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Economic Effects of Social Protection

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Preface

When I started writing this dissertation almost four years ago, I did not expect that the time would go so fast. Have I been in a kind of trance imposed by a high degree of concentration, the regularity of the days and the structured approach of science? The time might have gone, which also becomes clear when I notice a changed hairline in the mirror, but my mind is peaceful and I am proud of the result.

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1 Introduction

In developed economies, between 20% and 30% of GDP is spent on pensions, health care and benefits targeted at the elderly, unemployed, disabled, sick, families and the poor. In order to spend these amounts well-targeted and cost efficient, we need to know the economic effects of social protection. On the one hand, we have to find out whether the intended goals of social protection are achieved, such as the extent to which social insurance protects individuals against income shocks. On the other hand, social protection may also come with adverse behavioral effects, such as reduced job search effort caused by welfare benefits.

This thesis contributes to the literature by both studying some of the intended effects and some of the potential adverse effects. We start this introduction with a brief overview of the aims of social insurance and social protection more broadly, after which we describe some of the adverse behavioral effects that social protection may have. We next relate these positive and negative effects of social protection to the outcome variables that are studied in this PhD thesis.

Social insurance aims at protecting individuals against negative income shocks caused by unemployment, disability, sickness and old age (Barr 2012). In principle, this is welfare enhancing because individuals are risk averse and there is uncertainty about individuals' future income. Risk aversion implies that an increase in income risk lowers expected utility, because of diminishing marginal utility of consumption.[1] Social insurance helps to smooth income over the life cycle and over 'good' and 'bad' states and thereby reduces risks and increases lifetime utility. This is especially

[1]Diminishing marginal utility of consumption means that the utility that individuals derive from products and services decreases when they consume more and more of it.

true for liquidity constrained individuals, who have less possibilities to smooth consumption (Chetty 2008), and for individuals with time-inconsistent present-biased preferences, who save too little (Thaler and Shefrin 1981). Furthermore, social insurance may increase welfare because people are loss averse (e.g. Kahneman et al. (1991)) and social insurance reduces income losses.[2]

When taking a broader perspective, social protection also aims at reducing poverty and inequality and in some cases at increasing employment (Barr 2012). Reducing poverty and inequality may increase welfare and productivity among poor people (Baldacci et al. 2008; Cingano 2014; OECD 2014). Moreover, reducing inequality is expected to decrease rent-seeking behavior (Stiglitz 2012).[3] Further, social protection spending increases macroeconomic stability and by reducing inequality it may also increase political and social stability (Rodrik 1999; Kumhof et al. 2015). Finally, social protection may induce positive behavioral effects. Income security may increase risk taking, investments and thereby productivity (Acemoglu and Shimer 2000; Estevez-Abe et al. 2001).

Social protection can also provoke adverse behavioral effects. Social protection systems shift part of the costs associated with certain behavior (e.g. risk taking) to others. This generates a discrepancy between the individual costs and benefits and the social costs and benefits. Individuals tend to choose too much leisure (or consumption) from a social perspective when part of the costs of leisure (or consumption) are borne by others (Chetty 2008; Chetty and Finkelstein 2013). This inefficient behavior is called moral hazard. Examples of moral hazard caused by social protection are: decreases in job search efforts because of unemployment benefits (e.g. Krueger and Meyer (2002)) and opting out of employment because of early retirement benefits (e.g. Staubli and Zweimüller (2013)). Another distortionary effect arises from taxes that are needed to finance the welfare

[2]Loss aversion refers to people's tendency to prefer avoiding losses to acquiring equivalent gains.

[3]Rent-seeking refers to resources spent (by the rich) on increasing one's share of existing wealth without creating new wealth. Rent-seeking results in reduced economic efficiency through poor allocation of resources, reduced actual wealth-creation, lost government revenue and increased income inequality (Stiglitz 2012).

state. These taxes can reduce labour supply and private investments, which might lead to lower output.

In this thesis, we study both the achievement of some of the intended goals of social protection as well as some potential negative behavioral effects provoked by social protection.[4] We consider the following variables for studying the effectiveness of social protection: the number of NEETs (not in employment, education or training), social assistance coverage, different sources of compensation for wage losses caused by unemployment shocks (including within-household insurance), and poverty and inequality at an aggregated level. For studying potential adverse effects induced by social protection, we consider the effects on employment outcomes and on economic growth. In addition, we study how a constitutional right to social security is related to different kinds of social expenditure.

All together, we consider the welfare state from different perspectives, with a focus on both the redistributing effect of social protection and the employment effects of social protection. In the second and third chapter, we employ quasi-experimental methods using micro panel data from the Netherlands. In the fourth and fifth chapter, we use aggregated OECD panel data to perform international comparative research. The chapters can be read independently and all contain an extensive introduction. This introductory chapter aims to summarize the motivations, research questions, and outcomes of the four chapters.

1.1 Chapter 2: Preventing NEETs

Young individuals not in employment, education or training (NEETs) are a major policy concern, in particular during periods of recession. In line with this, NEETs are a prime concern for the European Commission (Carcillo et al. 2015). This increased policy attention for reducing the number of NEETs is accompanied with a trend towards stricter conditions for re-

[4] The effectiveness and efficiency should of course not be the only criteria by which a social protection system should be judged. Justice and fairness, public support, consistency within the legal framework and the social contract are other considerations that must be discussed and weighted by policy makers, politicians and the electorate more broadly.

ceiving welfare benefits, via e.g. the imposition of job search requirements and/or by making welfare benefits receipt conditional on participation in so-called work-learn programs.

In chapter 2, we study the effects of a mandatory activation program for young individuals on the NEETs rate during a severe economic recession. Specifically, we study the WIJ (*Wet Investeren in Jongeren*, Work Investment Act for Young Individuals) reform, introduced in the Netherlands at the end of 2009, just after the start of the Great Recession. The reform targeted individuals up to and including 26 years of age. The goal of the WIJ reform was to reduce the number of young NEETS. To this end, welfare benefits were made conditional on participation in 'work-learn programs'.

This chapter aims to answer the question: "*What is the effect of a mandatory activation program for young individuals on the NEETs rate during a severe economic recession?*" We consider the effects of the WIJ reform on key outcome variables: NEETs claiming welfare benefits, NEETs not claiming welfare benefits, the overall NEETs rate, the employment rate and the enrollment rate in education.

We use differences-in-differences and regression discontinuity and the large administrative dataset Labour Market Panel (Arbeidsmarktpanel) of Statistics Netherlands (2015) to estimate the causal effects of the WIJ reform. The Labour Market Panel tracks 1.2 million individuals over the period 1999-2012 and contains a large set of labour market outcomes and a large number of individual and household characteristics. We consider the treatment effect for three different age groups, 20-22, 23-24 and 25-26 years of age, while our base control group consists of individuals 27-28 years of age. A key challenge in the empirical analysis is to control for potentially different time effects between the treatment and control groups, due to e.g. differential trends or different business cycle responses (Bell and Blanchflower 2011). In our preferred specification we therefore include demographic controls, a full set of unemployment-age dummies, age-specific trends and control-specific trends. We also present an extensive placebo analysis, including placebo treatment dummies for the years just before the reform and placebo treatment dummies for the earlier economic downturn in 2002-2004.

Our main findings are as follows. First, we find that the reform had a statistically significant large negative effect on the number of young NEETs claiming welfare benefits of –24% in the age group 25-26 years of age, the only treatment group that passes all the placebo tests. Second, the reform had only a small and statistically insignificant effect on the total number of NEETs. The reform pushed young individuals out of welfare, but did not increase the number of young individuals in employment or education. We argue that this is likely to be due to the state of the business cycle, as the reform clashed head on with the start of the Great Recession. During these years it was hard for people, in particular young individuals, to find a job. Third, our analysis shows that controlling for differential trends in a differences-in-differences analysis may be important for some outcome variables, like the enrollment rate in education, when studying a reform that targets young individuals and using somewhat older individuals as a control group. Finally, we show that standard pre-reform placebo treatment dummies may fail to reject the common time effects assumption.

Chapter 3: The Added Worker effect 1.2

Since the start of the Great Recession, policymakers and academics have shown increased interest in the effect of unemployment shocks on the labour supply of partners of the unemployed workers– also known as added worker effects (henceforth AWE). The empirical literature generally finds the AWE to be small, see e.g. Hardoy and Schøne (2014), Halla et al. (2018) and Bredtmann et al. (2018) for recent contributions. Two pertaining questions are whether the AWE has become more important in the years following the onset of the Great Recession and whether the AWE declined over time as the female participation rate increased (leaving less space for increases in labour supply).

Chapter 3 answers the question: "*How did the Added Worker Effect change over time and over the business cycle in the Netherlands during the period 2003-2015?*" To shed more light on the relation between the AWE and the business cycle, we study the AWE for women whose male partner became unemployed in the years before and during the Great Recession.

This chapter assesses the importance of the AWE for the updated 'Labour Market Panel' of Statistics Netherlands (2017). The updated Labour Market Panel tracks the labour market outcomes of 1.8 million individuals for the period 1999-2015, as well as their social security records.

Our research strategy compares households with male partners who became unemployed to households with male partners that remained employed in a given year. Using a differences-in-differences setup with individual fixed effects, we estimate the impact of male partner's unemployment shock in a particular year on the earnings of both partners, the employment of the female partner, income from Unemployment Insurance (UI) and other social benefits, and profits from self-employment – all measured over a time window from 2 years before entering UI to three years after entering UI. By taking different reference years for the unemployment shocks occurring to cohorts in our sample, we assess how the effects vary over the business cycle and over time more generally.

Our main findings are as follows. First, we find that the unemployment shock of a male partner, causing a loss in gross income of 20 to 30 thousand euro, has a positive and statistically significant but small positive AWE of 2-5% (500-1,000 euros). Second, the AWE for women that we estimate is small and insignificant during the first years of the Great Recession (2008-2009). Third, our findings point to the existence of both intensive and extensive margin effects for the AWE. The decrease in the AWE at the start of the Great Recession is mostly driven by decreases at the intensive margin. And the extensive margin effect decreased over time. Finally, we find an AWE of about 2% (500 euro) of profits from self-employment of the female partner and the treatment effect on male partner's profits more than doubled from about 2000 euro 3 years after entering UI in 2004 to about 4500 euro 3 years after entering UI in 2012.

1.3 Chapter 4: Constitutional commitment to social security

In recent decades, politicians and academics have emphasized the role of social rights for social and economic development (Townsend 2007;

ILO 2014). The main argument for a rights-based approach to social development is that it gives an entitlement that can be enforced in court. Without such a right, people are dependent on the 'good-will' of the incumbent government for proper education, health care and social security. In theory, the constitution can play an important role for social rights, as constitutions provide universal rights and protect minorities against the majority. However, the number of empirical studies on the effect of social rights in the constitution is still limited.

Chapter 3 of this thesis answers the question: "*What is the effect of constitutional commitment to social security on different social expenditure schemes?*" First, we are interested in the effect of constitutional commitment to social security (CCSS) on total social expenditure, which shows whether CCSS has an effect at all. Second, we study whether the effect of CCSS is most sizable on social expenditure schemes for beneficiaries who are seen as less deserving by the public opinion. We expect a larger effect of CCSS on these social expenditure schemes if the median voter cares less about these social expenditure schemes, leaving a larger role for the constitution.

We use a panel data set for 17 EU-countries from 1990 until 2012. The data on social expenditures as a percentage of GDP are taken from the Social Expenditure Database (SOCX) of the OECD. For CCSS, we use the indicator created by Ben Bassat and Dahan (2018), which we define as one or zero, depending on the presence of a legal provision on assistance to old age, survivors, disability, unemployment, sickness, work injury or the poor in the constitution. We run OLS models, 2SLS regression models and the Heckman two step model with the rigidity of the constitution as an instrument, to correct for possible endogeneity and to derive the effect of CCSS on different social expenditure schemes. In line with our expectations, the rigidity of the constitution has a positive effect on CCSS, indicating that our instrument is relevant.

Our main findings are as follows. First, we find a positive significant effect of CCSS on total social expenditure of 3.8 percentage points. This includes a positive effect on spending on old age and survivor, incapacity, unemployment and active labour market policies. Second, the most sizable effects, expressed as a percentage of average spending, are found for spending on unemployment and active labour market policies. These

are the expenditure schemes targeted at people who are perceived as less deserving by the public opinion (Blekesaune and Quadagno 2003; Van Oorschot 2006). Third, no positive effect is found on expenditure on health and family, which are expenditure types that are not covered in CCSS. This suggests that the positive effect on social expenditure is due to CCSS and not caused by a positive attitude towards redistribution.

1.4 Chapter 5: Social expenditure and poverty, inequality and GDP growth

Since Piketty (2014) has published his work on income inequality, there is a resurgence of the public and academic debate on income and wealth inequality. This debate is strengthened by the rise of populist movements. For a long time, policymakers and academics assumed a trade-off between reducing income inequality and increasing GDP growth (Kaldor 1957; Okun 1975; Lazear and Rosen 1981; Benabou 2000; Arjona et al. 2003). More recent studies challenge this view and find a negative association between income inequality and economic growth (Persson and Tabellini 1994; Alesina and Rodrik 1994; Perotti 1996; Easterly 2007; OECD 2014; Dabla-Norris et al. 2015). Moreover, the latest empirical evidence does not support that redistribution is negatively related to economic growth (Thewissen 2013; Ostry et al. 2014). Nevertheless, redistribution is a broad concept and different kinds of redistribution, translated into different social expenditure types, have different effects on poverty, inequality and economic growth.

Therefore, we focus in this chapter on the question: "*How are different social expenditure types related to poverty, inequality and GDP growth*" First, we investigate how social expenditure at the aggregated level is related to poverty, inequality and GDP growth. This analysis provides insights in the potential trade-off between poverty and inequality on the one hand and GDP growth on the other hand. Second, we study the relationships for social expenditure on 1) old age and survivor, 2) incapacity, 3) health, 4) family, 5) unemployment and active labour market policies (ALMPs) and 6) housing and others. This analysis shows the importance of the

different expenditure types for reducing poverty and inequality and for the potential detrimental effects on GDP growth.

We use a panel data set of 22 EU-countries for the years 1990-2015 for our base results and a panel data set of 32 OECD countries in our robustness analysis. The data are taken from several OECD databases. We employ OLS and 2SLS regression models in which the lagged values of the different expenditure variables are used as explanatory variables. We use social expenditure in period (t-1) because social expenditure itself also depends on growth and potentially also on poverty and inequality. In our 2SLS model, we use the social expenditure variables in period (t-2) as instrumental variable. Our preferred model is an OLS model with panel corrected standard errors in which we correct for first order serial correlation and control for country and year fixed effects.

Our main findings are as follows. First, we find total public social expenditure to be negatively related to poverty and inequality and not significantly related to GDP growth. Hence, there seems to be no trade-off between reducing poverty and inequality on the one hand and higher economic growth on the other hand. Second, the different social expenditure schemes are differently related to poverty, inequality and economic growth, which makes more accurate targeting possible. For poverty, we find negative relations with expenditure on family, unemployment and ALMPs and housing and other.[5] For inequality, we find a strong negative association with social expenditure on old age and survivor and family. Finally, a strong positive relation with GDP growth is found for expenditure on housing and others.

[5]Social expenditure on “others" consists for the largest part of expenditure on social assistance.

2 Preventing NEETs During the Great Recession – The Effects of a Mandatory Activation Program for Young Welfare Recipients

Abstract

We study the impact of a mandatory activation program for young welfare recipients in the Netherlands. What makes this reform unique is that it clashed head on with the Great Recession. We use differences-in-differences and regression discontinuity and data for the period 1999–2012 to estimate the effects of this reform. We find that the reform reduced the number of welfare recipients but had no effect on the number of NEETs (individuals not in employment, education or training). This last finding contrasts with previous studies, which we argue is due to the reform taking place during a severe economic recession.

A working paper version of this chapter is published as Cammeraat et al. (2017) and is currently under review. The chapter is co-authored by Egbert Jongen en Pierre Koning. We are grateful to Ineke Bottelberghs, Marina Pool and Mirthe Bronsveld-de Groot of Statistics Netherlands for the data on participation in mandatory activation programs by young welfare recipients. Furthermore, we are grateful for comments and suggestions by Leon Bettendorf, Richard Blundell, Matz Dahlberg, Sander Gerritsen, Bas Jacobs, Max van Lent, Daniël van Vuuren and seminar and conference participants at Leiden University, CPB Netherlands Bureau for Economic Policy Analysis, the IIPF 2016 Doctoral School in Mannheim, the IIPF 2016 Conference in South Lake Tahoe, EALE 2016 in Ghent, NED 2016 in Amsterdam, LAGV 2017 in Aix-en-Provence and the RWI-GIZ Conference 'What Works? The Effectiveness of Youth Employment Programs' 2017 in Berlin. Remaining errors are my own.

2.1 Introduction

Young individuals not in employment, education or training (NEETs) are a major policy concern, in particular during periods of recession. NEETs are a prime concern for the European Commission (Carcillo et al. 2015). In his 2016 State of the Union speech, President Juncker of the European Commission stated he wants to "continue to roll out the Youth Guarantee across Europe, improving the skillset of Europeans and reaching out to regions and young people most in need." (European Commission 2016) This increased policy attention for reducing the number of NEETs is accompanied with a trend towards stricter conditions for receiving welfare benefits, via e.g. the imposition of job search requirements and/or by making welfare benefits receipt conditional on participation in so-called work-learn programs. Prominent examples of such policies that are targeted at young unemployed individuals include the New Deal for Young People in the UK and the Job Corps in the US (Kluve 2014). Previous studies have found that stricter conditionality of welfare benefits decreases welfare claims and increases employment rates (Blundell et al. 2004; Dahlberg et al. 2009; Persson and Vikman 2014; Hernæs et al. 2016; Kluve et al. 2016; Bolhaar et al. 2018).

In this paper, we study the effects of a mandatory activation program for young individuals during a severe economic recession. Specifically, we study the WIJ (*Wet Investeren in Jongeren*, Work Investment Act for Young Individuals) reform, introduced in the Netherlands at the end of 2009, just after the start of the Great Recession. The reform targeted individuals up to and including 26 years of age. The goal of the WIJ reform was to reduce the number of young NEETs. To this end, welfare benefits were made conditional on participation in 'work-learn programs'. We consider the effects of the WIJ reform on key outcome variables: NEETs claiming welfare benefits, NEETs not claiming welfare benefits, the overall NEETs rate, the employment rate and the enrollment rate in education.

We use differences-in-differences and regression discontinuity and the large administrative dataset Labour Market Panel (*Arbeidsmarktpanel*) of Statistics Netherlands (2015) to estimate the causal effects of the WIJ reform. The Labour Market Panel tracks 1.2 million individuals over the

period 1999–2012 and contains a large set of labour market outcomes and a large number of individual and household characteristics. We consider the treatment effect for three different age groups, 20–22, 23–24 and 25–26 years of age, while our base control group consists of individuals 27–28 years of age. A key challenge in the empirical analysis is to control for potentially different time effects between the treatment and control groups, due to e.g. differential trends or different business cycle responses (Bell and Blanchflower 2011). In our preferred specification we therefore include demographic controls, a full set of unemployment-age dummies, age-specific trends and control-specific trends. We also present an extensive placebo analysis, including placebo treatment dummies for the years just before the reform and placebo treatment dummies for the earlier economic downturn in 2002–2004.

Our main findings are as follows. First, we find that the reform had a statistically significant large negative effect on the number of young NEETs claiming welfare benefits of –24% in the age group 25–26 years of age, the only treatment group that passes all the placebo tests. Second, the reform had only a small and statistically insignificant effect on the total number of NEETs. The reform pushed young individuals out of welfare, but did not increase the number of young individuals in employment or education. Third, our analysis shows that controlling for differential trends in a differences-in-differences analysis may be important for some outcome variables, like the enrollment rate in education, when studying a reform that targets young individuals and using somewhat older individuals as a control group. Furthermore, we show that standard pre-reform placebo reform dummies may be insufficient to test for common time effects, as business cycle responses may differ still.

Our paper relates to a number of studies that consider the effects of mandatory activation programs for young individuals.[1] Blundell et al. (2004) use area-based piloting and age-related eligibility rules to identify

[1]Our analysis also contributes to a broader literature on the effect of training programs targeted at the youth. The overall success rate of programs on employment and wage earnings is found to be small, see e.g. a recent meta-analysis by Kluve et al. (2016). According to Kluve et al. (2016), one of the key determinants of success is that programs consist of a comprehensive set of interventions, like training, counseling, mediation and private sector incentives.

the employment impact of a mandatory job search programme in the UK, the New Deal for Young People. They find that the program increased the probability to find employment by about five percentage points. Dahlberg et al. (2009) and Persson and Vikman (2014) analyze respectively the effect on the number of welfare recipients and entry and exit effects of a welfare reform in Sweden where city districts in Stockholm implemented mandatory activation programs at different rates. They find that the reform reduced welfare participation and increased employment rates of younger individuals, with the main effect operating through the entry rate into welfare. Hernæs et al. (2016) exploit a geographically differentiated implementation of conditionality of welfare benefits for Norwegian youth and find that stricter conditionality reduces welfare claims and increases high school completion rates. These analyses suggest that the combination of welfare conditionality and welfare-to-work programs can reduce the number of NEETs and promote employment and enrollment in education among young individuals.

We make the following contributions to this literature. First, we show that stricter conditionality combined with welfare-to-work programs does not always increase employment or enrollment in education. Indeed, we find that for the WIJ reform there was no effect on the number of NEETs. The main effect of the reform was simply to push young individuals out of welfare. We argue that this is likely to be due to the state of the business cycle, as the reform clashed head on with the start of the Great Recession, during which it was hard for people, in particular young individuals, to find a job. Second, we consider all potential outcome states, not only NEETs on welfare but also NEETs not on welfare, and the enrollment in education next to employment (and we also consider the effect on entry and exit rates). Indeed, our analysis for young individuals in the treated group shows that when looking at the effects on the employment rate, it is important to study changes in the enrollment rate in education as well. Third, we use an exceptionally large and long data set, that allows us to study and account for differential trends and test for differences in business cycle responses across age groups in an earlier economic downturn. The latter turns out to be crucial, as standard pre-reform

placebo treatment dummies may fail to reject the common time effects assumption.

The outline of the paper is as follows. Section 2.2 describes the institutional setting and the main features of the reform. Section 2.3 discusses the empirical methodology. Section 2.4 discusses the dataset and gives descriptive statistics. In Section 2.5 we then present graphical evidence, the estimation results and a large number of robustness checks. Section 2.6 discusses our findings and concludes. An appendix contains supplementary material.

Institutional setting and the reform 2.2

Young NEETs are a policy concern in all OECD countries. However, there is considerable variation in the share of NEETs among the young across OECD countries, and the extent to which the share of NEETs has changed during the Great Recession, see Table 2.1. Panel A gives indicators for individuals 20–24 years of age, and panel B gives indicators for individuals 25–29 years of age. The Netherlands has one of the lowest NEETs shares among OECD countries, in 2015 only 8.9% of 20–24 year olds in the Netherlands were NEETs.[2] Over the period 2005–2015, there has been a moderate rise in the share of NEETs in the Netherlands. The low share of NEETs in the Netherlands is mirrored by the high share of 20–24 year olds that are in education, as well as by the high share of 20–24 year olds that are employed, whereas the share of unemployed 20–24 year olds is relatively low, see again Table 2.1.[3] Turning to individuals 25–29 years of age, the Netherlands also scores relatively favorable in terms of a low NEETs rate, a high enrollment rate in education, a high employment-to-population rate and a relatively low unemployment rate.

[2]In 2015, the only country in the OECD with a lower share of NEETs was Iceland (6.6%). Below we will compare our results to studies for e.g. Norway, Sweden and the UK. In this respect it is relevant to note that Norway had a NEETs rate that was only slightly higher than in the Netherlands, the NEETs rate in Sweden was somewhat higher still, whereas the NEETs rate in the UK was considerably higher (almost double the NEETs rate in the Netherlands for individuals 20–24 years of age).

[3]The shares of individuals in education and individuals in employment add up to more than 100% because individuals in education can be employed, and employed individuals can also be in education.

Table 2.1: An international perspective on NEETs

Year	NEETs-to-population rate 2005	NEETs-to-population rate 2015	Education-to-population rate 2015	Employment-to-population rate 2015	Unemployment-to-population rate 2015
Panel A: Individuals 20–24 years of age					
Continental Europe					
Netherlands	8.1	8.8	57.7	69.4	6.7
Belgium	18.3	15.8	45.3	42.0	9.8
France	17.8	20.9	44.4	46.2	14.2
Germany	18.7	9.3	54.4	64.3	5.1
Scandinavia					
Denmark	8.3	12.4	59.1	63.4	7.6
Finland	13.0	18.3	47.8	52.5	14.7
Norway	9.6	10.2	42.1	66.6	5.8
Sweden	13.4	11.8	46.0	56.4	13.0
Anglo-Saxon countries					
Australia	11.6	13.1	44.5	71.5	7.3
Canada	14.4	14.4	41.6	64.7	8.3
United Kingdom	16.8	15.6	33.8	65.3	8.2
Unites States	15.5	15.8	38.5	64.1	6.5
OECD average	17.3	16.9	44.8	53.4	9.9
Panel B: Individuals 25–29 years of age					
Continental Europe					
Netherlands	10.7	12.1	20.8	82.2	5.7
Belgium	17.7	20.2	8.5	74.4	11.0
France	19.8	23.4	8.5	72.1	12.5
Germany	21.2	12.8	20.8	77.9	5.0
Scandinavia					
Denmark	11.6	15.2	30.4	73.8	7.9
Finland	14.0	18.2	26.9	70.2	10.1
Norway	12.3	14.0	14.6	77.1	5.2
Sweden	10.0	10.8	25.1	75.6	8.7
Anglo-Saxon countries					
Australia	15.4	15.5	19.1	78.5	4.4
Canada	15.7	17.6	12.8	76.7	7.0
United Kingdom	16.6	16.2	12.7	79.4	5.0
Unites States	18.1	20.0	13.2	75.4	4.7
OECD average	19.0	19.3	16.3	73.5	9.4

Notes: Using data from OECD (2016a), OECD (2016b) and OECD (2016c). The education-to-population rate is the enrollment in education divided by the relevant age population. The unemployment-to-population rate is calculated as the unemployment rate multiplied by the labour force participation rate.

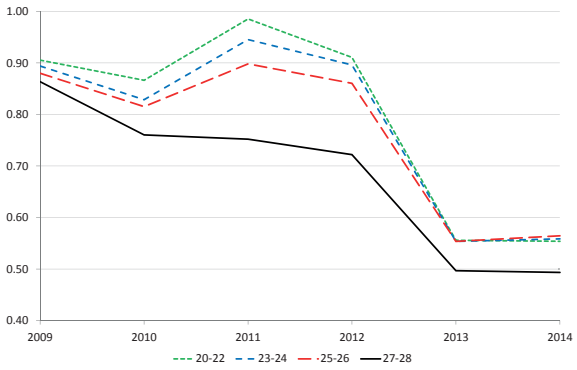
The reform we consider targets young individuals on welfare benefits. In the Netherlands, welfare benefits form a safety net that is provided by municipalities to support unemployed individuals who are not, or are no longer, entitled to other types of social insurance benefits like unemployment insurance. The vast majority of new welfare recipients consists of individuals with insufficient work history for entitlement to unemployment insurance.[4] Welfare benefits are means-tested and assets-tested.[5] The level of welfare benefits differs across household types and age groups. In 2008, before the start of the WIJ reform, welfare benefits ranged from 220 euro per month for singles of 18–20 years of age to 1,320 euros per month for couples with children (Ministry of Social Affairs and Employment 2008).

The Work Investment Act for Young Individuals (*Wet Investeren in Jongeren*, WIJ) came into force in October 2009 as a consequence of increased policy attention for NEETs and welfare dependency. The reform was designed before the start of the Great Recession, but the implementation was after the start of the Great Recession (Recession started in September 2008). Similar to e.g. the New Deal for Young People in the UK, the aim of the WIJ was both to activate young welfare recipients and to foster their human capital formation. The WIJ stipulated that for individuals below the age of 27, entitlement to welfare benefits became conditional on participation in a mandatory activation program. These mandatory programs were defined as 'work-learn offers' and consisted of public employment programs, apprenticeships and internships. Any wage earnings in these programs were supplemented up to the level of welfare benefits. As Figure 2.1 shows, the WIJ increased the coverage rate of activation programs for young welfare recipients in our respective treatment groups (individuals 20–22, 23–24 and 25–26 years of age, discussed below) from around 85% in January 2010 to around 95% in 2011 (on average). Hence, the reform restricted the discretionary room of caseworkers in administering welfare benefits and work-learn offers.

[4] In 2014, only 22% of all new welfare recipients consisted of unemployed workers who exhausted their unemployment insurance benefits (UWV 2014).

[5] For single individuals, net worth should not exceed 5,765 euro. For households with more persons, net worth should not exceed 11,895 euro.

Figure 2.1: Participation rate of individuals on welfare in activation programs



Notes: Statistics Netherlands (personal communication). This figure gives the share of individuals on welfare participating in an activation program in the respective treatment groups 20–22, 23–24 and 25–26 years of age and the control group 27–28 years of age.

The WIJ applied to all new entrants into welfare from October 2009 onward. However, as the enactment of the WIJ implied a substantial increase in the workload for municipalities, municipalities were given 9 additional months – until July 2010 – to increase coverage of the WIJ to 100% of the pre-existing stock of welfare recipients. Figure 2.1 suggests that in the end it took until January 2011 for the WIJ to achieve its largest coverage.

To get a better understanding of the implementation of the WIJ reform at the municipality level, we interviewed policymakers and caseworkers in the city of Amsterdam that were involved in the design and implementation of the WIJ. In Amsterdam, the majority of work-learn offers were provided by retail companies, local industries and welfare-to-work organizations. The respondents in our interviews stressed that some aspects of the WIJ were already common practice in Amsterdam. That is, apprenticeships, internships and public employment programs were already provided for individuals up to 23 years of age (Board of Amsterdam 2009). In effect, in Amsterdam the WIJ reform thus implied the extension of these programs to individuals with 24–26 years of age, together with the

imposition of welfare conditionality for all young individuals below the age of 27. In our empirical analysis, we focus on the group of individuals 25–26 years of age, because they are the most comparable to the control group of 27–28 years of age (as indicated by e.g. placebo pre-reform dummies), but we also consider the effects for younger age groups.

A previous social security reform implemented in the early 2000s already went in the direction of improving opportunities for youth who enter the labour market while tightening up their obligations to find work or improve their employability (OECD 2008; OECD 2010).[6] However, this reform was much smaller than the WIJ-reform and the obligations were enforced much less strictly, which also follows from Figure 2.1. The huge effect of the WIJ-reform on exit rates from welfare, which we will present in the results section, provides evidence that the WIJ-reform had a substantial additional effect on welfare conditionality on top of this previous reform.

Finally, next to the WIJ reform, there were two other reforms relevant for our analysis that took place in January 2012. First, the government replaced the mandatory acceptance of work-learn offers with 'work-first' arrangements. Specifically, the government introduced an initial one-month 'job-search period' during which individuals younger than 27 years of age did not receive welfare benefits. This may explain the small drop in the participation rate in activation programs in January 2012, and the larger drop in January 2013, see Figure 2.1. Second, adult children living at home were no longer eligible to welfare benefits when they lived in a household in which first-degree relatives had sufficient income or assets (the 'household-income test'). To study to what extent these additional reforms may affect our results, we also present treatment effects by individual treatment years, the treatment effect on the probability of being an adult child living at home and the treatment effects for the subgroup of adult children living at home.

[6]This reform was called "Chain for Work and Income" which was established in 2002 with the SUWI Law (Law on implementation structure for work and income)

2.3 Empirical methodology

We use differences-in-differences (DD) and regression discontinuity (RD) to estimate the effects of the WIJ reform on a number of outcome variables. Our preferred method is DD because this gives us an average treatment effect for a larger group than the local average treatment effect of regression discontinuity. Indeed, we are also interested in the treatment effect for individuals further away from the cutoff (20–24 years of age). Furthermore, we may be concerned that welfare recipients or their caseworkers might anticipate the 27th birthday of the welfare recipient, when participation in work-learn arrangements is no longer obligatory, or that participation in work-learn arrangements may continue after the 27th birthday of the welfare recipient.[7]

2.3.1 Differences-in-differences

The reform was targeted at individuals up to 27 years of age and started in October 2009. A key assumption of the DD approach is common time effects for the treatment and control group (in the absence of the reform). In this context, our preferred treatment group consists of individuals 25–26 years of age and a control group consisting of individuals 27–28 years of age. Our baseline model also considers the treatment effects for the treatment groups consisting of individuals 20–22 and 23–24 years of age, but we will show that changes in the enrollment in education complicate the analysis for these groups (young individuals in the treated group have a choice of staying in education, while this is hardly a choice for individuals in the older control group). The age variable is measured on the 1st of October of each year, and the outcome variables are averages for October each year.

As outcome variables we consider (i) the 'participation rate' in NEETs, defined as not being in employment or education[8], (ii) the participation

[7] A robustness analysis where we leave out observations close to the threshold using a so-called donut-RD design yields similar results as the base RD specification with these observations included though.

[8] Similar to the OECD, we do not observe participation in training programs in our dataset.

rate in NEETs on welfare, (iii) the participation rate in NEETs not on welfare, (iv) the employment rate, and (v) the enrollment rate in education. The participation rate in NEETs, the employment rate and the enrollment rate in education sum to one, but we analyse them independently.

