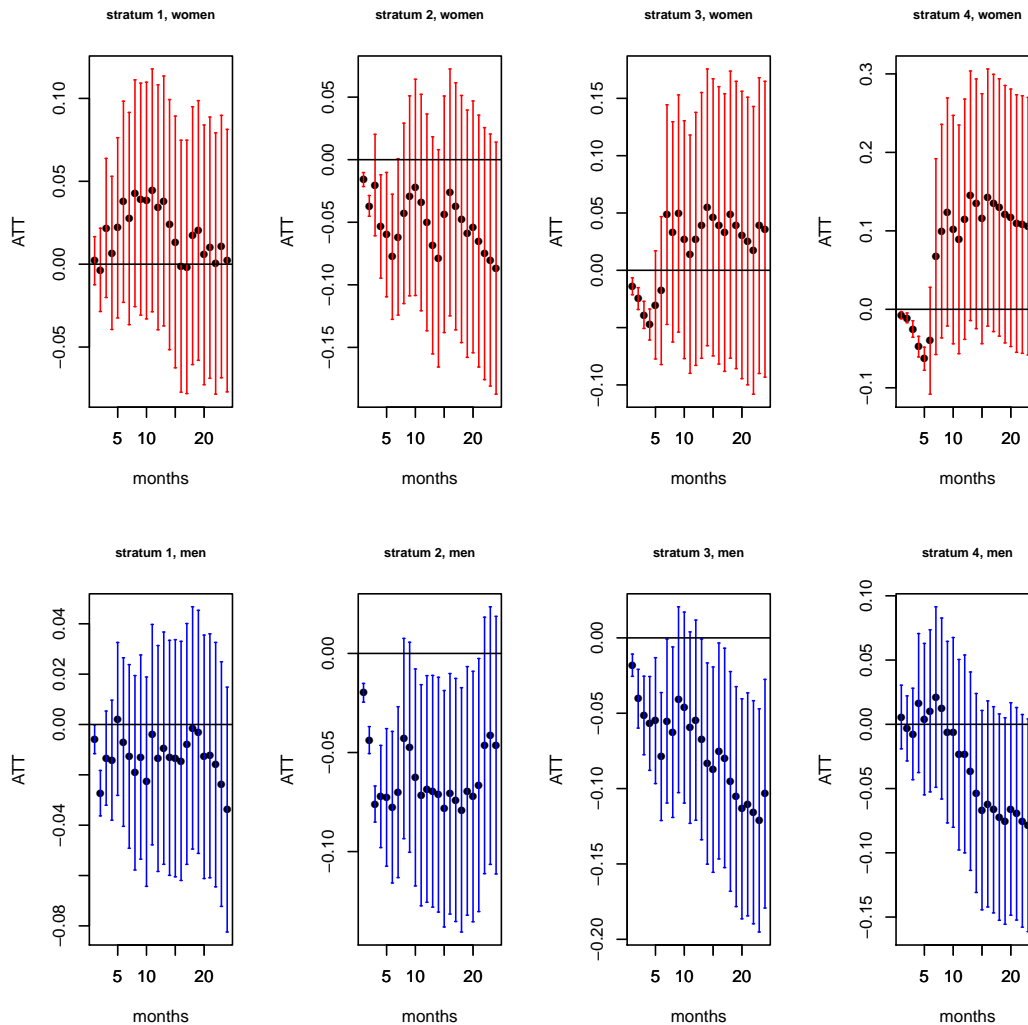


FIGURE A.4: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*” (O)) and its 90% confidence interval of *direct sanctions* for *unemployed* (UE) welfare recipients in *Western Germany* (WG) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

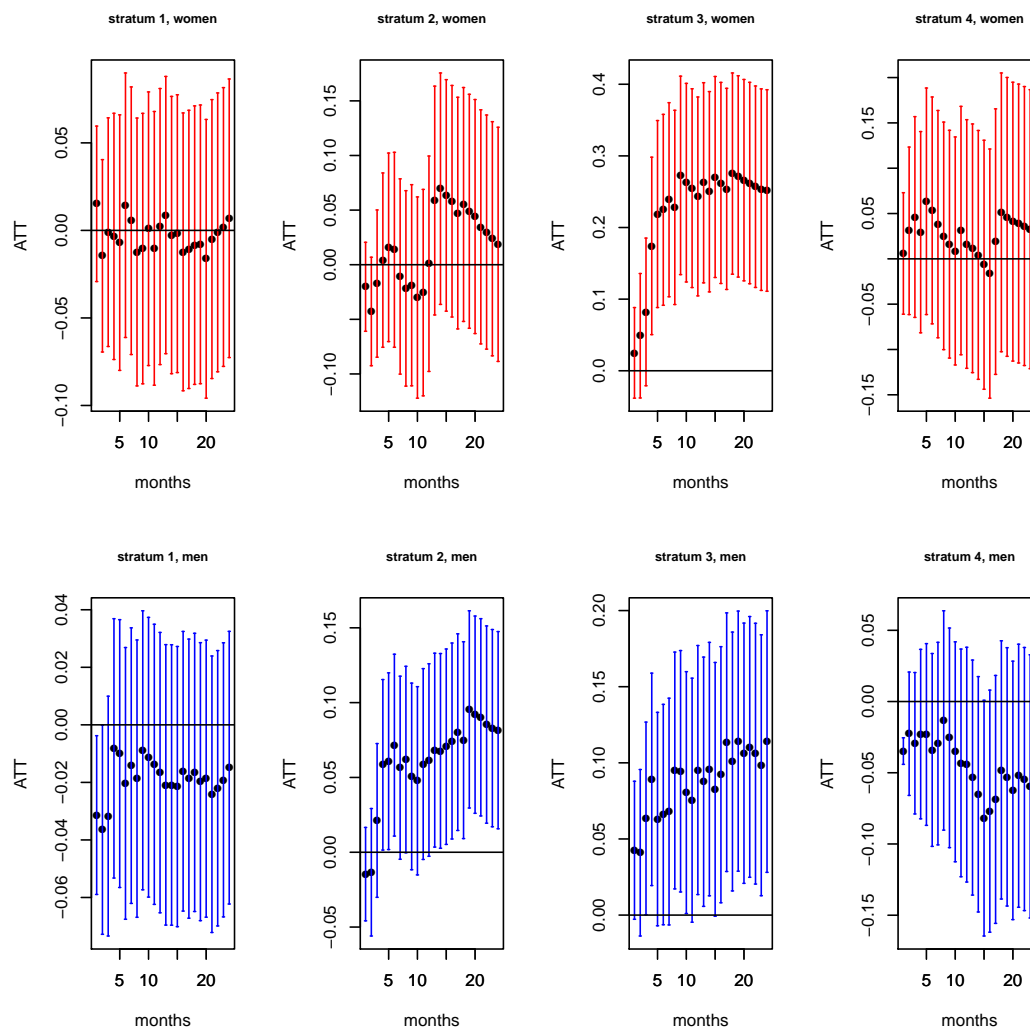


FIGURE A.5: The plots show the monthly updated ATT on the probability of exit into *employment with top-up benefits* (“*supplementary*” ( $S$ )) and its 90% confidence interval of *direct sanctions for unemployed* ( $UE$ ) welfare recipients in *Western Germany* ( $WG$ ) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

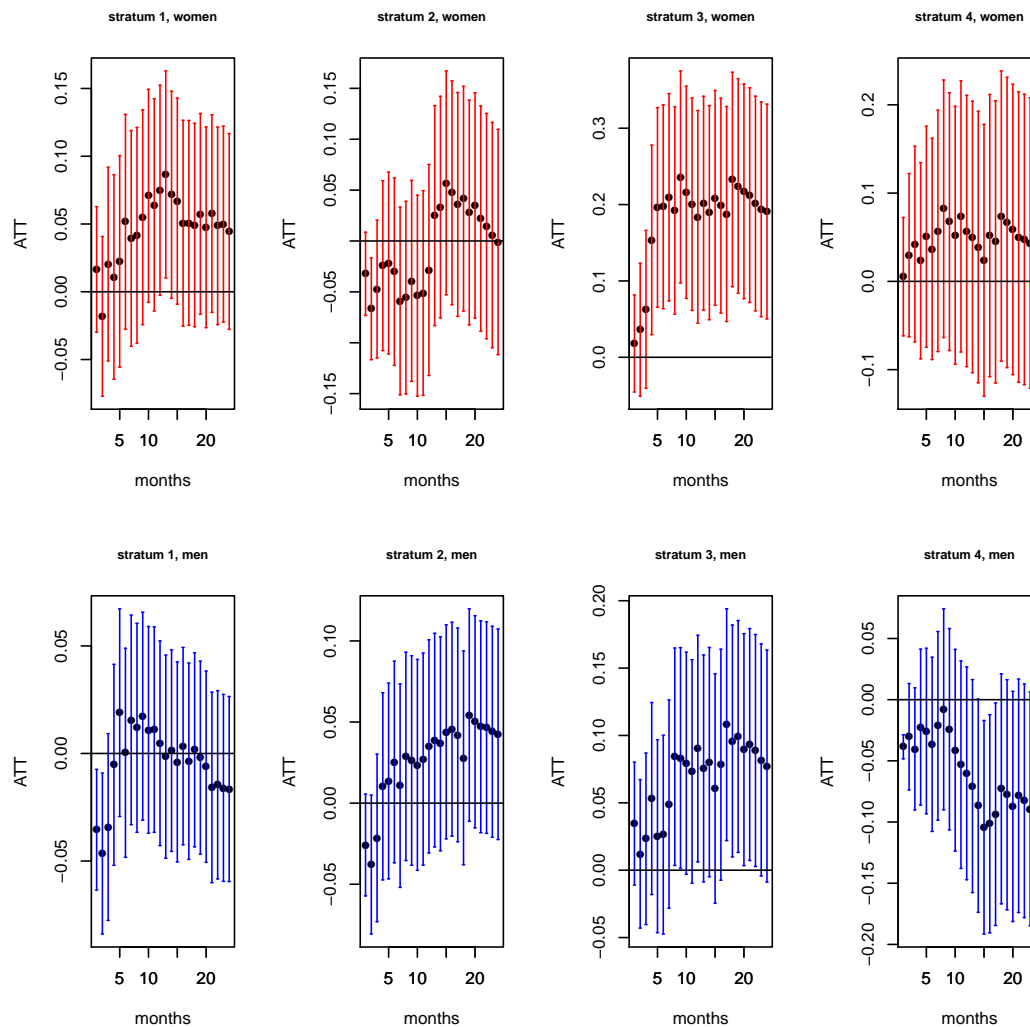


FIGURE A.6: The plots show the monthly updated ATT on the probability of exit into *employment* (“*job in general*” ( $G$ )) and its 90% confidence interval of *direct sanctions for unemployed* ( $UE$ ) welfare recipients in *Western Germany* ( $WG$ ) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

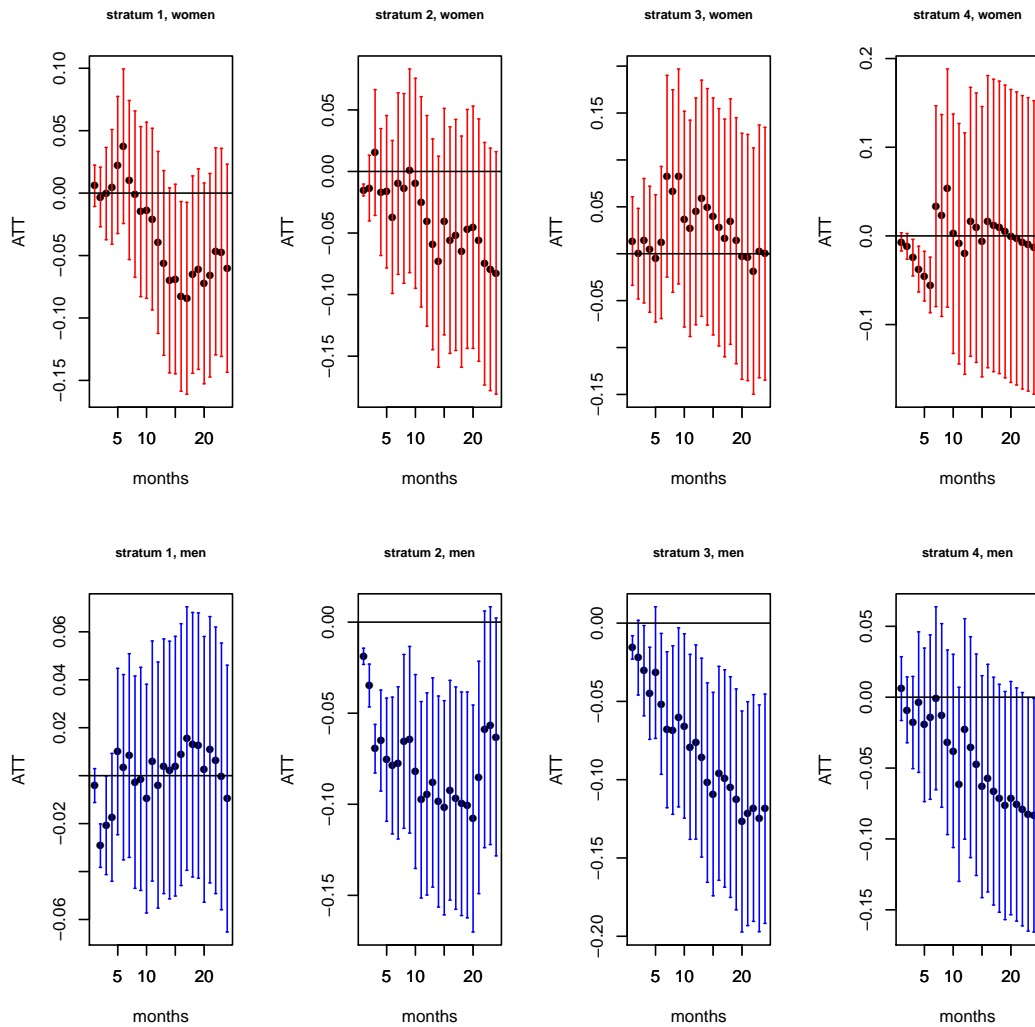


FIGURE A.7: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*” (O)) and its 90% confidence interval of *direct sanctions* for *unemployed (UE)* welfare recipients with *medium-level labor market access (LMA)* of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