For all these outcome variables we estimate a linear probability model (Angrist and Pischke 2009). Let y_{iat} be a dummy variable that is 1 if individual i in age group a is 'participating', 'employed' or 'enrolled' in period t. In our preferred DD specification, we regress the outcome variable on a set of year fixed effects (α_t), age fixed effects (β_a), age-specific trends (with coefficients γ_a), an interaction term between age and the unemployment rate (u_t) with age-specific coefficients ϕ_a, a set of demographic controls X_i (gender and ethnicity) with coefficients μ_x, a set of demographic-control-specific trends with coefficients ψ_x, a treatment effect (DD_{gt}) for individuals in the treatment group g in a given year t in the post-reform period with coefficient $\delta_{g,t}$, and an error term ϵ_{iat}:

$$y_{iat} = \alpha_t + \beta_a + \gamma_a t + \phi_a u_t + X_i' \mu_x + X_{it}' \psi_x t + \delta_{g,t} \mathrm{DD}_{gt} + \epsilon_{iat}. \quad (2.1)$$

We are primarily interested in the treatment coefficients $\delta_{g,t}$. We include an interaction term between age and the unemployment rate to allow for different business cycle responses across age groups (Bell and Blanchflower 2011). Furthermore, we include age-specific and demographic-control-specific trends to allow for trend differences.[9]

In an extension to this model, we add placebo treatment dummies for the pre-reform years 2008 and 2009. The coefficients on these placebo treatment dummies are informative about potential remaining differential time effects between the treatment and control groups, for example because of changes in group specific trends or differences in business cycle responses not captured by the age-specific unemployment terms, and also about potential anticipation effects of the reform.

Finally, to allow for correlation in the error terms at a higher level than the individual and over time, we use cluster-robust standard errors

[9] We have 10 years of pre-reform data to estimate the coefficients on these trends.

(Bertrand et al. 2004; Donald and Lang 2007). We cluster the standard errors by month of birth. This results in 264 clusters in our base DD specification, which is deemed sufficiently large by Angrist and Pischke (2009) to use the large-sample properties of the estimator.

2.3.2 Regression discontinuity

In the RD approach we estimate the impact of the policy by comparing differences in the outcome variables for individuals that are just younger than the cutoff of 27 years that determines treatment by the WIJ reform with individuals that are just older than this cutoff. The advantage of using an RD approach is that the treatment and control group are likely to share the same time-effects, however we only obtain a local average treatment effect and we need to assume that in the absence of the reform the outcome variables are a smooth function in age. Related to the last point, we assume that individuals and caseworkers did not anticipate the end of the WIJ obligations by already lowering their effort preceding the age cutoff of 27 years.[10]

Similar to our DD setup, we use linear probability models in our RD setup. In our preferred RD specification, we regress participation status y_{iat} on a year fixed effect (β_t), age in months a_{it} (recentered[11], with coefficient β_a), an interaction term that captures the additional effect of age when the person is younger than the cutoff a' (with coefficient $\beta_{a<a'}$) to allow for a different slope to the left of the discontinuity, a treatment effect if the age of the person is below 27 (with coefficient β_{RD}) capturing the discontinuity, individual characteristics X_i and an error term ϵ_{it}:[12]

$$y_{it} = \beta_t + \beta_a a_{it} + \beta_{a<a'} 1(a_{it} < a') a_{it} + \beta_{RD} \mathrm{RD}_{it} + X_i' \mu_x + \epsilon_{it}. \qquad (2.2)$$

[10] We will address this issue by also presenting 'donut' RD regressions (Barreca et al. 2011) that exclude months around the age cut-off of 27 years.

[11] Age is recentered so that individuals that have turned 27 in September have a value of 1, they are the first age group to the 'right' of the discontinuity.

[12] We also estimated models with a quadratic term in age, and with a different quadratic term in age to the left of the threshold. The estimated discontinuities are similar to the results of our preferred specification (available on request).

Our primary interest is in coefficient β_{RD}, which measures the size of the discontinuity in the relationship between the outcome variable and age due to the policy. For an accurate measurement of the discontinuity it is important to get a precise estimate of the relation between age and the outcome variables around the discontinuity. In the RD analysis we therefore use month of birth relative to the discontinuity as the running variable.[13] Since the identification in the RD approach comes from differences in month of birth, we cluster standard errors by month of birth, where we put persons born in the same month but in different years in different clusters. This generates 72 clusters in the base specification, again deemed sufficiently large to use the large-sample properties of the estimator.

In an extension of the RD analysis we consider a 'difference-in-discontinuity' setup, using both the pre- and post-reform data – see e.g. the analysis in Bettendorf et al. (2014). This specification may be relevant if the age cutoff of 27 years of age cannot be uniquely linked to the WIJ reform but that other pre-existing policies use a similar cutoff. To test for this possibility, we use observations both before and after the policy reform to control for a potential discontinuity before the reform. In this specification we include a treatment effect γ_{PRD} that captures the pre-reform discontinuity, and an additional treatment effect for the post-reform discontinuity relative to the pre-reform discontinuity γ_{DRD}. In the specification below, the discontinuity before the reform equals γ_{PRD} and the discontinuity after the reform equals $\gamma_{PRD} + \gamma_{DRD}$:

$$\begin{aligned} y_{it} &= \gamma_t + \gamma_a a_{it} + \gamma_{a^2} (a_{it})^2 + \gamma_{a<a'} 1(a_{it} < a') a_{it} \\ &\quad + \gamma_{PRD} \text{PRD}_{it} + \gamma_{DRD} \text{DRD}_{it} + X_i' \nu + v_{it}, \end{aligned} \tag{2.3}$$

where for the same reasons as in the RD analysis we use age measured in months relative to the discontinuity as the running variable, and we cluster the standard errors by month of birth.

[13] The exact date of birth during the month is not available in our data set.

2.4 Data

We use data from the Labour Market Panel (*Arbeidsmarktpanel*) of Statistics Netherlands (2015). The Labour Market Panel is a large and rich household panel data set, tracking 1.2 million individuals over the period 1999–2012.[14] We use the years 1999–2009 as the pre-reform years, and 2010–2012 as the treatment years.

We consider three treatment groups: i) individuals 25–26 years of age, ii) individuals 23–24 years of age and iii) individuals 20-22 years age. Our main control group consists of individuals 27–28 years of age. As we will see below, the treatment group of individuals 25–26 years of age is the most similar to our main control group in terms of demographic characteristics, levels of the outcome variables and business cycle responses. The other two treatment groups with younger individuals are more likely to differ from the main control group, and hence we have to be extra careful when interpreting the estimated treatment effects for these younger treatment groups.

The outcome variables are based on the social-economic classification (SEC) variable in the Labour Market Panel. The SEC variable classifies individuals according to their main source of income, where individuals in education are always classified as being in the state of education (even if their wage income is larger than their study grant) and individuals with profit income are always classified as being self-employed (even if their wage income exceeds their profit income). According to the SEC individuals can be in the following states: (1) employee, (2) owner of closely-held company, (3) self-employed, (4) another type of employment, (5) on unemployment insurance, (6) on welfare benefits, (7) on disability or sickness benefits, (8) on retirement benefits, (9) on other social insurance, (10) in education with income, (11) in education without income, (12) without income. We count individuals in states (1)-(4) as employed, in states (10)-(11) as in education, and in states (5)-(9) and (12) as NEETs. Within the state of NEETs we count individuals in state (6) as NEETs on welfare and individuals in states (5), (7)-(9) and (12) as NEETs not

[14] For a limited number of variables, not used in this study, the data set also contains data for 2013.

Table 2.2: Descriptive statistics treatment groups and control group

	Treatment Group (1999–2009)		Differences (treatment–control)		Normalized differences (treatment–control)	
	Mean	SD	1999–2009	2010–2012	1999–2009	2010–2012
Panel A: Treatment group 25–26						
Explanatory variables						
Female	0.506	0.500	−0.006	0.000	−0.009	0,000
Non-Western immigrant	0.102	0.302	0.001	−0.004	0.003	−0.008
Western immigrant	0.072	0.258	−0.003	−0.002	−0.007	−0.005
Dependent variables						
NEETS rate on welfare	0.025	0.155	−0.001	−0.004		
NEETs rate not on welfare	0.088	0.283	−0.011	0.005		
Total NEETs rate	0.112	0.316	−0.012	0.001		
Employment rate	0.818	0.386	−0.036	−0.065		
Enrollment rate education	0.069	0.254	0.048	0.063		
Panel B: Treatment group 23–24						
Explanatory variables						
Female	0.499	0.500	−0.013	−0.002	−0.018	−0.002
Non-Western immigrant	0.101	0.302	0.001	−0.004	0.002	−0.009
Western immigrant	0.069	0.253	−0.005	−0.004	−0.015	−0.010
Dependent variables						
NEETS rate on welfare	0.022	0.146	−0.004	−0.007		
NEETs rate not on welfare	0.078	0.268	−0.021	0.004		
Total NEETs rate	0.099	0.299	−0.025	−0.003		
Employment rate	0.714	0.452	−0.140	−0.212		
Enrollment rate education	0.187	0.390	0.165	0.215		
Panel C: Treatment group 20–22						
Explanatory variables						
Female	0.492	0.500	−0.020	−0.004	−0.029	−0.005
Non-Western immigrant	0.101	0.301	0.001	0.007	0.001	0.016
Western immigrant	0.067	0.249	−0.008	−0.006	−0.021	−0.017
Dependent variables						
NEETS rate on welfare	0.014	0.118	−0.012	−0.013		
NEETs rate not on welfare	0.063	0.243	−0.035	−0.012		
Total NEETs rate	0.077	0.267	−0.047	−0.025		
Employment rate	0.491	0.500	−0.363	−0.459		
Enrollment rate education	0.432	0.495	0.410	0.484		

Notes: Own calculations using the Labour Market Panel (Statistics Netherlands). Treatment groups: individuals 20–22, 23–24 and 25–26 years of age. Control group: individuals 27–28 years of age. Observations 1999-2012: treatment group 20–22: 582,364, treatment group 23–24: 375,182, treatment group 25–26: 376,083, control group 27-28: 391,627. Normalized differences are mean differences divided by the square root of the sum of the variances (see Imbens and Wooldridge 2009).

on welfare. As demographic control variables we include gender and ethnicity (native/Western immigrant/non-Western immigrant).

Table 2.2 gives descriptive statistics for the respective treatment groups, along with the differences and normalized differences (for the demographic control variables) with the control group in the pre- and the post-reform period. The differences in the demographic control variables gender and ethnicity are small for all treatment groups, in particular for the oldest treatment group with individuals 25–26 years of age. The same is true for the so-called normalized differences (mean differences divided by the square root of the sum of variances). Imbens and Wooldridge (2009) argue that these normalized differences are an informative way to check if the treatment and control group have sufficient overlap in the covariates, and as a rule of thumb they suggest that when the normalized difference exceeds a value of .25, linear regression becomes sensitive to the specification. The normalized differences for gender and ethnicity stay well below .25. Furthermore, the differences in the demographic control variables hardly change from the pre- to the post-reform period. Hence, there is no indication of differential changes in the composition of the treatment and control group.[15]

Table 2.2 also gives descriptive statistics for the outcome variables. The NEETs rate on welfare in the oldest treatment group is very similar to the control group in the pre-reform period, but drops relative to the control group in the post-reform period, suggesting a negative treatment effect on this outcome variable for this treatment group. The pre-reform differences in the NEETs rate on welfare are larger for the younger treatment groups, in particular for the youngest treatment group. Also for these groups the difference becomes larger in the post-reform period, suggesting a negative treatment effect for the NEETs rate on welfare benefits for these groups. The NEETs rate not on welfare is also quite similar for the older treatment group and the control group before the reform, though somewhat lower for the treatment group than the control group, and lower still for the younger treatment groups. After the reform, the NEETs rate not on welfare move closer to the control group, suggesting a positive treatment effect on this outcome variable. The total NEETs rate again is quite similar for the oldest treatment group and the control group before the reform,

[15]Figure A.2.1 in the Supplementary material plots the shares of the demographic control variables for the treatment and control group over time.

though again somewhat lower for this treatment group, and lower still for the younger treatment groups. After the reform, the total NEETs rate of the treatment groups move closer to the control group, which suggests a positive treatment effect for the total NEETs rate. The employment rate is lower for the treatment groups than the control group in the pre-reform period, and the difference becomes more negative in the post-reform period, suggesting a counterintuitive negative treatment effect on the employment rate. Finally, the enrollment rate in education shows the mirror image of the employment rate. The enrollment is higher in the treatment groups than in the control group in the pre-reform period, and this difference also becomes bigger in the post-reform period, suggesting a positive treatment effect on the enrollment in education. However, these simple treatment effects do not account for differential trends between the treatment and control groups. These differential trends will turn out to be important for some outcome variables, in particular for the younger treatment groups, in the empirical analysis below.

Results 2.5

Differences-in-differences 2.5.1

We first present graphical evidence on the treatment effects of the reform on the outcome variables, see Figure 2.2. The solid black line denotes the control group of individuals 27–28 years of age, whereas the red, blue and green lines denote the treatment groups of 25–26, 23–24 and 20-22 years of age, respectively. The dotted lines denote the difference between the respective treatment groups and the control group. Figure 2.2(a) shows that the NEETs rate on welfare moves very much in tandem for the treatment groups 23–24 and 25–26 years of age and the control group in the pre-reform period, and there is a clear negative treatment effect in 2010, which subsequently becomes smaller in 2011 and then remains roughly constant in 2012. For the youngest treatment group 20–22 years of age, the NEETs rate on welfare also shows a quite similar pattern to the control group prior to the reform, but there is no apparent treatment

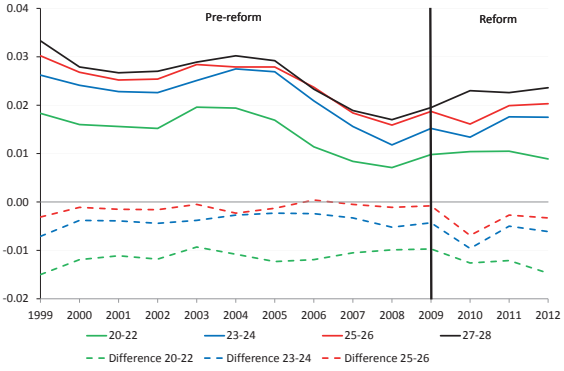
dip in 2010 (although the control group moves 'up', presumably due to the Great Recession, and the treatment group 20–22 does not) nor is there an apparent recovery in the NEETs rate in 2011 or 2012 for this treatment group relative to the control group. Figure 2.2(b)-(e) make clear that there are apparent trend differences between the treatment and control group for the other outcome variables, also for the treatment group 25–26 years of age. The main culprit here is the difference in trends in the enrollment in education by age groups, see Figure 2.2(e). Hence, accounting for differential trends will be important to isolate the treatment effect of the reform for these outcome variables.

Table 2.3 gives the base differences-in-differences regression results. In all specifications we use a single treatment dummy per treatment group for the post-reform years 2010–2012.[16] First consider the results for the treatment group 25–26 years of age in Panel A, the group that is the most similar to the control group in observable characteristics and means of the outcome variables. Column (1) shows the results of the basic DD setup, where we only include year dummies, a group dummy for each individual age group and a treatment dummy for the age group 25–26. This setup suggests a negative and statistically significant treatment effect of –0.30 percentage points on the NEETs rate on welfare. In column (2) we add demographic controls. Consistent with the observation that there were negligible compositional changes in these characteristics, this hardly affects the estimated treatment effect. In column (3) we add interaction terms for age and the national unemployment rate, to allow for a potential different business-cycle response by age. Again, this does not substantially affect the estimated treatment effect for the NEETs rate on welfare. In column (4) we then also allow for age-specific trends, and this leads to a somewhat larger treatment effect in absolute terms (more negative) of –0.44 percentage points. Finally, column (5), our richest and preferred specification, shows that the inclusion of demographic-control specific trends gives a treatment effect that is very similar to the treatment effect in column (4). The treatment effect in column (5) of –0.46 percentage points also suggests a sizable negative treatment effect on the NEETs rate on

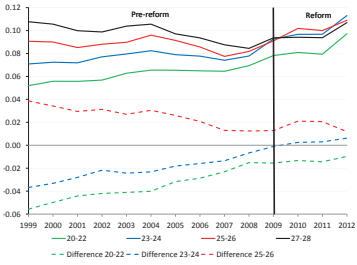
[16] Full regression results can be found in Table A.2.1 in the Supplementary material.

Figure 2.2: Means outcome variables treatment and control groups: 1999–2012

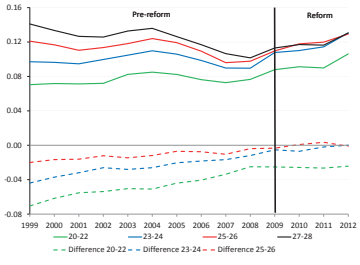
(a) NEETs rate on welfare



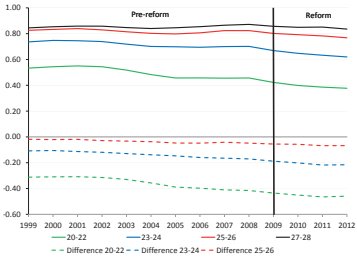
(b) NEETs rate not on welfare



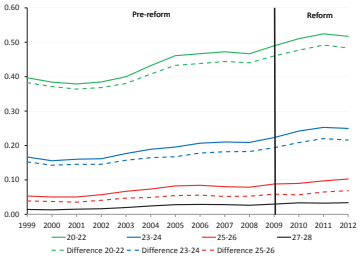
(c) Total NEETs rate



(d) Employment rate



(e) Enrollment rate in education



Notes: Own calculations using the Labour Market Panel (Statistics Netherlands). The solid black line denotes the control group of individuals 27–28 years of age, the red lines denote the treatment group 25–26 years of age, the blue lines denote the treatment group 23–24 years of age and the green lines denote the treatment group 20–22 years of age. The dotted lines denote the difference between the treatment group and the control group. NEETs rates are individuals not in employment or education relative to the relevant age population, employment rates are individuals in employment relative to the relevant age population and enrollment rates in education are individuals in education relative to the relevant age population.

welfare of –24% relative to a baseline of 1.9 percentage points in the last pre-reform year (2009).

As noted earlier, accounting for trend differences between the treatment and control group is important for the other outcome variables in Table 2.3. In particular, we find rather similar treatment effects for the specification in columns (1)-(3)[17], but allowing for differential trends in age in column (4) has an important impact on the treatment effects on the employment rate and the enrollment rate in education.[18] Our preferred specification is in column (5), with results suggesting a positive and statistically significant treatment effect on the NEETs rate not on welfare, but no effect on the total NEETs rate. Also, there appears to be no effect on the employment rate and the enrollment rate in education. Hence, the reform seems to have pushed or kept the treated individuals in this age group out of welfare without higher employment and/or enrollment in education. This is at odds with previous studies on related reforms, as we will discuss more extensively in the final section.

Panels B and C give the results for the younger age groups. We focus on our preferred specification controlling for differential trends in column (5). Similar to the age group of 25–26 years of age, negative and statistically significant treatments effects on the NEETs rate on welfare of about –0.4 percentage points are found for the age groups of 23–24 and 20–22 years of age. We find no effect on the overall NEETs rate for those aged 23–24, but a large statistically significant decrease for those aged 20–22. For the individuals aged 23–24, the suggested treatment effect on the enrollment rate in education is positive and the treatment effect on the employment rate is negative. An optimistic interpretation of this result is that this treatment group was stimulated to remain in (or return to) education following the WIJ reform. As we will show in our robustness tests, however, this finding should be interpreted with the appropriate care.

Turning to the placebo analyses, first consider the results in Table 2.4. In this table we take specification (5) of Table 2.3 and add placebo

[17] Although the 'treatment effect' for the employment rate and enrollment rate in education do vary in absolute size over the different specifications in columns (1)-(3).

[18] The inclusion of demographic-control specific trends in column (5) again hardly affects the results when compared to column (4).

Table 2.3: Differences-in-differences: base regression results

	(1)	(2)	(3)	(4)	(5)
Panel A: Treatment group 25–26					
NEETs rate on welfare	−0.0030***	−0.0028***	−0.0032***	−0.0044***	−0.0046***
	(0.0010)	(0.0010)	(0.0011)	(0.0014)	(0.0014)
NEETs rate not on welfare	0.0159***	0.0161***	0.0137***	0.0061***	0.0060***
	(0.0017)	(0.0016)	(0.0019)	(0.0023)	(0.0023)
NEETs rate	0.0129***	0.0133***	0.0105***	0.0017	0.0014
	(0.0019)	(0.0019)	(0.0020)	(0.0027)	(0.0028)
Employment rate	−0.0298***	−0.0303***	−0.0213***	−0.0027	−0.0027
	(0.0032)	(0.0031)	(0.0035)	(0.0036)	(0.0036)
Enrollment rate in education	0.0169***	0.0170***	0.0108***	0.0010	0.0013
	(0.0027)	(0.0027)	(0.0030)	(0.0032)	(0.0032)
Panel B: Treatment group 23–24					
NEETs rate on welfare	−0.0028***	−0.0027***	−0.0037***	−0.0039***	−0.0040***
	(0.0011)	(0.0010)	(0.0011)	(0.0015)	(0.0015)
NEETs rate not on welfare	0.0248***	0.0248***	0.0209***	0.0022	0.0022
	(0.0020)	(0.0019)	(0.0020)	(0.0026)	(0.0026)
NEETs rate	0.0220***	0.0220***	0.0172***	−0.0016	−0.0017
	(0.0023)	(0.0022)	(0.0023)	(0.0030)	(0.0030)
Employment rate	−0.0728***	−0.0730***	−0.0598***	−0.0145***	−0.0145***
	(0.0041)	(0.0040)	(0.0043)	(0.0047)	(0.0047)
Enrollment rate in education	0.0508***	0.0509***	0.0426***	0.0161***	0.0163***
	(0.0036)	(0.0036)	(0.0040)	(0.0049)	(0.0048)
Panel C: Treatment group 20–22					
NEETs rate on welfare	−0.0017*	−0.0024**	−0.0025**	−0.0043***	−0.0040***
	(0.0010)	(0.0010)	(0.0011)	(0.0014)	(0.0014)
NEETs rate not on welfare	0.0232***	0.0219***	0.0175***	−0.0061***	−0.0057***
	(0.0018)	(0.0017)	(0.0019)	(0.0021)	(0.0021)
NEETs rate	0.0215***	0.0195***	0.0151***	−0.0104***	−0.0097***
	(0.0022)	(0.0020)	(0.0023)	(0.0027)	(0.0027)
Employment rate	−0.0968***	−0.0946***	−0.0720***	0.0030	0.0027
	(0.0046)	(0.0045)	(0.0051)	(0.0054)	(0.0054)
Enrollment rate in education	0.0753***	0.0751***	0.0570***	0.0075	0.0070
	(0.0044)	(0.0044)	(0.0051)	(0.0055)	(0.0056)
Demographic controls	NO	YES	YES	YES	YES
Unemployment-age dummies	NO	NO	YES	YES	YES
Age-specific trends	NO	NO	NO	YES	YES
Control-specific trends	NO	NO	NO	NO	YES
Observations	1,725,256	1,725,256	1,725,256	1,725,256	1,725,256
Clusters	264	264	264	264	264

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: individuals 20–22, 23–24 and 25–26 years of age. Control group: individuals 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters), All specifications include age and year fixed effects. See Table A.2.1 in the Supplementary material for the full regression results.

Table 2.4: Differences-in-differences: pre-reform placebo's and annual treatment effects

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Panel A: Treatment group 25–26					
Placebo 2008	−0.0023	0.0046	0.0022	−0.0037	0.0014
	(0.0022)	(0.0038)	(0.0044)	(0.0053)	(0.0044)
Placebo 2009	−0.0022	0.0027	0.0005	−0.0032	0.0027
	(0.0023)	(0.0039)	(0.0048)	(0.0056)	(0.0048)
Treatment 2010	−0.0086***	0.0114***	0.0028	−0.0019	−0.0009
	(0.0021)	(0.0036)	(0.0045)	(0.0057)	(0.0047)
Treatment 2011	−0.0045*	0.0097**	0.0051	−0.0102	0.0050
	(0.0024)	(0.0038)	(0.0047)	(0.0062)	(0.0051)
Treatment 2012	−0.0052**	0.0039	−0.0013	−0.0038	0.0052
	(0.0022)	(0.0040)	(0.0047)	(0.0059)	(0.0054)
Panel B: Treatment group 23–24					
Placebo 2008	−0.0034**	0.0006	−0.0029	−0.0030	0.0058
	(0.0017)	(0.0031)	(0.0035)	(0.0051)	(0.0052)
Placebo 2009	−0.0026	0.0044	0.0018	−0.0109	0.0090
	(0.0020)	(0.0037)	(0.0043)	(0.0070)	(0.0073)
Treatment 2010	−0.0082***	0.0056	−0.0026	−0.0171**	0.0197**
	(0.0024)	(0.0035)	(0.0046)	(0.0076)	(0.0078)
Treatment 2011	−0.0041	0.0030	−0.0011	−0.0273***	0.0284***
	(0.0027)	(0.0041)	(0.0050)	(0.0068)	(0.0077)
Treatment 2012	−0.0059**	0.0038	−0.0021	−0.0154**	0.0175**
	(0.0026)	(0.0042)	(0.0049)	(0.0076)	(0.0076)
Panel C: Treatment group 20–22					
Placebo 2008	−0.0003	0.0015	0.0012	−0.0035	0.0023
	(0.0015)	(0.0026)	(0.0031)	(0.0050)	(0.0051)
Placebo 2009	−0.0004	−0.0019	−0.0023	−0.0077	0.0099
	(0.0017)	(0.0030)	(0.0037)	(0.0065)	(0.0063)
Treatment 2010	−0.0035*	−0.0032	−0.0067*	−0.0065	0.0132*
	(0.0019)	(0.0030)	(0.0037)	(0.0077)	(0.0076)
Treatment 2011	−0.0034	−0.0087***	−0.0121***	−0.0080	0.0201**
	(0.0022)	(0.0031)	(0.0040)	(0.0083)	(0.0082)
Treatment 2012	−0.0062***	−0.0067**	−0.0129***	0.0128	0.0002
	(0.0022)	(0.0034)	(0.0042)	(0.0091)	(0.0090)
Demographic controls	YES	YES	YES	YES	YES
Unemployment-age dummies	YES	YES	YES	YES	YES
Age-specific trends	YES	YES	YES	YES	YES
Control-specific trends	YES	YES	YES	YES	YES
Observations	1,725,256	1,725,256	1,725,256	1,725,256	1,725,256
Clusters	264	264	264	264	264

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: individuals 20–22, 23–24 and 25–26 years of age. Control group: individuals 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters). All specifications include demographic controls, unemployment-age interaction terms, age-specific trends and control-specific trends.

treatment dummies for the years 2008 and 2009. For each treatment group, we also split the single treatment dummy (for 2010–2012) into single-year treatment dummies for 2010, 2011 and 2012. With this specification, we can both test for common time effects as well as for anticipation effects and the evolution of the treatment effect of the WIJ reform over time. From the table, the general picture that emerges is that the placebo dummies are small and statistically insignificant. It is only for the NEETs rate on welfare in the treatment group of 23–24 years of age that we find a significant placebo dummy for 2008, but this effect would become insignificant if we would correct for multiple testing. Another finding is that treatment effects on NEETs rate on welfare for 2011 and 2012 are often smaller than for 2010, which is consistent with the pattern in Figure 2.2. Hence, most of the treatment effect seems to be confined to the first period of the reform. Also for the NEETs rate not on welfare, most of the effect appears to be in 2010, after which the effect becomes smaller again. Finally, it should be noted that there is still no statistically significant treatment effect for the total NEETs rate, the employment rate nor the enrollment rate in education when we consider single-year treatment dummies.

We also exploit the richness of our data by conducting additional placebo analyses that capture the economic downturn in 2002–2004 in the Netherlands – see Table 2.5 for the estimation results. The general idea here is to detect possible differences in responses to the business cycle between younger treatment groups and the control group of individuals aged 27–28 not accounted for by the interactions between the unemployment rate and individual ages. If such responses are different, this casts doubt on the common-time effects assumption underlying our DD approach. As the table shows, we do find placebo effects in the two youngest treatment groups. This particularly casts doubt on the large treatment effects on employment and education enrollment rates we find for these groups. As business cycle effects have been substantially different for the outcome measures between the group of 27–28 years of age and those below the age of 25, we cannot interpret the effect estimates as causal.

The Supplementary material presents some additional robustness checks. First, one may worry that the reform created spillovers for the control group via e.g. the job-finding rate (Blundell et al. 2004; Gautier

Table 2.5: Differences-in-differences: placebo treatment dummy economic downturn 2002-2004

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Panel A: Treatment group 25–26					
Treatment 2010–2012	−0.0045***	0.0060***	0.0015	−0.0023	0.0008
	(0.0014)	(0.0023)	(0.0027)	(0.0035)	(0.0032)
Placebo 2002–2004	0.0001	0.0003	0.0004	0.0018	−0.0023
	(0.0012)	(0.0021)	(0.0026)	(0.0030)	(0.0020)
Panel B: Treatment group 23–24					
Treatment 2010–2012	−0.0039***	0.0025	−0.0015	−0.0139***	0.0153***
	(0.0015)	(0.0026)	(0.0030)	(0.0047)	(0.0049)
Placebo 2002–2004	0.0004	0.0027	0.0032	0.0047	−0.0078**
	(0.0015)	(0.0023)	(0.0028)	(0.0038)	(0.0032)
Panel C: Treatment group 20–22					
Treatment 2010–2012	−0.0038***	−0.0058***	−0.0096***	0.0046	0.0050
	(0.0014)	(0.0021)	(0.0027)	(0.0055)	(0.0056)
Placebo 2002–2004	0.0021**	−0.0006	0.0015	0.0239***	−0.0255***
	(0.0010)	(0.0019)	(0.0022)	(0.0039)	(0.0035)
Demographic controls	YES	YES	YES	YES	YES
Unemployment-age dummies	YES	YES	YES	YES	YES
Age-specific trends	YES	YES	YES	YES	YES
Control-specific trends	YES	YES	YES	YES	YES
Observations	1,725,256	1,725,256	1,725,256	1,725,256	1,725,256
Clusters	264	264	264	264	264

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24, and 25–26 years of age. Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters). All specifications include demographic controls, unemployment rate-age interactions, age-specific trends and control-specific trends.

et al. 2018). In Table A.2.2 we address this concern by using individuals with 29–30 years of age as an alternative control group, and introduce 'treatment dummies' for our main control group of individuals 27–28 years of age. We then find rather similar treatment effects as in the base specification for the treatment groups 20-22, 23–24 and 25–26 years of age, and no statistically significant placebo treatment effects for our control group (27–28 years of age).[19] Second, Table A.2.3 addresses the concern that treatment effects may persist as individuals age into the control group, another type of spillover effect that may bias our estimates. Here we use individuals 30–31 years of age as the control group, as these were never in the treatment group during the WIJ reform period, and introduce 'treatment dummies' for individuals 27–29 years of age. Again, the results

[19]The proverbial exception is the employment rate, which is 'borderline' significant at the 10% level.

for the treatment groups 20–22, 23–24 and 25–26 years of age are (quite[20]) similar to the base specification, and the treatment effects for individuals 27–29 years of age are statistically insignificant. Third, Table A.2.4 shows that we obtain similar results when we narrow the treatment group down to individuals 26 years age and the control group to individuals 27 years of age. Finally, Table A.2.5 shows that the different levels of clustering (at the individual level, by month of birth or by year of birth, respectively) (virtually) does not affect the statistical significance of the results.

Table A.2.6 considers to what extent the changes in the stocks are driven by changes in the respective entry and exit rates.[21] When focussing on the older treatment group of individuals 25–26 years of age for which the baseline results turned out to be robust, we find that the effect on the NEETs rate on welfare runs entirely via an increased exit rate, with no effect on the entry rate (suggesting the 'threat effect' for new potential welfare recipients is limited). And vice versa, we find that the effect on the NEETs rate not on welfare is mainly due to an increase in the entry rate (although this coefficient is only statistically significant at the 10% level), with no effect on the exit rate. The exit and entry rates for the total NEETs rate, employment rate and the enrollment rate are statistically insignificant and typically small.

The Supplementary material section also presents the outcomes for selected other outcome variables and by subgroups. In light of our earlier results, we now focus on the treatment group of 25–26 years of age. Table A.2.7 shows that the effects of the WIJ on the enrollment rate in unemployment insurance (UI) and disability insurance (DI) are insignificant for this group. Next, Table A.2.8 gives the treatment effect on being in a particular household type. Distinguishing between adult children living at home, childless singles, single parents and couples, we do not find any

[20]Of course the control group becomes increasingly dissimilar to the main treatment groups, which results in some treatment effects (total NEETs rate and employment rate) for the age group 25–26 to become borderline significant at the 10% level, though with a counter-intuitive sign, also suggesting this is not a causal effect.

[21]Specifically, for entry the dependent variable equals 1 when, for each state, the current state is 1 and the previous state was a different state, and zero otherwise. For exit the dependent variable equals 1 when, for each state, the current state is a different state than the previous state, and the previous state is 1, and zero otherwise. We present results for our most elaborate specification, including demographic controls, unemployment-age interaction terms, age-specific trends and demographic-control-specific trends.

statistically significant treatment effects. Given that being in a particular household type seems largely exogenous to the treatment, Table A.2.9 then studies the treatment effects by household type. Focusing again on the treatment group of 25–26 year olds, the largest drop in the NEETs rate on welfare in absolute terms is for adult children living at home and single parents, –1.0 and –7.0 percentage points respectively. In percentages however, the drop for single parents is –22% (relative to the 2009 level), which is comparable to the average treatment effect over all household types. But for adult children living at home it is –45% (relative to the 2009 level), which can be explained by the additional reform in 2012, when adult children living at home were no longer eligible to welfare benefits when they lived in a household in which first-degree relatives had sufficient income or assets (see Section 2). The effect for childless singles is comparable to the average over all household types, whereas the effect for couples is close to zero. In line with the base results where we pool all household types, the NEETs rate not on welfare increases for all household types. The treatment effects for the other outcome variables are typically not statistically significant.

In addition to stratifying with respect to household types, Table A.2.10 gives the results by gender and ethnicity. The treatment effects for males and females are similar. The treatment effects for natives are somewhat smaller than the base results, whereas the results for immigrants are larger in absolute terms. But in percentage terms, the effects are much more comparable to the average, –29% for natives and –22% for immigrants for the NEETs rate on welfare (and a statistically significant effect on the total NEETs rate). Finally, Table A.2.11 considers the treatment effects for provinces that had a relatively low or a relatively high pre-reform unemployment rate. The treatment effect appears to be smaller (about half) in the provinces which had a lower pre-reform unemployment rate. However, the percentage drop is almost the same in regions with low and high pre-reform unemployment rates, 25% (relative to baseline 2009) for low unemployment regions and 24% for high unemployment regions.

Regression discontinuity 2.5.2

We next consider the more local treatment effect of the WIJ reform by considering outcomes around the cutoff age of 27, using regression discontinuity. To gauge the presence of such cutoff effects, Figure 2.3 shows the NEETs rate on welfare, the NEETs rate not on welfare and the total NEETs rate by month of birth of 25–28 year olds, relative to the discontinuity – both for the pre-reform period (2007–2009, left panels) and post-reform period (2010–2012, right panels).[22] In the figures, value averages are centered around the cutoff age of 27. The solid lines give the predictions from a RD regression without control variables, estimated separately on the left- and right-hand side of the discontinuity. The dashed lines give the corresponding 95% confidence intervals. These graphs suggest a small positive pre-reform discontinuity in the NEETs rate on welfare and a small negative post-reform discontinuity in the NEETs rate on welfare, and no pre-reform discontinuity for the NEETs rate not on welfare but a small positive post-reform discontinuity for the NEETs rate not on welfare. Finally, we observe a small and positive but similar pre- and post-reform discontinuity in the total NEETs rate.

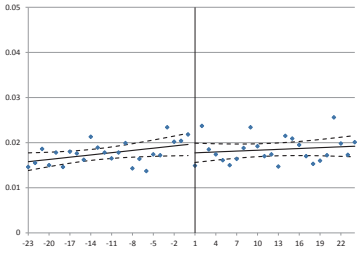
Table 2.6 gives the RD regression results. The RD dummy captures a different intercept on the left-hand side of the discontinuity, but we also allow for a different slope on the left-hand side of the discontinuity and include year fixed effects and demographic control variables. We present results for the pre- and post-reform period, in Panel A and B respectively.[23] We find a small positive but statistically insignificant pre-reform treatment effect for the NEETs rate on welfare, the NEETs rate not on welfare and the total NEETs rate. In addition, both the treatment effect on the employment rate and the education enrollment rate are negative and statistically insignificant. For the post-reform period we find a small

[22] Similar plots for the employment rate and the enrollment rate in education are given in Figure A.2.2 in the Supplementary material.

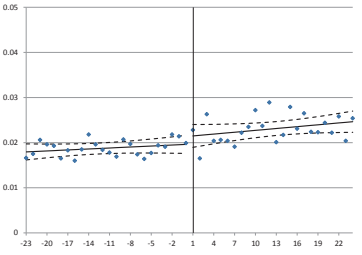
[23] Full regression results for the preferred RD specifications, for the pre- and post-reform period respectively, can be found in Table A.2.12 and Table A.2.13 in the Supplementary material. Furthermore, results for different RD specifications, for the pre- and post-reform period respectively, can be found in Table A.2.14 and Table A.2.15 in the Supplementary material. Figure A.2.3 shows that there is no manipulation in the running variable (age of the child in months), and Figure A.2.4 and A.2.5 show that there are also no discontinuities in the demographic control variables, either pre- or post-reform.

Figure 2.3: Pre-reform (2007–2009) and post-reform (2010–2012) outcome variables relative to the age threshold

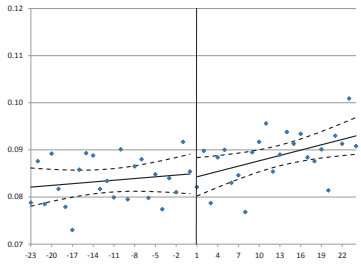
(a) NEETs rate on welfare: pre-reform



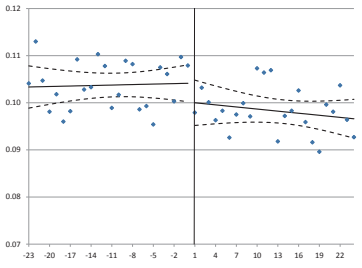
(b) NEETs rate on welfare: post-reform



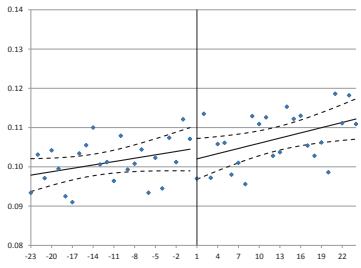
(c) NEETs rate not on welfare: pre-reform



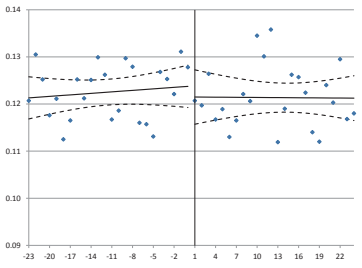
(d) NEETs rate not on welfare: post-reform



(e) Total NEETs rate: pre-reform



(f) Total NEETs rate: post-reform



Notes: Own calculations using the Labour Market Panel (Statistics Netherlands). Age is recentered around the discontinuity (outcomes are measured in October, 1 is a person who has turned 27 in September). The solid lines give the predictions from a RD regression without control variables, estimated separately on the left- and right-hand side of the discontinuity. The dashed lines give the corresponding 95% confidence interval. NEETs rates are individuals not in employment or education relative to the relevant age population.

Table 2.6: Regression discontinuity: base regression results

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Panel A: RD for the period 2007–2009					
RD dummy	0.0020	0.0011	0.0031	−0.0044	0.0013
(placebo)	(0.0014)	(0.0023)	(0.0030)	(0.0034)	(0.0022)
Observations	157,543	157,543	157,543	157,543	157,543
Clusters	72	72	72	72	72
Panel B: RD for the period 2010–2012					
RD dummy	−0.0014	0.0044**	0.0030	−0.0022	−0.0008
	(0.0013)	(0.0022)	(0.0023)	(0.0030)	(0.0024)
Observations	158,195	158,195	158,195	158,195	158,195
Clusters	72	72	72	72	72
Panel C: DRD for the period 2007–2012					
DRD dummy	−0.0033*	0.0032	−0.0001	0.0022	−0.0021
	(0.0020)	(0.0031)	(0.0038)	(0.0046)	(0.0030)
Observations	315,738	315,738	315,738	315,738	315,738
Clusters	108	108	108	108	108

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Cluster-robust standard errors in parentheses, clustered by month of birth (72 cluster for the RD estimates, 108 clusters for the DRD estimates). The RD parameter estimates are for the RD dummy capturing a different intercept on the left-hand side of the discontinuity, and also allow for a different slope on the left-hand side of the discontinuity, include year fixed effects and include demographic control variables. Full regression results for the RD specifications for the period 2007–2009 and 2010–2012 can be found in Table A.2.12 and A.2.13 in the Supplementary material, respectively. The DRD parameter estimates are for the DRD dummy capturing the difference in the different intercept on the left-hand side of the discontinuity from the period 2007–2009 to the period 2010–2012, and also allow for a different slope on the left-hand side of the discontinuity, a change in the different slope on the left-hand side of the discontinuity, include year fixed effects and include demographic control variables. Full regression results for the DRD specification can be found in Table A.2.16 in the Supplementary material.

but now negative treatment effect for the NEETs rate on welfare, though not statistically significant, a bigger positive and statistically significant treatment effect for the NEETs rate not on welfare (at the 5% level), and a small positive treatment effect for the total NEETs rate that is similar to the effect in the pre-reform period. Furthermore, the post-reform treatment effect is somewhat larger for the employment rate and somewhat smaller for the enrollment rate in education.

Panel C of Table 2.6 then gives the coefficient on the 'difference-in-discontinuity' dummy, which is very close to the difference in the discon-

tinuity between the pre- and post-reform period.[24] The results are similar to the DD analysis. That is, there is a negative treatment effect on the NEETs rate on welfare, statistically significant at the 10% level, a positive treatment effect on the NEETs rate not on welfare and essentially no effect on the total NEETs rate (and the treatment effects for the employment rate and enrollment rate in education are insignificant).

The Supplementary material gives some additional analyses for the RD analysis as well. RD plots by year are given in Figure A.2.6, A.2.7 and A.2.8. Consistent with the DD analysis, these graphs show that most of the effect on the NEETs rate on welfare and the NEETs rate not on welfare was confined to the year 2010, whereas there is no apparent effect on the total NEETs rate in any year. Table A.2.17, A.2.18 and A.2.19 show that we obtain qualitatively similar results when we use quarter of birth instead of month of birth, or use a smaller or a larger bandwidth in age, respectively. To control for potential anticipation and adaptation effects close to and after reaching the age of 27, Table A.2.20 gives results of a so-called donut RD (and DRD) analysis where we drop observations of individuals three months on either side of the cutoff.[25] These results are very similar to the base RD and DRD specifications (and even closer to the DD results than the base RD and DRD analysis). Finally, Table A.2.21 gives the difference-in-discontinuity results for entry and exit probabilities. The difference-in-discontinuity analysis also suggests a positive effect on the exit probability from welfare, in line with the DD analysis, significant at the 10 percent level. At the same time, however, it also suggests a negative effect on the entry probability into welfare, significant at the 10 percent level. Hence, the DRD analysis suggests there may have been some 'threat effect' of the WIJ reform.

[24] Full regression results for the difference-in-discontinuity specification can be found in Table A.2.16 in the Supplementary material.

[25] For an analysis of the implementation of donut RD designs, see e.g. Barreca et al. (2011) or Barreca et al. (2016).

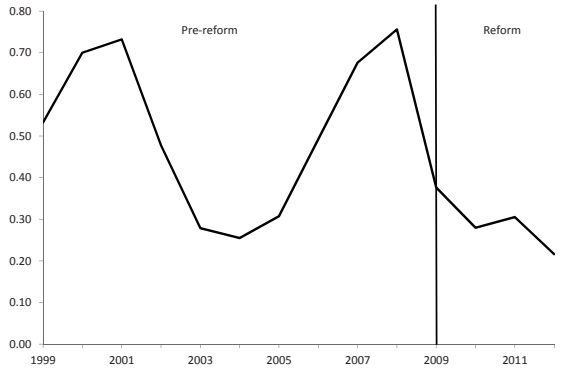
Discussion and conclusion 2.6

In this paper we have studied the labour market effects of a Dutch mandatory activation program for individuals up to 26 years of age in The Netherlands. We used differences-in-differences and regression discontinuity, and a long and rich administrative dataset to uncover the effect of the WIJ reform on the NEETs rate on welfare, the NEETs rate not on welfare, the total NEETs rate, the employment rate and the enrollment rate in education. We considered the separate treatment effects on individuals 20–22, 23–24 and 25–26 years of age, using individuals 27–28 years of age as the main control group. An extensive number of placebo tests suggests that we can interpret the effects on the group 25–26 years of age as causal, whereas the assumption of common-time effects seems questionable for the the younger treatment groups. Focusing on the results for the group 25–26 years of age, we find that the reform reduced the number of NEETS on welfare with a substantial 24%, with most of the effect in the first year of the reform. However, the reform did not reduce the overall NEETs rate, neither did it increase the employment rate nor did in increase the enrollment rate in education. The reform mainly pushed individuals out of welfare, where most of the effect appears to have come from an increase in the exit rate from welfare rather than a decrease in the entry rate into welfare.

Part of our findings are in line with previous studies on mandatory activation programs targeted at young individuals. Consistent with Blundell et al. (2004), Dahlberg et al. (2009), Persson and Vikman (2014) and Hernæs et al. (2016), we find a substantial negative effect on the number of young individuals on welfare. In line with Blundell et al. (2004), we find a substantial positive effect of the reform on the exit rate out of welfare.[26] Consistent with Blundell et al. (2004) and Dahlberg et al. (2009) we also find that most of the effect was in the beginning of the reform period, and then the effect diminishes in subsequent periods. As a potential explanation Blundell et al. (2004) consider 'cleaning up the registers', which

[26]For a reform in Sweden, Persson and Vikman (2014) find no significant effect on the exit rate from welfare, but a negative and statistically significant effect on the entry rate into welfare. We find that the effect on entry is insignificant in our DD setup, but is also negative and statistically significant in our DRD setup.

Figure 2.4: Vacancy-to-unemployment ratio: 1999–2012



Source: Statistics Netherlands (Statline).

have been noted of previous UK labour market reforms (Blundell et al. 2004, p. 594). A similar mechanism could be at work in the Dutch case. Also consistent with Blundell et al. (2004), we find no evidence of spillover effects to other groups. In particular, we find no effects on the group of individuals that is one or two years older than the treatment group.

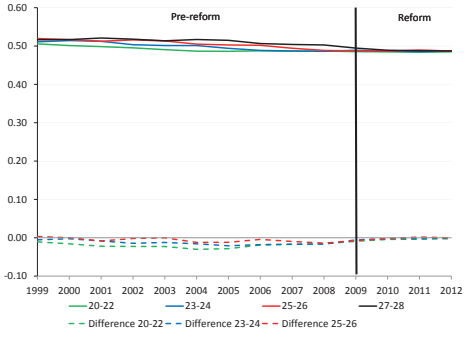
That being said, part of our findings are also at odds with previous studies. In particular, while mandatory programs for young individuals are usually associated with increased employment (Blundell et al. 2004; Dahlberg et al. 2009; Persson and Vikman 2014) or education enrollment (Hernæs et al. 2016), we find no evidence in this direction. One potential explanation for this difference in findings is that we consider a country where the NEETs rate is relatively low, see Table 2.1. The findings of Hernæs et al. (2016) for Norway, a country with comparable level of NEETs rates, however point at substantial program effects on employment and education enrollment. Furthermore, also note that we find rather similar treatment effects for regions with relatively low and relatively high pre-reform unemployment rates. We argue that a more plausible explanation for the absence of program effects on employment is that the reform clashed head on with the Great Recession that started just prior to the start of the WIJ reform. The Great Recession made it inherently

more difficult for individuals, especially young individuals, to find work. Indeed, Figure 2.4 shows the steep drop in the vacancy-to-unemployment ratio during the reform period. This was quite different for the reforms considered in previous studies. For the UK, (Blundell et al. 2004) note that the New Deal was introduced at a favorable point of the business cycle by historical standards, while stressing that "[C]learly, the program in this favorable climate may not apply to less favorable periods."Likewise, the reforms in Sweden and Norway studied by Dahlberg et al. (2009), Persson and Vikman (2014) and Hernæs et al. (2016) were implemented in relatively favorable periods (the end of the 1990s). Our results thus suggest that mandatory activation programs and work-learn arrangements are a much less effective policy tool during a recession.

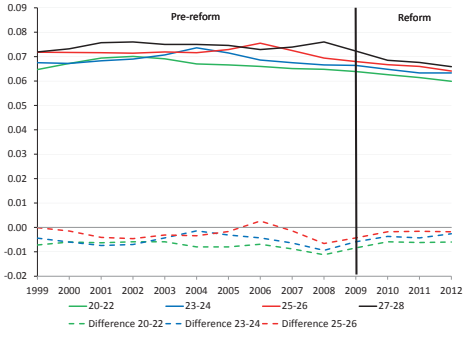
2.A Supplementary material

Figure A.2.1: Means of the control variables treatment and control groups: 1999–2012

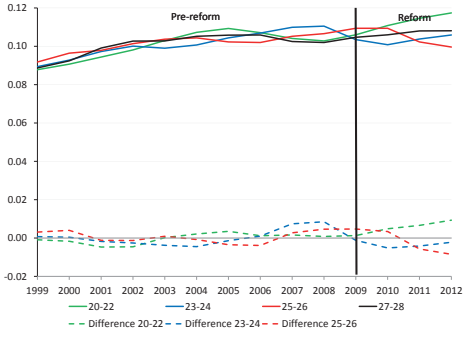
(a) Share female



(b) Share Western immigrant



(c) Share non-Western immigrant



Notes: Own calculations using the Labour Market Panel (Statistics Netherlands). The solid black line denotes the control group (27–28 years of age), the green lines denote treatment group 20–22, the blue lines denote treatment group 23–24 and the red lines denote the treatment group 25–26 years of age. The dotted lines denote the difference between the treatment group and the control group.

Table A.2.1: Differences-in-differences: full results base regressions

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treatment group 25–26	−0.0046***	0.0060***	0.0014	−0.0027	0.0013
	(0.0014)	(0.0023)	(0.0028)	(0.0036)	(0.0032)
Treatment group 23–24	−0.0040***	0.0022	−0.0017	−0.0145***	0.0163***
	(0.0015)	(0.0026)	(0.0030)	(0.0047)	(0.0048)
Treatment group 20–22	−0.0040***	−0.0057***	−0.0097***	0.0027	0.0070
	(0.0014)	(0.0021)	(0.0027)	(0.0054)	(0.0056)
Group age 20	−0.0113***	−0.0523***	−0.0636***	−0.3659***	0.4296***
	(0.0021)	(0.0044)	(0.0049)	(0.0108)	(0.0109)
Group age 21	−0.0088***	−0.0532***	−0.0620***	−0.2886***	0.3506***
	(0.0023)	(0.0044)	(0.0050)	(0.0093)	(0.0092)
Group age 22	−0.0114***	−0.0436***	−0.0550***	−0.2069***	0.2619***
	(0.0024)	(0.0048)	(0.0054)	(0.0087)	(0.0085)
Group age 23	−0.0088***	−0.0369***	−0.0457***	−0.1333***	0.1790***
	(0.0026)	(0.0050)	(0.0053)	(0.0086)	(0.0083)
Group age 24	−0.0065**	−0.0325***	−0.0390***	−0.0674***	0.1065***
	(0.0029)	(0.0049)	(0.0056)	(0.0083)	(0.0075)
Group age 25	−0.0037	−0.0231***	−0.0269***	−0.0151**	0.0419***
	(0.0030)	(0.0052)	(0.0056)	(0.0071)	(0.0054)
Group age 26	−0.0015	−0.0193***	−0.0209***	0.0036	0.0173***
	(0.0026)	(0.0052)	(0.0057)	(0.0065)	(0.0042)
Group age 27	−0.0001	−0.0095***	−0.0096**	0.0033	0.0063**
	(0.0018)	(0.0036)	(0.0040)	(0.0045)	(0.0030)
Female	0.0203***	0.0675***	0.0878***	−0.0728***	−0.0151***
	(0.0008)	(0.0027)	(0.0029)	(0.0035)	(0.0026)
Non-Western immigrant	0.1046***	0.1271***	0.2317***	−0.2087***	−0.0229***
	(0.0031)	(0.0034)	(0.0051)	(0.0062)	(0.0041)
Western immigrant	0.0241***	0.0411***	0.0652***	−0.0828***	0.0176***
	(0.0019)	(0.0031)	(0.0038)	(0.0045)	(0.0033)
Unemployment rate	11.241***	−1.5904**	−0.4663	−2.2783**	27.446***
	(0.3483)	(0.7155)	(0.7731)	(1.1161)	(0.9373)
Unemployment rate*	−0.1330***	−0.2315**	−0.3645***	−0.3974*	0.7619***
1(age=20)	(0.0440)	(0.0928)	(0.0972)	(0.2334)	(0.2389)
Unemployment rate*	−0.0526	−0.1317	−0.1844*	−0.0603	0.2447
1(age=21)	(0.0489)	(0.0916)	(0.0978)	(0.2062)	(0.2042)
Unemployment rate*	0.0572	−0.2257**	−0.1685	0.0558	0.1126
1(age=22)	(0.0484)	(0.0966)	(0.1032)	(0.1937)	(0.1920)
Unemployment rate*	0.0697	−0.1697*	−0.1000	0.1004	−0.0004
1(age=23)	(0.0529)	(0.1028)	(0.1070)	(0.1810)	(0.1802)
Unemployment rate*	0.0614	−0.0737	−0.0124	−0.0911	0.1034
1(age=24)	(0.0635)	(0.1071)	(0.1174)	(0.1833)	(0.1685)

Table A.1: *Continued*

Unemployment rate*	0.0170	−0.0496	−0.0326	−0.2982**	0.3308***
1(age=25)	(0.0645)	(0.1093)	(0.1166)	(0.1501)	(0.1194)
Unemployment rate*	−0.0177	0.0302	0.0125	−0.2024	0.1899**
1(age=26)	(0.0558)	(0.1110)	(0.1189)	(0.1397)	(0.0888)
Unemployment rate*	−0.0143	−0.0180	−0.0323	−0.0081	0.0405
1(age=27)	(0.0408)	(0.0756)	(0.0845)	(0.0998)	(0.0634)
Trend age 20	0.0035**	−0.0034	0.0001	−0.0121***	0.0121***
	(0.0014)	(0.0029)	(0.0032)	(0.0045)	(0.0038)
Trend age 21	0.0032**	−0.0034	−0.0002	−0.0108**	0.0110***
	(0.0014)	(0.0029)	(0.0032)	(0.0045)	(0.0038)
Trend age 22	0.0032**	−0.0030	0.0002	−0.0093**	0.0091**
	(0.0014)	(0.0029)	(0.0032)	(0.0044)	(0.0037)
Trend age 23	0.0030**	−0.0038	−0.0008	−0.0069	0.0076**
	(0.0014)	(0.0029)	(0.0032)	(0.0045)	(0.0037)
Trend age 24	0.0030**	−0.0046	−0.0015	−0.0039	0.0054
	(0.0014)	(0.0029)	(0.0031)	(0.0045)	(0.0038)
Trend age 25	0.0032**	−0.0057**	−0.0026	−0.0017	0.0042
	(0.0014)	(0.0029)	(0.0032)	(0.0045)	(0.0038)
Trend age 26	0.0032**	−0.0065**	−0.0032	0.0004	0.0028
	(0.0014)	(0.0029)	(0.0032)	(0.0045)	(0.0038)
Trend age 27	0.0030**	−0.0069**	−0.0039	0.0018	0.0021
	(0.0014)	(0.0030)	(0.0033)	(0.0045)	(0.0037)
Trend age 28	0.0030**	−0.0080***	−0.0050	0.0035	0.0015
	(0.0014)	(0.0029)	(0.0032)	(0.0045)	(0.0037)
Trend female	−0.0012***	−0.0049***	−0.0061***	0.0041***	0.0020***
	(0.0001)	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Trend non-Western	−0.0059***	−0.0043***	−0.0102***	0.0023***	0.0079***
immigrant	(0.0003)	(0.0004)	(0.0006)	(0.0007)	(0.0005)
Trend Western	−0.0011***	−0.0001	−0.0013***	−0.0012**	0.0025***
immigrant	(0.0002)	(0.0004)	(0.0004)	(0.0006)	(0.0005)
Observations	1,725,256	1,725,256	1,725,256	1,725,256	1,725,256
Clusters	264	264	264	264	264

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24 and 25–26 years of age.Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters). All specifications include demographic controls, unemployment rate-age interactions, age-specific trends and control-specific trends.

Table A.2.2: Differences-in-differences: 27–28 as placebo treatment group and 29–30 as control group

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treatment group 27–28	−0.0003	0.0038	0.0035	−0.0052*	0.0017
(placebo)	(0.0014)	(0.0025)	(0.0031)	(0.0031)	(0.0017)
Treatment group 25–26	−0.0048***	0.0097***	0.0048	−0.0076*	0.0028
	(0.0015)	(0.0030)	(0.0033)	(0.0040)	(0.0032)
Treatment group 23–24	−0.0042***	0.0059**	0.0017	−0.0194***	0.0177***
	(0.0014)	(0.0028)	(0.0030)	(0.0046)	(0.0046)
Treatment group 20–22	−0.0043***	−0.0021	−0.0064**	−0.0021	0.0085
	(0.0012)	(0.0024)	(0.0026)	(0.0054)	(0.0053)
Observations	2,143,282	2,143,282	2,143,282	2,143,282	2,143,282
Clusters	288	288	288	288	288

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24, and 25–26 years of age, placebo treatment group: 27–28 years of age. Control group: 29–30 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (288 clusters).

Table A.2.3: Differences-in-differences: 27–29 as placebo treatment group and 30–31 as control group

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treatment group 27–29	0.0010	0.0027	0.0038	−0.0029	−0.0009
(placebo)	(0.0015)	(0.0024)	(0.0027)	(0.0028)	(0.0013)
Treatment group 25–26	−0.0037**	0.0093***	0.0057*	−0.0067*	0.0010
	(0.0016)	(0.0030)	(0.0032)	(0.0040)	(0.0031)
Treatment group 23–24	−0.0030**	0.0056*	0.0025	−0.0185***	0.0160***
	(0.0014)	(0.0030)	(0.0030)	(0.0047)	(0.0045)
Treatment group 20–22	−0.0032***	−0.0025	−0.0056**	−0.0012	0.0068
	(0.0012)	(0.0026)	(0.0026)	(0.0054)	(0.0053)
Observations	2,362,916	2,362,916	2,362,916	2,362,916	2,362,916
Clusters	300	300	300	300	300

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24, and 25–26 years of age and placebo treatment group: 27–29 years of age. Control group: 30–31 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (300 clusters).

Table A.2.4: Differences-in-differences: 26 as treatment group and 27 as control group

	(1)	(2)	(3)	(4)	(5)
NEETs rate on welfare	−0.0031***	−0.0028***	−0.0031***	−0.0043***	−0.0044***
	(0.0010)	(0.0010)	(0.0011)	(0.0016)	(0.0016)
NEETs rate not on welfare	0.0078***	0.0080***	0.0063***	0.0019	0.0019
	(0.0016)	(0.0016)	(0.0019)	(0.0025)	(0.0026)
NEETs rate	0.0047**	0.0052***	0.0031	−0.0024	−0.0025
	(0.0020)	(0.0019)	(0.0022)	(0.0033)	(0.0033)
Employment rate	−0.0110***	−0.0118***	−0.006**	0.0041	0.0041
	(0.0025)	(0.0024)	(0.0026)	(0.0036)	(0.0036)
Enrollment rate in education	0.0063***	0.0065***	0.0033	−0.0017	−0.0016
	(0.0021)	(0.0020)	(0.0021)	(0.0029)	(0.0029)
Demographic controls	NO	YES	YES	YES	YES
Unemployment-age dummies	NO	NO	YES	YES	YES
Age-specific trends	NO	NO	NO	YES	YES
Control-specific trends	NO	NO	NO	NO	YES
Observations	381,495	381,495	381,495	381,495	381,495
Clusters	180	180	180	180	180

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment group: 26 years of age. Control group: 27 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth 180 clusters).

Table A.2.5: Differences-in-differences: estimated standard errors for different levels of clustering

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treatment group 25–26	−0.0046	0.0060	0.0014	−0.0027	0.0013
Cluster(Individual)	(0.0012)***	(0.0025)**	(0.0027)	(0.0032)	(0.0020)
Cluster(Month of birth)	(0.0014)***	(0.0023)***	(0.0028)	(0.0036)	(0.0032)
Cluster(Year of birth)	(0.0014)***	(0.0013)***	(0.0022)	(0.0035)	(0.0043)
Treatment group 23–24	−0.0040)***	0.0022)	−0.0017)	−0.0145)***	0.0163)***
Cluster(Individual)	(0.0013)***	(0.0027)	(0.0030)	(0.0039)***	(0.0030)***
Cluster(Month of birth)	(0.0015)***	(0.0026)	(0.0030)	(0.0047)***	(0.0048)***
Cluster(Year of birth)	(0.0014)***	(0.0016)	(0.0025)	(0.0059)**	(0.0059)**
Treatment group 20–22	−0.0040	−0.0057	−0.0097	0.0027	0.0070
Cluster(Individual)	(0.0012)**	(0.0024)**	(0.0026)***	(0.0035)	(0.0029)**
Cluster(Month of birth)	(0.0014)***	(0.0021)***	(0.0027)***	(0.0054)	(0.0056)
Cluster(Year of birth)	(0.0014)***	(0.0013)***	(0.0023)***	(0.0056)	(0.0054)
Observations	1,725,256	1,725,256	1,725,256	1,725,256	1,725,256

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24 and 25–26 years of age. Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by individuals (321,474 clusters), month of birth (264 clusters) and year of birth (23 clusters).

Table A.2.6: Differences-in-differences: entry and exit

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Panel A: Treatment group 25–26					
Entry	0.0000	0.0028*	0.0007	0.0034	−0.0008
	(0.0008)	(0.0015)	(0.0016)	(0.0026)	(0.0008)
Exit	0.0022***	−0.0003	−0.0002	−0.0025	0.0060**
	(0.0008)	(0.0015)	(0.0016)	(0.0017)	(0.0024)
Panel B: Treatment group 23–24					
Entry	−0.0004	0.0023	0.0004	0.0005	−0.0003
	(0.0007)	(0.0017)	(0.0016)	(0.0024)	(0.0009)
Exit	0.0022***	−0.0033**	−0.0026*	−0.0027*	0.0058**
	(0.0007)	(0.0015)	(0.0016)	(0.0016)	(0.0024)
Panel C: Treatment group 20–22					
Entry	−0.0011*	−0.0031**	−0.0042***	−0.0127***	−0.0003
	(0.0006)	(0.0014)	(0.0014)	(0.0021)	(0.0008)
Exit	0.0020***	−0.0045***	−0.0025*	−0.0065***	−0.0082***
	(0.0006)	(0.0013)	(0.0014)	(0.0014)	(0.0022)
Observations	1,588,817	1,588,817	1,588,817	1,588,817	1,588,817
Clusters	252	252	252	252	252

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24 and 25–26 years of age. Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (252 clusters). Entry and exit are measured relative to the relevant age population. In this way entry and exit effects are directly comparable to the effects on the stocks, and to each other.

Table A.2.7: Differences-in-differences: treatment effect enrollment rate in other types of social insurance

	(1) Unemployment insurance	(2) Disability insurance
Treatment group 25–26	−0.0012	0.0009
	(0.0010)	(0.0009)
Treatment group 23–24	−0.0019**	−0.0010
	(0.0009)	(0.0009)
Treatment group 20–22	−0.0033***	−0.0024***
	(0.0007)	(0.0009)
Observations	1,725,256	1,725,256
Clusters	264	264

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24 and 25–26 years of age. Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters).

Table A.2.8: Differences-in-differences: treatment effect on the probability of being a particular household type

	(1) Adult children living with parent	(2) Singles	(3) Single parents	(4) Couples
Treatment group 25–26	−0.0002	−0.0016	−0.0020	0.0047
	(0.0051)	(0.0034)	(0.0013)	(0.0059)
Treatment group 23–24	−0.0019	0.0029	−0.0020	0.0036
	(0.0077)	(0.0039)	(0.0014)	(0.0074)
Treatment group 20–22	−0.0039	−0.0016	−0.0016	0.0099
	(0.0072)	(0.0038)	(0.0012)	(0.0061)
Observations	1,725,256	1,725,256	1,725,256	1,725,256
Clusters	264	264	264	264

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24 and 25–26 years of age. Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters).