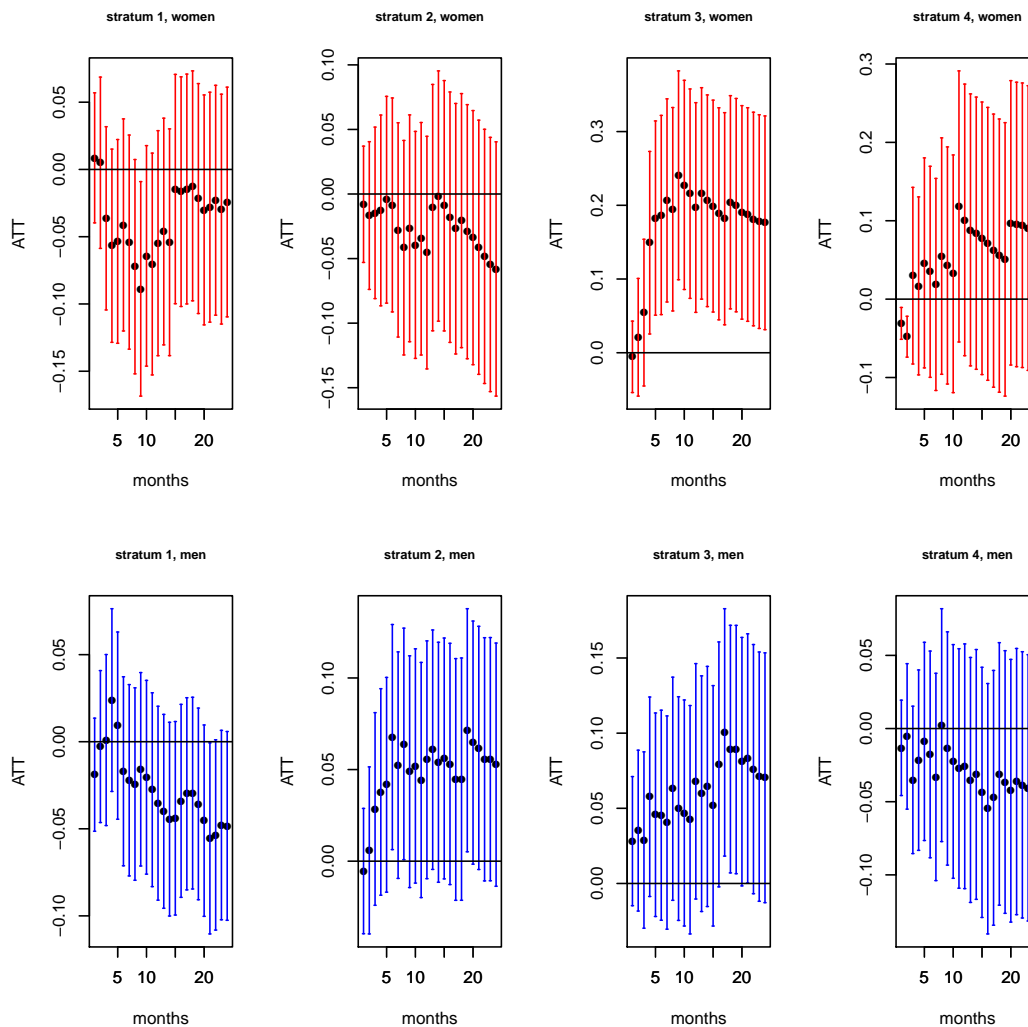


FIGURE A.8: The plots show the monthly updated ATT on the probability of exit into *employment with top-up benefits* (“*supplementary*” (*S*)) and its 90% confidence interval of *direct sanctions for unemployed* (*UE*) welfare recipients with *medium-level labor market access* (*LMA*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.



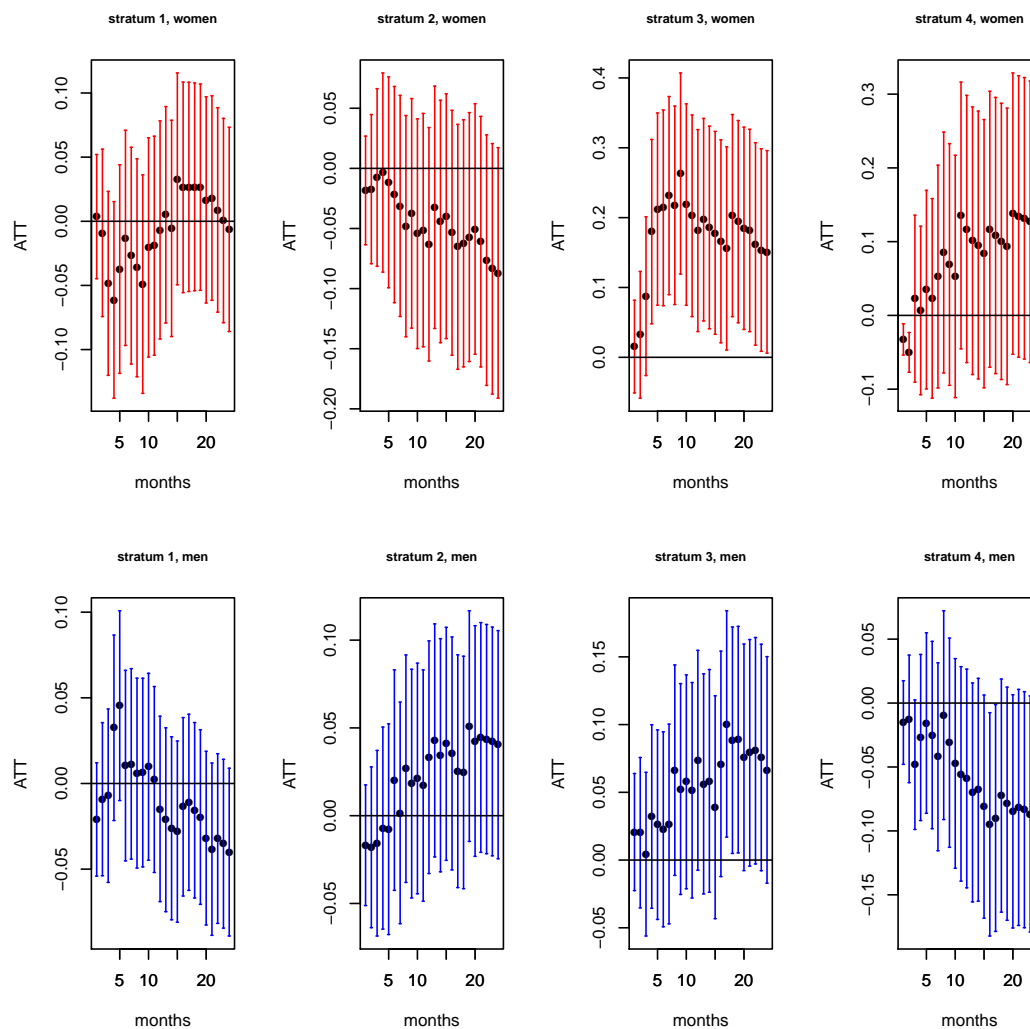


FIGURE A.9: The plots show the monthly updated ATT on the probability of exit into *employment* (“*job in general*” ( $G$ )) and its 90% confidence interval of *direct sanctions* for *unemployed* ( $UE$ ) welfare recipients with *medium-level labor market access* ( $LMA$ ) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

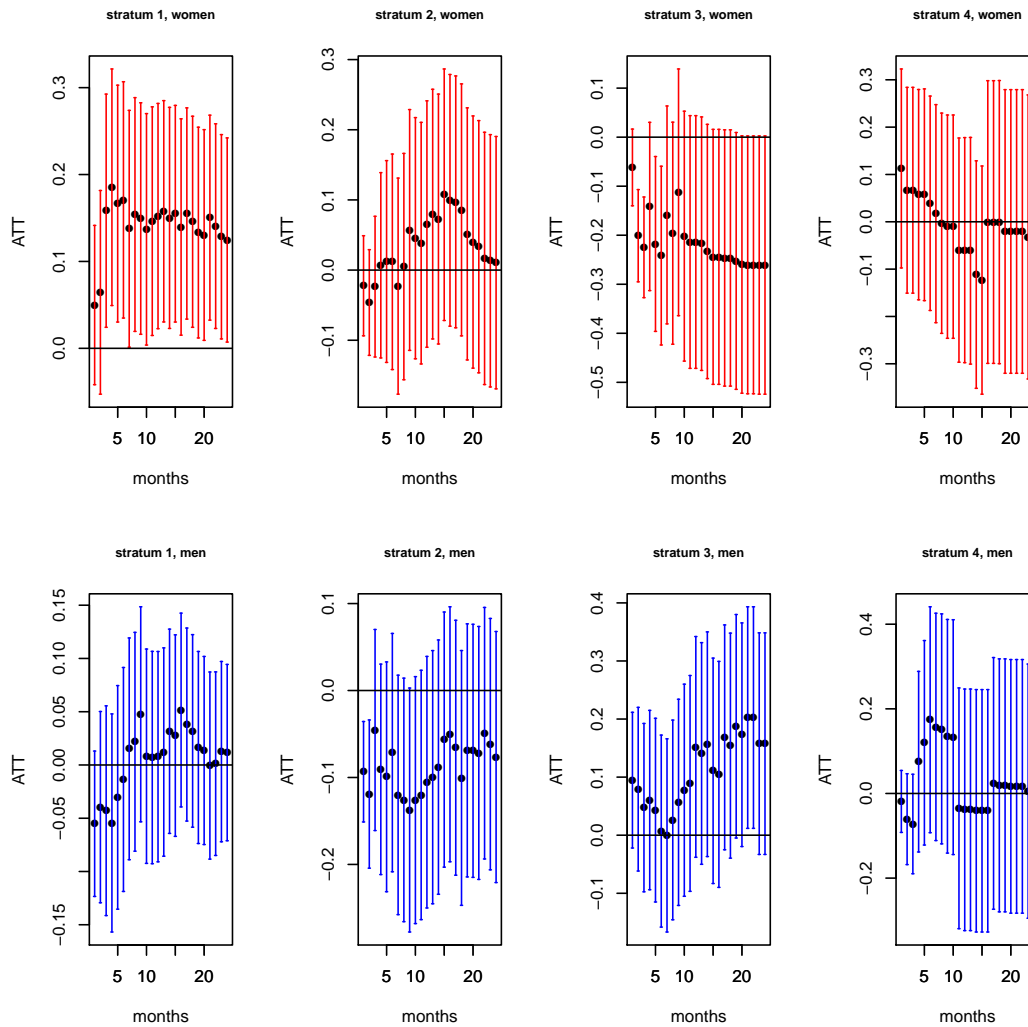


FIGURE A.10: The plots show the monthly updated ATT on the probability of exit into *employment* (“*job in general*” ( $G$ )) and its 90% confidence interval of *direct sanctions for unemployed* ( $UE$ ) welfare recipients *under 25 years* ( $u25$ ) in *Western Germany* ( $WG$ ) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

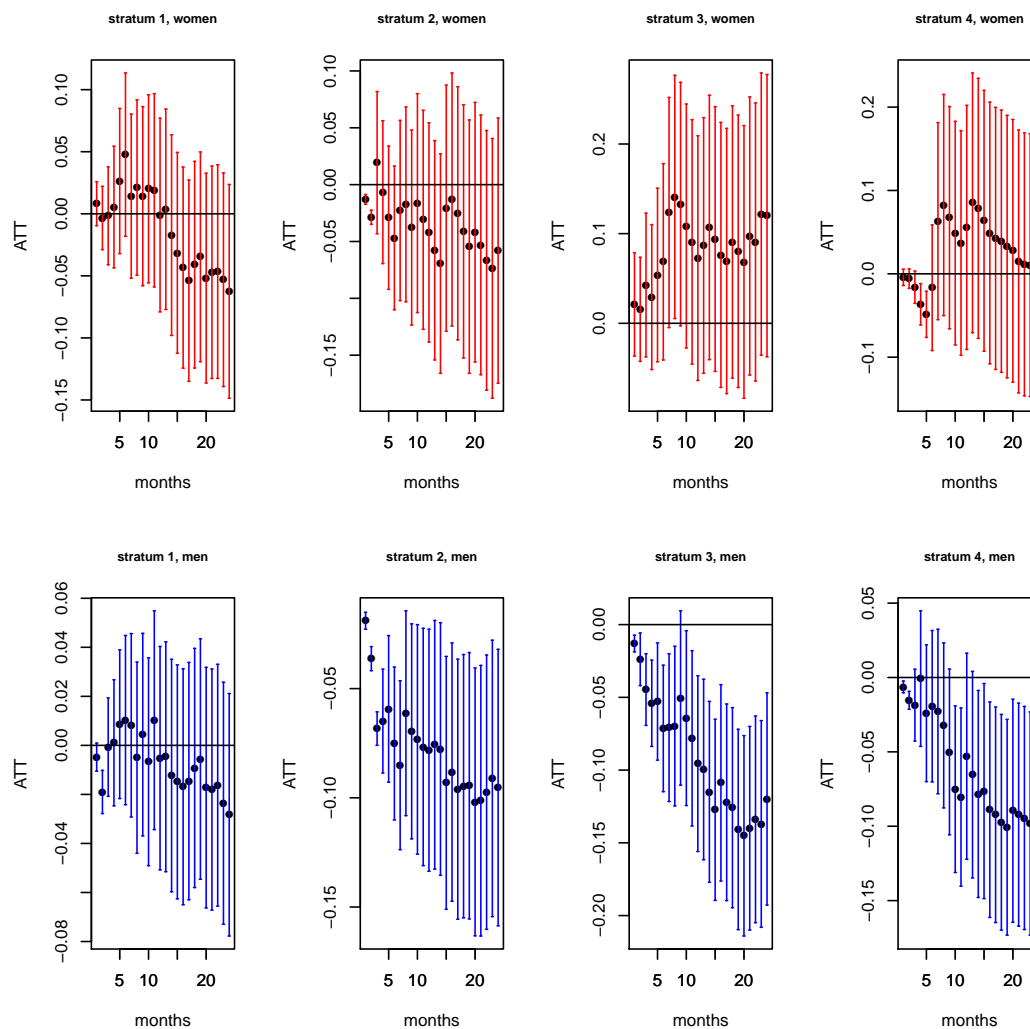


FIGURE A.11: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*” (O)) and its 90% confidence interval of *direct sanctions for unemployed (UE) welfare recipients over 25 years (o25)* of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