Table A.2.9: Differences-in-differences: treatment effects by household types

	Adult children living with parent	Singles	Single parents	Couples
Panel A: Treatment group 25–26				
NEETs rate on welfare	−0.0099***	−0.0058*	−0.0697**	0.0000
	(0.0029)	(0.0031)	(0.0298)	(0.0010)
NEETs rate not on welfare	0.0006	0.0062	0.0745***	0.0052*
	(0.0070)	(0.0041)	(0.0229)	(0.0031)
NEETs rate	−0.0094	0.0003	0.0048	0.0052
	(0.0071)	(0.0055)	(0.0289)	(0.0032)
Employment rate	0.0049	0.0063	−0.0025	−0.0082**
	(0.0083)	(0.0082)	(0.0300)	(0.0036)
Enrollment rate in education	0.0045	−0.0067	−0.0023	0.0029
	(0.0061)	(0.0068)	(0.0164)	(0.0024)
Panel B: Treatment group 23–24				
NEETs rate on welfare	−0.0096***	−0.0056*	−0.0731**	0.0019
	(0.0026)	(0.0032)	(0.0370)	(0.0015)
NEETs rate not on welfare	−0.0036	0.0022	0.0893***	0.0021
	(0.0062)	(0.0046)	(0.0249)	(0.0039)
NEETs rate	−0.0132**	−0.0034	0.0162	0.0041
	(0.0064)	(0.0057)	(0.0343)	(0.0040)
Employment rate	−0.0131	−0.0035	−0.0052	−0.0162***
	(0.0086)	(0.0100)	(0.0349)	(0.0055)
Enrollment rate in education	0.0262***	0.0069	−0.0110	0.0122**
	(0.0069)	(0.0091)	(0.0237)	(0.0051)
Panel C: Treatment group 20–22				
NEETs rate on welfare	−0.0091***	−0.0090***	−0.0161	0.0020
	(0.0025)	(0.0029)	(0.0387)	(0.0015)
NEETs rate not on welfare	−0.0129**	−0.0052	0.0391	0.0098**
	(0.0056)	(0.0041)	(0.0267)	(0.0041)
NEETs rate	−0.0220***	−0.0142***	0.0230	0.0118***
	(0.0058)	(0.0055)	(0.0401)	(0.0044)
Employment rate	0.0010	0.0269***	0.0433	0.0004
	(0.0088)	(0.0079)	(0.0330)	(0.0081)
Enrollment rate in education	0.0210***	−0.0126	−0.0663**	−0.0122
	(0.0077)	(0.0077)	(0.0281)	(0.0077)
Observations	605,177	361,488	23,566	714,313
Clusters	264	264	264	264

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24 and 25–26 years of age. Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters). All specifications include demographic controls, unemployment rate-age interactions, age-specific trends and control-specific trends.

Table A.2.10: Differences-in-differences: treatment effect by gender and ethnicity

	Females	Males	Natives	Immigrants
Panel A: Treatment group 25–26				
NEETs rate on welfare	−0.0049**	−0.0044***	−0.0032***	−0.0120**
	(0.0021)	(0.0015)	(0.0011)	(0.0052)
NEETs rate not on welfare	0.0057	0.0058**	0.0019	0.0244***
	(0.0036)	(0.0029)	(0.0025)	(0.0064)
NEETs rate	0.0009	0.0013	−0.0014	0.0124
	(0.0041)	(0.0032)	(0.0027)	(0.0079)
Employment rate	−0.0030	−0.0023	−0.0002	−0.0117
	(0.0047)	(0.0045)	(0.0036)	(0.0090)
Enrollment rate in education	0.0021	0.0010	0.0016	−0.0007
	(0.0037)	(0.0036)	(0.0031)	(0.0066)
Panel B: Treatment group 23–24				
NEETs rate on welfare	−0.0050**	−0.0035**	−0.0040***	−0.0062
	(0.0024)	(0.0014)	(0.0012)	(0.0056)
NEETs rate not on welfare	0.0028	0.0015	−0.0017	0.0179**
	(0.0042)	(0.0031)	(0.0027)	(0.0076)
NEETs rate	−0.0022	−0.0020	−0.0057*	0.0117
	(0.0049)	(0.0033)	(0.0030)	(0.0094)
Employment rate	−0.0071	−0.0220***	−0.0133***	−0.0141
	(0.0061)	(0.0060)	(0.0050)	(0.0118)
Enrollment rate in education	0.0093	0.0240***	0.0190***	0.0024
	(0.0060)	(0.0054)	(0.0049)	(0.0094)
Panel C: Treatment group 20–22				
NEETs rate on welfare	−0.0047**	−0.0039***	−0.0036***	−0.0084*
	(0.0022)	(0.0014)	(0.0011)	(0.0050)
NEETs rate not on welfare	−0.0088**	−0.0028	−0.0085***	0.0044
	(0.0036)	(0.0029)	(0.0023)	(0.0071)
NEETs rate	−0.0135***	−0.0067**	−0.0121***	−0.0040
	(0.0044)	(0.0032)	(0.0028)	(0.0090)
Employment rate	0.0179***	−0.0117*	−0.0019	0.0324***
	(0.0064)	(0.0065)	(0.0055)	(0.0107)
Enrollment rate in education	−0.0044	0.0182***	0.0139**	−0.0285***
	(0.0065)	(0.0061)	(0.0055)	(0.0095)
Observations	858,695	866,561	1,429,549	295,707
Clusters	264	264	264	264

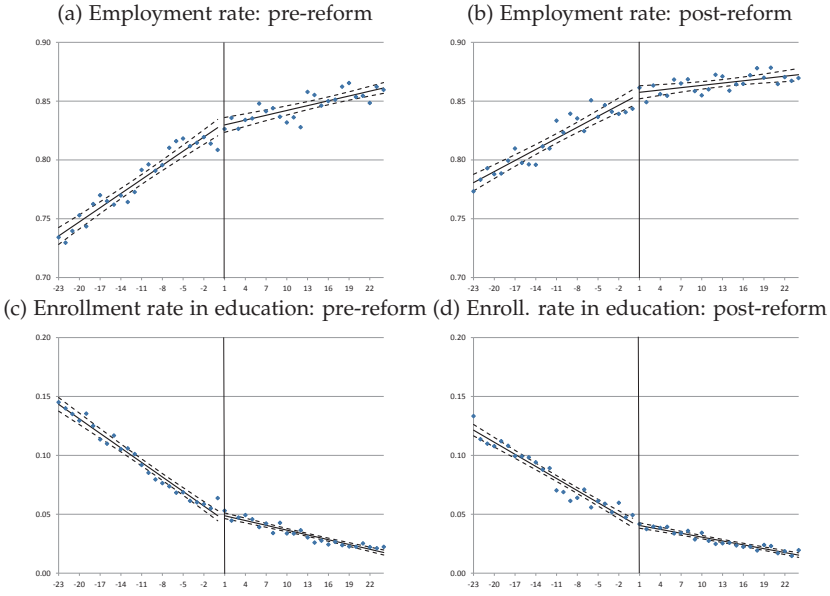
Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24 and 25–26 years of age.Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters). All specifications include demographic controls, unemployment rate-age interactions, age-specific trends and control-specific trends.

Table A.2.11: Differences-in-differences: treatment effect by pre-reform regional unemployment rate

	(1)	(2)
Pre-reform regional unemployment	Low	High
Panel A: Treatment group 25–26		
NEETs rate on welfare	−0.0031**	−0.0059***
	(0.0055)	(0.0058)
NEETs rate not on welfare	0.0041	0.0081**
	(0.0030)	(0.0035)
NEETs rate	0.0010	0.0022
	(0.0035)	(0.0038)
Employment rate	−0.0034	−0.0024
	(0.0045)	(0.0046)
Enrollment rate in education	0.0024	0.0002
	(0.0038)	(0.0037)
Panel B: Treatment group 23–24		
NEETs rate on welfare	−0.0034**	−0.0044**
	(0.0017)	(0.0020)
NEETs rate not on welfare	0.0016	0.0032
	(0.0035)	(0.0037)
NEETs rate	−0.0018	−0.0011
	(0.0040)	(0.0043)
Employment rate	−0.0202***	−0.0095
	(0.0059)	(0.0063)
Enrollment rate in education	0.0220***	0.0106*
	(0.0055)	(0.0058)
Panel C: Treatment group 20–22		
NEETs rate on welfare	−0.0033**	−0.0048***
	(0.0015)	(0.0018)
NEETs rate not on welfare	−0.0081***	−0.0035
	(0.0029)	(0.0031)
NEETs rate	−0.0114***	−0.0083**
	(0.0035)	(0.0038)
Employment rate	0.0023	0.0035
	(0.0064)	(0.0064)
Enrollment rate in education	0.0091	0.0048
	(0.0062)	(0.0065)
Observations	859,405	865,851
Clusters	264	264

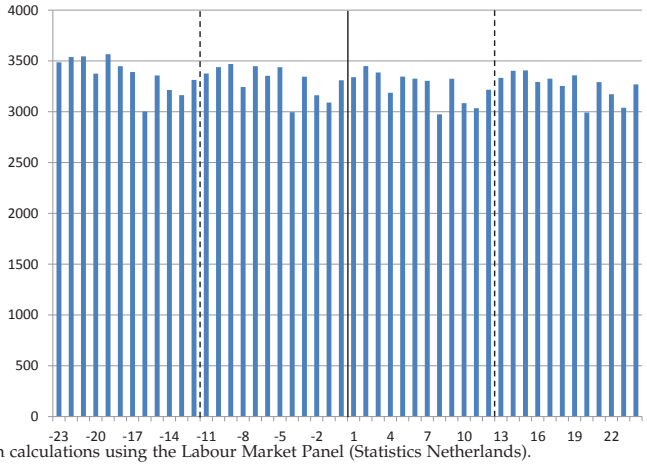
Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 1999–2012. Treatment groups: 20–22, 23–24 and 25–26 years of age. Control group: 27–28 years of age. Cluster-robust standard errors in parentheses, clustered by month of birth (264 clusters). 5 regions with on average the lowest unemployment in 1999-2009: Utrecht, Noord-Brabant, Zeeland, Gelderland, Noord-Holland, 7 regions with on average the highest unemployment in 1999-2009:Zuid-Holland, Overijssel, Limburg, Flevoland, Friesland, Drenthe, Groningen.

Figure A.2.2: Regression discontinuity: pre-reform (2007–2009) and post-reform (2010–2012) other outcome variables



Notes: Own calculations using the Labour Market Panel (Statistics Netherlands). Employment rates are the employed relative to the population and enrollment rates are individuals in education relative to the population.

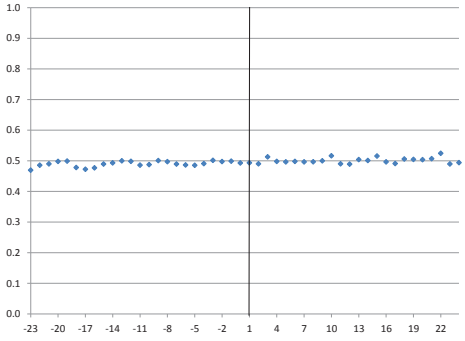
Figure A.2.3: Regression discontinuity: observations by month of birth (2010–2012)



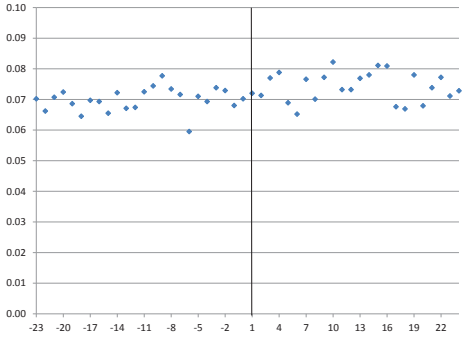
Notes: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.2.4: Regression discontinuity: control variables relative to discontinuity (2007–2009)

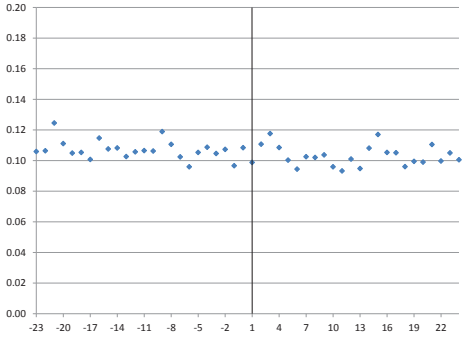
(a) RD: pre-reform share female



(b) RD: pre-reform share Western immigrant



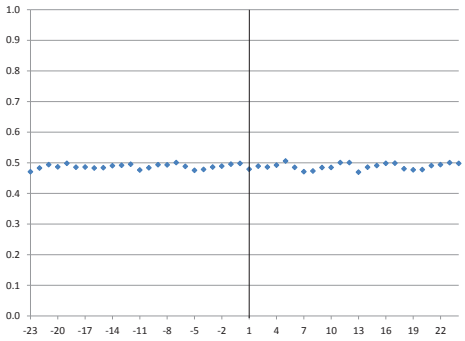
(c) RD: pre-reform share non-Western immigrant



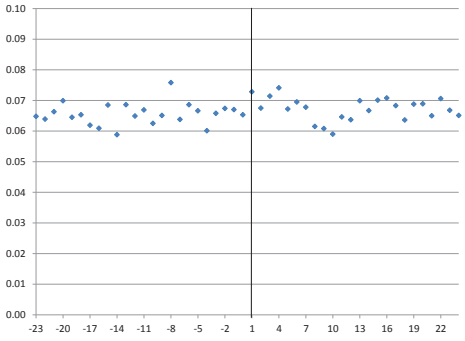
Notes: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.2.5: Regression discontinuity: control variables relative to discontinuity (2010–2012)

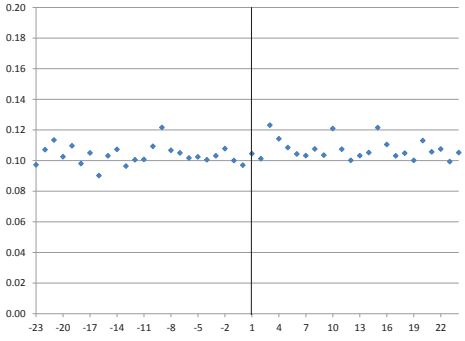
(a) RD: post-reform share female



(b) RD: post-reform share Western immigrant

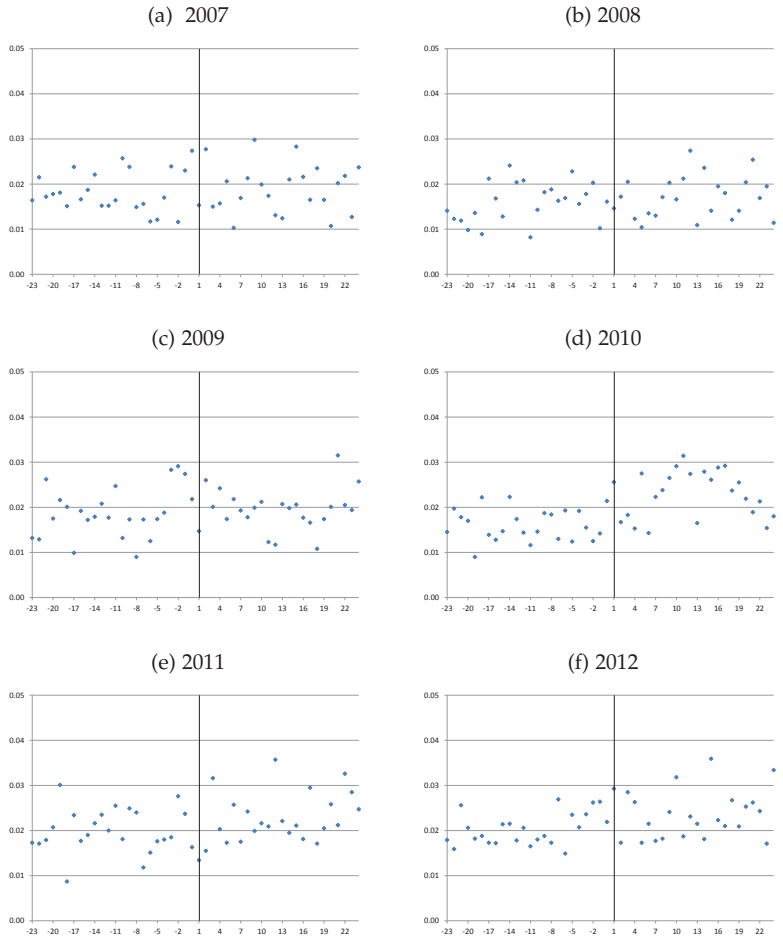


(c) RD: post-reform share non-Western immigrant



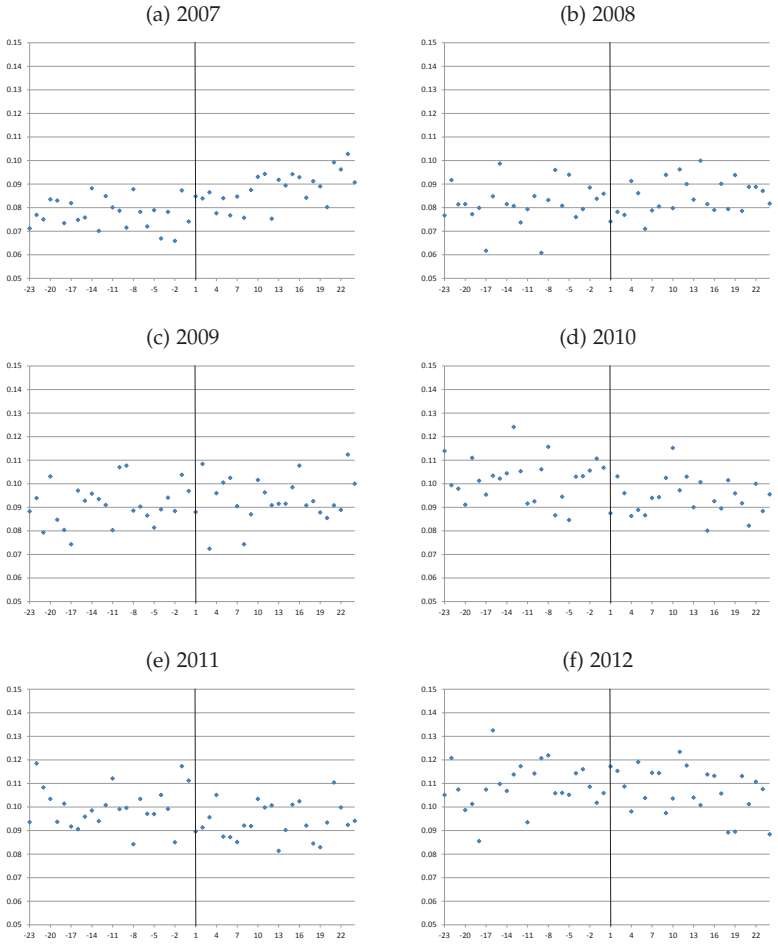
Notes: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.2.6: Regression discontinuity: NEETs rate on welfare by year



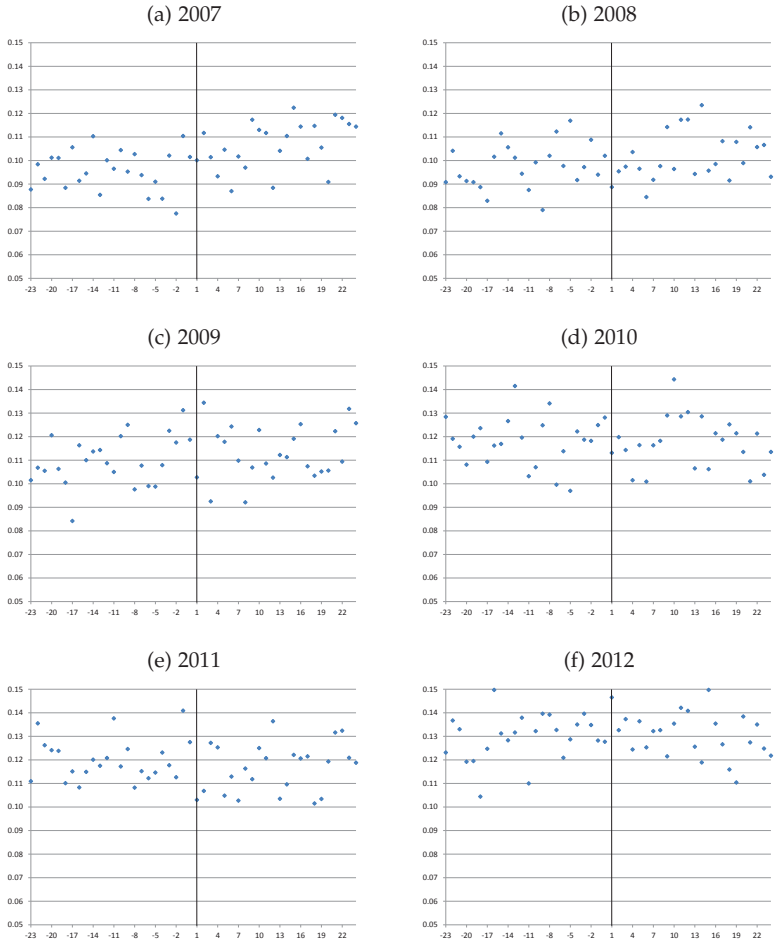
Notes: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.2.7: Regression discontinuity: NEETs rate not on welfare by year



Notes: Own calculations using the Labour Market Panel (Statistics Netherlands).

Figure A.2.8: Regression discontinuity: total NEETs rate by year



Notes: Own calculations using the Labour Market Panel (Statistics Netherlands).

Table A.2.12: Regression discontinuity: pre-reform full regression results (2007–2009)

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treat RD	0.0020	0.0011	0.0031	−0.0044	0.0013
	(0.0014)	(0.0023)	(0.0030)	(0.0034)	(0.0022)
Age in months	0.0001	0.0004***	0.0004***	0.0007***	−0.0011***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Age in months -left from cutoff	0.0001	−0.0002	−0.0001	0.0024***	−0.0023***
	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Year 2008	−0.0022***	0.0007	−0.0015	0.0033**	−0.0018
	(0.0006)	(0.0012)	(0.0013)	(0.0016)	(0.0011)
Year 2009	0.0004	0.0099***	0.0103***	−0.0145***	0.0042***
	(0.0009)	(0.0014)	(0.0016)	(0.0019)	(0.0012)
Female	0.0107***	0.0400***	0.0507***	−0.0328***	−0.0179***
	(0.0007)	(0.0025)	(0.0027)	(0.0035)	(0.0015)
Non-Western immigrant	0.0610***	0.1002***	0.1612***	−0.2077***	0.0464***
	(0.0029)	(0.0038)	(0.0051)	(0.0055)	(0.0034)
Western immigrant	0.0165***	0.0331***	0.0496***	−0.0881***	0.0385***
	(0.0020)	(0.0039)	(0.0046)	(0.0063)	(0.0037)
Observations	157,543	157,543	157,543	157,543	157,543
Clusters	72	72	72	72	72

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 2007–2009. Treatment group 25–26 and control group 27–28. Cluster-robust standard errors in parentheses, clustered by month of birth (72 clusters).

Table A.2.13: Regression discontinuity: post-reform full regression results (2010–2012)

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treat RD	−0.0014	0.0044**	0.0030	−0.0022	−0.0008
	(0.0013)	(0.0022)	(0.0023)	(0.0030)	(0.0024)
Age in months	0.0001*	−0.0001	0.0000	0.0013***	−0.0013***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Age in months -left from cutoff	−0.0001	0.0002	0.0001	0.0026***	−0.0028***
	(0.0001)	(0.0002)	(0.0002)	(0.0003)	(0.0002)
Year 2011	0.0019**	−0.0008	0.0011	−0.0036*	0.0025
	(0.0008)	(0.0012)	(0.0013)	(0.0018)	(0.0015)
Year 2012	0.0027***	0.0104***	0.0132***	−0.0195***	0.0063***
	(0.0008)	(0.0015)	(0.0017)	(0.0021)	(0.0015)
Female	0.0073***	0.0247***	0.0319***	−0.0157***	−0.0163***
	(0.0009)	(0.0021)	(0.0022)	(0.0030)	(0.0018)
Non-Western immigrant	0.0570***	0.0969***	0.1539***	−0.2164***	0.0626***
	(0.0027)	(0.0039)	(0.0041)	(0.0054)	(0.0039)
Western immigrant	0.0165***	0.0466***	0.0630***	−0.0982***	0.0352***
	(0.0020)	(0.0045)	(0.0044)	(0.0053)	(0.0033)
Observations	158,195	158,195	158,195	158,195	158,195
Clusters	72	72	72	72	72

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 2010–2012. Treatment group 25–26 and control group 27–28. Cluster-robust standard errors in parentheses, clustered by month of birth (72 clusters).

Table A.2.14: Regression discontinuity: different sets of control variables (2007–2009)

	(1)	(2)	(3)	(4)	(5)	(6)
NEETs rate on welfare	0.0020	0.0020	0.0019	0.0020	0.0020	0.0020
	(0.0016)	(0.0016)	(0.0016)	(0.0014)	(0.0014)	(0.0014)
NEETs rate not on welfare	0.0009	0.0009	0.0010	0.0010	0.0010	0.0011
	(0.0025)	(0.0025)	(0.0025)	(0.0022)	(0.0022)	(0.0023)
Total NEETs rate	0.0028	0.0028	0.0029	0.0030	0.0030	0.0031
	(0.0035)	(0.0035)	(0.0035)	(0.0030)	(0.0030)	(0.0030)
Employment rate	−0.0032	−0.0031	−0.0041	−0.0035	−0.0034	−0.0044
	(0.0043)	(0.0038)	(0.0041)	(0.0038)	(0.0032)	(0.0034)
Enrollment rate in education	0.0003	0.0003	0.0012	0.0004	0.0003	0.0013
	(0.0031)	(0.0020)	(0.0022)	(0.0031)	(0.0020)	(0.0022)
Age in months squared	NO	YES	NO	NO	YES	NO
(Age in months) x 1(age<27)	NO	NO	YES	NO	NO	YES
Demographic controls	NO	NO	NO	YES	YES	YES
Observations	157,543	157,543	157,543	157,543	157,543	157,543
Clusters	72	72	72	72	72	72

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 2007–2009. Treatment group 25–26 and control group 27–28. Cluster-robust standard errors in parentheses, clustered by month of birth (72 clusters).

Table A.2.15: Regression discontinuity: different sets of control variables (2010–2012)

	(1)	(2)	(3)	(4)	(5)	(6)
NEETs rate on welfare	−0.0018	−0.0018	−0.0017	−0.0014	−0.0014	−0.0014
	(0.0014)	(0.0014)	(0.0014)	(0.0013)	(0.0013)	(0.0013)
NEETs rate not on welfare	0.0041	0.0041	0.0040	0.0045**	0.0044**	0.0044**
	(0.0029)	(0.0029)	(0.0029)	(0.0022)	(0.0022)	(0.0022)
Total NEETs rate	0.0023	0.0023	0.0023	0.0031	0.0030	0.0030
	(0.0032)	(0.0032)	(0.0032)	(0.0023)	(0.0023)	(0.0023)
Employment rate	0.0005	0.0002	−0.0009	−0.0008	−0.0011	−0.0022
	(0.0047)	(0.0038)	(0.0041)	(0.0038)	(0.0027)	(0.0030)
Enrollment rate in education	−0.0028	−0.0025	−0.0013	−0.0023	−0.0019	−0.0008
	(0.0035)	(0.0022)	(0.0024)	(0.0035)	(0.0021)	(0.0024)
Age in months squared	NO	YES	NO	NO	YES	NO
(Age in months) x 1(age<27)	NO	NO	YES	NO	NO	YES
Demographic controls	NO	NO	NO	YES	YES	YES
Observations	158,195	158,195	158,195	158,195	158,195	158,195
Clusters	72	72	72	72	72	72

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.Sample period 2010–2012. Treatment group 25–26 and control group 27–28. Cluster-robust standard errors in parentheses, clustered by month of birth (72 clusters).

Table A.2.16: Difference-in-discontinuity: full regression results (2007–2012)

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treat RD x 1(year>2009)	−0.0033*	0.0032	−0.0001	0.0022	−0.0021
	(0.0020)	(0.0031)	(0.0038)	(0.0046)	(0.0030)
Treat RD	0.0020	0.0011	0.0031	−0.0044	0.0013
	(0.0014)	(0.0022)	(0.0030)	(0.0034)	(0.0022)
Age in months	0.0001	0.0004***	0.0004***	0.0006***	−0.0011***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Age in months x 1(year>2009)	0.0001	−0.0005***	−0.0004**	0.0007***	−0.0003**
	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Age in months x 1(age<27)	0.0001	−0.0002	−0.0001	0.0024	−0.0023
	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Age in months x 1(age<27)	−0.0002	0.0004	0.0002	0.0002	−0.0004
x1(year>2009)	(0.0001)	(0.0003)	(0.0003)	(0.0004)	(0.0003)
Year 2008	−0.0022***	0.0007	−0.0015	0.0033**	−0.0018
	(0.0006)	(0.0012)	(0.0013)	(0.0016)	(0.0011)
Year 2009	0.0004	0.0098***	0.0102***	−0.0144***	0.0042***
	(0.0009)	(0.0014)	(0.0016)	(0.0019)	(0.0012)
Year 2010	0.0014	0.0167***	0.0181***	−0.0243***	0.0061***
	(0.0018)	(0.0028)	(0.0033)	(0.0036)	(0.0022)
Year 2011	0.0033*	0.0159***	0.0192***	−0.0278***	0.0086***
	(0.0019)	(0.0028)	(0.0034)	(0.0035)	(0.0023)
Year 2012	0.0041**	0.0271***	0.0313***	−0.0437***	0.0124***
	(0.0018)	(0.0027)	(0.0032)	(0.0033)	(0.0022)
Female	0.0090***	0.0323***	0.0413***	−0.0242***	−0.0171***
	(0.0007)	(0.0018)	(0.0020)	(0.0025)	(0.0011)
Non-Western immigrant	0.0590***	0.0987***	0.1577***	−0.2122***	0.0545***
	(0.0022)	(0.0029)	(0.0035)	(0.0043)	(0.0029)
Western immigrant	0.0165***	0.0397***	0.0562***	−0.0931***	0.0370***
	(0.0015)	(0.0031)	(0.0035)	(0.0045)	(0.0026)
Observations	315,738	315,738	315,738	315,738	315,738
Clusters	108	108	108	108	108

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 2007–2012. Treatment group 25–26 and control group 27–28. Cluster-robust standard errors in parentheses, clustered by month of birth (72 clusters).

Table A.2.17: Difference-in-discontinuity: wider bandwith, quarter of birth

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treat RD x 1(year>2009)	−0.0030	0.0023	−0.0007	0.0030	−0.0023
	(0.0019)	(0.0034)	(0.0043)	(0.0054)	(0.0030)
Treat RD	0.0016	0.0017	0.0033	−0.0072	0.0039
	(0.0013)	(0.0025)	(0.0032)	(0.0043)	(0.0025)
Observations	315,738	315,738	315,738	315,738	315,738
Clusters	36	36	36	36	36

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 2007–2012. Treatment group 25–26 and control group 27–28. Cluster-robust standard errors in parentheses, clustered by month of birth (108).

Table A.2.18: Difference-in-discontinuity: smaller age range 26–27

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treat RD x 1(year>2009)	−0.0013	0.0081	0.0068	−0.0027	−0.0041
	(0.0036)	(0.0059)	(0.0072)	(0.0076)	(0.0046)
Treat RD	0.0033	0.0024	0.0058	−0.0130**	0.0072**
	(0.0026)	(0.0041)	(0.0052)	(0.0053)	(0.0030)
Observations	157,399	157,399	157,399	157,399	157,399
Clusters	84	84	84	84	84

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 2007–2012. Treatment group 26 and control group 27 Cluster-robust standard errors in parentheses, clustered by month of birth (84).

Table A.2.19: Difference-in-discontinuity: wider age range 24–29

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Treat RD x 1(year>2009)	−0.0030*	0.0050*	0.0020	−0.0000	−0.0019
	(0.0017)	(0.0026)	(0.0032)	(0.0046)	(0.0031)
Treat RD	0.0013	0.0009	0.0021	0.0036	−0.0057**
	(0.0011)	(0.0018)	(0.0024)	(0.0033)	(0.0024)
Observations	475,213	475,213	475,213	475,213	475,213
Clusters	132	132	132	132	132

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 2007–2012. Treatment group 24–26 and control group 27–29. Cluster-robust standard errors in parentheses, clustered by month of birth (132).

Table A.2.20: Regression discontinuity and difference-in-discontinuity: treatment effects using donut regression discontinuity and donut difference-in-discontinuity (2007–2012)

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Panel A: Donut RD for the period 2007–2009					
RD dummy	0.0017	–0.0011	0.0006	0.0024	–0.0031
(placebo)	(0.0016)	(0.0026)	(0.0031)	(0.0036)	(0.0022)
Observations	137,698	137,698	137,698	137,698	137,698
Clusters	72	72	72	72	72
Panel B: Donut RD for the period 2010–2012					
RD dummy	−0.0024	0.0032	0.0008	0.0042	–0.0050**
	(0.0013)	(0.0025)	(0.0028)	(0.0033)	(0.0022)
Observations	138,456	138,456	138,456	138,456	138,456
Clusters	72	72	72	72	72
Panel C: Donut DRD for the period 2007–2012					
DRD dummy	−0.0041*	0.0042	0.0001	0.0018	−0.0019
	(0.0023)	(0.0037)	(0.0044)	(0.0055)	(0.0033)
Observations	276,154	276,154	276,154	276,154	276,154
Clusters	108	108	108	108	108

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Sample period 2007–2012. Treatment group 25–26 and control group 27–28. RD and DRD without observations for 3 age months before and after the cutoff. Cluster-robust standard errors in parentheses, clustered by month of birth. The RD parameter estimates are for the RD dummy capturing a different intercept on the left hand side of the discontinuity, and also allow for a different slope on the left hand side of the discontinuity, include year fixed effects and include demographic control variables. The DRD parameter estimates are for the DRD dummy capturing the difference in the different intercept on the left hand side of the discontinuity from the period 2007–2009 to the period 2010–2012, and also allow for a different slope on the left hand side of the discontinuity, a change in the different slope on the left hand side of the discontinuity, include year fixed effects and include demographic control variables.