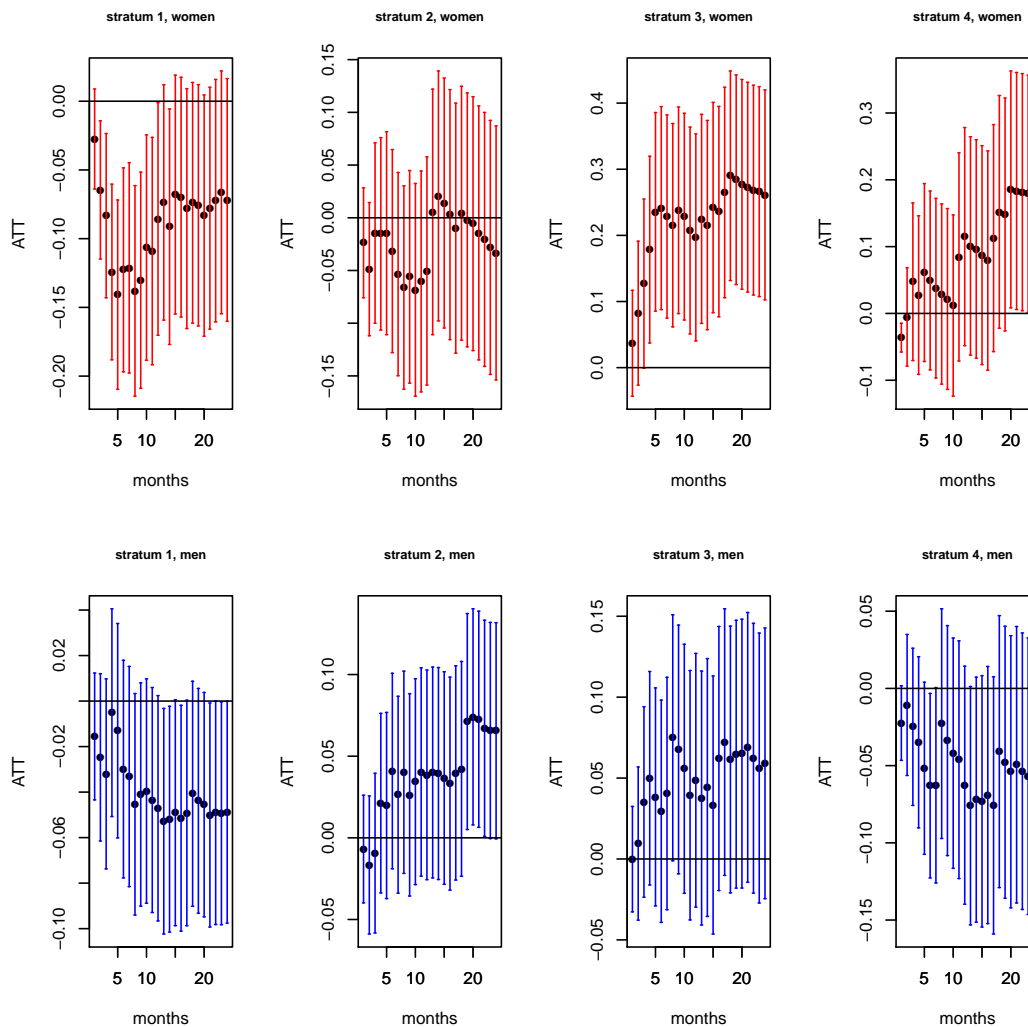


FIGURE A.12: The plots show the monthly updated ATT on the probability of exit into *employment with top-up benefits* (“*supplementary*” ( $S$ )) and its 90% confidence interval of *direct sanctions for unemployed* ( $UE$ ) welfare recipients *over 25 years* ( $o25$ ) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

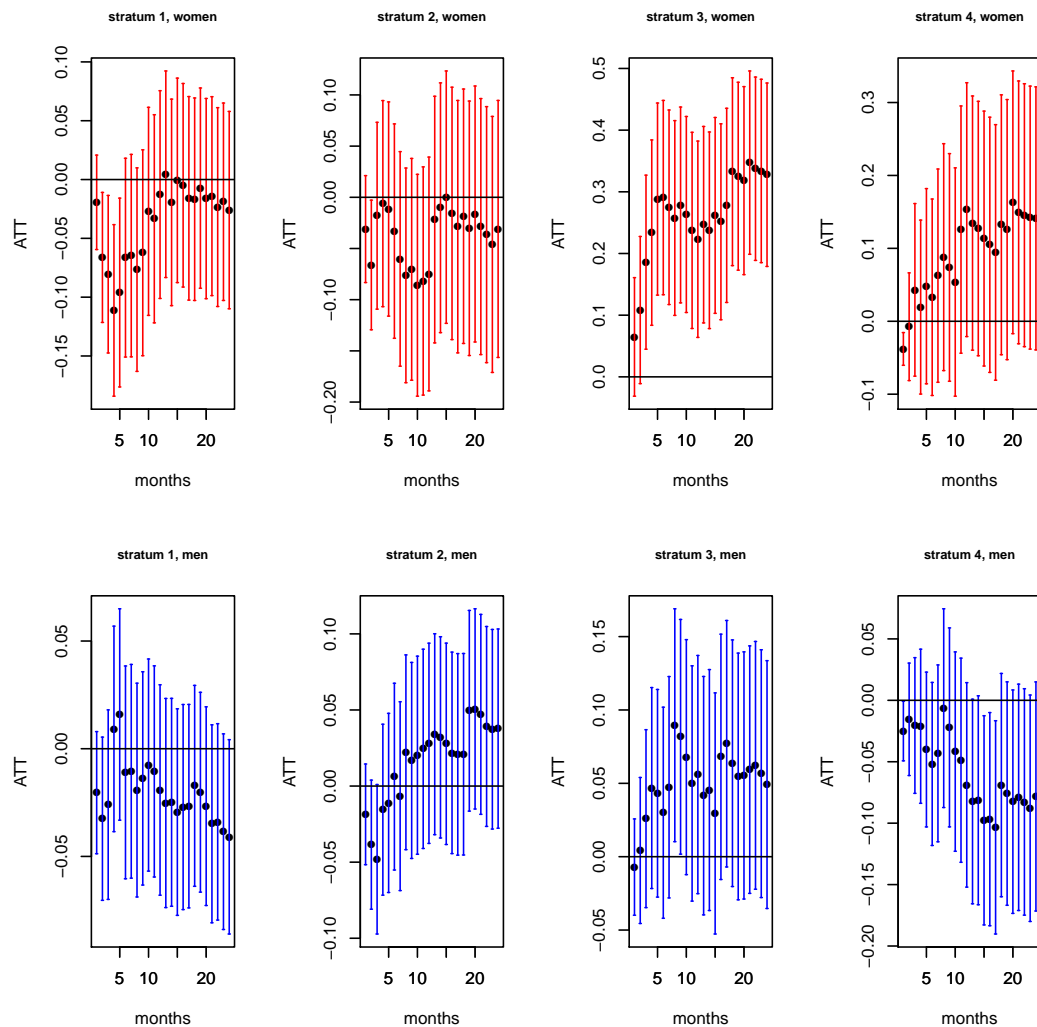


FIGURE A.13: The plots show the monthly updated ATT on the probability of exit into *employment* (“*job in general*” ( $G$ )) and its 90% confidence interval of *direct* sanctions for *unemployed* ( $UE$ ) welfare recipients over 25 years ( $o_{25}$ ) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

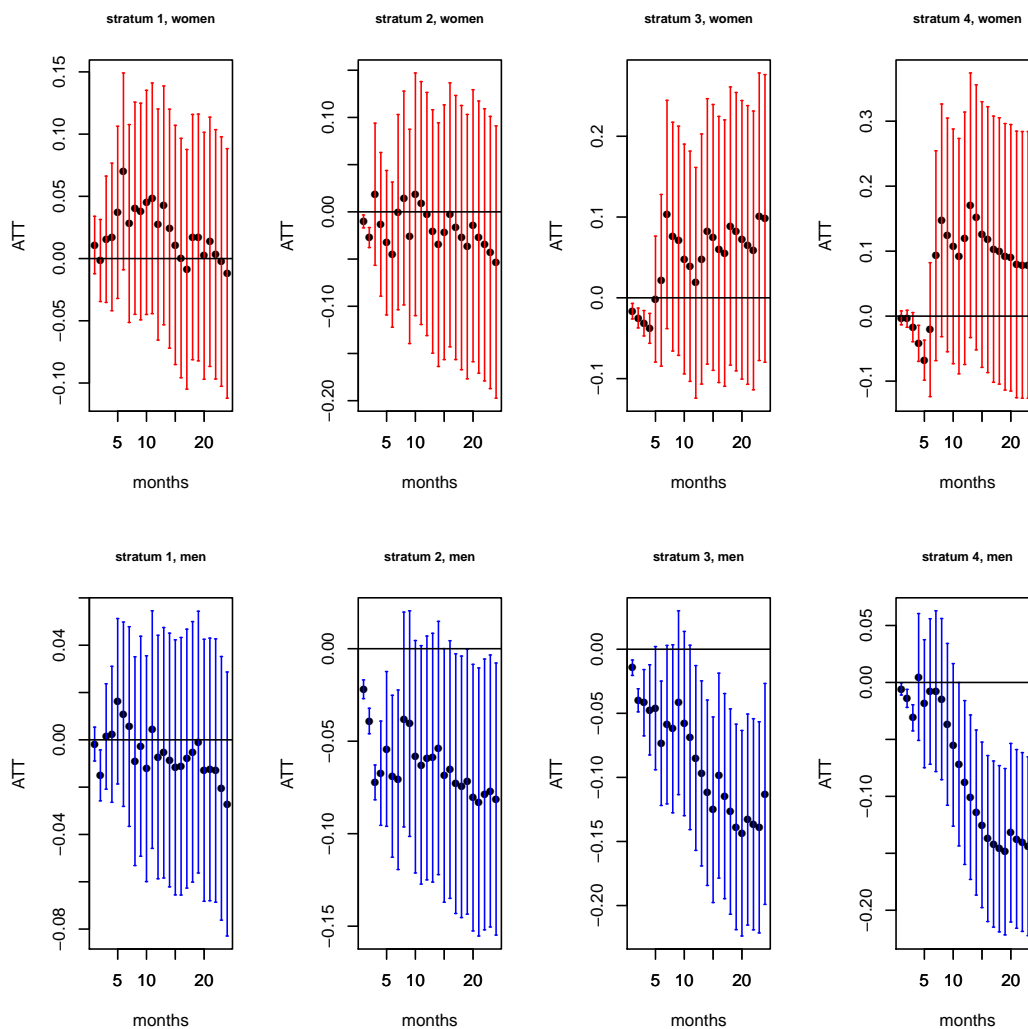


FIGURE A.14: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*” (O)) and its 90% confidence interval of *direct sanctions for unemployed (UE) welfare recipients over 25 years* (o25) in *Western Germany (WG)* of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

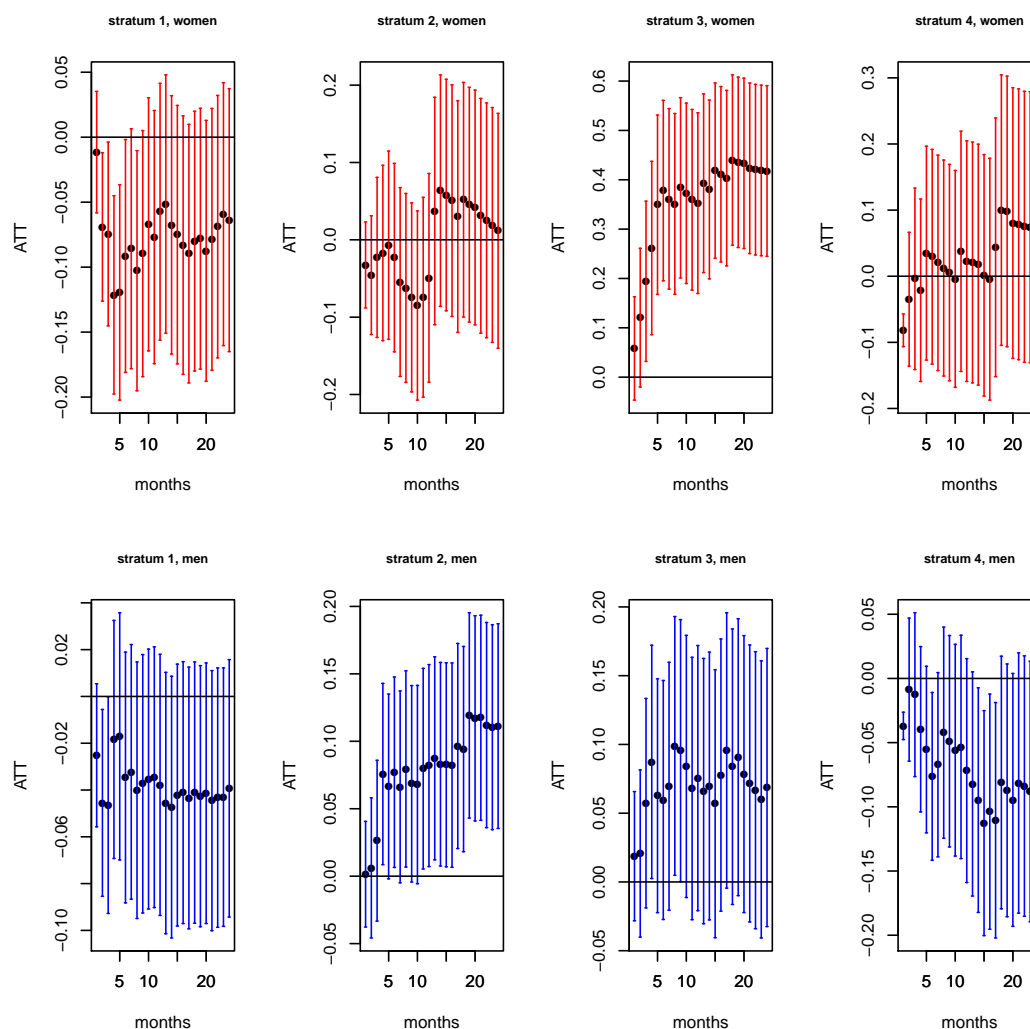


FIGURE A.15: The plots show the monthly updated ATT on the probability of exit into *employment with top-up benefits* (“*supplementary*” ( $S$ )) and its 90% confidence interval of *direct sanctions for unemployed* ( $UE$ ) welfare recipients *over 25 years* ( $o_{25}$ ) in *Western Germany* ( $WG$ ) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

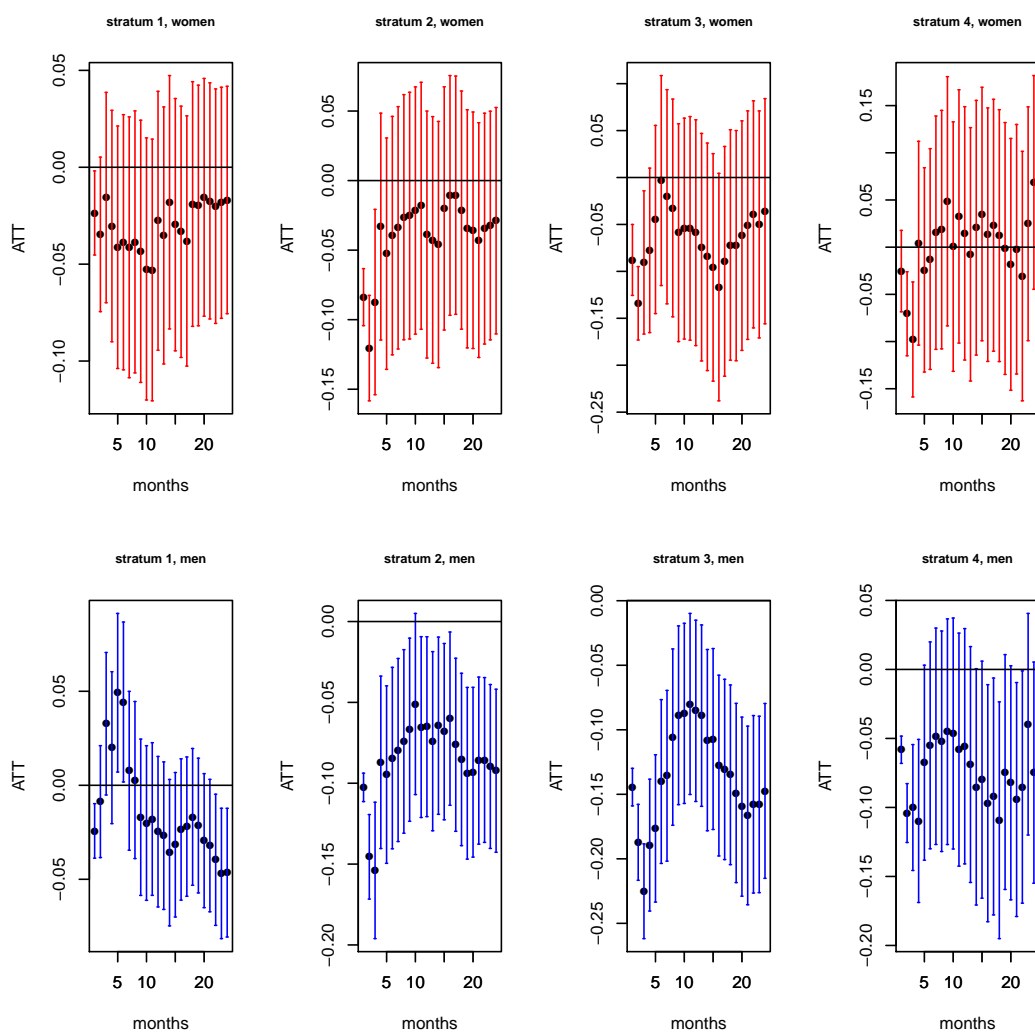


FIGURE A.16: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for *unemployed (UE)* welfare recipients of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.



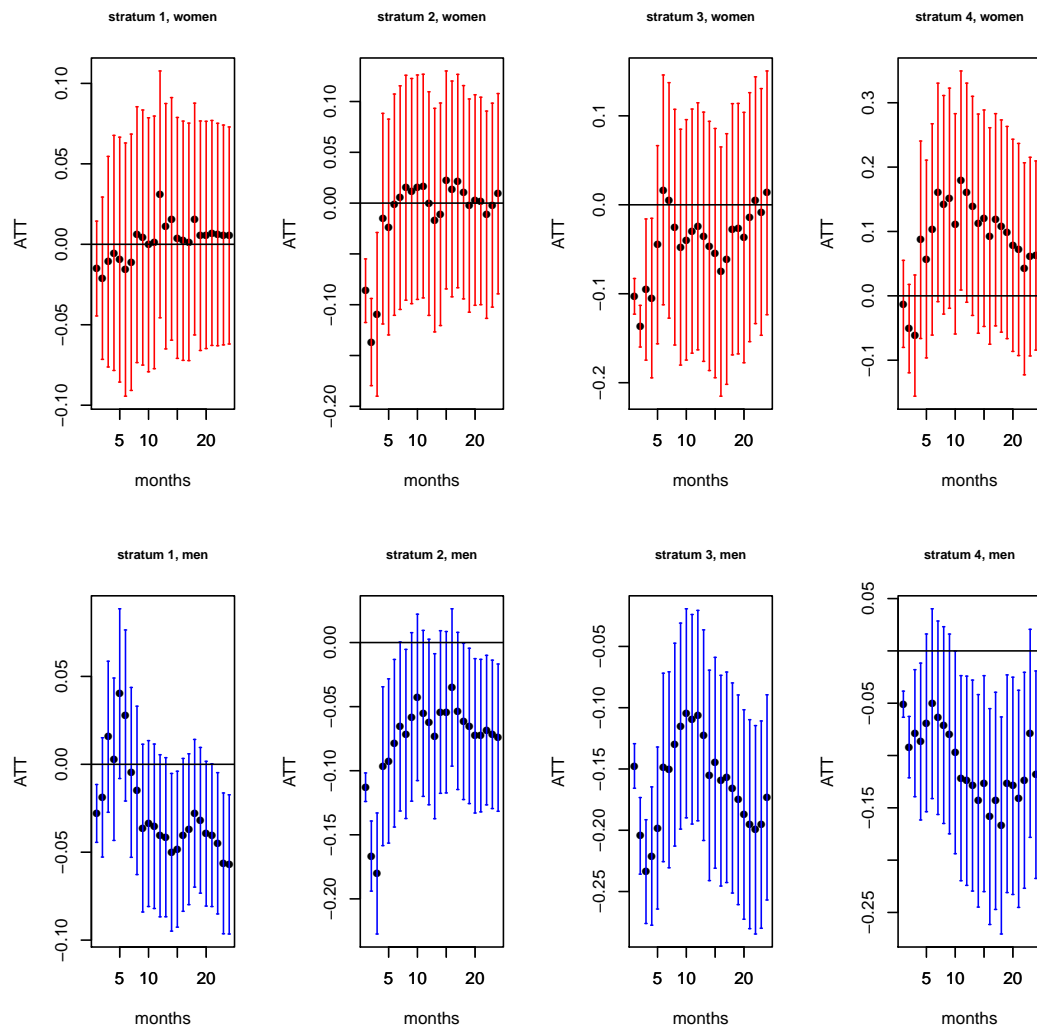


FIGURE A.17: The plots show the monthly updated ATT on the probability of *exiting welfare (ExWel)* and its 90% confidence interval of *direct sanctions* for *unemployed (UE)* welfare recipients in *Western Germany (WG)* of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

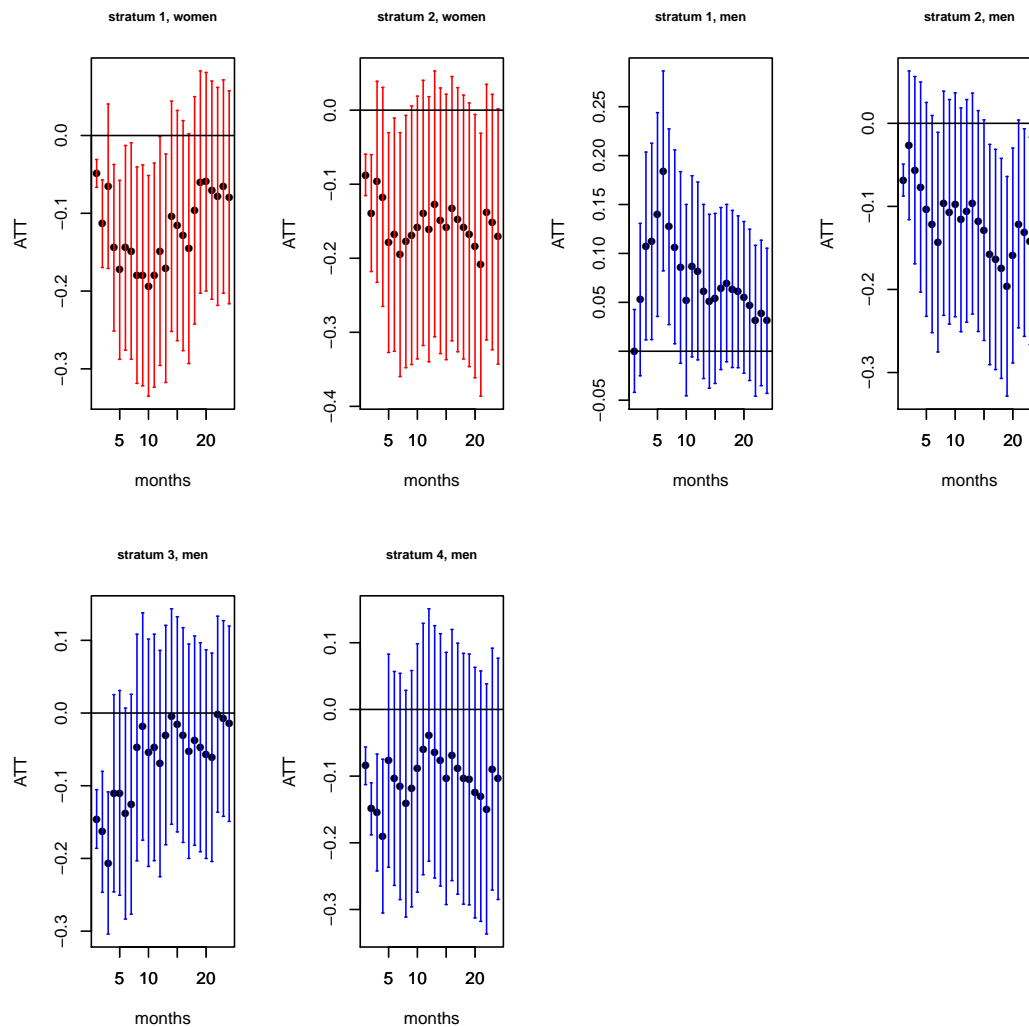


FIGURE A.18: The plots show the monthly updated ATT on the probability of *exiting welfare* (*ExWel*) and its 90% confidence interval of *direct* sanctions for *unemployed* (*UE*) welfare recipients in *Eastern Germany* (*EG*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

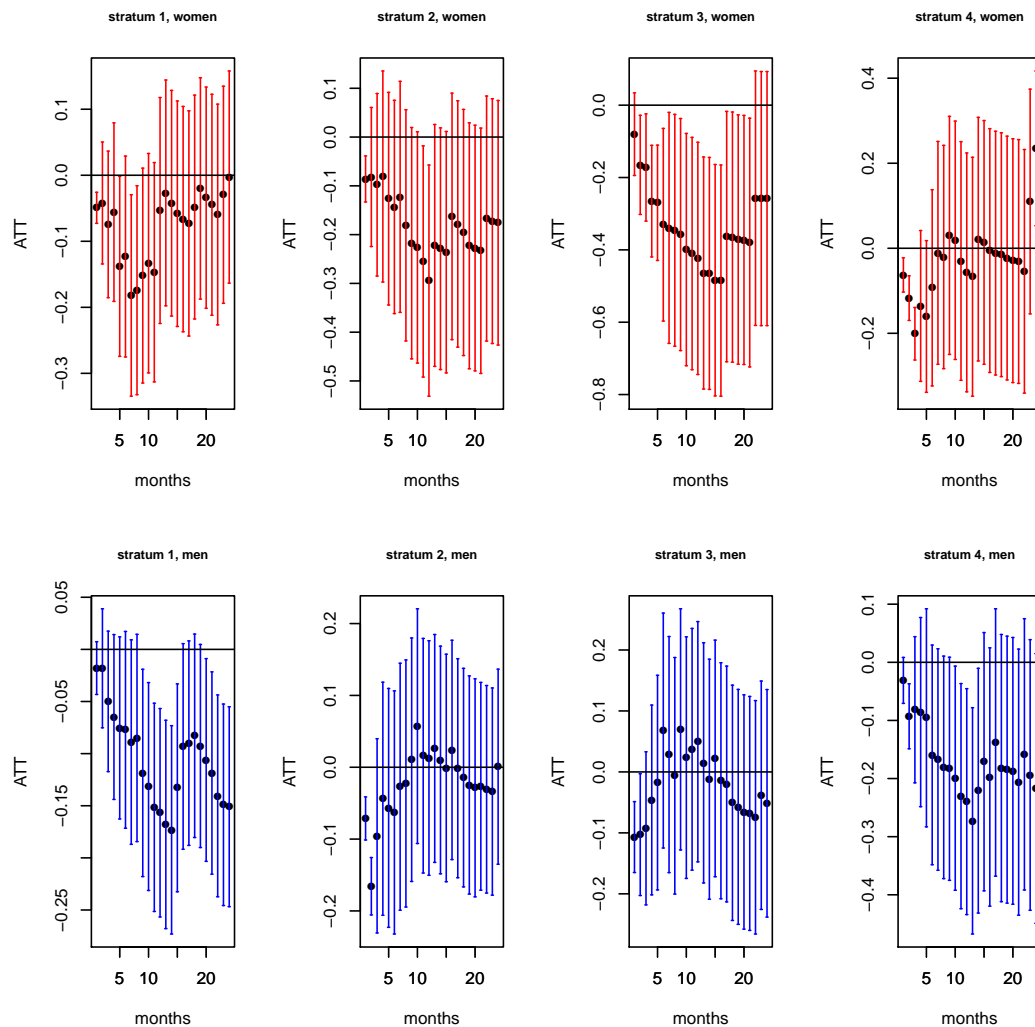


FIGURE A.19: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for *unemployed* (*UE*) welfare recipients with *low-level labor market access* (*LMA*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

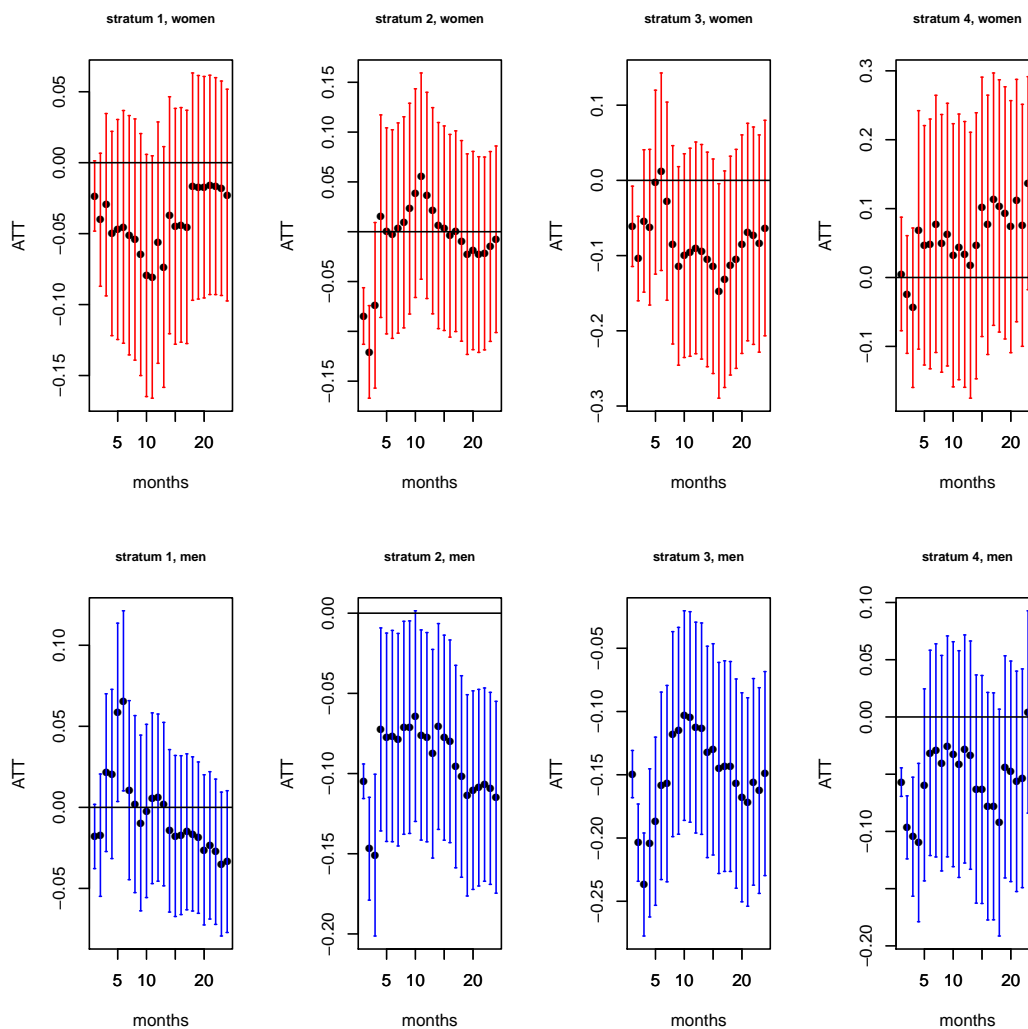


FIGURE A.20: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for unemployed (*UE*) welfare recipients with *medium-level labor market access* (*LMA*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