Table A.2.21: Difference-in-discontinuity: entry and exit

	(1) NEETs rate on welfare	(2) NEETs rate not on welfare	(3) Total NEETs rate	(4) Employment rate	(5) Enrollment rate in education
Entry					
DRD dummy	−0.0023*	0.0014	−0.0005	0.0024	−0.0005
	(0.0013)	(0.0028)	(0.0028)	(0.0040)	(0.0014)
Exit					
DRD dummy	0.0021*	−0.0026	−0.0001	−0.0011	0.0026
	(0.0012)	(0.0024)	(0.0023)	(0.0030)	(0.0030)
Observations	315,495	315,495	315,495	315,495	315,495
Clusters	108	108	108	108	108

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Cluster-robust standard errors in parentheses, clustered by month of birth (108 clusters). The DRD parameter estimates are for the DRD dummy capturing the difference in the different intercept on the left hand side of the discontinuity from the period 2007–2009 to the period 2010–2012, and also allow for a different slope on the left hand side of the discontinuity, a change in the different slope on the left hand side of the discontinuity, include year fixed effects and include demographic control variables.

3 The Added Worker Effect in the Netherlands Before and During the Great Recession

Abstract

We study the added worker effect in the Netherlands before and during the Great Recession. We use a large administrative panel dataset for the period 1999–2015 and employ differences-in-differences to estimate the effect of male partner's unemployment shock on female partner's income. We find a modest added worker effect of 2-5% of the male partner's income loss. The added worker effect disappeared in the beginning of the Great Recession, but resurfaced a few years later. Furthermore, we show that self-employment has become more important in dealing with unemployment shocks.

The chapter is co-authored by Egbert Jongen en Pierre Koning. We are grateful for comments and suggestions by seminar and conference participants at CPB Netherlands Bureau for Economic Policy Analysis and the IIPF 2018 Conference in Tampere. Remaining errors are my own.

3.1 Introduction

Since the start of the Great Recession, policymakers and academics have shown increased interest in the effect of unemployment shocks on the labor supply of partners of the unemployed workers – also known as the added worker effect (henceforth AWE). While the empirical literature generally finds the AWE to be small – see e.g. Hardoy and Schøne (2014), Halla et al. (2018) and Bredtmann et al. (2018) for recent contributions – a pertaining question is whether the AWE has grown in importance in the years following the onset of the Great Recession in 2008. With markedly higher unemployment risks and larger shocks in wage earnings that have occurred in this period, one may expect the AWE to have become more sizable. At the same time, however, increases in labor supply may to a lesser extent have been translated into more employment during an economic downturn and high unemployment rates may have discouraged partners from entering the labor market. From a theoretical perspective, the overall effect of changes in business cycles on the AWE is thus ambiguous.

This paper studies how the AWE is related to changes over the business cycle in the Netherlands during the period 2003-2015. For this purpose, we use administrative data from the Labour Market Panel of Statistics Netherlands. The Labour Market Panel tracks the labor market histories of 1.8 million individuals for the period 1999-2015, as well as their social security records and profits from self-employment. In addition, the panel contains information on demographics, household characteristics and education levels of individuals.

We contribute to the literature by investigating how AWE changed over the business cycle in the Netherlands, using data that cover periods before and during the Great Recession. We study the AWE for couples who are confronted with large and persistent income shocks in comparison with other studies on the AWE. These larger income shocks follow both from the Great Recession and from studying the effects of entering unemployment insurance (UI) rather than studying the effects of mass layoffs.[1] We further

[1] In contrast with studies on mass layoffs, we do not study the effects for households that were displaced but never entered UI.

shed light on two distinctive features of the Dutch labour market. First, we assess the importance of the substantial and increasing share of self-employed that may have provided increasing opportunities to mitigate partners' income shocks. Second, the Netherlands is a country that has seen a steep rise in the employment rate of women, while remaining the country with the highest share of part-time employment in the OECD. In this context, it is interesting to study the AWE at the extensive and intensive margin, and potential changes in the role of these margins over time.

Our research strategy compares women with male partners who became unemployed to women with male partners that remained employed in a given year. Using a differences-in-differences design with individual fixed effects, we estimate the impact of a male partners unemployment shock in a particular year on the earnings of both partners, the employment of the female partner, income from unemployment insurance (UI) and other social benefits, and profits from self-employment – all measured over a time window from 4 years before entering UI, the year of entering UI and 3 years after entering UI. With these results, we assess the importance of a rich set of income sources that may mitigate the drop in household income due to the job loss of the male partner. By taking different reference years for the unemployment shocks occurring in our sample, we assess how the effects vary over the business cycle and over time more broadly.

Throughout the empirical analysis, a key challenge is to construct treatment and control groups that have common time effects. For this reason, we select a sample of individuals between the age of 25 and 55 who received annual earnings from labor of at least 5,000 euro and no income from benefits in the 4 years preceding the possible receipt of UI benefits. As such, each yearly cohort consists of individuals with relatively stable positions on the labour market for which income shocks are plausibly exogenous. In addition, the inclusion of time dummies in the two years before becoming unemployed allows us to conduct placebo analyses on the assumption of common time effects and whether or not we observe any anticipation effects.

Our main findings are as follows. First, we find that the unemployment shock of a male partner, causing a loss in gross income of 20 to 30 thousand euro, has a small, positive and statistically significant AWE of 2-5% (500-1,000 euros). This is comparable to the AWE estimates of Juhn and Potter (2007), Hardoy and Schøne (2014), Starr (2014), Halla et al. (2018) and Bredtmann et al. (2018).[2] Second, the AWE that we estimate largely disappears during the first years of the Great Recession (2008-2009). While this may appear at odds with earlier research in this field – see e.g. Mattingly and Smith (2010) and Bredtmann et al. (2018) – it is in line with Halla et al. (2018) who find AWE on earnings to be confined to districts with low unemployment rates.[3] Third, our findings point to the existence of both intensive and extensive margin added worker effects. As such, we add to a literature that provides mixed evidence on the importance of intensive and extensive margin effects – see e.g. Hardoy and Schøne (2014), Halla et al. (2018) and Bredtmann et al. (2018). The decrease in the AWE at the start of the Great Recession is mostly driven by decreases at the intensive margin, i.e. less additional hours worked by partners that were already employed. Finally, we find an AWE of about 2% (500 euro) of profits from self-employment of the female partner and the treatment effect on male partner's profits more than doubled from about 2,000 euro 3 years after entering UI in 2004 to about 4,500 euro 3 years after entering UI in 2012.

The outline of the paper is as follows. Section 3.2 gives background information on the Dutch labor market and the UI system. Section 3.3 considers the empirical methodology. Section 3.4 discusses the dataset and gives descriptive statistics. Section 3.5 presents the estimation results. Section 3.6 concludes.

[2]Table A.3.1 in the appendix gives a detailed overview of the literature on the AWE.

[3]In addition, Juhn and Potter (2007) and Bryan and Longhi (2013) find evidence of positive labor force participation effects of partners in an economic downturn that do not translate into increases of employment.

Institutional setting 3.2

Bearing in mind that the room for an AWE is likely to be driven by contextual factors, this section sheds light on the institutions and the labor market situation in the Netherlands in the time period under investigation. In particular, we highlight the high share of part-time employment among women and the increasing and substantial share of self-employment in the labor force.

Figure 3.1a presents the labor force participation rates for women in 2000 and 2015 for 16 developed OECD countries. The Netherlands has experienced one of the fastest increases in the female labor force participation rate over the period 2000-2015 (amounting to almost 10 percentage points). As a result, the Netherlands has reached female participation levels that are close to those in Scandinavian countries.[4] As Bredtmann et al. (2018) argue, higher female labor force participation rates are expected to limit the room for extensive margin effects. At the same time, panel (b) of Figure 3.1 suggests that the high share of part-time employment still provides room for women to increase working hours. This makes the Netherlands a particularly interesting case to study AWE effects at the intensive margin.

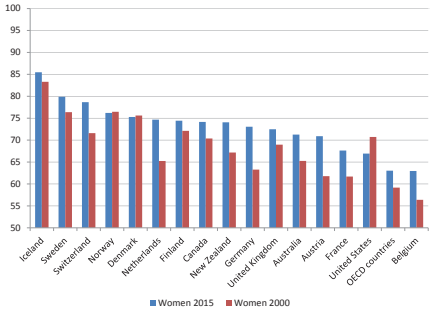
Between 2000 and 2015, the Dutch labour market has also been marked by a strong increase in the share of employees on fixed-term contracts and the increase in the share of self-employed. The share of employees on fixed-term contracts increased from around 15% in 2000 to slightly more than 20% in 2016, which is one of the highest across OECD countries (OECD 2018c). As panel (c) of Figure 3.1 shows, the increase in the share of self-employed in the Netherlands was the largest for OECD countries (OECD 2018c). Self-employment may have increasingly been used to mitigate income shocks caused by unemployment (OECD 2018c).

To provide insight in the economic conditions over time, Figure 3.1d shows the unemployment rate for the Netherlands and several other OECD countries. The unemployment rate of the Netherlands, denoted by the blue dotted line, was very low from an international perspective in the

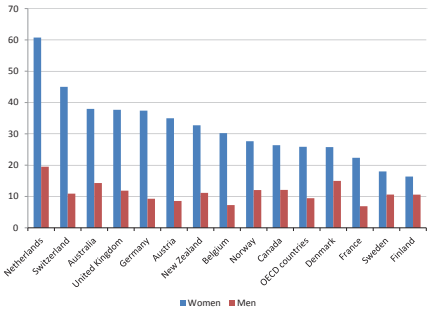
[4] For men, the Netherlands has the third highest labor force participation rate of the OECD in 2015, see Figure A.3.1 in the Supplementary Material.

Figure 3.1: International comparison of labor markets

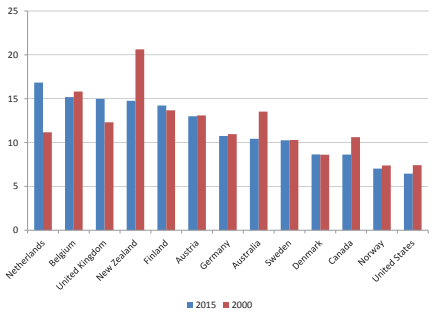
(a) Labour force participation rates for women



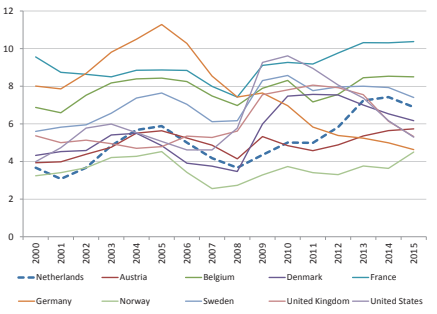
(b) Incidence of part-time employment in 2015



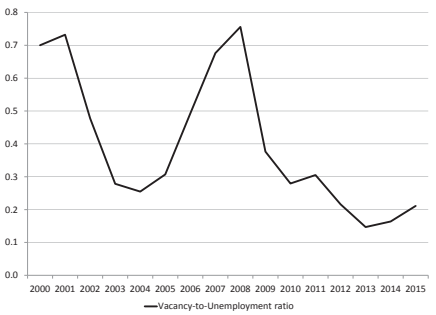
(c) Share of self-employed as a % of total employed



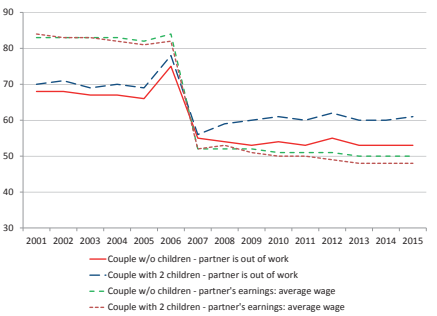
(d) Unemployment rates



(e) Vacancy-to-unemployment ratio



(f) Net repl. rates after 24 months unemployment



Notes: Data are obtained from OECD (2018a,d,e, 2019a,b) and from Statistics Netherlands (Statline).

beginning of the 21st century. The unemployment rate increased from 3.1 in 2001 to almost 5.8 percent in 2005 due to the burst of the dot-com bubble, after which it decreased again to 3.7% in 2008. The increase in the unemployment rate in 2009 was smaller in the Netherlands than in most other OECD countries affected by the Great Recession, but the increase persisted for a longer period of time, reaching a peak of 7.4% in 2014. To complement this data, Figure 3.1e pictures the vacancy-to-unemployment ratio in the Netherlands between 2000 and 2015. This shows that there was an economic downturn in the years 2003-2005 and 2009-2015.

Finally, it is worth noting that UI reforms were implemented in 2006. This implied that the maximum entitlement period was reduced from 60 to 38 months. As panel (f) of Figure 3.1 shows, this has caused a drop in the net replacement rate for individuals that are long-term unemployed, from about 70% to 50%. This in turn may have increased the need for intra-household insurance via an AWE.

Empirical strategy 3.3

Essentially, empirical analyses on the AWE require two major ingredients. First, the idea is to follow behavioral responses to an income shock that is plausibly exogenous and cannot be anticipated by workers' partners. Obvious candidates for such shocks are plant closures, mass layoffs or involuntary firings. Second, one needs to construct control groups of workers that are not hit by these shocks, but do have time effects that are common to the treatment group. Accordingly, the estimation of AWE typically follows a differences-in-differences design to estimate the effect of income shocks on outcome measures. This is also the approach we follow.

While most studies consider the effect of bankruptcies or mass-layoffs to define treatment groups – see e.g. Hardoy and Schøne (2014) and Halla et al. (2018) – we use the inflow into UI benefits. Bearing in mind that the UI benefits are only received for those fired involuntarily, our key assumption is that workers cannot anticipate the timing of this event. We argue that this assumption is not necessarily stronger than in the case of

mass layoffs or plant closures. Similar to these cases, testing potential anticipation effects remains a crucial part of our estimation approach. An advantage of our approach is that we consider income shocks that are expected to be more sizable than income shocks in case of mass-layoffs. In particular, including couples with male partners finding a job after displacement would limit the shock effect, making it harder to infer an AWE, which are typically found to be relatively small.

As a second ingredient of our analysis, we select couples 25–55 years of age with male partners with an income from work of at least 5,000 euro and with no income from UI, social assistance or other benefits in the years before becoming unemployed. These sample selection criteria ensure that the treatment and control groups have similar (stable) labor market positions for a long stretch of time.

To formalize matters, we define the treatment group as those women with a partner who worked in *t*-1 and started receiving UI benefits in period *t*. The control group contains women with a partner who did not receive UI benefits in both period *t-1* and *t*. For each year in our sample, we construct treatment and cohort groups this way. In effect, this means that we have 10 cohort years (2003-2012) for which we constructed balanced samples including 4 years before becoming unemployed, the year of the income shock, and 3 years thereafter. For these samples, we estimate linear models that are specified as follows:

$$Y_{it} = X'_{it}\,\beta_x + \tau_t + \alpha_i + \sum_{j=-2}^{3} d_{it}^{\,j}\,\gamma_j + \epsilon_{it}. \tag{3.1}$$

for individual i in year t. In the above specification, the outcome variables Y are regressed on a set of time-varying demographic controls (age) X_{it}, year fixed effects (τ_t), individual fixed effects (α_i), and the treatment dummies $d_{it}^{\,j}$ which are equal to one if the partner of woman i became unemployed in year t, j years from year t, and zero otherwise. The residual

term ϵ_{it} is assumed to be i.i.d.[5] Equation [3.1] can be estimated with fixed effects estimation.[6] As such, we control for a priori differences in outcome values between the treatment and control groups.

Our parameters of interest that describe the AWE are included in vector γ. For values of j that are zero or positive, γ equals the short- and longer-term effects of the unemployment shock. For the two pre-treatment dummies, the values of j are negative and γ captures potential anticipation effects or different trends in the two years before the husbands' income shock, hence these are placebo tests.

Data 3.4

We use administrative data from the Labour Market Panel (In Dutch: *Arbeidsmarktpanel*) of Statistics Netherlands (2015). The Labour Market Panel is a large and rich household panel data set, tracking 1.8 million individuals over the period 1999–2015. The main outcome variables we consider are female partner's wages and profits from self-employment, male partner's wages and profits from self-employment, income from UI benefits, social assistance benefits, welfare benefits, disability benefits and other benefits. In addition, we estimate the AWE on the participation rate and on the number of hours worked that are observed in the data.[7] All variables are measured on an annual basis.

As argued earlier, we select couples in which both partners are 25–55 years of age to make sure that the treatment and control group are comparable. While younger individuals are often studying or living with their parents, older individuals may anticipate old age benefits in the years before retirement. Also, note that we restrict the sample to heterosexual

[5] In the results section we consider different levels of clustering of the standard error, which may be at the level of provinces, provinces interacted with nationality and the individual level, and show that our results are robust in terms of statistical significance using different levels of clustering.

[6] Note that the group dummy is absorbed by the individual fixed effects.

[7] Unfortunately, data on hours worked is only available for the shorter period 2006-2015.

couples, who also stay together during the full 8 years in the balanced samples.[8]

Table 3.1 presents sample characteristics for our balanced panel consisting of 'treated' individuals and untreated individuals, for selected cohorts (2004, 2008 and 2012) to ease the exposition. The table shows the values that are averaged over the pre-treatment period, consisting of the four periods before the 'treated' individuals enter UI. First, the table shows the mean values of demographic variables. Comparing treatment and control groups, we find relatively small differences in age for both male and female partners. There are some differences in the treatment group and control group regarding ethnicity and the level of education, however, below we show that we obtain similar results for the AWE when we exclude or include demographic control variables (and exclude individual fixed effects).

Regarding the outcome variables in our analysis, Table 3.1 shows some differences in earnings in the pre-treatment period for the treatment and control groups. Men in the treatment group earned 3,000-4,000 euro (about 8%) less in the treatment group compared to the control group for the treatment years 2004 and 2012, whereas men who became unemployed in 2008 earned slightly more than the control group. Male partner's income from profits is slightly smaller in the treatment group than in the control group for the treatment years 2008 and 2012. Female partner's income from work and from profits as well as their employment rates are all about the same for the treatment and control groups for the different treatment years.

[8]We do not consider same-sex couples because the distinction between same-sex couples and friends living together is harder to make with the data. Furthermore, we do not consider the effect of entering UI on the stability of relationships.

Table 3.1: Sample characteristics (standard deviations in parentheses)

	2004		2008		2012	
	Treatment group (before unemployment) (2000-2003)	Control group	Treatment group (before unemployment) (2004-2007)	Control group	Treatment group (before unemployment) (2008-2011)	Control group
Explanatory variables						
Men						
Age	40.908	41.190	42.349	42.151	43.459	43.430
	(7.534)	(7.322)	(7.069)	(7.023)	(6.840)	(6.653)
Western immigrant	0.087	0.067	0.087	0.066	0.068	0.064
	(0.281)	(0.250)	(0.282)	(0.248)	(0.251)	(0.245)
Non-Western immigrant	0.068	0.033	0.065	0.043	0.064	0.050
	(0.252)	(0.180)	(0.247)	(0.202)	(0.244)	(0.218)
Medium education level	0.447	0.434	0.427	0.439	0.455	0.446
	(0.497)	(0.496)	(0.495)	(0.496)	(0.498)	(0.497)
High education level	0.272	0.334	0.334	0.344	0.283	0.350
	(0.445)	(0.472)	(0.472)	(0.475)	(0.451)	(0.477)
Women						
Age	38.745	39.051	40.216	40.018	41.306	41.338
	(7.592)	(7.376)	(7.245)	(7.140)	(6.989)	(6.833)
Western immigrant	0.087	0.075	0.112	0.075	0.071	0.075
	(0.281)	(0.263)	(0.316)	(0.263)	(0.257)	(0.263)
Non-Western immigrant	0.067	0.037	0.066	0.048	0.071	0.055
	(0.251)	(0.190)	(0.248)	(0.214)	(0.256)	(0.229)
Medium education level	0.436	0.471	0.435	0.485	0.491	0.497
	(0.496)	(0.499)	(0.496)	(0.500)	(0.500)	(0.500)
High education level	0.253	0.247	0.281	0.275	0.265	0.294
	(0.435)	(0.431)	(0.449)	(0.446)	(0.441)	(0.455)
Number of children	1.435	1.558	1.540	1.655	1.675	1.749
	(1.082)	(1.128)	(1.091)	(1.088)	(1.044)	(1.048)
Dependent variables						
Men						
Income from work	36,627	39,710	46,622	45,793	49,999	53,978
	(21,578)	(23,953)	(45,905)	(32,151)	(35,841)	(39,956)
Income from profits	170	189	151	311	−58	294
	(7,722)	(4,826)	(3,978)	(6,340)	(10,520)	(7,273)
Women						
Income from work	12,609	12,353	14,900	15,082	18,651	18,950
	(12,357)	(12,141)	(14,478)	(15,322)	(17,219)	(17,583)
Income from profits	626	420	857	626	912	1040
	(7,329)	(4,904)	(8,235)	(6,411)	(7,832)	(9,009)
Employment rate	0.752	0.757	0.765	0.796	0.801	0.826
	(0.432)	(0.429)	(0.424)	(0.403)	(0.399)	(0.379)
Hours worked	876	817	885	882	925	944
	(974)	(916)	(920)	(968)	(667)	(639)
Observations	7,952	483,240	3,632	437,252	8,552	353,204
Number of individuals	1,988	120,810	908	109,313	2,138	88,301

3.5 Results

3.5.1 The added worker effect

Figure 3.2 presents graphical evidence of the AWE, showing the average income of female partners from 4 years before the male partner starts to receive UI benefits until 3 years thereafter. The solid black lines denote the control group (women whose male partner did not enter UI), the dashed red lines denote the 'treatment' group (women whose partner did enter UI) and the dotted blue lines denote the differences between the treatment group and the control group. For the years 2003–2006 and 2010–2012, income from work for both groups appears to move parallel, consistent with the assumption of common time effects. Similar eyeball tests suggests the presence of small and positive AWE in most years. For the years 2007–2009, however, we observe small differences in the time pattern between the treatment and control group before the unemployment shock. In what follows, we thus should interpret the estimation results for these years with the appropriate care.

Table 3.2 gives the 'treatment effect' on the income of the male partner, i.e. the direct effect of the unemployment shock on the wage income of the male partner. The different columns present the results for different treatment years (years in which male partners enter UI) and the rows show the treatment effect from two years before the treatment (t-2) up to 3 years after the treatment (t+3). The pre-treatment placebo dummies are (typically) small and statistically insignificant.[9] For most treatment years we observe a negative treatment effect on male partner's income of about 15 thousand euro in the year that the male partner becomes unemployed. This effect increases to about 25 thousand euro in the year after becoming unemployed, which is more than 50% of the income before unemployment. This increase from year t to year $t+1$ stems from the fact that we use annual data wherein not all male partners become unemployed in the beginning of the year. Three years after the unemployment shock, we still observe a negative treatment effect of about 20 thousand euro. This

[9] The proverbial exception is the placebo for 2006, which is however still small when compared to the 'treatment effect' that follows.

indicates a sizable, persistent negative effect of becoming unemployed on income, which is substantially larger than the loss in income that is typically observed in the literature. Using mass layoffs, Hardoy and Schøne (2014) find a 5% reduction in income which remains approximately the same level in the 4 years after displacement and Halla et al. (2018) find a relatively stable decrease of 21-24% of the pre-displacement mean earnings. As argued earlier, our treatment group does not include men that did not transit to a new job without going through UI. To further understand the large income drop in our case, Table A.3.2 in the appendix shows the treatment effect on male partner's probability of being employed. For most treatment years, the employment rate is about 22 percentage points lower in the year after the unemployment shock. Hence, 40 to 45% of the negative treatment effect on men's wage income can be explained by being unemployed and more than half appears to be due to lower wages in subsequent employment. This is more than is typically found in the literature using mass layoffs. Deelen et al. (2018) estimate a decrease in the employment rate in the year after displacement of 18 percentage points for older age workers (45-54) and 12 percentage points for prime-age workers (35-44) in the Netherlands. Meekes and Hassink (2019) find a displacement effect on employment of –20% for the Netherlands, which remains stable between 1 and 3 years after displacement. Also, both Deelen et al. (2018) and Meekes and Hassink (2019) find substantially lower but stable treatment effects on wages, ranging from –3 to –8%.

Table 3.3 shows the AWE estimates – that is, the treatment effect on the female partner's wage income from work for all year cohorts in our sample. First, we consider the placebo treatment dummies for t-2 and t-1, which are typically small and statistically insignificant.[10] The treatment effect varies across years, but is typically in the order of 500-1,000 euro in the years after the male partner enters UI. The AWE is rather stable over the years following entry into UI, corresponding to 2–5% of the income shock for the male partner. Hardoy and Schøne (2014) find an AWE of 7–18% of a much smaller income shock and Halla et al. (2018) find an

[10] Again with one exception, the dummy for t-1 for female partners of male partners that become unemployed in 2007, where the placebo dummy is significant at the 10% level.

Figure 3.2: Wage income for women whose male partner enters UI in a specific year (treatment group) or not (control group)

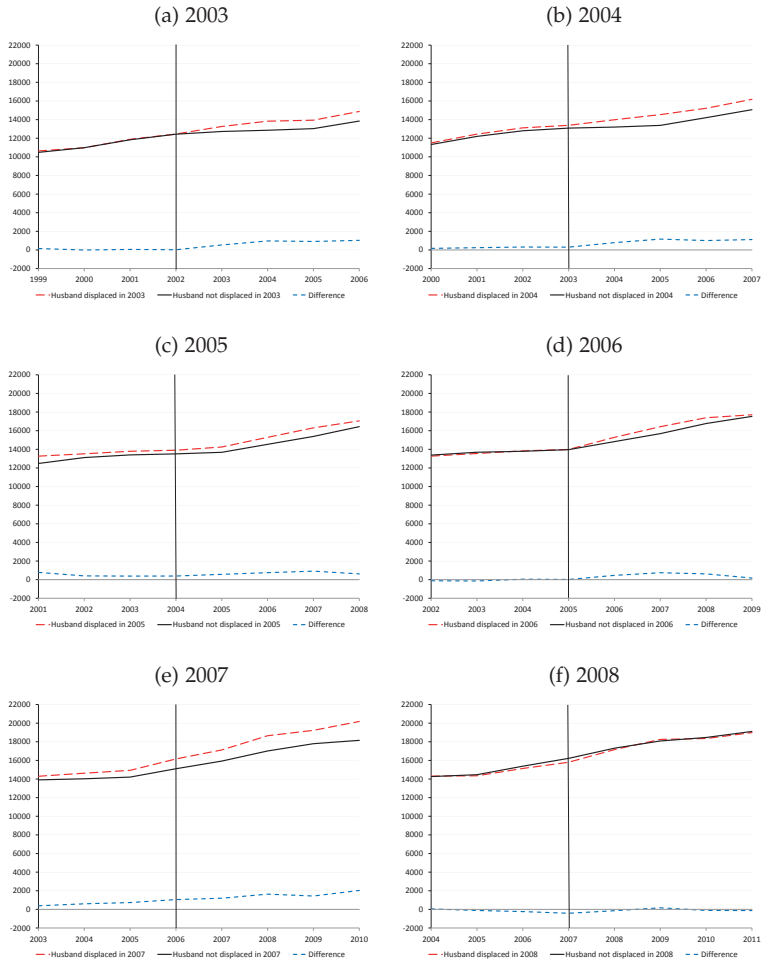
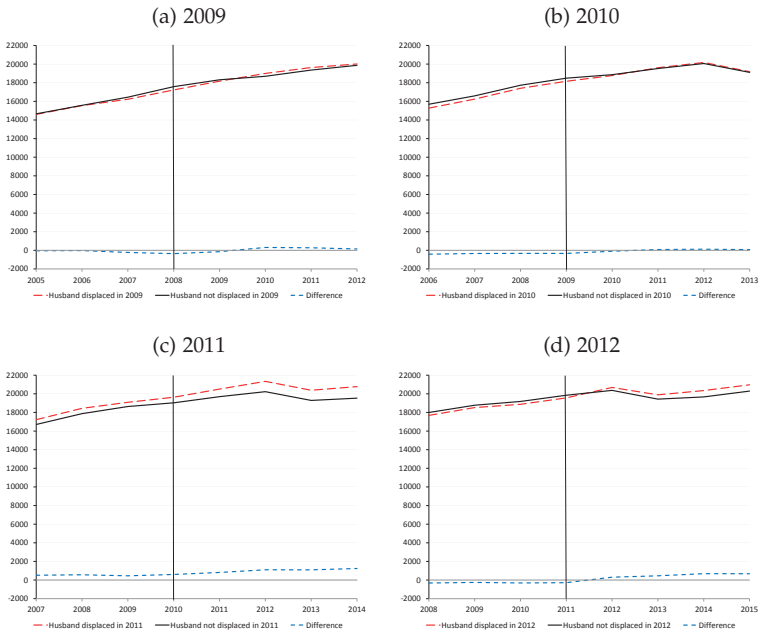


Figure 2: *Continued*



Notes: Own calculations using the Labour Market Panel (Statistics Netherlands). The solid black lines denotes the control groups, the red lines denote the treatment groups and the dotted blue lines denote the differences between the treatment group and the control group. Figures for individual treatment years are based on a sample of individuals: with observations available for the full 8-year period, couples that stay together during the full period, 25-55 years of age for both the female and male partner in the year before the treatment year, and with husbands that earn at least 5,000 euro and receive no UI, social assistance or other benefits in the years before the treatment.

Table 3.2: Treatment effect of entering UI on wage income male partner

	(1)	(2)	(3)	(4)	(5)
	2003	2004	2005	2006	2007
Male partner displaced in t-2	407	−560	−696	−273	−211
	(412)	(463)	(564)	(681)	(820)
Male partner displaced in t-1	−106	−273	−382	1,364**	152
	(412)	(463)	(564)	(681)	(820)
Male partner displaced in t	−12,223***	−13,176***	−12,621***	−17,005***	−13,417***
	(412)	(463)	(564)	(681)	(820)
Male partner displaced in t+1	−21,793***	−19,532***	−21,599***	−23,434***	−21,498***
	(412)	(463)	(564)	(681)	(820)
Male partner displaced in t+2	−17,697***	−15,953***	−17,882***	−19,751***	−19,575***
	(412)	(463)	(564)	(681)	(820)
Male partner displaced in t+3	−16,091***	−13,733***	−17,011***	−20,279***	−20,112***
	(412)	(463)	(564)	(681)	(820)
Demographic controls	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Individual fiixed effects	YES	YES	YES	YES	YES
Observations	999,744	982,384	966,104	940,136	912,104
Number of individuals	124,968	122,798	120,763	117,517	114,014

	(6)	(7)	(8)	(9)	(10)
	2008	2009	2010	2011	2012
Male partner displaced in t-2	−48	−221	−224	124	−599
	(879)	(545)	(544)	(605)	(680)
Male partner displaced in t-1	1,666*	−830	−1,015*	−367	−883
	(879)	(545)	(544)	(605)	(680)
Male partner displaced in t	−14,710***	−16,945***	−19,471***	−17,566***	−18,613***
	(879)	(545)	(544)	(605)	(680)
Male partner displaced in t+1	−26,172***	−26,377***	−27,116***	−27,204***	−30,220***
	(879)	(545)	(544)	(605)	(680)
Male partner displaced in t+2	−22,810***	−20,898***	−24,107***	−23,696***	−25,387***
	(879)	(545)	(544)	(605)	(680)
Male partner displaced in t+3	−21,108***	−19,790***	−22,834***	−22,565***	−23,893***
	(879)	(545)	(544)	(605)	(680)
Demographic controls	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Individual fiixed effects	YES	YES	YES	YES	YES
Observations	881,768	853,176	809,928	768,176	723,512
Number of individuals	110,222	106,648	101,242	96,023	90,441

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the male partner has an annual income from work of at least 5,000 euro in the 4 years before the treatment and does not receive UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table 3.3: Treatment effect wage income female partner (added worker effect via wages)

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	−1	105	−182	151	220
	(136)	(155)	(182)	(226)	(299)
Male partner displaced in t-1	−31	96	−176	112	528*
	(136)	(155)	(182)	(226)	(299)
Male partner displaced in t	495***	607***	24	557**	669**
	(136)	(155)	(182)	(226)	(299)
Male partner displaced in t+1	926***	998***	225	849***	1,102***
	(136)	(155)	(182)	(226)	(299)
Male partner displaced in t+2	855***	858***	396**	729***	897***
	(136)	(155)	(182)	(226)	(299)
Male partner displaced in t+3	968***	970***	107	297	1,482***
	(136)	(155)	(182)	(226)	(299)
Demographic controls	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Individual fiixed effects	YES	YES	YES	YES	YES
Observations	999,744	982,384	966,104	940,136	912,104
Number of individuals	124,968	122,798	120,763	117,517	114,014

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−214	−178	53	−86	−26
	(313)	(188)	(187)	(217)	(181)
Male partner displaced in t-1	−392	−308	38	68	2
	(313)	(188)	(187)	(217)	(181)
Male partner displaced in t	−124	−99	285	293	604***
	(313)	(188)	(187)	(217)	(181)
Male partner displaced in t+1	195	344*	470**	585***	761***
	(313)	(188)	(187)	(217)	(181)
Male partner displaced in t+2	−77	294	501***	574***	992***
	(313)	(188)	(187)	(217)	(181)
Male partner displaced in t+3	−80	156	461**	718***	1,001***
	(313)	(188)	(187)	(217)	(181)
Demographic controls	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Individual fiixed effects	YES	YES	YES	YES	YES
Observations	881,768	853,176	809,928	768,176	723,512
Number of individuals	110,222	106,648	101,242	96,023	90,441

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the male partner has an annual income from work of at least 5,000 euro in the 4 years before the treatment and does not receive UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table 3.4: Treatment effect female partner's income from work and profits ('total' added worker effect)

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	-99	119	-138	352	216
	(155)	(174)	(201)	(248)	(323)
Male partner displaced in t-1	-131	161	-137	242	626*
	(155)	(174)	(201)	(248)	(323)
Male partner displaced in t	624***	618***	217	1,095***	842***
	(155)	(174)	(201)	(248)	(323)
Male partner displaced in t+1	1,152***	994***	506**	1,026***	1,703***
	(155)	(174)	(201)	(248)	(323)
Male partner displaced in t+2	1,122***	1,102***	749***	1,076***	1,393***
	(155)	(174)	(201)	(248)	(323)
Male partner displaced in t+3	1,313***	1,322***	798***	850***	2,151***
	(155)	(174)	(201)	(248)	(323)
Demographic controls	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Individual fiixed effects	YES	YES	YES	YES	YES
Observations	999,744	982,384	966,104	940,136	912,104
Number of individuals	124,968	122,798	120,763	117,517	114,014

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	115	-178	38	-26	12
	(336)	(204)	(203)	(236)	(201)
Male partner displaced in t-1	-3	-147	51	214	-104
	(336)	(204)	(203)	(236)	(201)
Male partner displaced in t	697**	174	283	673***	658***
	(336)	(204)	(203)	(236)	(201)
Male partner displaced in t+1	882***	727***	593***	1,064***	853***
	(336)	(204)	(203)	(236)	(201)
Male partner displaced in t+2	737**	661***	839***	1,100***	979***
	(336)	(204)	(203)	(236)	(201)
Male partner displaced in t+3	623*	565***	611***	1,272***	865***
	(336)	(204)	(203)	(236)	(201)
Demographic controls	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES
Individual fixed effects	YES	YES	YES	YES	YES
Observations	881,768	853,176	809,928	768,176	723,512
Number of individuals	110,222	106,648	101,242	96,023	90,441

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the male partner has an annual income from work of at least 5,000 euro in the 4 years before the treatment and does not receive UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

AWE of 0.6–1.5%. For 2008 and 2009, the start of the Great Recession, AWE estimates on female partner's wage income from work are statistically insignificant.[11] In line with the findings of Halla et al. (2018), depressed labour demand may have muted the AWE on realized income increases, as female partners could not find a job or extend their working hours. Finding a smaller AWE, during an economic downturn is also in line with Maloney (1987), Maloney (1991), Juhn and Potter (2007) and Bryan and Longhi (2013). Later on, from 2010 onwards, the AWE resurfaces.