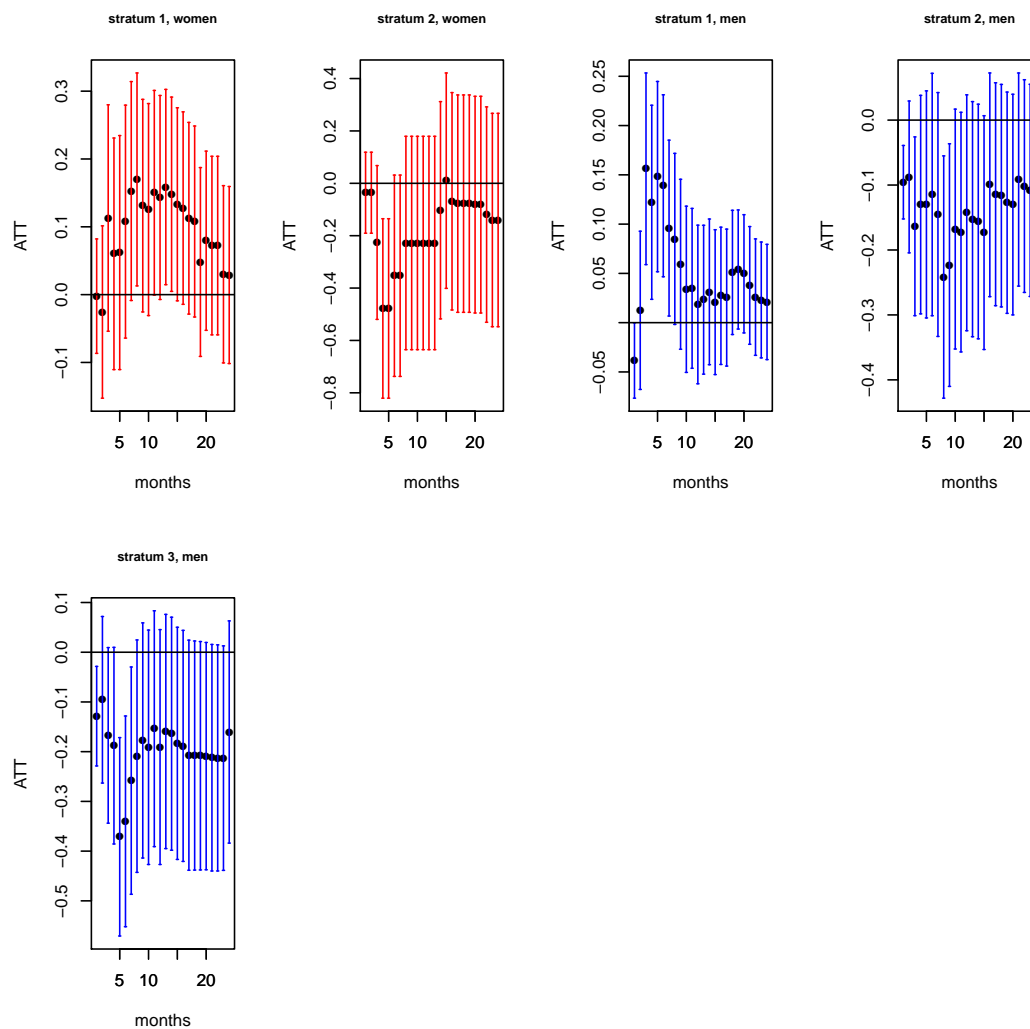


FIGURE A.21: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for *unemployed* (*UE*) welfare recipients with *high-level labor market access* (*LMA*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

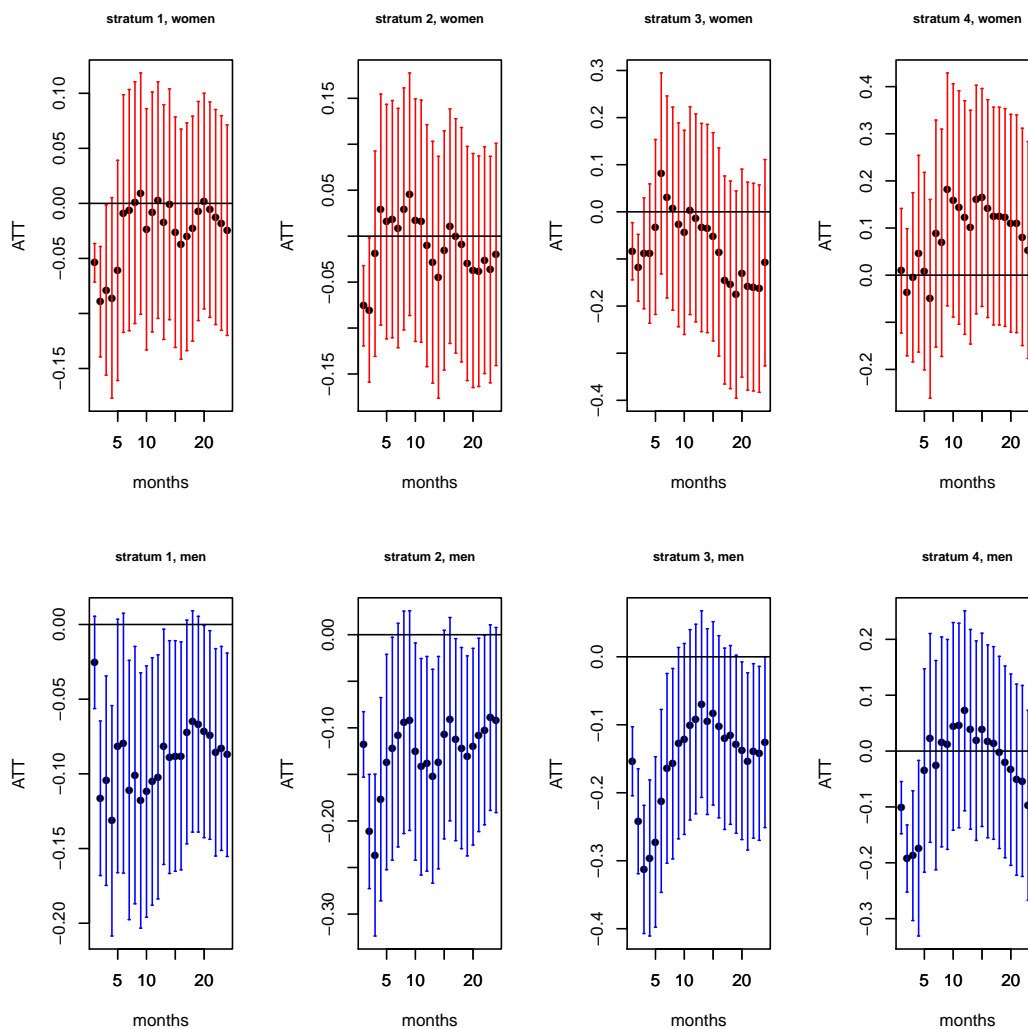


FIGURE A.22: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for *unemployed* (*UE*) welfare recipients *under 25 years* (*u25*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

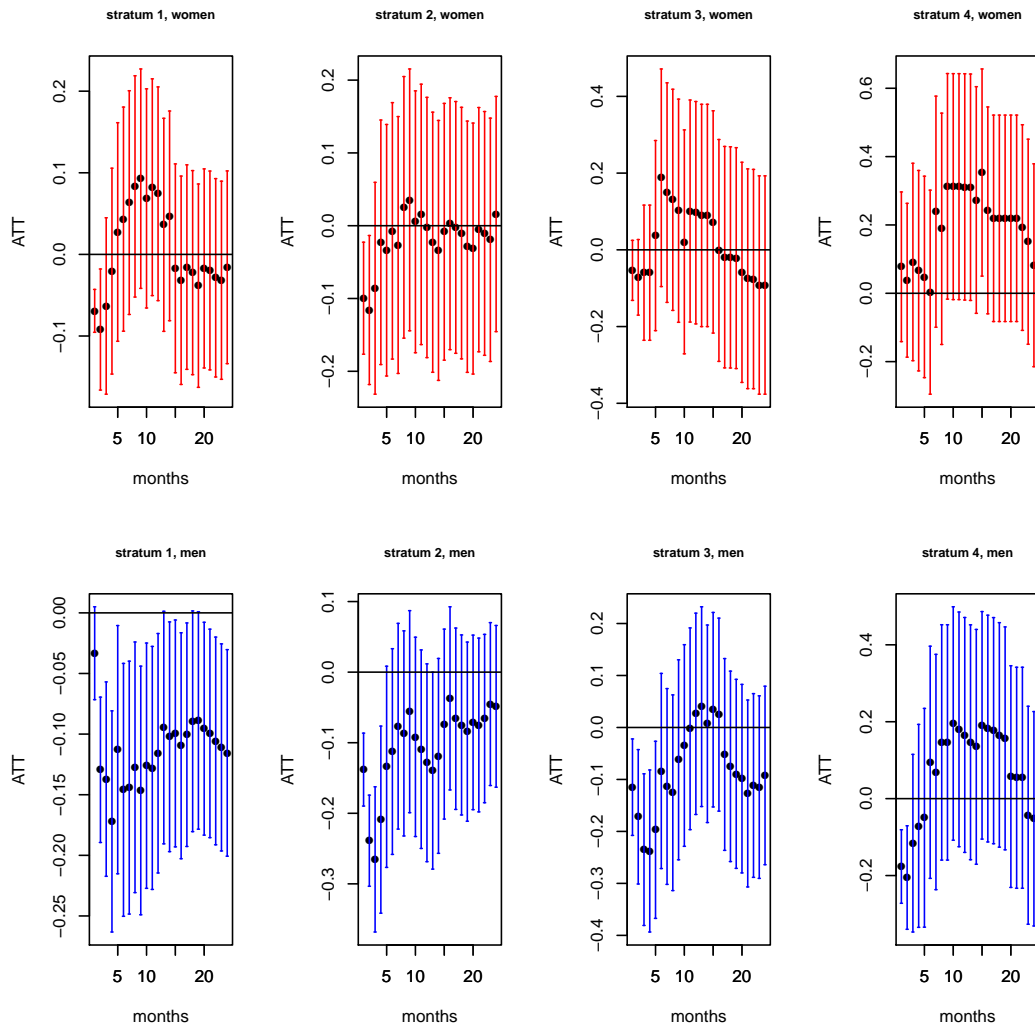


FIGURE A.23: The plots show the monthly updated ATT on the probability of *exiting welfare (ExWel)* and its 90% confidence interval of *direct sanctions* for *unemployed (UE)* welfare recipients *under 25 years (u25)* in *Western Germany (WG)* of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

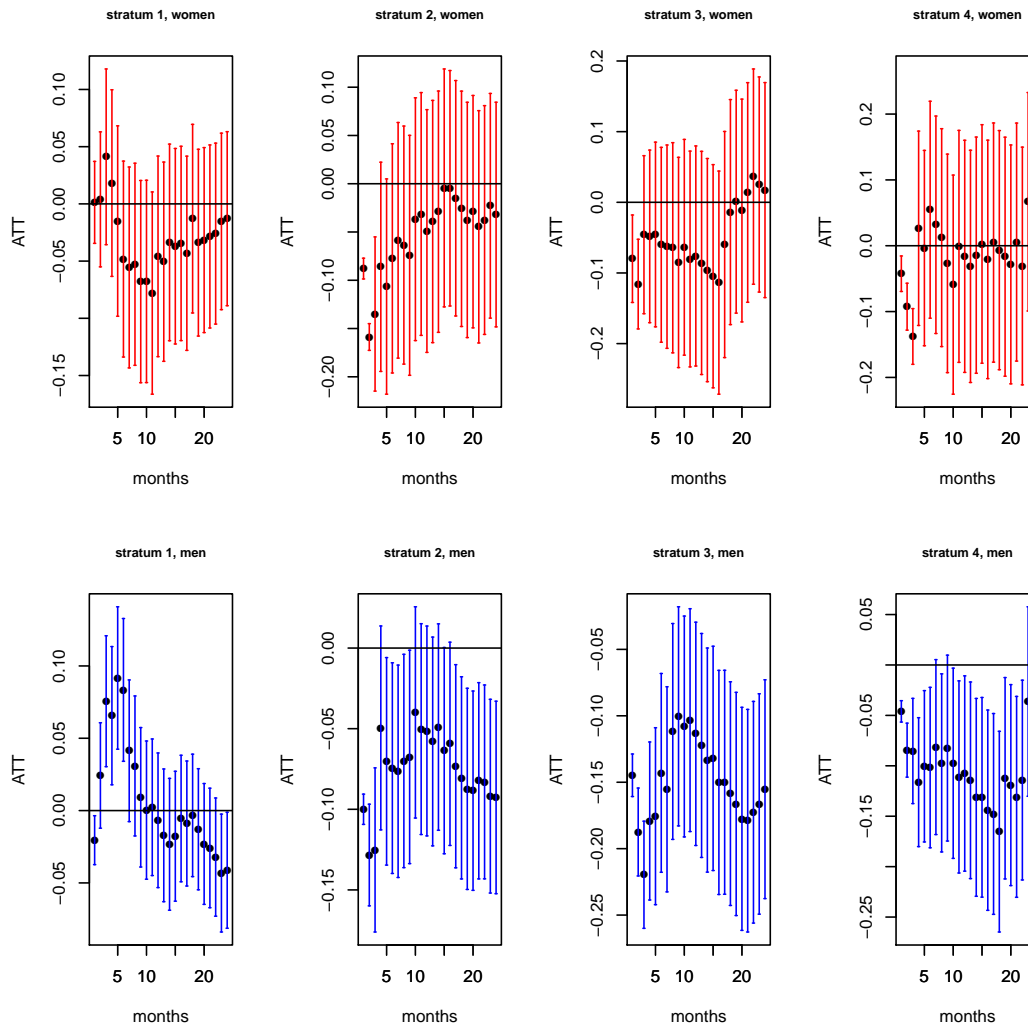


FIGURE A.24: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for *unemployed (UE)* welfare recipients over 25 years (o25) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.



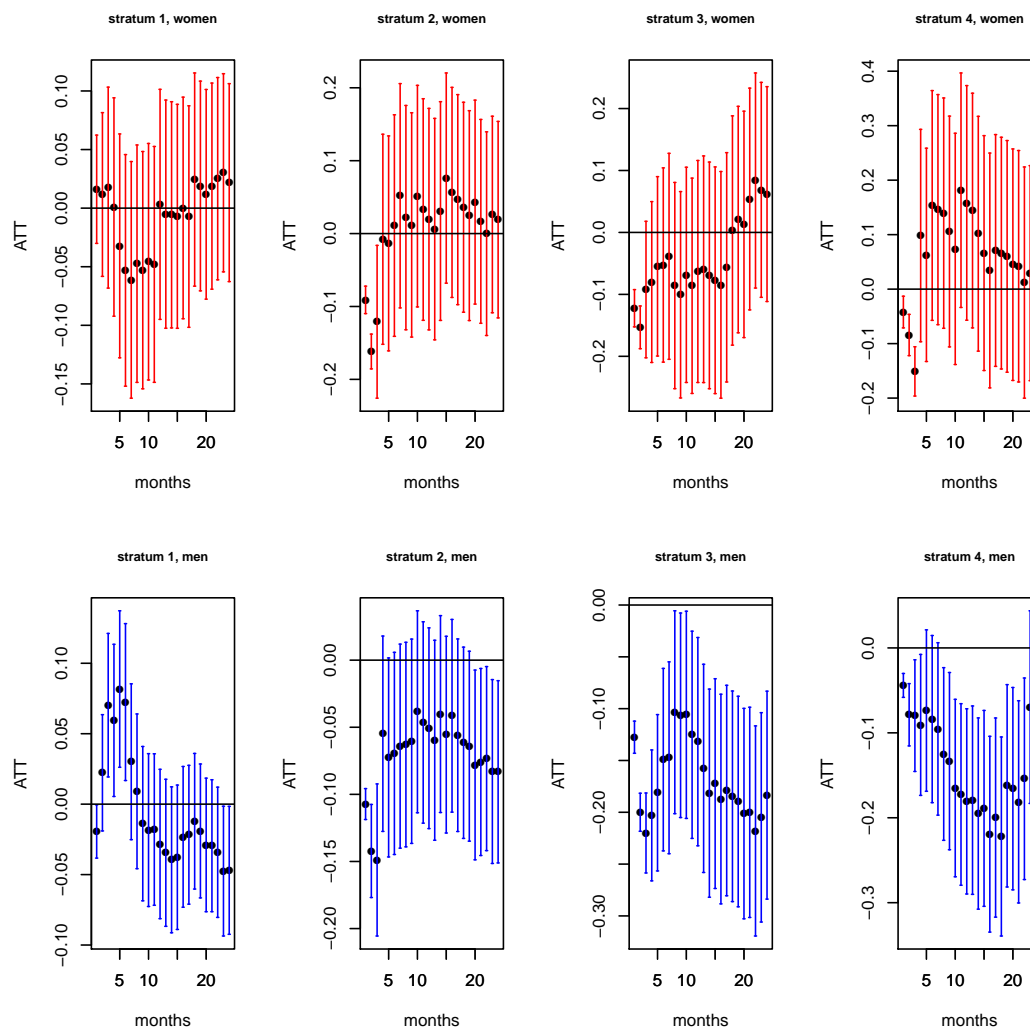


FIGURE A.25: The plots show the monthly updated ATT on the probability of *exiting welfare (ExWel)* and its 90% confidence interval of *direct sanctions* for *unemployed (UE)* welfare recipients over 25 years (o25) in *Western Germany (WG)* of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

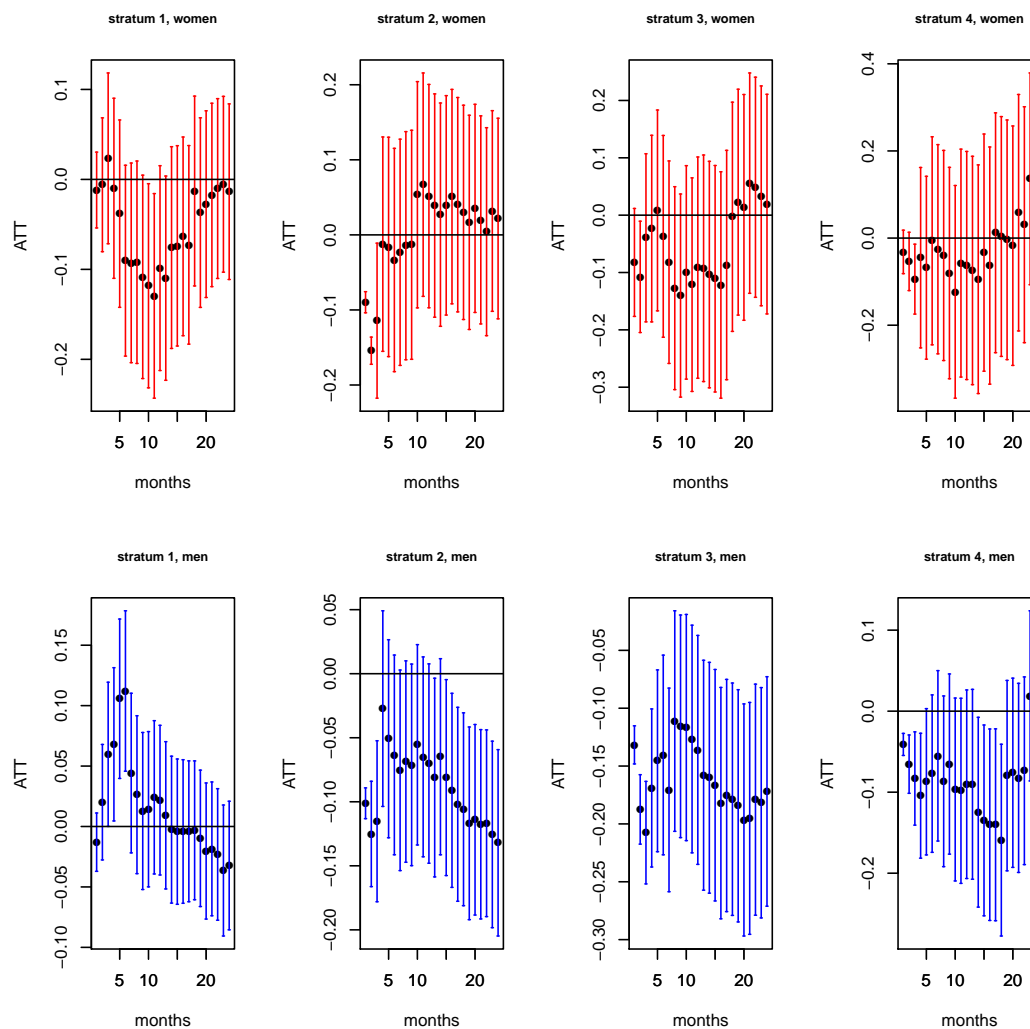


FIGURE A.26: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for *unemployed (UE)* welfare recipients over 25 years with *medium-level labor market access (LMA)* of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

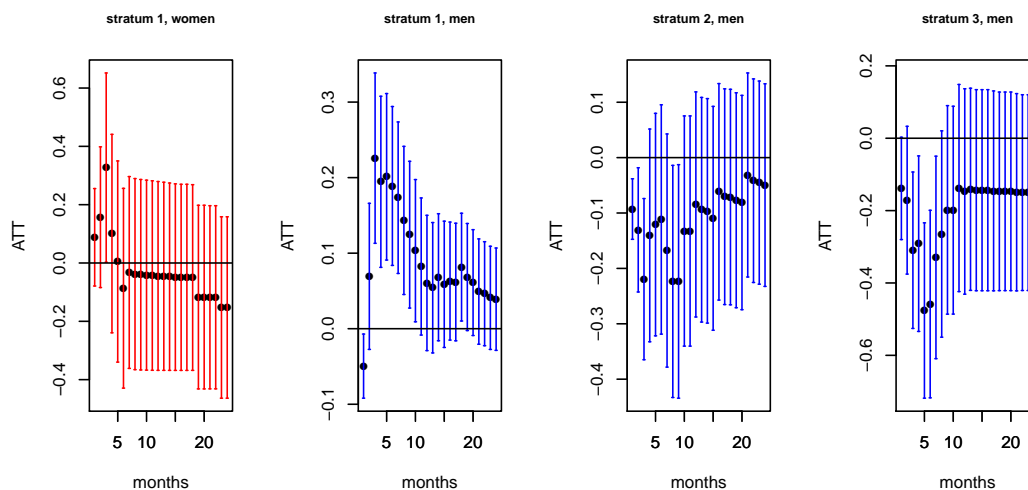


FIGURE A.27: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for *unemployed* (*UE*) welfare recipients over 25 years (*o25*) with *high-level labor market access* (*LMA*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

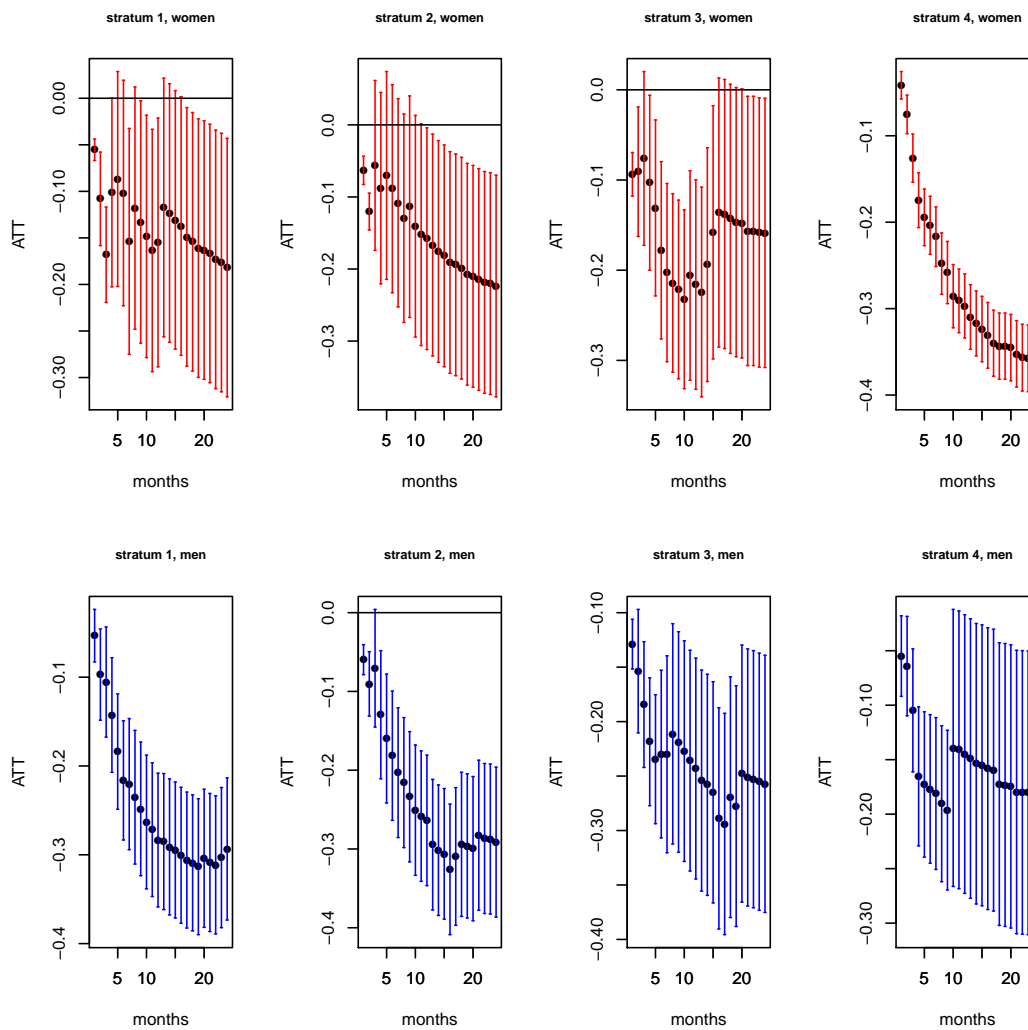


FIGURE A.28: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*” (*O*)) and its 90% confidence interval of *direct sanctions for employed* (*Emp*) welfare recipients of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

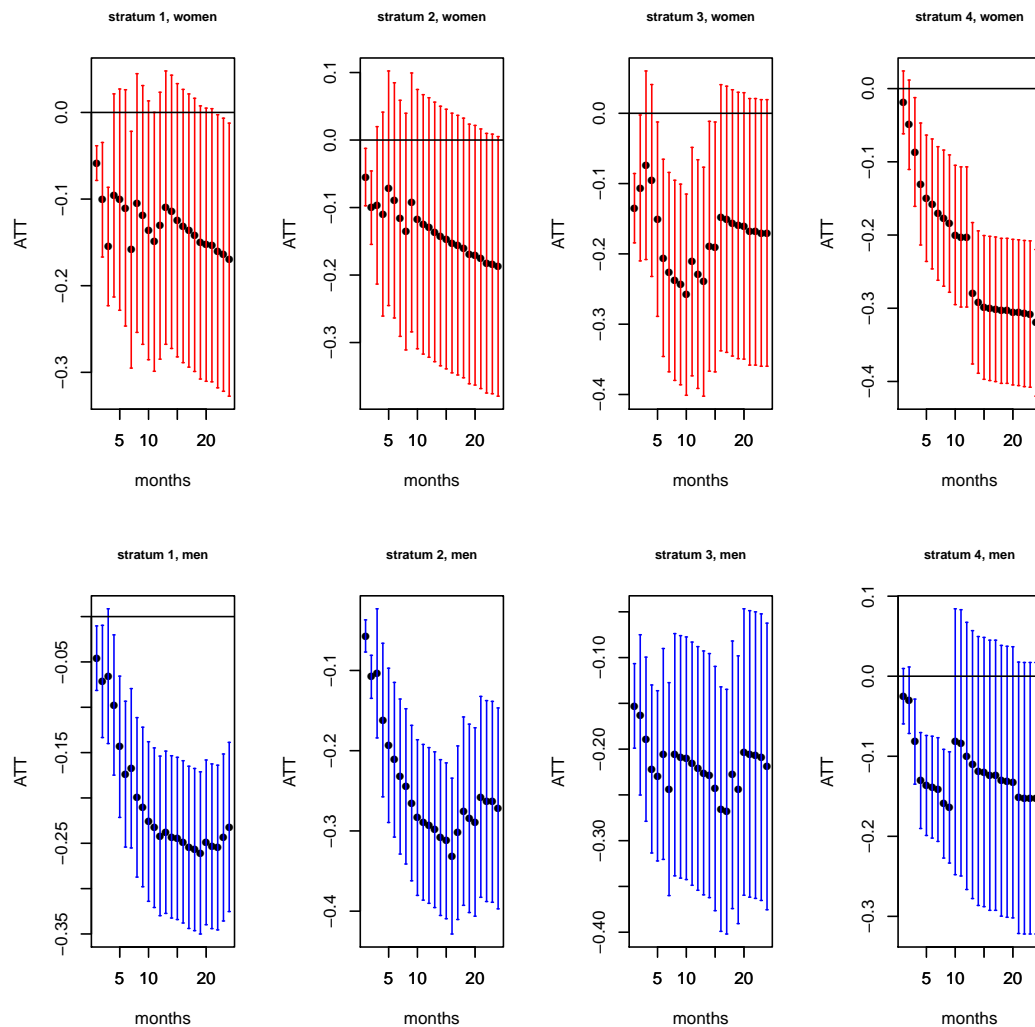


FIGURE A.29: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*” (O)) and its 90% confidence interval of *direct sanctions* for *employed* (*Emp*) welfare recipients in *Western Germany* (WG) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