We next broaden our analysis to income from profits of female partners as self-employed, defining the 'total AWE' as the effect on the sum of wage and profits. Table 3.4 presents this combined treatment effect on female partner's wage income and female partner's profits from self-employment. Again, the placebo dummies are typically small and statistically insignificant.[12] We find a total AWE for the different treatment years, in the order of 800-2,100 euro, which is 3–10% of male partner's income loss. Table A.3.3 shows the effects on mere profits, which contains the difference between Table 3.3 and 3.4. According to these estimates, there is a positive AWE via profits of the female partner rising to about 500 euro three years after the male entered UI.

Robustness checks and additional analyses 3.5.2

Some robustness checks and a heterogeneity analysis are given in the appendix to this paper. For expositional reasons, most tables in the appendix present our results on the 'total' AWE (that includes profit) for the years 2004, 2008 and 2012. Table A.3.4 shows the results for different model specifications. The first column presents the results when the model only controls for year fixed effects and a group dummy. Demographic controls are added in the second model and the third model gives our preferred model where we add individual fixed effects. The results hardly change over these three models. Table A.3.5 shows that the levels of

[11] However, Table 3.4 shows that we still find an AWE for 2008 en 2009 on female partner's profits.

[12] Again with the exception of the dummy for t-1 for male partners that become unemployed in 2007, where the placebo dummy is significant at the 10% level.

significance do not change when we use different levels of clustering of the standard errors.[13] We consider cluster-robust standard errors at the level of province, province interacted with ethnicity, individual and no clustering at all. Following Angrist and Pischke (2008), we prefer to be conservative by reporting the largest standard errors.

Next to considering couples where the male partner enters into UI, we also have estimated the total AWE induced by a large negative shock on male partner's (total) income (wages plus profits). Table A.3.6 and Table A.3.7 consider the AWE of a negative income shock of 20 and 50%, respectively, in total income of the male. Many of the pre-treatment placebo dummies are statistically significant for this treatment group, which violates the assumption of common time effects. Hence, this appears to be a problematic research strategy, and we do not consider the treatment effects. This violation of the assumption of common time effects when considering income shocks provides additional evidence that not finding significant pre-treatment placebo dummies for unemployment shocks means that the unemployment shocks are indeed exogenous as endogenous shocks would cause significant anticipation effects. As another robustness test, we also varied our sample by using different threshold values for the male partners earned income. As Table A.3.8 shows, excluding couples in which the male partners earned an income of less than 0, 5,000 or 15,000 euro in the years before the male partner became unemployed yields similar AWE estimates.[14] We also find a similar AWE when we shorten our samples to 6 year periods in which we observe couples that are together and observed in the data for 6 years, see Table A.3.10.[15] Using 6-year samples also allows

[13] The exception is the placebo for $t-1$ for 2008 that changes from statistically significant at the 10% level in our preferred specification with 'clustering' at the individual level to insignificant with the other levels of clustering.

[14] In Table A.3.9 we exclude couples working in the same sector, so that the AWE is not contaminated by common sectoral shocks. This yields AWE estimates that are slightly larger (one tenth to one fifth), indicating that we may underestimate the AWE somewhat in our base specification because of common sectoral shocks (Hardoy and Schøne 2014).

[15] Using a 6 year rather than an 8 year period addresses the concern that our samples may not be representative for the full population. About 40% of our couples are excluded from our samples because they do not stay together for 8 years or are not observed during the full 8-year period. Finding a slightly smaller AWE for our 6 year samples suggests a slightly lower willingness to compensate for each others income shocks when partners are together for a shorter period.

us to study the effect for the years 2013 and 2014, for which we find an AWE of 700 and 510 euro one year after the male entered UI.

We also analyze whether the AWE has operated mainly at the extensive or the intensive margin. Tables A.3.11 and A.3.12 give the treatment effect on female partner's income from work at the extensive and the intensive margin, respectively. The extensive margin refers to the increase in employment by female partners who didn't work, whereas the intensive margin refers to the intensity of work supplied by female partners already in work. In the current context, the extensive margin effect gives the effect on female partner's wage income for a sample of households in which the female partner was not employed in year t-4. The intensive margin effect gives the effect on female partner's wage income for the remaining sample of households in which the female partner was employed in year t-4. Generally, extensive margin effects are larger than intensive margin effects for the treatment years 2003-2009. For the treatment years 2010-2012, however, extensive margin effects seem absent.[16] When interpreting these findings, one should bear in mind that there was a strong increase in the female employment rate in the time period under consideration. This trend may have limited the room for extensive margin effects over time.

In addition, Table A.3.12 shows no evidence of intensive margin effects during the first years of the Great Recession (2008-2010), whereas the extensive margin effect is not affected by the business cycle. This is in line with Bredtmann et al. (2018), who argue that firms might first cut down the working hours of those already employed, before having to rely on layoffs to reduce their overall costs. These hoarding effects may render it difficult to increase hours worked in the firm in which someone is employed than to find a job at another firm during the beginning of a recession.

To shed more light on intensive and extensive margin effects, Table A.3.14 shows the effect on female partner's participation instead of female partner's income. Participation is measured by either being employed or

[16]We have to interpret the results of Table A.3.11 for the treatment year 2012 with the appropriate care, as we find counter-intuitive negative treatment effects as well as a negative statistically significant pre-treatment placebo dummy. We do not find negative effects when we consider the extensive margin effect on participation (rather than on income) – see Table A.3.13.

having an income from profits. For most treatment years the treatment effect estimates of the participation rate are about 1–2 percentage points for the full sample, which is 1–3% relative to the participation rate in the years before entering UI.[17] Table A.3.15 shows that the treatment effect on female partner's annual hours worked for the treatment years 2010-2012 is 21-43 hours three years after the treatment.[18] This is 2-5% relative to the hours worked in the years before entering UI.

Finally, we study the AWE for various demographic and income groups for the treatment years 2004, 2008 and 2012. Table A.3.16 gives the AWE for different age groups. For the treatment years 2004 and 2012, we find a larger AWE for young (25–35) and middle aged (36–45) women, but no AWE effect for women 46-55 years of age. For the treatment year 2008, there only is evidence for AWE for the middle aged but not for the young. Hence, not finding an overall AWE on wage income for 2008 can be explained by not finding an AWE for the young (25–35). The reason for this may be that it was more difficult for young individuals to increase employment at the beginning of the Great Recession. Table A.3.17 shows the AWE for couples with and without children. The AWE for couples with children is about half the size of the AWE for couples without children. A plausible explanation is that the costs of changing roles within the household are larger when couples have children. Table A.3.18 presents the AWE for women with a low, middle or high level of education. For high educated women, we find a higher AWE and for low educated women we find no AWE at all. This could be explained by difficulties for low educated women to find a job, especially if they have not been employed for years. Table A.3.19 gives the AWE for female partners with different ethnicities. The largest effects are obtained for natives and Western-immigrants and no effect for Non-Western immigrants. For the treatment year 2008, the treatment effect on female partner's income for Western and Non-Western immigrants is negative. This may be explained by correlated shocks for male and female partner, as immigrants may be disproportionately affected at the beginning of the Great Recession.

[17] Table A.3.13 shows that the effects on participation for the extensive margin sample, consisting of women who did not yet work in t-4, is 3–7 percentage points.

[18] Data on hours worked is only available for the shorter period 2006-2015.

Finally, Table A.3.20 shows the AWE for women with male partners within different income groups (measured before unemployment shock). The AWE increases with the income of the male partner (before unemployment shock). This larger AWE for women with high-income partners could be explained by a larger income shock for these households.

How much of the income shock is covered? 3.5.3

Following Hardoy and Schøne (2014), we consider how much of the income shock from unemployment is covered by various types of benefits and other sources of income, such as the AWE, and how much remains uncovered. To ease the exposition, we only report results for a number of representative years: 2004, 2008 and 2012; these are shown in Table 3.5, 3.6 and 3.7, respectively.

Table 3.5 shows the effect of a male worker entering UI on different income sources, for treatment year 2004. Column (1) shows a negative effect on male partner's wage income of –19,532 euro in the year after becoming unemployed, which then becomes less negative over time, to –13,733 three years after entering UI. Income from self-employment for the worker increase up to 2,139 euro three years after entering UI (column (2)). UI benefits compensate 8,777 euro of the wage loss in the year of the unemployment shock, but this drops to only 2,376 euro three years after the unemployment shock (column (3)). Treatment effects on income from welfare benefits, disability benefits and other benefits, which are relatively small, are given in columns (4), (5) and (6). The AWE operating via wage and profit income is presented in columns (7) and (8), respectively. Three years after the unemployment shock, the AWE from wage income is 970 euro and from profits is 352 euro. Finally, column (9) gives the total amount of the wage income loss that is covered. The total compensated amount is 10,254 euro in year t, 11,103 euro in year t+1 and this decreases to 6,598 euro in t+3. This implies that about 78% of the income loss is compensated in the year of the unemployment shock, and subsequently decreases to 48% of the remaining wage income shock 3 years after entering UI. The main reason for the lower 'coverage rate' is the drop in UI benefits. The

Table 3.5: Effect of male partner becoming unemployed in 2004 on different income sources

	(1) Wage man 2004	(2) Profit man 2004	(3) Unemp. benefits man 2004	(4) Welfare benefits man 2004	(5) Disab. benefits man 2004	(6) Other benefits man 2004
Male partner displaced in t-2	−560	−243	−0	0	−0	−1
	(463)	(166)	(35)	(2)	(25)	(39)
Male partner displaced in t-1	−273	−365**	−0	0	−1	−2
	(463)	(166)	(35)	(2)	(25)	(39)
Male partner displaced in t	−13,176***	143	8,777***	0	174***	542***
	(463)	(166)	(35)	(2)	(25)	(39)
Male partner displaced in t+1	−19,532***	1,181***	7,859***	7***	177***	885***
	(463)	(166)	(35)	(2)	(25)	(39)
Male partner displaced in t+2	−15,953***	1,679***	4,481***	18***	169***	787***
	(463)	(166)	(35)	(2)	(25)	(39)
Male partner displaced in t+3	−13,733***	2,139***	2,376***	27***	232***	502***
	(463)	(166)	(35)	(2)	(25)	(39)
Demographic controls	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Individual fixed effects	YES	YES	YES	YES	YES	YES
Observations	982,384	982,384	982,384	982,384	982,384	982,384
Number of individuals	122,798	122,798	122,798	122,798	122,798	122,798

	(7) Wage woman 2004	(8) Profit woman 2004	(9) Total Comp. 2004	(10) Total Comp. in % 2004
Male partner displaced in t-2	105	15	−125	
	(155)	(101)	(256)	
Male partner displaced in t-1	96	65	−207	
	(155)	(101)	(256)	
Male partner displaced in t	607***	11	10,254***	77.8%
	(155)	(101)	(256)	
Male partner displaced in t+1	998***	−4	11,103***	56.8%
	(155)	(101)	(256)	
Male partner displaced in t+2	858***	243**	8,236***	51.6%
	(155)	(101)	(256)	
Male partner displaced in t+3	970***	352***	6,598***	48.0%
	(155)	(101)	(256)	
Demographic controls	YES	YES	YES	
Year fixed effects	YES	YES	YES	
Individual fiixed effects	YES	YES	YES	
Observations	982,384	982,384	982,384	
Number of individuals	122,798	122,798	122,798	

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table 3.6: Effect of male partner becoming unemployed in 2008 on different income sources

	(1) Wage man 2008	(2) Profit man 2008	(3) Unemp. benefits man 2008	(4) Welfare benefits man 2008	(5) Disab. benefits man 2008	(6) Other benefits man 2008
Male partner displaced in t-2	−48	46	−2	0	−1	−1
	(879)	(312)	(71)	(5)	(38)	(59)
Male partner displaced in t-1	1,666*	37	−2	0	−1	−1
	(879)	(312)	(71)	(5)	(38)	(59)
Male partner displaced in t	−14,710***	1,031***	8,139***	0	−14	353***
	(879)	(312)	(71)	(5)	(38)	(59)
Male partner displaced in t+1	−26,172***	2,578***	9,678***	10**	10	1,054***
	(879)	(312)	(71)	(5)	(38)	(59)
Male partner displaced in t+2	−22,810***	3,410***	5,772***	64***	97**	1,133***
	(879)	(312)	(71)	(5)	(38)	(59)
Male partner displaced in t+3	−21,108***	2,986***	3,430***	123***	256***	1,112***
	(879)	(312)	(71)	(5)	(38)	(59)
Demographic controls	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Individual fiixed effects	YES	YES	YES	YES	YES	YES
Observations	881,768	881,768	881,768	881,768	881,768	881,768
Number of individuals	110,222	110,222	110,222	110,222	110,222	110,222

	(7) Wage woman 2008	(8) Profit woman 2008	(9) Total Comp. 2008	(10) Total Comp. in % 2008
Male partner displaced in t-2	−214	330*	156	
	(313)	(182)	(478)	
Male partner displaced in t-1	−392	389**	31	
	(313)	(182)	(478)	
Male partner displaced in t	−124	821***	10,206***	69.4%
	(313)	(182)	(478)	
Male partner displaced in t+1	195	687***	14,213***	54.3%
	(313)	(182)	(478)	
Male partner displaced in t+2	−77	814***	11,213***	49.2%
	(313)	(182)	(478)	
Male partner displaced in t+3	−80	703***	8,530***	40.4%
	(313)	(182)	(478)	
Demographic controls	YES	YES	YES	
Year fixed effects	YES	YES	YES	
Individual fiixed effects	YES	YES	YES	
Observations	881,768	881,768	881,768	
Number of individuals	110,222	110,222	110,222	

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table 3.7: Effect of male partner becoming unemployed in 2012 on different income sources

	(1) Wage man 2012	(2) Profit man 2012	(3) Unemp. benefits man 2012	(4) Welfare benefits man 2012	(5) Disab. benefits man 2012	(6) Other benefits man 2012
Male partner displaced in t-2	−599	108	−2	0	−0	−1
	(680)	(190)	(56)	(3)	(26)	(129)
Male partner displaced in t-1	−883	−436**	−2	0	−0	−2
	(680)	(190)	(56)	(3)	(26)	(129)
Male partner displaced in t	−18,613***	469**	10,968***	−0	−14	906***
	(680)	(190)	(56)	(3)	(26)	(129)
Male partner displaced in t+1	−30,220***	2,747***	11,654***	14***	−36	717***
	(680)	(190)	(56)	(3)	(26)	(129)
Male partner displaced in t+2	−25,387***	3,997***	6,865***	51***	4	839***
	(680)	(190)	(56)	(3)	(26)	(129)
Male partner displaced in t+3	−23,893***	4,499***	3,110***	127***	304***	374***
	(680)	(190)	(56)	(3)	(26)	(129)
Demographic controls	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Individual fiixed effects	YES	YES	YES	YES	YES	YES
Observations	723,512	723,512	723,512	723,512	723,512	723,512
Number of individuals	90,441	90,441	90,441	90,441	90,441	90,441

	(7) Wage woman 2012	(8) Profit woman 2012	(9) Total Comp. 2012	(10) Total Comp. in% 2012
Male partner displaced in t-2	−26	38	116	
	(181)	(129)	(319)	
Male partner displaced in t-1	2	−105	−544*	
	(181)	(129)	(319)	
Male partner displaced in t	604***	54	12,986***	69.8%
	(181)	(129)	(319)	
Male partner displaced in t+1	761***	92	15,949***	52.8%
	(181)	(129)	(319)	
Male partner displaced in t+2	992***	−13	12,735***	50.2%
	(181)	(129)	(319)	
Male partner displaced in t+3	1,001***	−137	9,279***	38.8%
	(181)	(129)	(319)	
Demographic controls	YES	YES	YES	
Year fixed effects	YES	YES	YES	
Individual fiixed effects	YES	YES	YES	
Observations	723,512	723,512	723,512	
Number of individuals	90,441	90,441	90,441	

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

AWE covered only 10% of the remaining wage income shock 3 years after entering UI, which is only a fraction of the shock.

Table 3.6 provides the results for couples where the male enters UI in 2008, the year the Great Recession started. The negative treatment effects on the wage income of the male are larger and more persistent than in 2004. Compensation from the UI of the male partner increases as well, but decreases as a percentage of the wage income shock. Compensation from the profit income from the male partners increases. There is no significant AWE from wage income of the female, as noted before, though there does appear to be a positive AWE from profit income.[19] The total compensated amount is higher in 2008 compared to 2004, but is a smaller percentage of the (larger) loss in wage income of the male, leaving a larger part of this negative shock uncompensated.

Finally, Table 3.7 gives the results for couples where the male enters UI in 2012, which was the second period ('double dip') of the Great Recession in the Netherlands. The loss in wage income of the male is larger than for 2008, but the treatment effect on male partner's profits is also larger than in the earlier years, rising to 4,499 euros three years after entering UI. It thus seems that the extent to which self-employment contributes to compensating male partner's wage loss has increased over time. We further find that for the 2012 period, the AWE returns.

Conclusion 3.6

In this paper we have studied the AWE in the Netherlands before and during the Great Recession, using a large and rich administrative panel dataset for the period 1999-2015. We have used a differences-in-differences setup with couples where the men enter UI as the treatment group and couples where the men do not enter UI as the control group. We find a negative and persistent effect of the male partner's unemployment shock on his income from work, of about 25 thousand euro one year after becoming unemployed. This corresponds to more than 50% of his income

[19] However, the statistically significant placebo for the women's profit income suggests that we should interpret this latter AWE with the appropriate care.

before becoming unemployed. This loss in wage income from the male leads to a small positive added worker effect on the wage income of the females of about 500-1,000 euro, which compensates 2-5% of the income loss of the male partner. The AWE estimate on wage income is statistically insignificant during the first period of the Great Recession (2008-2009), but resurfaces during the second period of the Great Recession (2010-2015). The AWE at the extensive margin decreased over time, probably because of the strong increase in female employment in the time period under consideration. We also find that profit income becomes a more important insurance tool for dealing with negative wage income shocks over time, from 2,139 euro 3 years after the unemployment shock in 2004 to 4,499 euro 3 years after the unemployment shock in 2012. Finally, when we consider all sources of compensation, including different types of benefits, only 40-50% of the wage income loss from unemployment is compensated three years after entering UI.

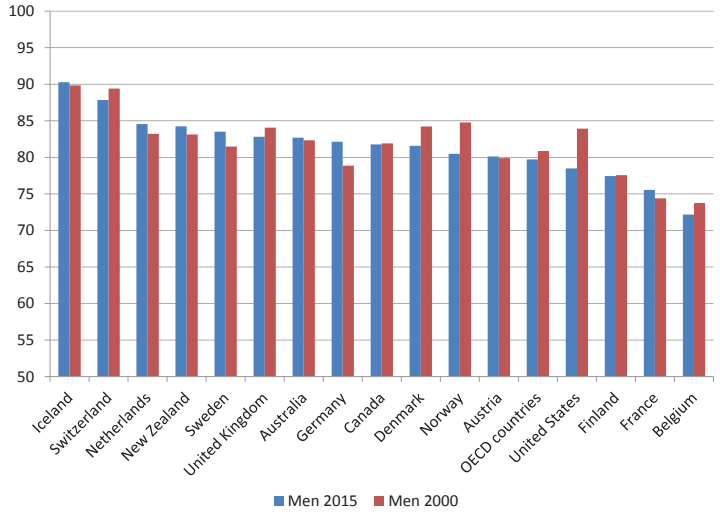
In this paper we have looked at the AWE for couples where the man enters UI, whereas most of the literature has focused on mass layoffs. Our approach yields much larger impulse estimates that are expected to cause larger behavioural responses. We show that our approach of entering UI also gives plausibly exogenous variation in male incomes. In future research it would be interesting to split the analysis of individuals that are laid off during mass layoffs into workers that go straight to another job and workers that first go through UI, and consider whether there is a different AWE for these groups. Decomposing these effects gives insights in the importance of the size of the income shock for the AWE. Further, we study the AWE on employment and income and not on labor supply or job search effort. Future research is needed to study if the AWE is small (even for the large negative shocks we study) because of small labor supply responses or because increases in labor supply do not translate into increases in employment (Juhn and Potter 2007; Bryan and Longhi 2013). The latter could be explained by difficulties in finding a job or getting working hours extended. This is expected to be particularly relevant during an economic downturn because demand side constraints are making it harder to find a job or to increase working hours. Studying both labor supply responses and employment responses in one study

could resolve that some studies (e.g. Bredtmann et al. (2018)) find the AWE to be larger when unemployment is higher and others find the AWE to be smaller when unemployment is higher (e.g. Halla et al. (2018)).

Furthermore, in this paper we have focused on the effect of entering UI by the male on subsequent income and employment of the male and the female. Future research could look at the effect of entering UI on other outcomes, like the stability of relations and fertility, as in Halla et al. (2018), and outcomes like health and happiness.

3.A Supplementary material

Figure A.3.1: Labour force participation rate for men



Notes: Using data from OECD (2018b)

Table A.3.1: Literature review:AWE

Study	Country	Period	Data	Empirical method	Findings
Heckman and MaCurdy (1980)	USA	1968-1975	Panel data PSID	Fixed effects Tobit model	No AWE, no evidence of a married female labour supply response to transitory income variation
Lundberg (1985)	USA	1969-1973	Panel data (monthly) SIME/DIME	Dynamic simulation of estimated employment transition probabilities	2 extra wives employed for 100 additional men unemployed No AWE for black families
Maloney (1987)	USA	1975	Cross-section from PSID	Censored Tobit model	638 hours increase (single censored model), 1706 hours increase (double censored model)
Maloney (1991)	USA	1982	Cross-section from PSID	Double selection model	No AWE, but wives with frequently unemployed husbands have lower reservation wages
Cullen and Gruber (2000)	USA	1984-1988 1990-92	Panel data SIPP	TSLS potential UI as instrument	Wives of unemployed husbands would work 30% more hours if there were no UI income
Stephens (2002)	USA	1968-92	Panel data PSID	Linear fixed effect model and a censored dependent variable model	Long-run AWE increases compensate for over 25% of the husbands'lost income
Juhn and Potter (2007)	USA	1968-2005	Pooled cross-sections CPS files	Simulated employment-pop. ratios using couples' joint transition matrix	AWE: 5.6% point higher chance entering the labour market during expansion and 9.1% during recession
Hardoy and Schøne (2014)	Norway	2000-2005	Panel data	Differences-in-differences	No AWE for full sample, 1% increase in income for wives working in different industries and 2% for wives who did not work full-time
Starr (2014)	USA	2007-2009	Pooled cross-section ACS	Differences-in-differences	4.0% increase in change of being employed, 2.6% increase in chance of being unemployed
Bredtmann et al. (2018)	28 EU-countries	2004-2013	4-year rotating panel EU-SILC	Probit models	Increase in labor supply, job search and hours worked, but no increase in employment rate. Cont. EU (incl. NL): only intens. marg. effect
Halla et al. (2018)	Austria	1990-2007	Panel data ASSD	Differences-in-differences	AWE: 1–2% increase in earnings as well as in employment rate

Table A.3.2: Treatment effect on male partner's employment probability

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	−0.000	0.000	0.000	−0.000	−0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Male partner displaced in t-1	−0.000	0.000	0.000	−0.000	−0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Male partner displaced in t	−0.092***	−0.082***	−0.100***	−0.067***	−0.031***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Male partner displaced in t+1	−0.235***	−0.211***	−0.211***	−0.220***	−0.182***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Male partner displaced in t+2	−0.193***	−0.159***	−0.167***	−0.180***	−0.184***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Male partner displaced in t+3	−0.161***	−0.133***	−0.152***	−0.181***	−0.189***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Observations	917,712	904,704	891,112	868,920	844,944
Number of individuals	114,714	113,088	111,389	108,615	105,618

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−0.000	0.000	0.000	0.000	−0.000
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Male partner displaced in t-1	−0.000	0.000	0.000	0.000	0.000
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Male partner displaced in t	−0.051***	−0.033***	−0.032***	−0.034***	−0.038***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Male partner displaced in t+1	−0.217***	−0.212***	−0.219***	−0.237***	−0.286***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Male partner displaced in t+2	−0.201***	−0.160***	−0.202***	−0.218***	−0.224***
	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Male partner displaced in t+3	−0.183***	−0.150***	−0.203***	−0.200***	−0.180***
	(0.0042)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	817,688	779,560	743,528	661,744	581,808
Number of individuals	102,211	97,445	92,941	82,718	72,726

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.3: Treatment effect on female partner's income from profit

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	−98	15	44	200	−4
	(91)	(101)	(117)	(145)	(175)
Male partner displaced in t-1	−100	65	38	130	98
	(91)	(101)	(117)	(145)	(175)
Male partner displaced in t	129	11	193*	538***	173
	(91)	(101)	(117)	(145)	(175)
Male partner displaced in t+1	226**	−4	281**	177	600***
	(91)	(101)	(117)	(145)	(175)
Male partner displaced in t+2	267***	243**	354***	347**	496***
	(91)	(101)	(117)	(145)	(175)
Male partner displaced in t+3	345***	352***	692***	553***	669***
	(91)	(101)	(117)	(145)	(175)
Observations	999,744	982,384	966,104	940,136	912,104
Number of individuals	124,968	122,798	120,763	117,517	114,014

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	330*	0	−15	60	38
	(182)	(118)	(120)	(141)	(129)
Male partner displaced in t-1	389**	160	13	146	−105
	(182)	(118)	(120)	(141)	(129)
Male partner displaced in t	821***	273**	−2	380***	54
	(182)	(118)	(120)	(141)	(129)
Male partner displaced in t+1	687***	383***	124	479***	92
	(182)	(118)	(120)	(141)	(129)
Male partner displaced in t+2	814***	367***	338***	526***	−13
	(182)	(118)	(120)	(141)	(129)
Male partner displaced in t+3	703***	409***	150	554***	−137
	(182)	(118)	(120)	(141)	(129)
Observations	881,768	853,176	809,928	768,176	723,512
Number of individuals	110,222	106,648	101,242	96,023	90,441

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the male partner has an annual income from work of at least 5,000 euro in the 4 years before the treatment and does not receive UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.4: Treatment effect on female partner's income (wage+profit) - different models

	(1)	(2)	(3)
	2004	2004	2004
Male partner displaced in t-2	119	118	119
	(175)	(174)	(174)
Male partner displaced in t-1	157	162	161
	(175)	(174)	(174)
Male partner displaced in t	591***	616***	618***
	(175)	(174)	(174)
Male partner displaced in t+1	950***	990***	994***
	(175)	(174)	(174)
Male partner displaced in t+2	1,046***	1,097***	1,102***
	(175)	(174)	(174)
Male partner displaced in t+3	1,260***	1,319***	1,322***
	(175)	(174)	(174)
Year fixed effects	YES	YES	YES
Demographic controls (age)	NO	YES	YES
Fixed Effects	NO	NO	YES
Observations	982,384	982,384	982,384
Number of individuals	122,798	122,798	122,798
	(4)	(5)	(6)
	2008	2008	2008
Male partner displaced in t-2	121	117	115
	(336)	(336)	(336)
Male partner displaced in t-1	3	−2	−3
	(336)	(336)	(336)
Male partner displaced in t	700**	696**	697**
	(336)	(336)	(336)
Male partner displaced in t+1	882***	883***	882***
	(336)	(336)	(336)
Male partner displaced in t+2	728**	737**	737**
	(336)	(336)	(336)
Male partner displaced in t+3	602*	624*	623*
	(336)	(336)	(336)
Year fixed effects	YES	YES	YES
Demographic controls (age)	NO	YES	YES
Individual fixed effects	NO	NO	YES
Observations	881,768	881,768	881,768
Number of individuals	110,222	110,222	110,222
	(7)	(8)	(9)
	2012	2012	2012
Male partner displaced in t-2	22	13	12
	(201)	(201)	(201)
Male partner displaced in t-1	−99	−103	−104
	(201)	(201)	(201)
Male partner displaced in t	658***	657***	658***
	(201)	(201)	(201)
Male partner displaced in t+1	840***	852***	853***
	(201)	(201)	(201)
Male partner displaced in t+2	959***	976***	979***
	(201)	(201)	(201)
Male partner displaced in t+3	831***	863***	865***
	(201)	(201)	(201)
Year fixed effects	YES	YES	YES
Demographic controls (age)	NO	YES	YES
Fixed Effects	NO	NO	YES
Observations	723,512	723,512	723,512
Number of individuals	90,441	90,441	90,441

Notes: *denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies and a group dummy.