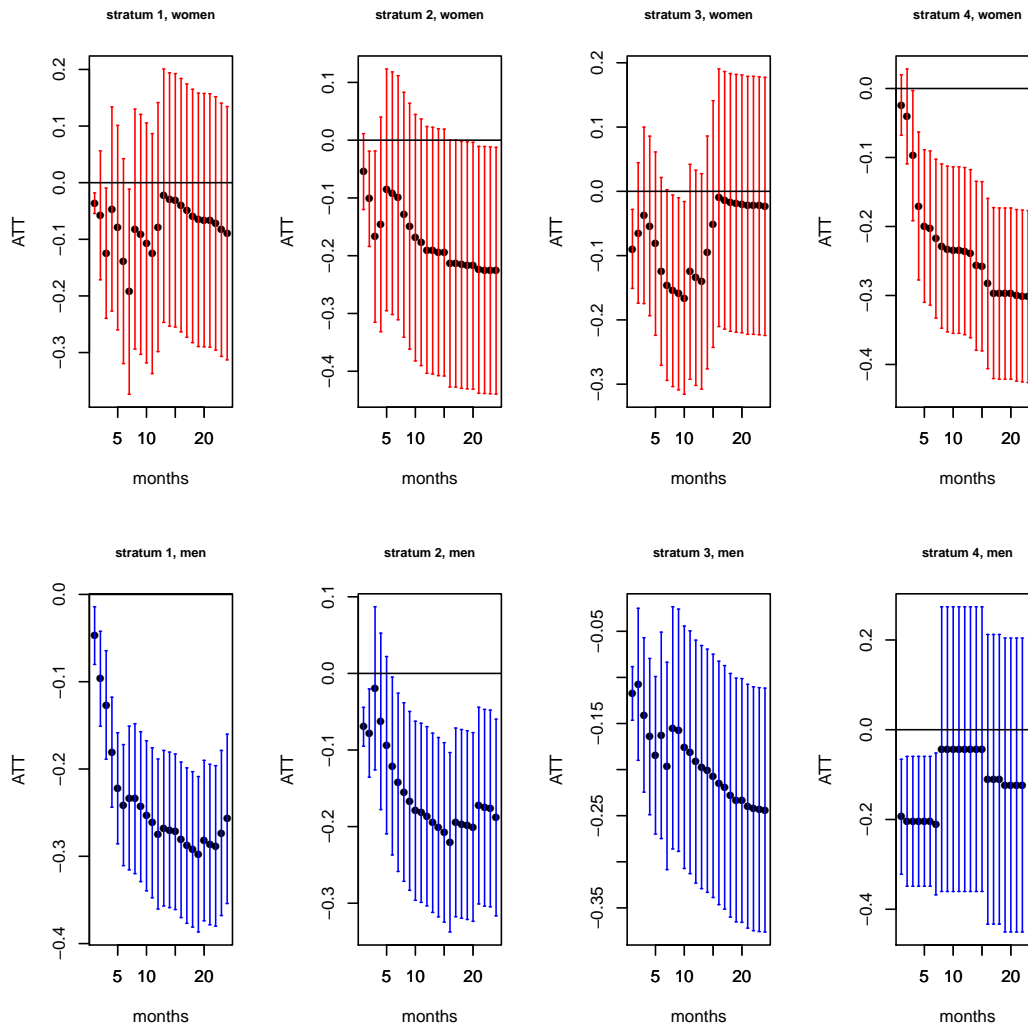


FIGURE A.30: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*” (O)) and its 90% confidence interval of *direct sanctions* for *employed* (*Emp*) welfare recipients with *medium-level labor market access* (LMA) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

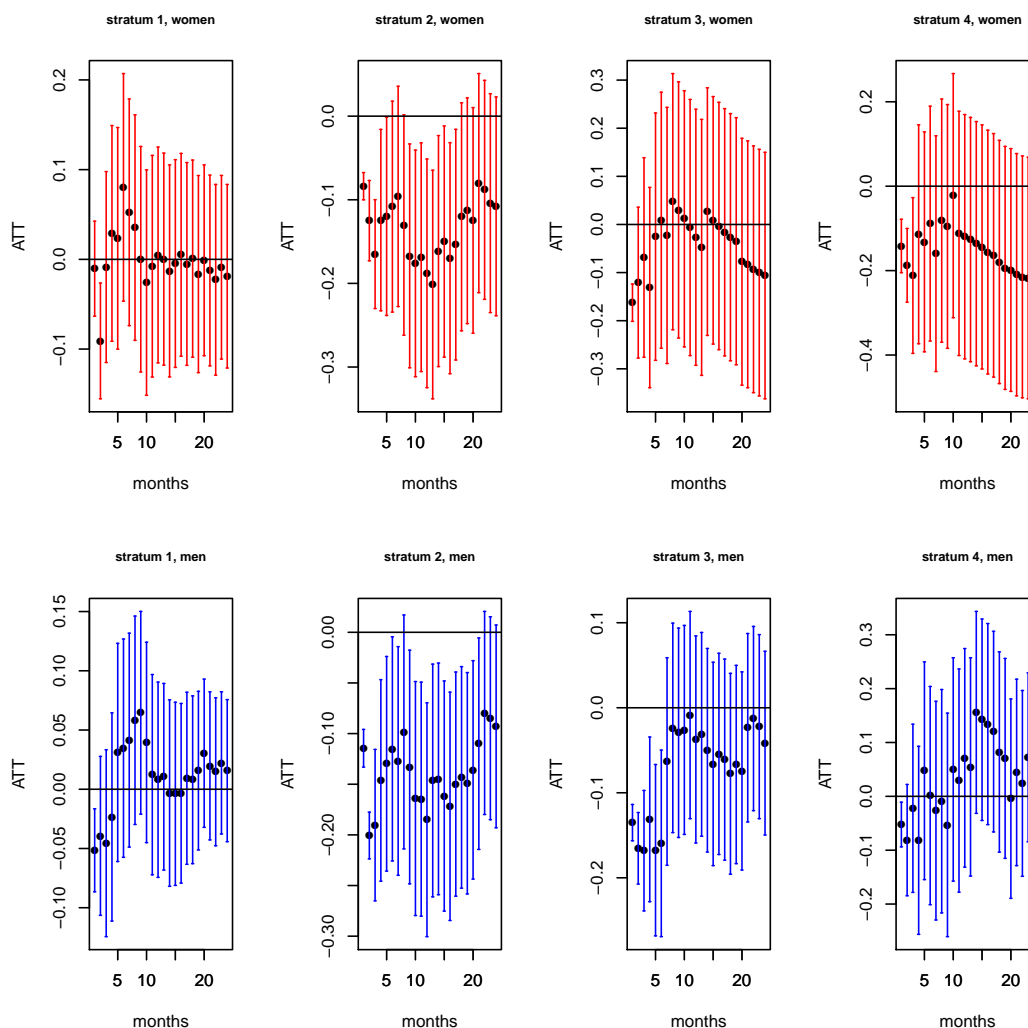


FIGURE A.31: The plots show the monthly updated ATT on the probability of *exiting welfare (ExWel)* and its 90% confidence interval of *direct sanctions* for *employed (Emp)* welfare recipients of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

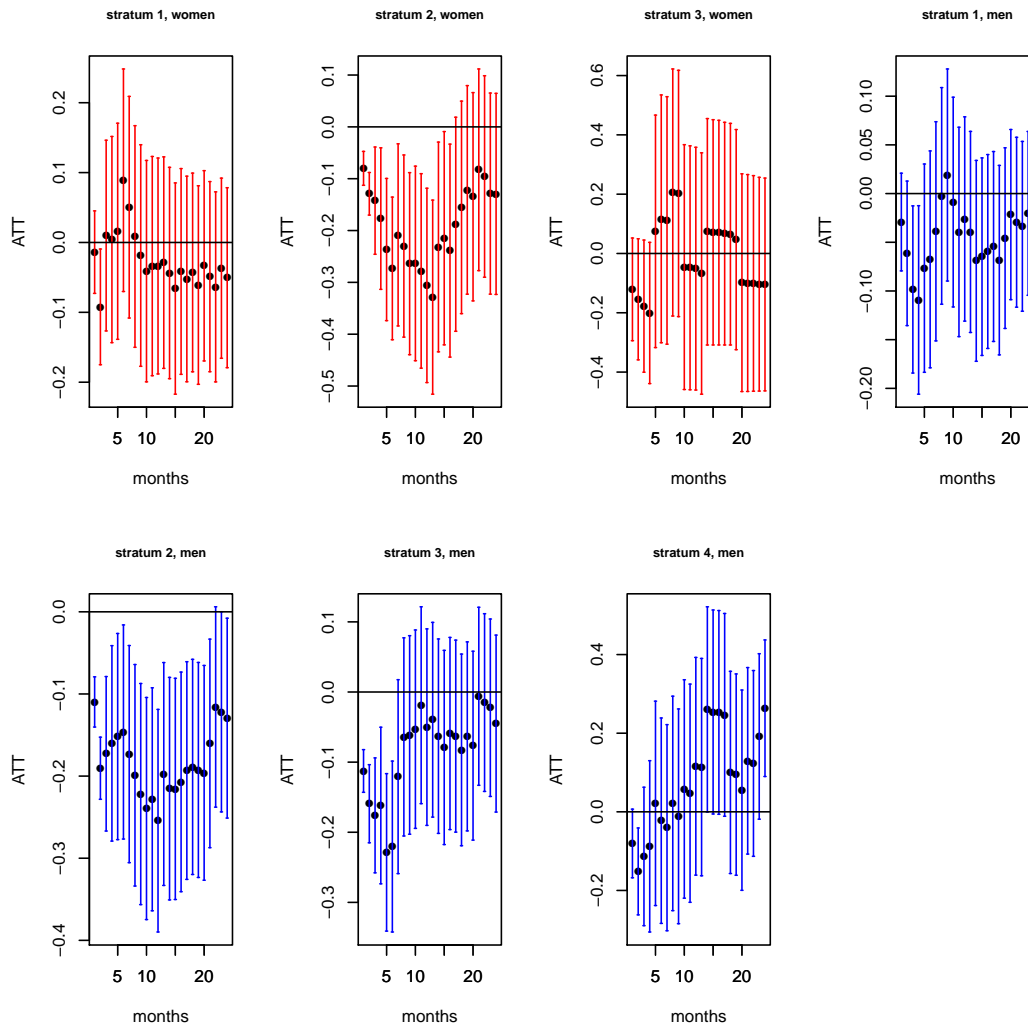


FIGURE A.32: The plots show the monthly updated ATT on the probability of exiting welfare (*ExWel*) and its 90% confidence interval of *direct* sanctions for *employed* (*Emp*) welfare recipients with *medium-level* labor market access (*LMA*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.



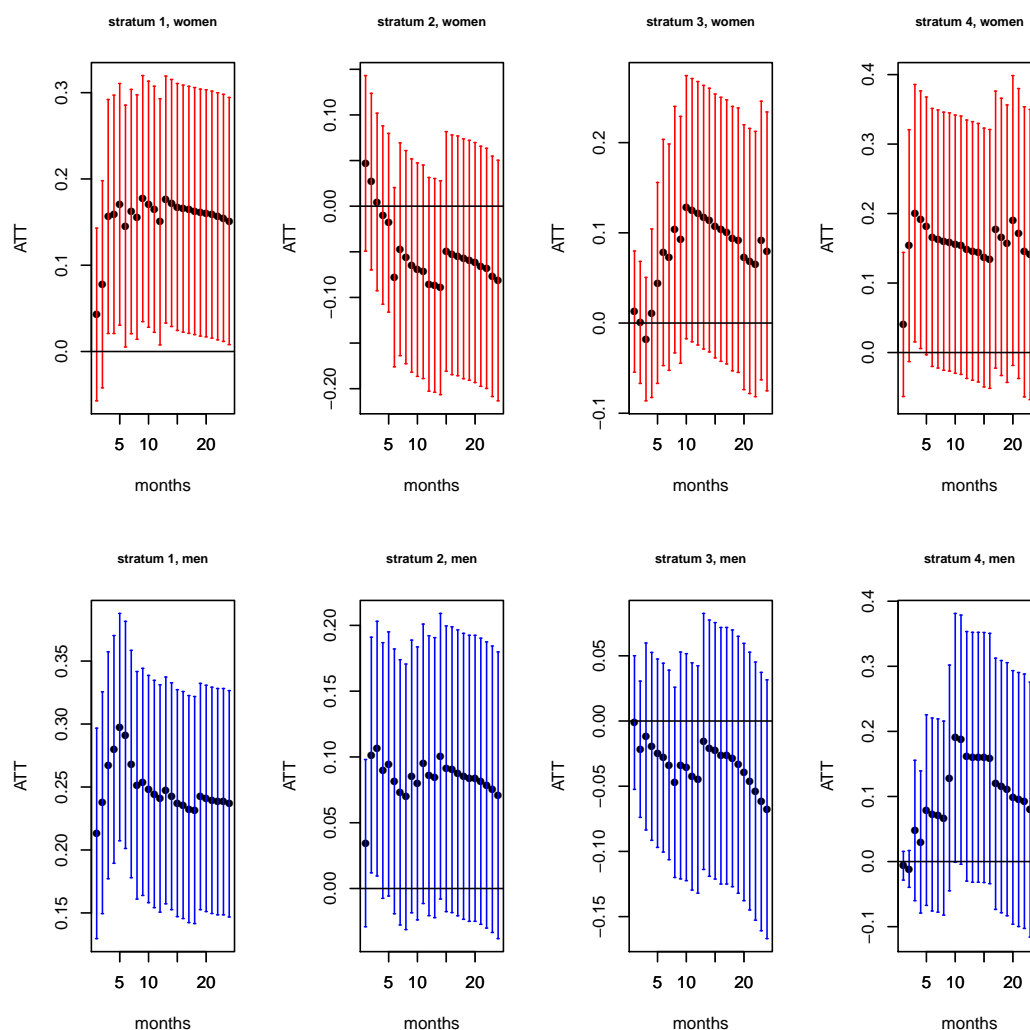


FIGURE A.33: The plots show the monthly updated ATT on the probability of exiting employment (*ExJob*) for mere welfare receipt and its 90% confidence interval of direct sanctions for employed (*Emp*) welfare recipients of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

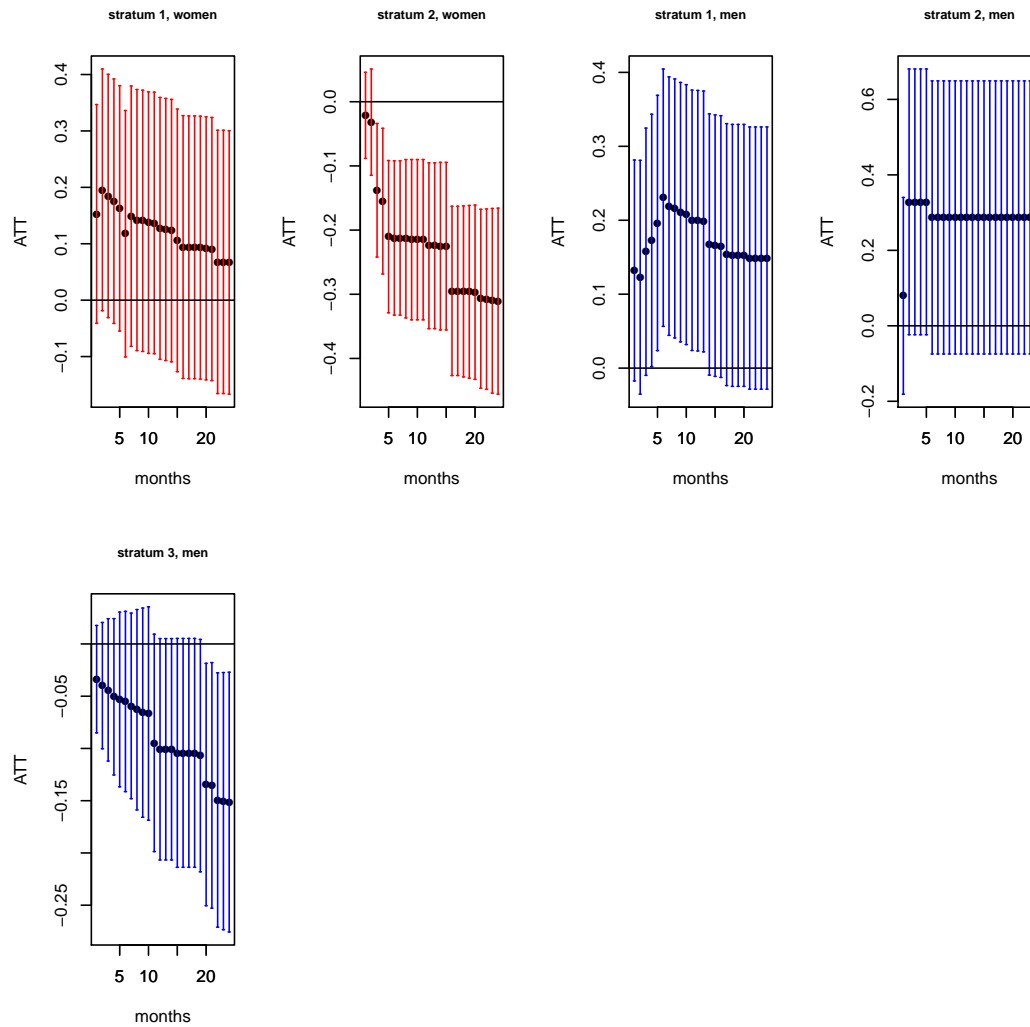


FIGURE A.34: The plots show the monthly updated ATT on the probability of exiting employment (*ExJob*) for mere welfare receipt and its 90% confidence interval of direct sanctions for employed (*Emp*) welfare recipients with high-level labor market access (LMA) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

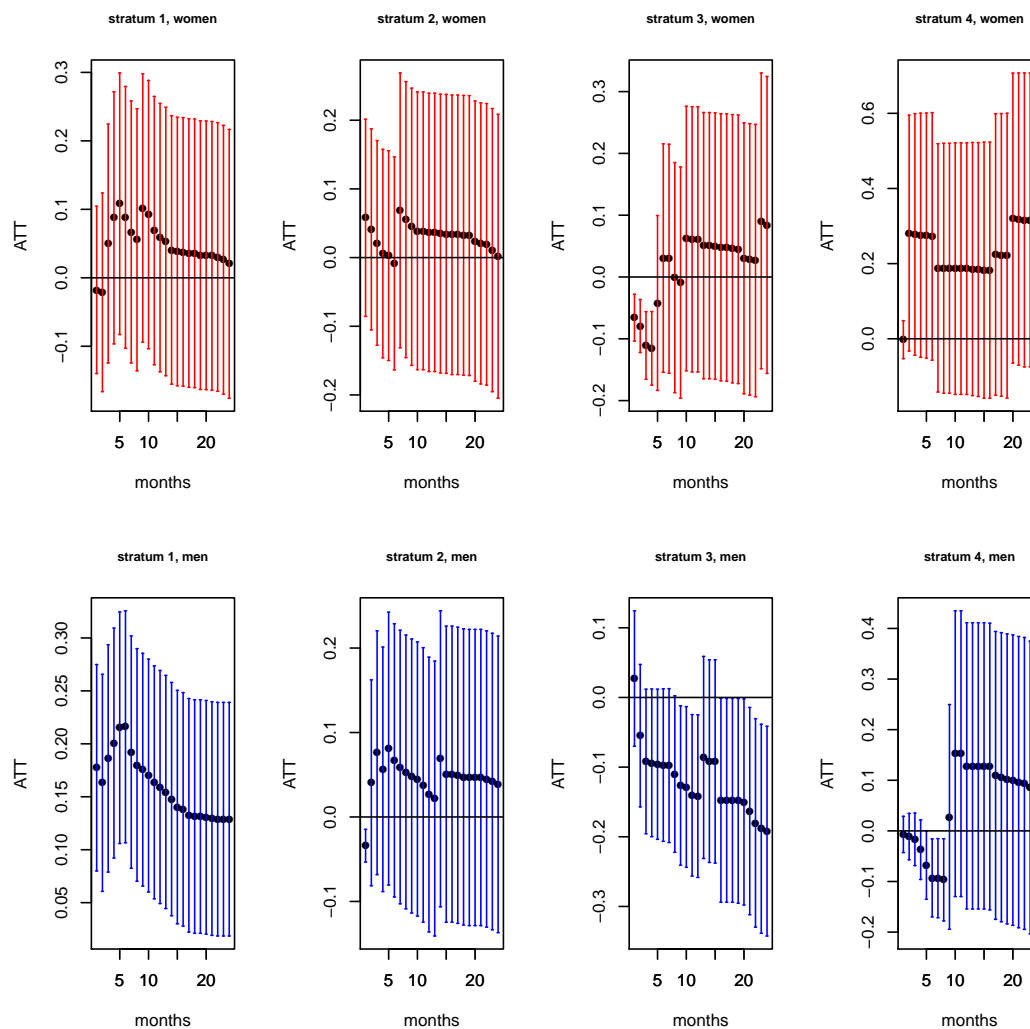


FIGURE A.35: The plots show the monthly updated ATT on the probability of exiting employment (*ExJob*) for mere welfare receipt and its 90% confidence interval of direct sanctions for employed (*Emp*) welfare recipients over 25 years (o25) in Western Germany (WG) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (quarterly) stratum  $S_i$  of welfare duration (with  $i=1-4$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-24$  counting the months after the beginning of stratum.

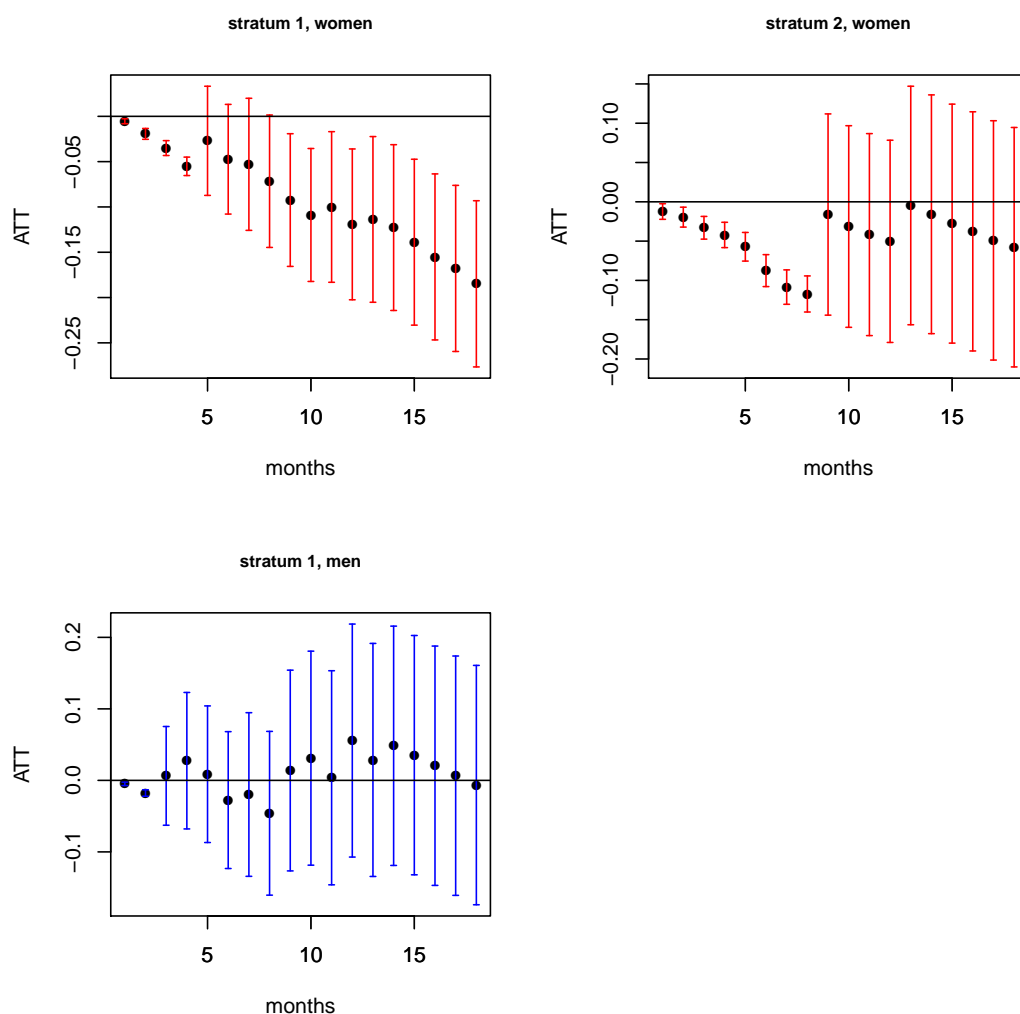


FIGURE A.36: The plots show the monthly updated ATT on the probability of exit into *mere employment* (“*job only*” (*O*)) and its 90% confidence interval of *indirect* (*ind*) sanctions for *unemployed* (*UE*) welfare recipients in *Western Germany* (*WG*) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (half-yearly) stratum  $S_i$  of welfare duration (with  $i=1-2$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-18$  counting the months after the beginning of stratum.

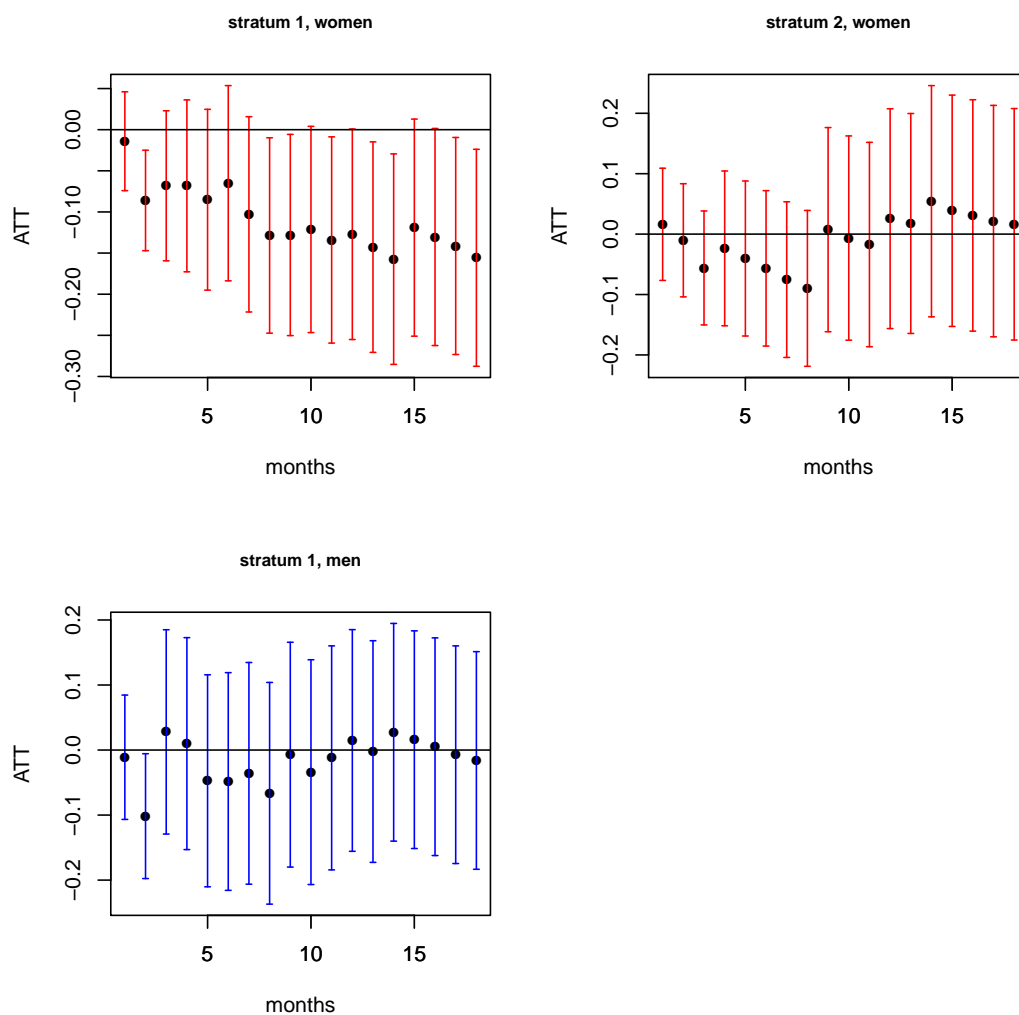


FIGURE A.37: The plots show the monthly updated ATT on the probability of exit into *employment* (“*job in general*” (G)) and its 90% confidence interval of *indirect* (*ind*) sanctions for *unemployed* (UE) welfare recipients in *Western Germany* (WG) of the inflow cohort 2008, separately for women (red) and men (blue). They illustrate the development of the sanction effect for overlapping periods  $P_j$ , each starting with the beginning of the (half-yearly) stratum  $S_i$  of welfare duration (with  $i=1-2$ ) and finishing with the end of ongoing final months  $m_j$ , with  $j=1-18$  counting the months after the beginning of stratum.



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