Table A.3.5: Treatment effect on female partner's income from work - different ways of clustering standard errors

	(1)	(2)	(3)	(4)
	2004	2004	2004	2004
Level of clustering	None	Individual	Province	Province* ethnicity
Male partner displaced in t-2	105	105	104	104
	(155)	(121)	(146)	(130)
Male partner displaced in t-1	96	96	77	77
	(155)	(146)	(130)	(161)
Male partner displaced in t	607***	607***	605***	605***
	(155)	(178)	(132)	(196)
Male partner displaced in t+1	998***	998***	805***	805***
	(155)	(207)	(160)	(176)
Male partner displaced in t+2	858***	858***	715***	715***
	(155)	(217)	(142)	(190)
Male partner displaced in t+3	970***	970***	804***	804***
	(155)	(252)	(144)	(173)
Observations	982,384	982,392	942,624	942,624
Number of individuals	122,798	122,799	117,828	117,828

	(5)	(6)	(7)	(8)
	2008	2008	2008	2008
Level of clustering	None	Individual	Province	Province* ethnicity
Male partner displaced in t-2	−214	−214	−151	−151
	(313)	(194)	(162)	(159)
Male partner displaced in t-1	−392	−392*	−333	−333
	(313)	(231)	(188)	(202)
Male partner displaced in t	−124	−124	−100	−100
	(313)	(303)	(198)	(247)
Male partner displaced in t+1	195	195	106	106
	(313)	(418)	(418)	(330)
Male partner displaced in t+2	−77	−77	−115	−115
	(313)	(450)	(457)	(358)
Male partner displaced in t+3	−80	−80	−111	−111
	(313)	(471)	(443)	(405)
Observations	881,768	881,768	855,608	855,608
Number of individuals	110,222	110,222	106,951	106,951

	(9)	(10)	(11)	(12)
	2012	2012	2012	2012
Level of clustering	None	Individual	Province	Province* ethnicity
Male partner displaced in t-2	−26	−26	−13	−13
	(181)	(120)	(106)	(121)
Male partner displaced in t-1	2	2	13	13
	(181)	(169)	(184)	(198)
Male partner displaced in t	604***	604***	588**	588**
	(181)	(200)	(217)	(220)
Male partner displaced in t+1	761***	761***	706***	706***
	(181)	(225)	(206)	(231)
Male partner displaced in t+2	992***	992***	955***	955***
	(181)	(256)	(253)	(273)
Male partner displaced in t+3	1,001***	1,001***	944***	944***
	(181)	(280)	(291)	(307)
Observations	723,512	723,512	710,456	710,456
Number of individuals	90,441	90,441	88,807	88,807

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.6: Treatment effect of male partner's income shock of 20% on female partner's income (wage+profit)

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	2	80	−160	64	203
	(104)	(115)	(121)	(134)	(159)
Male partner displaced in t-1	237**	90	237**	543***	435***
	(104)	(115)	(121)	(134)	(159)
Male partner displaced in t	210**	60	−358***	−41	−202
	(104)	(115)	(121)	(134)	(159)
Male partner displaced in t+1	672***	491***	411***	381***	71
	(104)	(115)	(121)	(134)	(159)
Male partner displaced in t+2	800***	691***	609***	519***	96
	(104)	(115)	(121)	(134)	(159)
Male partner displaced in t+3	935***	855***	503***	517***	318**
	(104)	(115)	(121)	(134)	(159)
Observations	999,744	982,384	966,104	940,136	912,104
Number of individuals	124,968	122,798	120,763	117,517	114,014

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−118	−379***	−599***	−234	55
	(166)	(146)	(148)	(172)	(157)
Male partner displaced in t-1	15	−257*	−285*	143	48
	(166)	(146)	(148)	(172)	(157)
Male partner displaced in t	−820***	−1,015***	−822***	−21	64
	(166)	(146)	(148)	(172)	(157)
Male partner displaced in t+1	95	−543***	−109	818***	567***
	(166)	(146)	(148)	(172)	(157)
Male partner displaced in t+2	−20	61	121	1,222***	727***
	(166)	(146)	(148)	(172)	(157)
Male partner displaced in t+3	2	378***	166	1,175***	855***
	(166)	(146)	(148)	(172)	(157)
Observations	881,768	853,176	809,928	768,176	723,512
Number of individuals	110,222	106,648	101,242	96,023	90,441

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.7: Treatment effect of male partner's income shock of 50% on female partner's income (wage+profit)

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	138	−79	−81	−37	736***
	(158)	(180)	(184)	(206)	(272)
Male partner displaced in t-1	264*	−11	350*	763***	838***
	(158)	(180)	(184)	(206)	(272)
Male partner displaced in t	298*	−435**	−581***	−533***	−701***
	(158)	(180)	(184)	(206)	(272)
Male partner displaced in t+1	911***	363**	399**	198	−474*
	(158)	(180)	(184)	(206)	(272)
Male partner displaced in t+2	1,312***	358**	914***	780***	−27
	(158)	(180)	(184)	(206)	(272)
Male partner displaced in t+3	1,51***	258	690***	844***	431
	(158)	(180)	(184)	(206)	(272)
Observations	999,744	982,384	966,104	940,136	912,104
Number of individuals	124,968	122,798	120,763	117,517	114,014

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−55	−786***	−391	−891***	393
	(264)	(236)	(241)	(294)	(267)
Male partner displaced in t-1	−96	−372	78	−266	−256
	(264)	(236)	(241)	(294)	(267)
Male partner displaced in t	−1,777***	−1,957***	−742***	−1,126***	−760***
	(264)	(236)	(241)	(294)	(267)
Male partner displaced in t+1	519**	−586**	86	307	−125
	(264)	(236)	(241)	(294)	(267)
Male partner displaced in t+2	237	−119	576**	756**	483*
	(264)	(236)	(241)	(294)	(267)
Male partner displaced in t+3	125	−246	502**	564*	1,145***
	(264)	(236)	(241)	(294)	(267)
Observations	881,768	853,176	809,928	768,176	723,512
Number of individuals	110,222	106,648	101,242	96,023	90,441

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.8: Treatment effect on female partner's income from work - different samples

	(1)	(2)	(3)
	2004	2004	2004
Male partners income	>0	>5000	>15000
Male partner displaced in t-2	104	105	100
	(158)	(155)	(146)
Male partner displaced in t-1	107	96	80
	(158)	(155)	(146)
Male partner displaced in t	644***	607***	605***
	(158)	(155)	(146)
Male partner displaced in t+1	1,001***	998***	1,03***
	(158)	(155)	(146)
Male partner displaced in t+2	806***	858***	878***
	(158)	(155)	(146)
Male partner displaced in t+3	1,052***	970***	995***
	(158)	(155)	(146)
Observations	1.131.768	982,384	964,352
Number of individuals	141,471	122,798	120,544

	(4)	(5)	(6)
	2008	2008	2008
Male partners income	>0	>5000	>15000
Male partner displaced in t-2	−80	−214	−229
	(309)	(313)	(299)
Male partner displaced in t-1	−103	−392	−436
	(309)	(313)	(299)
Male partner displaced in t	212	−124	−143
	(309)	(313)	(299)
Male partner displaced in t+1	541*	195	194
	(309)	(313)	(299)
Male partner displaced in t+2	198	−77	−76
	(309)	(313)	(299)
Male partner displaced in t+3	175	−80	−138
	(309)	(313)	(299)
Observations	1,021,432	881,768	866,064
Number of individuals	127,680	110,222	108,259

	(7)	(8)	(9)
	2012	2012	2012
Male partners income	>0	>5000	>15000
Male partner displaced in t-2	53	−26	−14
	(187)	(182)	(183)
Male partner displaced in t-1	70	2	−6
	(187)	(182)	(183)
Male partner displaced in t	1,011***	921***	953***
	(186)	(181)	(182)
Male partner displaced in t+1	175	209	239
	(186)	(181)	(182)
Male partner displaced in t+2	717***	751***	785***
	(186)	(181)	(182)
Male partner displaced in t+3	1,395***	1,479***	1,478***
	(186)	(181)	(182)
Observations	849,136	723,512	714,008
Number of individuals	106,144	90,441	89,253

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.9: Treatment effect on female partner's income from work in sample without partners working in the same sector in the years before husband's unemployment shock

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	24	109	−188	183	129
	(134)	(155)	(179)	(212)	(265)
Male partner displaced in t-1	26	140	−178	183	641**
	(134)	(155)	(179)	(212)	(265)
Male partner displaced in t	548***	714***	82	581***	708***
	(134)	(155)	(179)	(212)	(265)
Male partner displaced in t+1	1,025***	1,094***	426**	996***	1,187***
	(134)	(155)	(179)	(212)	(265)
Male partner displaced in t+2	960***	944***	539***	931***	901***
	(134)	(155)	(179)	(212)	(265)
Male partner displaced in t+3	1,089***	1,083***	249	474**	1,327***
	(134)	(155)	(179)	(212)	(265)
Observations	918,288	901,264	884,776	862,096	835,632
Number of individuals	114,786	112,658	110,597	107,762	104,455

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−140	−101	62	79	18
	(301)	(178)	(171)	(221)	(182)
Male partner displaced in t-1	−360	−223	37	207	179
	(301)	(178)	(171)	(221)	(182)
Male partner displaced in t	−118	94	336**	594***	787***
	(301)	(178)	(171)	(221)	(182)
Male partner displaced in t+1	333	597***	586***	829***	1,066***
	(301)	(178)	(171)	(221)	(182)
Male partner displaced in t+2	19	606***	574***	798***	1,272***
	(301)	(178)	(171)	(221)	(182)
Male partner displaced in t+3	41	493***	543***	935***	1,245***
	(301)	(178)	(171)	(221)	(182)
Observations	807,448	781,200	745,672	705,632	665,848
Number of individuals	100,932	97,651	93,210	88,205	83,233

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.10: Treatment effect on female partner's income from work in 6-year samples

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	−20	93	−199	133	237
	(119)	(138)	(160)	(190)	(258)
Male partner displaced in t-1	−14	112	−171	82	531**
	(119)	(138)	(160)	(190)	(258)
Male partner displaced in t	468***	558***	28	486**	695***
	(119)	(138)	(160)	(190)	(258)
Male partner displaced in t+1	853***	951***	240	717***	1,026***
	(119)	(138)	(160)	(190)	(258)
Observations	767,820	757,242	747,294	730,152	709,398
Number of individuals	127,970	126,207	124,549	121,692	118,233

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−259	−182	133	−95	−59
	(289)	(176)	(174)	(173)	(157)
Male partner displaced in t-1	−458	−336*	85	41	−60
	(289)	(176)	(174)	(173)	(157)
Male partner displaced in t	−165	−78	383**	322*	532***
	(289)	(176)	(174)	(173)	(157)
Male partner displaced in t+1	130	293*	701***	538***	687***
	(289)	(176)	(174)	(173)	(157)

	(11) 2013	(12) 2014
Male partner displaced in t-2	33	−78
	(175)	(136)
Male partner displaced in t-1	96	−106
	(175)	(136)
Male partner displaced in t	486***	283**
	(175)	(136)
Male partner displaced in t+1	700***	510***
	(175)	(136)
Observations	522,888	795,834
Number of individuals	87,149	132,639

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.11: Treatment effect on female partner's income from work - extensive margin

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	165	23	17	−118	534
	(167)	(186)	(230)	(264)	(340)
Male partner displaced in t-1	317*	119	294	56	208
	(167)	(186)	(230)	(264)	(340)
Male partner displaced in t	644***	763***	371	665**	762**
	(167)	(186)	(230)	(264)	(340)
Male partner displaced in t+1	768***	1,188***	813***	1,093***	980***
	(167)	(186)	(230)	(264)	(340)
Male partner displaced in t+2	1,024***	1,141***	1,090***	971***	789**
	(167)	(186)	(230)	(264)	(340)
Male partner displaced in t+3	1,238***	1,280***	1,195***	586**	1,135***
	(167)	(186)	(230)	(264)	(340)
Observations	285,808	257,376	220,560	204,632	191,560
Number of individuals	35,726	32,172	27,570	25,579	23,945

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−49	62	247	308	−393
	(375)	(245)	(251)	(311)	(281)
Male partner displaced in t-1	−17	13	565**	−162	−488*
	(375)	(245)	(251)	(311)	(281)
Male partner displaced in t	57	414*	772***	25	−516*
	(375)	(245)	(251)	(311)	(281)
Male partner displaced in t+1	1,619***	692***	914***	121	−587**
	(375)	(245)	(251)	(311)	(281)
Male partner displaced in t+2	1,127***	582**	930***	273	−508*
	(375)	(245)	(251)	(311)	(281)
Male partner displaced in t+3	1,300***	971***	785***	552*	−614**
	(375)	(245)	(251)	(311)	(281)
Observations	183,920	174,216	156,944	137,168	122,752
Number of individuals	22,990	21,777	19,618	17,146	15,344

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.12: Treatment effect on female partner's income from work - intensive margin

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	−71	133	−240	209	103
	(179)	(198)	(225)	(284)	(374)
Male partner displaced in t-1	−184	93	−308	77	632*
	(179)	(198)	(225)	(284)	(374)
Male partner displaced in t	417**	558***	−79	469*	652*
	(179)	(198)	(225)	(284)	(374)
Male partner displaced in t+1	971***	936***	45	715**	1,171***
	(179)	(198)	(225)	(284)	(374)
Male partner displaced in t+2	762***	762***	181	614**	969***
	(179)	(198)	(225)	(284)	(374)
Male partner displaced in t+3	838***	863***	−229	172	1,637***
	(179)	(198)	(225)	(284)	(374)
Observations	713,936	725,008	745,544	735,504	720,544
Number of individuals	89,242	90,626	93,193	91,938	90,069

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−262	−247	2	−164	49
	(387)	(228)	(224)	(254)	(212)
Male partner displaced in t-1	−506	−400*	−94	121	101
	(387)	(228)	(224)	(254)	(212)
Male partner displaced in t	−171	−245	161	349	850***
	(387)	(228)	(224)	(254)	(212)
Male partner displaced in t+1	−213	239	353	679***	1,028***
	(387)	(228)	(224)	(254)	(212)
Male partner displaced in t+2	−416	203	384*	651**	1,295***
	(387)	(228)	(224)	(254)	(212)
Male partner displaced in t+3	−469	−74	368	765***	1,335***
	(387)	(228)	(224)	(254)	(212)
Observations	697,848	678,960	652,984	631,008	600,752
Number of individuals	87,232	84,871	81,624	78,877	75,095

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.13: Treatment effect on female partner's participation (employed or self-employed) - extensive margin

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	-0.0112	-0.0204	-0.0080	-0.0277	0.0141
	(0.0146)	(0.0156)	(0.0181)	(0.0192)	(0.0231)
Male partner displaced in t-1	0.0058	0.0031	0.0120	0.0115	-0.0184
	(0.0146)	(0.0156)	(0.0181)	(0.0192)	(0.0231)
Male partner displaced in t	0.0323**	0.0450***	0.0340*	0.0532***	0.0285
	(0.0146)	(0.0156)	(0.0181)	(0.0192)	(0.0231)
Male partner displaced in t+1	0.0196	0.0621***	0.0106	0.0720***	0.0477**
	(0.0146)	(0.0156)	(0.0181)	(0.0192)	(0.0231)
Male partner displaced in t+2	0.0397***	0.0276*	0.0120	0.0533***	0.0233
	(0.0146)	(0.0156)	(0.0181)	(0.0192)	(0.0231)
Male partner displaced in t+3	0.0374**	0.0352**	0.0095	0.0353*	0.0137
	(0.0146)	(0.0156)	(0.0181)	(0.0192)	(0.0231)
Observations	285,808	257,376	220,560	204,632	191,560
Number of individuals	35,726	32,172	27,570	25,579	23,945

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	0.0328	0.0054	0.0053	0.0216	0.0060
	(0.0244)	(0.0157)	(0.0164)	(0.0193)	(0.0155)
Male partner displaced in t-1	0.0162	0.0001	0.0122	0.0145	-0.0240
	(0.0244)	(0.0157)	(0.0164)	(0.0193)	(0.0155)
Male partner displaced in t	0.0043	0.0382**	0.0370**	0.0394**	-0.0123
	(0.0244)	(0.0157)	(0.0164)	(0.0193)	(0.0155)
Male partner displaced in t+1	0.0334	0.0356**	0.0506***	0.0443**	-0.0234
	(0.0244)	(0.0157)	(0.0164)	(0.0193)	(0.0155)
Male partner displaced in t+2	0.0105	0.0211	0.0381**	0.0420**	0.0006
	(0.0244)	(0.0157)	(0.0164)	(0.0193)	(0.0155)
Male partner displaced in t+3	-0.0013	0.0298*	0.0469***	0.0167	0.0266*
	(0.0244)	(0.0157)	(0.0164)	(0.0193)	(0.0155)
Observations	183,920	174,216	156,944	137,168	122,752
Number of individuals	22,990	21,777	19,618	17,146	15,344

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.14: Treatment effect on female partner's participation (employed or self-employed)

	(1) 2003	(2) 2004	(3) 2005	(4) 2006	(5) 2007
Male partner displaced in t-2	−0.0064	−0.0100	−0.0069	−0.0057	0.0029
	(0.0063)	(0.0061)	(0.0063)	(0.0070)	(0.0080)
Male partner displaced in t-1	−0.0034	−0.0038	−0.0094	0.0080	0.0015
	(0.0063)	(0.0061)	(0.0063)	(0.0070)	(0.0080)
Male partner displaced in t	0.0068	0.0068	−0.0012	0.0218***	0.0114
	(0.0063)	(0.0061)	(0.0063)	(0.0070)	(0.0080)
Male partner displaced in t+1	0.0042	0.0202***	0.0012	0.0278***	0.0122
	(0.0063)	(0.0061)	(0.0063)	(0.0070)	(0.0080)
Male partner displaced in t+2	0.0147**	0.0053	−0.0008	0.0230***	0.0144*
	(0.0063)	(0.0061)	(0.0063)	(0.0070)	(0.0080)
Male partner displaced in t+3	0.0190***	0.0117*	−0.0021	0.0161**	0.0161**
	(0.0063)	(0.0061)	(0.0063)	(0.0070)	(0.0080)
Observations	999,744	982,384	966,104	940,136	912,104
Number of individuals	124,968	122,798	120,763	117,517	114,014

	(6) 2008	(7) 2009	(8) 2010	(9) 2011	(10) 2012
Male partner displaced in t-2	−0.0058	−0.0009	0.0010	0.0006	0.0024
	(0.0079)	(0.0048)	(0.0047)	(0.0052)	(0.0046)
Male partner displaced in t-1	−0.0125	−0.0024	0.0028	0.0029	−0.0090*
	(0.0079)	(0.0048)	(0.0047)	(0.0052)	(0.0046)
Male partner displaced in t	−0.0045	0.0089*	0.0019	0.0054	−0.0010
	(0.0079)	(0.0048)	(0.0047)	(0.0052)	(0.0046)
Male partner displaced in t+1	0.0072	0.0112**	0.0023	0.0101*	−0.0020
	(0.0079)	(0.0048)	(0.0047)	(0.0052)	(0.0046)
Male partner displaced in t+2	0.0153*	0.0075	0.0006	0.0046	0.0054
	(0.0079)	(0.0048)	(0.0047)	(0.0052)	(0.0046)
Male partner displaced in t+3	0.0089	0.0085*	0.0018	−0.0005	0.0109**
	(0.0079)	(0.0048)	(0.0047)	(0.0052)	(0.0046)
Observations	881,768	853,176	809,928	768,176	723,512
Number of individuals	110,222	106,648	101,242	96,023	90,441

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.15: Treatment effect on female partner's annual hours worked

VARIABLES	(1) 2010	(2) 2011	(3) 2012
Male partner displaced in t-2	3	−2	−7
	(7)	(8)	(7)
Male partner displaced in t-1	7	−1	2
	(7)	(8)	(7)
Male partner displaced in t	23***	11	20***
	(7)	(8)	(7)
Male partner displaced in t+1	26***	20**	36***
	(7)	(8)	(7)
Male partner displaced in t+2	27***	23***	40***
	(7)	(8)	(7)
Male partner displaced in t+3	28***	21***	43***
	(7)	(8)	(7)
Observations	809,928	768,176	723,512
Number of individuals	101,242	96,023	90,441

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.16: Effect of male partner being displaced on female partner's income (wage + profit) - different age groups female partner

	(1)	(2)	(3)
	2004	2004	2004
Age female partner	25-35	36-45	46-55
Male partner displaced in t-2	355	−10	−174
	(368)	(290)	(363)
Male partner displaced in t-1	411	−130	243
	(368)	(290)	(363)
Male partner displaced in t	1,104***	599**	63
	(368)	(290)	(363)
Male partner displaced in t+1	1,228***	1,061***	412
	(368)	(290)	(363)
Male partner displaced in t+2	1,117***	1,152***	581
	(368)	(290)	(363)
Male partner displaced in t+3	1,578***	1,325***	595
	(368)	(290)	(363)
Observations	301,256	467,728	302,960
Number of individuals	37,657	58,466	37,870

	(4)	(5)	(6)
	2008	2008	2008
Age female partner	25-35	36-45	46-55
Male partner displaced in t-2	−134	302	−179
	(613)	(590)	(595)
Male partner displaced in t-1	166	−258	−187
	(613)	(590)	(595)
Male partner displaced in t	733	958	138
	(613)	(590)	(595)
Male partner displaced in t+1	390	1,370**	804
	(613)	(590)	(595)
Male partner displaced in t+2	513	789	998*
	(613)	(590)	(595)
Male partner displaced in t+3	451	979*	470
	(613)	(590)	(595)
Observations	212,608	445,312	312,480
Number of individuals	26,576	55,665	39,060

	(7)	(8)	(9)
	2012	2012	2012
Age wife	25-35	36-45	46-55
Male partner displaced in t-2	126	−35	78
	(448)	(325)	(356)
Male partner displaced in t-1	629	38	−556
	(448)	(325)	(356)
Male partner displaced in t	1,225***	804**	350
	(448)	(325)	(356)
Male partner displaced in t+1	1,657***	1,013***	214
	(448)	(325)	(356)
Male partner displaced in t+2	1,084**	1,291***	396
	(448)	(325)	(356)
Male partner displaced in t+3	1,418***	909***	584
	(448)	(325)	(356)
Observations	129,424	367,648	311,048
Number of individuals	16,179	45,956	38,881

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.17: Treatment effect on female partner's income (wage + profit)- Couple with and without children

	(1) Couples with children 2004	(2) Couples with children 2008	(3) Couples with children 2012	(4) Couples without children 2004	(5) Couples without children 2008	(6) Couples without children 2012
Male partner displaced in t-2	−31	−70	13	566	793	90
	(193)	(391)	(217)	(373)	(644)	(521)
Male partner displaced in t-1	43	−204	−225	598	761	567
	(193)	(391)	(217)	(373)	(644)	(521)
Male partner displaced in t	481**	374	428**	1,102***	1,875***	1,858***
	(193)	(391)	(217)	(373)	(644)	(521)
Male partner displaced in t+1	772***	626	857***	1,691***	1,871***	970*
	(193)	(391)	(217)	(373)	(644)	(521)
Male partner displaced in t+2	824***	508	917***	1,923***	1,670***	1,420***
	(193)	(391)	(217)	(373)	(644)	(521)
Male partner displaced in t+3	967***	251	690***	2,331***	2,041***	1,836***
	(193)	(391)	(217)	(373)	(644)	(521)
Observations	722,768	693,744	603,560	259,600	188,024	119,944
Number of individuals	90,346	86,719	75,446	32,450	23,503	14,993

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.18: Effect of male partner being displaced on female partner's income (wage + profit) - different level of education female partner

	(1)	(2)	(3)
	2004	2004	2004
Level of education wife	Low	Middle	High
Male partner displaced in t-2	−348	115	284
	(315)	(253)	(492)
Male partner displaced in t-1	−369	139	514
	(315)	(253)	(492)
Male partner displaced in t	−311	631**	1,428***
	(315)	(253)	(492)
Male partner displaced in t+1	116	890***	1,813***
	(315)	(253)	(492)
Male partner displaced in t+2	−20	923***	2,128***
	(315)	(253)	(492)
Male partner displaced in t+3	107	1,113***	2,531***
	(315)	(253)	(492)
Observations	298,688	506,744	263,872
Number of individuals	37,336	63,343	32,984

	(4)	(5)	(6)
	2008	2008	2008
Level of education wife	Low	Middle	High
Male partner displaced in t-2	−4	−204	460
	(520)	(405)	(977)
Male partner displaced in t-1	−165	−33	−235
	(520)	(405)	(977)
Male partner displaced in t	397	327	1,398
	(520)	(405)	(977)
Male partner displaced in t+1	503	882**	1,620*
	(520)	(405)	(977)
Male partner displaced in t+2	866*	871**	718
	(520)	(405)	(977)
Male partner displaced in t+3	309	1,345***	211
	(520)	(405)	(977)
Observations	230,568	472,536	264,872
Number of individuals	28,821	59,067	33,109

	(7)	(8)	(9)
	2012	2012	2012
Level of education wife	Low	Middle	High
Male partner displaced in t-2	484*	−110	−84
	(287)	(241)	(593)
Male partner displaced in t-1	−93	−40	−128
	(287)	(241)	(593)
Male partner displaced in t	292	655***	1,283**
	(287)	(241)	(593)
Male partner displaced in t+1	52	861***	1,437**
	(287)	(241)	(593)
Male partner displaced in t+2	−159	1,055***	1,680***
	(287)	(241)	(593)
Male partner displaced in t+3	−171	1,288***	1,271**
	(287)	(241)	(593)
Observations	169,088	402,512	234,576
Number of individuals	21,136	50,314	29,322

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.19: Effect of male partner being displaced on female partner's income (wage + profit) - different ethnicity

	(1)	(2)	(3)
	2004	2004	2004
Ethnicity		Western	Non-Western
	Native	immigrant	immigrant
Male partner displaced in t-2	184	−426	−167
	(188)	(629)	(655)
Male partner displaced in t-1	162	−56	274
	(188)	(629)	(655)
Male partner displaced in t	705***	−253	515
	(188)	(630)	(655)
Male partner displaced in t+1	1,094***	552	199
	(188)	(630)	(655)
Male partner displaced in t+2	1,125***	1,556**	114
	(188)	(630)	(655)
Male partner displaced in t+3	1,510***	643	−271
	(188)	(630)	(655)
Observations	871,568	73,608	37,208
Number of individuals	108,946	9,201	4,651

	(4)	(5)	(6)
	2008	2008	2008
Ethnicity		Western	Non-Western
	Native	immigrant	immigrant
Male partner displaced in t-2	224	−857	267
	(373)	(1,002)	(1,072)
Male partner displaced in t-1	214	−1,474	−344
	(373)	(1,002)	(1,072)
Male partner displaced in t	1,022***	−997	−767
	(373)	(1,002)	(1,072)
Male partner displaced in t+1	1,351***	−1,633	−804
	(373)	(1,002)	(1,073)
Male partner displaced in t+2	1,410***	−1,897*	−3,285***
	(373)	(1,002)	(1,073)
Male partner displaced in t+3	1,403***	−2,717***	−3,574***
	(373)	(1,002)	(1,073)
Observations	773,032	66,360	42,376
Number of individuals	96,630	8,295	5,297

	(7)	(8)	(9)
	2012	2012	2012
Ethnicity		Western	Non-Western
	Native	immigrant	immigrant
Male partner displaced in t-2	26	93	−279
	(216)	(852)	(685)
Male partner displaced in t-1	−128	−136	163
	(216)	(852)	(685)
Male partner displaced in t	728***	671	−242
	(216)	(852)	(685)
Male partner displaced in t+1	937***	822	−177
	(216)	(852)	(685)
Male partner displaced in t+2	1,093***	1,190	−623
	(216)	(852)	(685)
Male partner displaced in t+3	839***	2,407***	−311
	(216)	(852)	(686)
Observations	629,288	53,856	40,368
Number of individuals	78,662	6,733	5,046

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

Table A.3.20: Effect male partner displaced in 2004 on income female partner (wage + profit) - different income groups male partner

	(1)	(2)	(3)	(4)
	2004	2004	2004	2004
Income husband	<30.000	30-40.000	40-50.000	50.000+
Male partner displaced in t-2	−108	435	−305	307
	(278)	(298)	(589)	(762)
Male partner displaced in t-1	−143	86	524	1,451*
	(278)	(298)	(589)	(762)
Male partner displaced in t	367	516*	537	2,453***
	(278)	(298)	(589)	(762)
Male partner displaced in t+1	539*	1,088***	646	3,338***
	(278)	(298)	(589)	(762)
Male partner displaced in t+2	464*	612**	2,001***	4,391***
	(278)	(298)	(589)	(762)
Male partner displaced in t+3	351	827***	2,473***	6,32***
	(278)	(298)	(589)	(763)
Observations	503,840	317,096	128,344	122,600
Number of individuals	62,980	39,637	16,043	15,325

	(5)	(6)	(7)	(8)
	2008	2008	2008	2008
Income husband	<30.000	30-40.000	40-50.000	50.000+
Male partner displaced in t-2	−540	560	−592	451
	(671)	(403)	(768)	(1,115)
Male partner displaced in t-1	−362	504	−818	−381
	(671)	(403)	(768)	(1,115)
Male partner displaced in t	209	517	−802	2,182*
	(671)	(403)	(768)	(1,115)
Male partner displaced in t+1	627	766*	−931	2,803**
	(671)	(403)	(768)	(1,115)
Male partner displaced in t+2	23	1,063***	−794	2,332**
	(671)	(403)	(768)	(1,115)
Male partner displaced in t+3	−17	1,505***	−140	866
	(671)	(403)	(768)	(1,115)
Observations	291,728	324,352	167,224	186,816
Number of individuals	36,466	40,544	20,903	23,352

	(9)	(10)	(11)	(12)
	2012	2012	2012	2012
Income husband	<30.000	30-40.000	40-50.000	50.000+
Male partner displaced in t-2	−184	485*	127	−443
	(544)	(267)	(388)	(482)
Male partner displaced in t-1	−510	311	13	−334
	(544)	(267)	(388)	(482)
Male partner displaced in t	424	831***	831**	675
	(544)	(267)	(388)	(482)
Male partner displaced in t+1	178	819***	793**	1,245***
	(544)	(267)	(388)	(482)
Male partner displaced in t+2	345	629**	853**	1,67***
	(544)	(267)	(388)	(482)
Male partner displaced in t+3	602	711***	468	1,589***
	(544)	(267)	(388)	(482)
Observations	164,040	214,920	162,616	266,392
Number of individuals	20,505	26,865	20,327	33,300

Notes: * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level. Standard errors in parentheses. Our sample consists of couples 25–55 years of age in the year before the unemployment shock. Further, we select couples in which the husband has an annual income from work of at least 5,000 euro in the 4 years before the treatment and with the husband not receiving income from UI, social assistance or other benefits in the pre-treatment period. All specifications include year dummies, time-varying demographic controls (age) and individual fixed effects.

4 The Effect of Constitutional Commitment to Social Security on Social Expenditure Schemes

Abstract

This paper studies the effect of constitutional commitment to social security (CCSS) on different categories of social expenditure. For this purpose, we use a pooled cross sectional database for 17 EU-countries from 1990 till 2012. We run OLS models, 2SLS regression models and the Heckman two step model, using the rigidity of the constitution as instrumental variable to correct for possible endogeneity. A positive effect of constitutional commitment to social security is found on total social expenditure and on all four categories of social security spending: old age and survivor, incapacity, unemployment and active labor market policies (ALMPs). The largest effect sizes, expressed as a percentage of average spending, are found for expenditure on unemployment and ALMPs. This shows that constitutional commitment to social security has the largest effect on social expenditure schemes targeted at people who are perceived as less deserving by the public opinion.

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4.1 Introduction

In recent decades, politicians and academics have emphasized the role of social rights for providing social and economic development (Townsend 2007; ILO 2014). The main argument for a rights-based approach to development is that it gives an entitlement that can be enforced in court. Without such a right, people are fully dependent on the 'good-will' of the government of that time for proper education, health care and social security. In theory, the constitution can play an important role for social rights, as constitutions provide universal rights for everyone and protect minorities against the majority. However, the number of empirical studies on the effect of social rights in the constitution is still very limited.

In this paper, we study the effect of constitutional commitment to social security (CCSS) on different kinds of social expenditure.[1] We define CCSS as a dichotomous variable, being 1 if there is at least a general statement in the constitution on a social right to income, unemployment, sickness, work injury, old age, survivor or disability and 0 if there is no statement on any of these categories. First, we are interested in the effect of CCSS on total social expenditure, which shows whether CCSS has an effect at all. Second, we study if the effect of CCSS is most sizable on social expenditure schemes for beneficiaries who are seen as less deserving by the public opinion. We expect this if the median voter cares less about these social expenditure schemes, leaving a larger role for the constitution.

We run OLS models, 2SLS regression models and the Heckman two step model with the rigidity of the constitution as an instrument to correct for possible endogeneity. In line with our expectations, we find a positive effect of the rigidity of the constitution on CCSS. First, this is in accordance with Landes and Posner (1975), who argue that the discounted value of the constitution is larger when the constitution is more durable. Second, uncertainty increases over time and thereby the risk that politicians themselves, but also the median voter, or their offspring become dependent on social security is larger for the distant future rather than the near. These

[1] The use of the acronym CCSS for constitutional commitment to social security is in line with Ben-Bassat and Dahan (2008, 2016).

two reasons show an added value of the constitution that supplements laws and policies.

We use a panel data set for 17 EU-countries from 1990 until 2012. The data on social expenditures as a % of GDP are taken from the Social Expenditure Database (SOCX) of the OECD. For CCSS, we use the indicator created by Ben-Bassat and Dahan (2008), which we defined as one or zero, depending on the presence of a legal provision on assistance to old age, survivors, disability, unemployment, sickness, work injury or the poor in the constitution.

Our main findings are as follows. First, we find a positive significant effect of CCSS on total social expenditure. This includes a positive effect on spending on old age and survivor, incapacity, unemployment and active labour market policies. Second, the most sizable effects, expressed as a percentage of average spending, are found for spending on unemployment and active labor market policies. These are the expenditure schemes targeted at people who are perceived as less deserving by the public opinion. Thirdly, no positive effect is found on expenditure on health and family, which are not covered in CCSS. This suggests that the positive effect we observe for social security types of social expenditure is due to CCSS and not caused by a positive attitude towards redistribution.

Our paper relates to two important studies that consider the effect of commitment to social rights in the constitution. Ben-Bassat and Dahan (2008) were the first to investigate the effects of the rights to social security, education, health, housing and workers' rights. They find no relation between these rights and expenditure on these different categories, except for the positive relation between the degree of constitutional commitment to social security and transfer payments and between constitutional commitment to health and health policy performance. In a more recent paper, Ben-Bassat and Dahan (2016) find a positive relationship between CCSS and the extent and coverage of actual measures of social security laws. The studies of Ben-Bassat and Dahan (2008, 2016) are the only two studies on the relation between CCSS and spending on social security, which makes more research on this topic a valuable contribution.

We make the following contributions to the literature. First, knowing if there is an effect of CCSS on total social expenditure contributes to research

on the popular rights-based approach, as it tells us if social rights in the constitution contribute to social security. Second, studying the effects on different kinds of social expenditure allows us to explain if CCSS has the strongest effect on social expenditure targeted at people who are perceived as less deserving by the public opinion. Third, an important contribution is how we correct for possible endogeneity in our empirical methodology. We select similar EU-countries and correct for the endogeneity problem by using both 2SLS models and the Heckman two step model. We use the rigidity of the constitution as an instrument in order to derive the effect of CCSS on social expenditure. This contributes to the current literature, which does not go beyond correlations inferred from OLS models for a sample with a wide variety of countries, which we deem as insufficient to deal with the endogeneity issue as well as to draw conclusions for the more homogeneous group of EU-countries.

The outline of the paper is as follows. Section 4.2 gives a literature review, in which we start with a theoretical framework on the effects of CCSS in Section 4.2.1 and proceed with the related empirical literature in Section 4.2.2. We continue with describing the methodology with an elaborate discussion on the causes of endogeneity and the methodological solutions to deal with this in Section 4.3.1. We give the empirical specification in Section 4.3.2. We discuss the data in Section 4.4 and the results in Section 4.5. We conclude with a discussion on the implications of the results in Section 4.6.

4.2 Constitutional rights and social security

4.2.1 Theories on the effects of CCSS

In this section we discuss the mechanisms how CCSS is expected to have an effect on top of normal laws and policies by the rigidity of the constitution, the interdependence cost calculus and the expressive function of law. We end this section with explaining the role of the median voter for social expenditure and how we expect the effects of CCSS for the different

expenditure schemes to be dependent on the preferences of the median voter.

The difference between constitutional law and normal laws and policies lies mainly in the more durable character of the constitution. Landes and Posner (1975) argue that benefits for interest groups are larger if policies or laws are more durable. This gives value to constitutional rights on top of normal laws.[2] This means that more rigid constitutions, which are more durable constitutions, are more valuable. This greater value makes it more likely that politicians will implement CCSS when the constitution is more rigid, as it is more worthwhile to put a statement on social security in the constitution when it is more durable. A second reason why we expect CCSS to be more valuable when constitutions are more durable is that uncertainty increases over time. The risk that politicians themselves, but also the current median voter, or their offspring become dependent on social security is larger for the distant future than the near future, making the willingness to pay for social security in the future larger than for social security in the present.[3] Hence, there is enough theoretical support for our empirical finding of a positive effect of the rigidity of the constitution on CCSS. Therefore, we can use the rigidity of the constitution as an instrument to derive the effect of CCSS on social expenditure in our empirical part, as we will explain in the methodology section.

Another economic rationale for CCSS is given by the interdependence cost calculus, which is about the trade-off between external costs and decision making costs (Buchanan and Tullock 1962). Rights in the constitution can protect minorities and thereby reduce external costs of political decisions. Therefore, the number of people involved in the decision making

[2]Politicians know that the durable character of the constitution will be questioned when they abolish or dramatically change the constitution. They also know that this would decrease the value of the constitution. For this reason, politicians are in favor of constitutions even when it limits their power, as they can use the constitution as a tool to extract rents related to a longer period than the time being an elected politician. Hence, we could explain the existence of constitutions in a multiparty system where different political parties alternate power. We can show in a game theoretical framework in which a tit for tat strategy is applied, like in Ordeshook (1992), that the Nash equilibrium is to respect the constitution.

[3]The risk-averse nature of humans may even increase this difference in willingness to pay for the uncertain future compared to the present in which the politician or median voter is unlikely to rely on social security.

can go down, reducing decision making cost. Hence, CCSS can reduce the sum of external costs and decision making costs. Also, more potential efficiency gains will be realized, as decision making costs can be an obstacle to implement efficient policies when these decision making costs outweigh the efficiency gains. We can compare this with an efficient contract that is not concluded when transaction costs are too high (Coase 1960). Besides, a reduction of the external costs affects policies through political stability. A right to social security protects the lower and middle class, making them less willing to resist against the government.

A third way in which CCSS can have an effect on social expenditure can be explained by the expressive function of law, in which CCSS gives information and thereby influences behavior. A provision in the constitution indicates that it is more fundamental and thereby provides a reference point, allowing lower decision making costs. Funk (2007) finds in her research on voting turnout that a law without penalties, targeted at the civic duty, might have a bigger impact on behavior than actions which affect the costs of provision for the public good. We expect this mechanism to be important as CCSS may have an effect on the political duty to care about social security.

We also study if CCSS has the largest effect on expenditure schemes that are preferred by the median voter or on expenditure schemes targeted at groups who are perceived as less deserving.[4] Blekesaune and Quadagno (2003) and Van Oorschot (2006) show that elderly people are seen as most deserving, closely followed by sick and disabled, whereas the unemployed are seen as less deserving. Knowing the preferences of the median voter allows us to test empirically if CCSS has the largest effects on the expenditure schemes preferred by the median voter or on

[4]In a democracy, we expect the preferences of the median voter to be the most important determinant for the level of social expenditure (Hotelling 1929; Black 1948; Downs 1957). Firstly, the median voter attaches more value to universal kinds of social expenditure, compared to targeted forms of social expenditure, as not only the poor but also the middle class benefits from these types of expenditure. This is supported by Korpi and Palme (1998), Rothstein (2001) and Larsen (2008) who show that a more universal character of entitlements causes higher levels of redistribution. Secondly, the median voter is expected to be more in favor of supporting social expenditure targeted at groups who are perceived as more deserving by the public opinion.

the expenditure schemes targeted at groups that are perceived as less deserving.

Empirical literature 4.2.2

In this section, we discuss the empirical literature on the effects of social rights in the constitution. Ben-Bassat and Dahan (2008) studied the effects of the rights to social security, education, health, housing and worker rights in the constitution. They constructed quantitative indicators for constitutional commitment for these five categories for 68 different countries. For social security, they studied the relationship between CCSS and the size of government and between CCSS and redistribution policy. They find no robust relation between constitutional commitment and public policy, except for the statistically significant association between CCSS and government transfers and between constitutional commitment to health and health policy performance. They find that an increase of one standard deviation in their CCSS index is associated with an increase of 1.7 percentage points in the share of transfers in GDP.

In a more recent study, Ben-Bassat and Dahan (2016) find a positive relation between their indicator of CCSS and the extent and coverage of actual measures of social security laws. The constitution seems to explain part of the cross country variation in welfare coverage around the world. They also tested for interaction effects between CCSS and the degree of constitutional review, the ease of amending the constitution, the power of NGOs and international organizations and ethnic fractionalization. In contrast with theoretical predictions, they find that these institutional factors do not have a significant influence on the effect of CCSS on social security policy.

Two other related studies look at a right to social security in the constitution on poverty and inequality. Bjørnskov and Mchangama (2019) find no evidence for an effect of the introduction of a right to social security in the constitution on inequality. Minkler and Prakash (2017) find no association between constitutional rights generally framed and poverty. These findings are in contrast with what we would expect based on the positive association between CCSS and social expenditure found

by Ben-Bassat and Dahan (2008, 2016). Both Bjørnskov and Mchangama (2019) and Minkler and Prakash (2017) use large panels covering 160 and 195 countries, whereas we are interested in the effect for the more homogeneous EU-countries. The results of these two studies may be driven by endogeneity as country characteristics are likely to play a role in explaining both social rights and poverty and inequality in a sample covering such a variety of countries. Furthermore, we look at the effect on social expenditure on which we expect a more direct effect than on inequality or poverty which can only indirectly be affected by a social right.

When taking a broader perspective, Espinosa (2016) finds that countries that spend more tend to inscribe fewer rights in their constitution. In line with this, social expenditure may have a negative effect on CCSS. Hence, the positive estimate of the effect of CCSS on social expenditure is a conservative estimate if the effect of social expenditure on CCSS is negative. Further, Espinosa (2016) finds fragile evidence that constitutional rights are more likely to induce larger governments only for a sample of democratic countries. Our sample exists of merely democratic countries and we use social expenditure rather than government expenditure and CCSS instead of their more general constitutional rights indicator, which makes it more likely that we find a positive significant effect. Another way by which constitutions affect government spending is by constitutional entrenched spending limits (Blume and Voigt 2013).

4.3 Methodology

4.3.1 Endogeneity issues

One way by which endogeneity may be a problem is by reverse causality, as the political conditions and the state of public opinions may cause constitutional structure, rather than the other way around (Riker 1976). Another endogeneity issue may be that societies with a culture that cares more about social security are expected to have both higher CCSS and higher total social expenditure. The latter indicates that third variables

as history, culture and religion my explain correlation between CCSS and social expenditure, rather than an effect of CCSS on social expenditure. In this section, we explain to what extent endogeneity may cause problems to find the effect of CCSS on social expenditure and how we address this endogeneity issues to find an effect that goes beyond mere correlations.

Constitutions can be considered as a representation or expression of social and political preferences, which have a deeper root in history, culture and religion. Ben-Bassat and Dahan (2008, 2016) show that cultural values and history, like religion and legal origin, indeed have an effect on both constitutional commitment and social benefits. They find that CCSS is on average higher in countries that share the tradition of French civil law. They also find that common law countries exhibit on average a lower CCSS. Constitutional commitments for socialist countries are closer to French civil law whereas German and Scandinavian traditions resemble the English common law more closely (Ben-Bassat and Dahan 2008). Part of the endogenous variation in CCSS can be explained by legal origin, which is related to geographical location and religion. Therefore, we control for legal origin to determine the partial effect of CCSS.

However, Ben-Bassat and Dahan (2016) argue that the endogeneity issue is less of a problem than we would expect, as it is hard to find common economic, cultural or other characteristics among countries that share a similar degree of constitutional commitment to social security. For example Scandinavian countries, which are known for their broad welfare state, have very different levels of CCSS. Finland has very high CCSS whereas Norway has a CCSS of zero. The same large differences for similar countries exist all over the sample with Greece having zero commitment whereas Italy has a very high CCSS. Hungary has a high CCSS while the Czech Republic has zero CCSS.

Ben-Bassat and Dahan (2016) also argue that endogeneity problems are absent if the effect of CCSS is interpreted as a proxy for stated preferences of the past, embedded in the culture. This is in line with Acemoglu et al. (2005), who argue that economic outcomes and the distribution of resources determine de facto political power, which has an effect on political institutions such as the constitution. In turn, these institutions have an effect on future redistribution of resources and future political

power. In this chain of causation, we measure the effect of the latest step, that is the effect of the 'stated public preferences in the constitution' on 'future political power', namely redistribution of resources and future public preferences. In this interpretation, we recognize that CCSS is affected by political preferences at the time when the constitution came into force. Finding an effect of CCSS indicates that former political preferences have a stronger effect on preferences of current politicians when these are stated in the constitution. Hence, culture is not a third variable that makes CCSS endogenous, but CCSS is a proxy of stated public preferences of the past. Finding a positive effect makes us conclude that political or public preferences are more durable if they are stated in the constitution.

To avoid biases in estimating the effect of CCSS, we use the rigidity of the constitution as an instrumental variable to derive the effect of CCSS on social expenditure in a 2SLS model and in the Heckman two step model. In these models, we assume that the rigidity of the constitution has an effect on CCSS, but no independent effect on social expenditure schemes. It is easy to imagine that CCSS is positively affected by the rigidity of the constitution, as explained in the theory part. First of all, because a higher level of rigidity implies a more durable character of the constitution and this would, according to Landes and Posner (1975), lead to a higher value for interest groups. After all, the added value of CCSS on top of normal policies and laws is expected to be very limited when constitutions are very adaptable. Second, preferences for CCSS are expected to be larger for more durable constitutions because uncertainty increases when time elapses and thereby the risk that the politician, the median voter, or their offspring become dependent on social security is larger in the far than the near future. For these two reasons, we expect a positive effect of the rigidity of the constitution on CCSS.

Regarding the exclusivity condition of our instrumental variable, there is no theoretical basis for an independent effect of the rigidity of the constitution on social expenditure. A potential risk is that another variable closely correlated with rigidity might have an effect on both CCSS and social expenditure directly. Trust might be such a variable that explains both

the rigidity of the constitution, CCSS and social expenditure.[5] However, Bjørnskov and Voigt (2014) argue that high trust levels reduce the need for statements in the constitution. In line with this it would also reduce the need for a more rigid constitution, suggesting a negative relation between trust and the rigidity of the constitution. But we find that trust and rigidity are only weakly (and positively) correlated, indicating that trust does not cause problems for the validity of our instrument. When we have a closer look at the data on the rigidity of the constitution, again no clear pattern appears between similar groups of countries and the rigidity of the constitution. All in all, we expect exclusivity of our instrument to be a justified assumption.

In the Heckman correction model, we correct for unobserved correlation between the selection model and the second stage. We expect a positive correlation, when CCSS is a complement to political decision making. This is the case when endogeneity is mainly driven by a welfare state culture explaining both CCSS and social expenditure. But we expect a negative correlation when CCSS is a substitute to political decision making. This implies that unobserved characteristics have a negative effect on the probability of CCSS and a positive effect on social expenditure. For instance, if the added value of a statement in the constitution would be smaller when policies or other laws are already inducing high social expenditure.

We also study the effect of CCSS on social expenditures on health and family. We expect no significant positive effects on these expenditure schemes as these are not taken into account in the CCSS indicator. However, we would still expect to find a positive significant effect of CCSS on social expenditure on family and health if part of the effect we measure is due to larger welfare regimes. Not finding such a positive effect can be interpreted as indication that the effect we find on social security expenditure is due to CCSS and not due to cultural factors that are both related with CCSS and social expenditure.

[5] Bjørnskov and Voigt (2014) show that social trust is negatively associated with the length of countries' constitutions. Although they are not studying constitutional rigidity, nor CCSS or social expenditure, social trust might also be important for explaining these variables

4.3.2 Empirical specification

We use various model specifications to estimate the effect of CCSS on different kinds of social expenditure. Regression equation (1) is used as a framework for the first three empirical model specifications:

$$y_{it} = \alpha_t + \gamma CCSS_i + X'_{it}\beta_x + \epsilon_{it}. \tag{4.1}$$

The dependent variables of interest are public and mandatory private gross total social expenditure and spending on old age and survivors, incapacity, unemployment, ALMPs, health and family, denoted by y_{it}. This outcome variables vary by country $(i = 1, ..., N)$ and years $(t = 1, ..., T)$. We regress the outcome variables on a set of year fixed effects (α_t), the control variables old age dependency ratio and GDP per capita X'_{it} with coefficients β_x and the explanatory variable of interest $CCSS_i$ with coefficient γ. The choice of these two control variables in the baseline model are in line with the literature (Ben-Bassat and Dahan 2016; Rodrik 1998; Mulligan et al. 2010). Note that CCSS is constant over time. Therefore the first specification is cross sectional, as we only use the data for 2008, which is the year in which CCSS is measured. From specification 2 onward we use the time period 1990-2012 and include year dummies to obtain more accurate estimates for our control variables and for CCSS. In specifications 2 and 3, we also include a first order serial correlation component in the error term and replace robust standard errors by panel corrected standard errors. Control variables for legal origin and unemployment are added in specification 3.

In specification 4, we control for endogeneity by using a 2SLS model using the rigidity of the constitution as instrument. Our first stage equation is given by regression equation (2):

$$CCSS_i = \alpha_t + \delta Z_i + X'_{it}\beta_x + \mu_{it} \tag{4.2}$$

In which Z_i denotes the rigidity of the constitution, our instrumental variable, with coefficient δ. As constitutions are constant, the rigidity of the constitution is constant over time as well. The second stage is still equal to equation (1). The rigidity of the constitution is expected to have an effect on CCSS but no direct effect on social expenditure. As explained

earlier, we can use this instrument to control for possible endogeneity to find a more accurate effect of CCSS on the different kinds of benefits. The rigidity of the constitution is expected to have a positive effect on CCSS, as the added value of CCSS on top of laws and policies is larger when the constitution is more rigid.[6]

Finally, specification 5 is our preferred model. Here we use the rigidity of the constitution to estimate the effect by using the Heckman two step model in which a correction for the correlation between unobserved characteristics in the selection model and unobserved characteristics in the second stage is applied (Heckman 1979). This yields:

$$Prob(CCSS_i = 1|Z_i, X'_{it}) = Prob(-\mu_{it} < \theta Z_i + \nu_x X'_{it}) \tag{4.3}$$

$$= \Phi(\theta Z_i + \nu_x X'_{it})$$

$$\begin{aligned} y_{it} &= \alpha_t + \gamma CCSS_i + X'_{it}\beta_x + \rho\sigma_\epsilon \Bigg[CCSS_i \frac{\phi(\widehat{\theta} Z_i + \widehat{\nu_x} X'_{it})}{\Phi(\widehat{\theta} Z_i + \widehat{\nu_x} X'_{it})} \\ &\quad -(1 - CCSS_i) \frac{\phi(\widehat{\theta} Z_i + \widehat{\nu_x} X'_{it})}{1 - \Phi(\widehat{\theta} Z_i + \widehat{\nu_x} X'_{it})} \Bigg] + \epsilon_{it}. \end{aligned} \tag{4.4}$$

where

$$\epsilon_{it} \sim N(0, \sigma_\epsilon)$$
$$\mu_{it} \sim N(0, 1)$$

and

$$\rho = \frac{cov(\epsilon, \mu)}{\sigma_\epsilon}$$

The first stage, follows from a probit regression model for the probability of CCSS, see equation (3). Z_{it} denotes the rigidity of the constitution,

[6] In our robustness analysis, we find that the rigidity of the constitution is a weak instrument for the OECD sample, which is denoted by a low F-statistic. Therefore, for the OECD sample, we can only use our first three (OLS) specifications.

which is our exclusion restriction, with parameter θ and X'_{it} give the explanatory variables GDP per capita and the old age dependency ratio with parameters ν_x. Estimation of this first stage model yields results that can be used to predict the probability that a country has CCSS. Equation (4) is our second stage equation, where γ gives the effect of CCSS when we control for selectivity. We assume that the error terms are jointly normal and are independent and identically distributed. ρ is the correlation between unobserved determinants of $CCSS_{it}$ and unobserved determinants of social expenditure. σ_ϵ is the standard deviation of ϵ_{it}. We use the inverse mills ratio to correct for selectivity, in which ϕ denotes the standard normal density function and Φ the standard normal cumulative distribution function. We use robust standard errors to correct for possible heteroscedasticity.

4.4 Data

We use a pooled cross sectional data set for 17 EU-countries covering 23 years from 1990 to 2012.[7] We choose to focus on EU-countries that are represented in the OECD for the reason of data availability and because there is less heterogeneity between these developed countries, making cross-country comparison more reliable. The countries Estonia, Latvia, Luxembourg and Slovenia were removed from the database, because both the index for CCSS and the index for the rigidity of the constitution are not available for these countries (Ben-Bassat and Dahan 2008; Lorenz 2005). This makes our selected countries even more comparable with regard to GDP per capita, geographical location and being consolidated democracies, reducing the risk that third factors obscure our results. We focus on the period from 1990 onwards, making the data set highly balanced, as this enables us to take the post-Soviet countries into account; a substantial share of the data is missing for these countries for the period before 1990.

We choose to use the CCSS indicator created by Ben-Bassat and Dahan (2008, 2016) as we consider this data of higher quality than the data sets

[7] Countries in EU sample: Austria, Belgium, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Netherlands, Poland, Portugal, Spain, Sweden, United Kingdom.

from the Toronto Initiative for Economic and Social Rights (TIESR) (Jung and Rosevear 2011) and the data set from the Comparative Constitutions Project (CCP) (Elkins et al. 2014). First, because Ben Bassat and Dahan studied the various constitutions thoroughly to code both different types of commitment to social security and different degrees of commitment. The other data sets only capture a dummy if there is a social right to social security in the constitutions.[8] Looking more carefully in the constitutions reduces the risk of making mistakes. Second, we consider the indicator of Ben Bassat and Dahan most accurate as The Netherlands and Sweden do commit to social security in the constitution, which is not given for the Netherlands in the CCP data set and not for Sweden in both the TIESR and the CCP data sets.[9] Third, a lot of data on commitment to social security is missing for different OECD countries and years in the CCP dataset. We could have exploited the panel nature of this dataset if this dataset was more complete regarding social rights to social security. However, the added value of using this panel nature is limited as constitutions hardly change over time. Looking at the CCP dataset, I only found 4 changes in CCSS for our sample of 28 OECD countries during the period 1990-2012.[10] Besides, constitutional changes are expected to be highly correlated with other political or economic shocks, making it difficult to separate the effect of social rights from the effects of these other shocks.

Another reason why we accept that we lose some countries when we use these CCSS and rigidity indicators is that Ben-Bassat and Dahan (2008,

[8]The CCP project is unique as it codes constitutions in hundreds of variables and is therefore of great value for studying the empirical effects of constitutions, but its limitation is that by looking at so many different aspects, less accuracy is expected for individual indicators.

[9]Article 20 of the Dutch constitution: 1. It shall be the concern of the authorities to secure the means of subsistence of the population and to achieve the distribution of wealth. 2. Rules concerning entitlement to social security shall be laid down by Act of Parliament. 3. Dutch nationals resident in the Netherlands who are unable to provide for themselves shall have a right, to be regulated by Act of Parliament, to aid from the authorities.

Article 2(2) of Chapter 1 of the Swedish constitution (headed 'Basic Principles'): "The personal, economic and cultural welfare of the individual shall be fundamental aims of public activity. In particular, the public institutions shall secure the right to employment, housing and education, and shall promote social care and social security, as well as favourable conditions for good health".

[10]Changes in CCSS are found for Belgium (1994), Finland (1998), France (2000), Poland (1997) in the CCP dataset.

2016) already studied the association between CCSS and social expenditure for a large variety of countries from all over the world. Our contribution is in finding effects rather than associations, using an IV approach, and looking at different social expenditure schemes. For these purposes, it is important that countries are not too different in unobserved characteristics that might drive our results. Therefore, we value quality of the data over quantity.

We transform the CCSS indicator of Ben-Bassat and Dahan (2008, 2016) in a dichotomous variable, being 1 if there is at least a general statement in the constitution on a social right to income, unemployment, sickness, work injury, old age, survivor or disability and 0 if there is no statement on any of these categories. The choice of taking the sum of these five categories corresponds to Ben-Bassat and Dahan (2008, 2016). A high overlap and substitutability between the different types of commitment to social security, caused by the abstract formulation of the legal provisions, makes us believe that the sum has more explanatory power than the individual commitment to social security variables.[11] We use a dichotomous variable in our baseline models, because we expect the existence of a legal provision in the constitution to be more important than the concreteness of this legal provision.

The outcome variables we consider are social expenditure variables for which we use the Social Expenditure Database (SOCX) of the OECD. Our main variable for social security is public and mandatory private gross total social expenditure as a % of GDP, which we define as total social expenditure.[12] This total social expenditure consists of spending on old age and survivor, incapacity, unemployment, ALMPs, health and family, which are our next dependent variables.[13] A description of the different social expenditure variables is given in the appendix in Table A.5.1.

[11] For example, Article 2 of the Swedish constitution and article 20 of the Dutch constitution may explain an effect on spending in multiple categories, see footnote 9.

[12] Data on net total social expenditure is not available for the different expenditure types, and very limited for total social expenditure, therefore we use data on gross social expenditure.

[13] A very small part of total social expenditure consists of expenditure on housing and others. we choose not to analyze these kinds of social expenditure separately because of the low significance, on average 0.33% and 0.46% of GDP in the period 1990-2012.

Table 4.1: Descriptive statistics: differences in means between countries with and without constitutional commitment to social security (CCSS) for the different social expenditure variables shown as % of GDP

	Countries with CCSS	Countries without CCSS	Differences in Means	Differences (in %) relative to Countries without CCSS
Total social expenditure	23.5	22.0	1.5	6.9
Old age and Survivor	9.3	9.5	0.2	−2.0
Incapacity	3.1	2.4	0.7	29.1
Unemployment	1.5	0.8	0.6	76.3
ALMPs	0.9	0.5	0.4	88.4
Health	5.6	5.8	-0.2	−3.5
Family	2.2	2.1	0.1	4.2
Observations	12	5		

Sample: 17 EU countries in the years 1990-2012.

Table 4.1 gives the descriptive statistics on the different social expenditure schemes for countries with and without CCSS, for EU countries over the period 1990-2012. We find that total social expenditure is on average 23.5 percent of GDP in the countries with CCSS and 22.0 in the countries without CCSS. This difference is 7 percent relative to the mean of total social expenditure for countries without CCSS. The relative differences are the largest for spending on unemployment and ALMPs (respectively 76 percent and 88 percent relative to the means in countries without CCSS). Further, in countries with CCSS, we observe less spending on old age and survivor (-2 percent) and more spending on incapacity ($+29$ percent). Regarding social expenditure which is not taken into account in CCSS, we find slightly larger spending in countries with CCSS on family (+4 percent), whereas we find less spending on health (-3 percent).

Our instrumental variable, the the rigidity of the constitution, is the average of the standardized indices for rigidity in Lorenz (2005). This index considers the factors: kinds of majority, success rate, times of voting, unicameral/bicameral legislature, initiative actors, special body or regulator legislature, need of elections between two votes, electoral system, approval by referendum, approval by states' legislatures. The rigidity of the constitution, CCSS and total social expenditure are given

for the different countries in the year 2008 in Table A.4.2. We consider the rigidity of the constitution of Lorenz (2005) a better instrument than the amendment rate because it contains more dimensions and has the highest correlation with most other variables for the rigidity of the constitution (Ginsburg and Melton 2015).

Further, we create an interaction variable between CCSS and political party in office to study how constitutional commitment and political party in office have a combined effect on social security benefits. For politics, we use left-wing/center/right-wing cabinet posts in percentage of total cabinet posts from the comparative Political Data Set (Armingeon et al. 2013).

The control variables we use are GDP per head of population (USD in thousands, constant prices, 2010 PPPs), the old age dependency ratio (percentage of 65+ relative to 15-64 years old), dummies for legal origin, unemployment rate (standardized unemployment rate, all persons) and year dummies, see the appendix Table A.4.3 for the descriptive statistics. These control variables are chosen as they have the largest effects on the social expenditure schemes and are expected to influence the effect of CCSS on social expenditure. These control variables are in line with the literature (Kittel et al. 2003; Mulligan et al. 2010; Ben-Bassat and Dahan 2008; Ben-Bassat and Dahan 2016). For legal origin we use dummies for French, English, German and Scandinavian legal origin, where we use socialist legal origin as the reference category.

In our robustness analysis, we also investigate the effect for the sample of EU-countries together with Iceland, Switzerland and Norway, as well as for a sample of OECD countries without Japan and Korea.[14] Japan and Korea are outliers as they have a different Asian system with very low levels of social spending.[15] Hence, Japan and Korea are outliers for

[14]Countries in OECD sample: Australia, Austria, Belgium, Canada, Chile, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Spain, Sweden, Switzerland, Turkey, United Kingdom and the United States

[15]Reasons for low spending in Japan and Korea can be found in social policies as means rather than as goals, larger involvement of family and private sector in the welfare mix, late start of welfare system, top-down development of social policies, colonial past and the neglect for social services targeting woman (Hong, 2014).

reasons independent of CCSS and therefore we decided to drop these two countries from our database.

Results 4.5

Table 4.2 presents the regression results of CCSS on total social expenditure. Column (1) shows the results for the year 2008 where we only control for GDP per capita and the old age dependency ratio. This specification suggests a positive effect, significant at a 10 percent level, indicating that countries with CCSS spend on average 2.0 percentage points of GDP more on total social expenditure. This implies that the mean total social expenditure rate for countries with CCSS is 9% higher than for countries without CCSS. In Column (2), we use the years 1990-2012 and add year dummies to our empirical specification. The robust standard errors are replaced by panel corrected standard errors and we include a first order serial correlation component. The effect of CCSS on total social expenditures slightly increases and is highly significant now.

Adding control variables for the unemployment rate and legal origin, in column (3), does not change much. German legal origin and Scandinavian legal origin have the largest positive effect on total social expenditure, socialist and English legal origin the lowest. The unemployment rate increases total social expenditure, which we expected as it controls for business cycle differences.

The effect of CCSS on total social expenditure increases to 3.1 percentage points in our 2SLS model, in which we use the rigidity of the constitution as instrument. An increase in the coefficient suggests an underestimation of the effect size when we do not control for endogeneity. This could be explained by the constitution being a substitute for political decision making. In the first stage regression, we find a large positive effect of the rigidity of the constitution on CCSS. The F-test of excluded instruments is easily rejected with an F-value of 73.6, see Table 4.3, suggesting sufficient relevance of our instrument.

In column (5), the effect size increases to 3.8 percentage points when we control for unobserved correlation between our selection model and

Table 4.2: Estimation results of constitutional commitment to social security (CCSS) on total social expenditure

	(1)	(2)	(3)	(4)	(5)
CCSS	1.990*	2.198***	2.261***	3.053***	3.763***
	(1.099)	(0.664)	(0.862)	(0.939)	(1.015)
Old age dependency ratio	0.574***	0.599***	0.214**	0.692***	0.708***
	(0.104)	(0.087)	(0.083)	(0.059)	(0.061)
GDP per capita	1.081	−0.858**	−1.534***	1.336***	1.282***
	(0.671)	(0.402)	(0.518)	(0.243)	(0.249)
Unemployment rate			0.185***		
			(0.034)		
French legal origin			4.325***		
			(1.277)		
English legal origin			1.577		
			(1.268)		
German legal origin			10.170***		
			(1.451)		
Scandinavian legal origin			8.432***		
			(1.450)		
Constant	3.957	8.243***	12.230***	0.111	−0.614
	(2.602)	(1.853)	(2.068)	(1.651)	(1.720)
Year dummies	No	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	2SLS	Heckman
Standard errors	Robust	PCSE	PCSE	Robust	Robust
AR(1) component	NO	YES	YES	NO	NO
Years	2008	1990-2012	1990-2012	1990-2012	1990-2012
Countries	17	17	17	17	17
Observations	17	382	359	382	382
R-squared	0.601	0.748	0.843	0.459	

Sample: EU-countries. Instrument: the rigidity of the constitution. * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table 4.3: First stage results: the rigidity of the constitution on constitutional commitment to social security (CCSS)

	(1)
VARIABLES	CCSS
Rigidity constitution	0.225***
	(0.026)
Old age dependency ratio	−0.016**
	(0.007)
GDP per capita	0.040
	(0.028)
Observations	382
R-squared	0.131
F-statistic	73.59

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

second stage regressions by using the Heckman two step model. The effect size in our preferred specification, column 5, is a substantial 17% relative to the mean of total social expenditure for countries without CCSS. Also the extent to which the Heckman two step model is correcting for unobserved correlation, denoted by rho, is with a value of -0.4 within acceptable proportions. The negative rho means that there is a lower probability of CCSS when other factors already take care of social expenditure (e.g. politicians or labor unions).

Table 4.4 presents the effect of CCSS on the different social spending categories when we split up total social expenditure. The largest effect sizes, expressed as a percentage of average spending, are found for expenditure on unemployment and ALMPs. In our preferred specification, column 5, a positive effect of 2.1 percentage points is found for expenditure on unemployment, which is an increase of 248% relative to the mean of expenditure on unemployment in countries without CCSS. For expenditure on ALMPs we find a coefficient of 0.5, which is an increase of 99% relative to the mean. Although smaller in relative size, we still find large positive significant effects of CCSS on expenditure on old age and survivor and on incapacity. A positive coefficient of 2.46 is found for old age and survivor, which is about 26% relative to the mean in countries without CCSS and we find a positive effect of 0.7 percentage points for incapacity which is about 28% relative to the mean. However, for the effect on old age and survivor we find no significant effect in specifications (2) and (3), suggesting that we have to be more careful in drawing conclusions. We find a negative rho for all kinds of social expenditure except for expenditure on family benefits. Meaning that for all these other social expenditure schemes, there is a lower probability of CCSS when other factors (e.g. politicians or labor unions) already take care of social expenditure.

No significant positive effect is found on health and family spending, which are not taken into account in the CCSS variable. This provides extra evidence that the effects we find on social security expenditure are due to CCSS and not caused by a third factor, such as a large welfare state. We even observe a negative significant effect on family, suggesting that this social expenditure type is substituted by expenditure on social security. This may be explained by government budget constraints or because the

Table 4.4: Estimation results of constitutional commitment to social security (CCSS) on different kinds of social expenditure

	(1)	(2)	(3)	(4)	(5)	%Δ
Total social expenditure	1.990*	2.198***	2.261***	3.053***	3.763***	17%
	(1.099)	(0.664)	(0.862)	(0.939)	(1.015)	
Correlation (rho)					−0.363	
Old age and Survivor	0.698	−0.005	0.063	3.233***	2.464***	26%
	(1.154)	(0.525)	(0.620)	(0.688)	(0.522)	
Correlation (rho)					−0.609	
Incapacity	0.684	0.681***	0.736***	0.784**	0.691**	28%
	(0.433)	(0.243)	(0.219)	(0.329)	(0.343)	
Correlation (rho)					−0.020	
Unemployment	0.475	0.757***	0.797***	2.122***	2.107***	248%
	(0.290)	(0.195)	(0.187)	(0.351)	(0.098)	
Correlation (rho)					−0.924	
ALMPs	0.282**	0.427***	0.479***	0.492***	0.475***	99%
	(0.100)	(0.074)	(0.073)	(0.110)	(0.067)	
Correlation (rho)					−0.108	
Health	−0.066	0.055	0.004	−0.030	0.056	1%
	(0.375)	(0.223)	(0.335)	(0.237)	(0.145)	
Correlation (rho)					−0.142	
Family	−0.063	0.119	0.218	−2.303***	−1.684***	−79%
	(0.404)	(0.160)	(0.203)	(0.409)	(0.0742)	
Correlation (rho)					0.983	
Year dummies	No	Yes	Yes	Yes	Yes	
controls legal origin	No	No	Yes	No	No	
controls unemployment	No	No	Yes	No	No	
Method	OLS	OLS	OLS	2SLS	Heckman	
Standard errors	Robust	PCSE	PCSE	Robust	Robust	
AR(1) component	NO	YES	YES	NO	NO	
Years	2008	1990-2012	1990-2012	1990-2012	1990-2012	
Countries	17	17	17	17	17	

Sample: EU-countries. Instrument: the rigidity of the constitution. * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

government takes into account the total budget of the needy, which is already higher when they can rely on generous social security benefits.

The results remain the same in the robust analysis. Table A.4.4 in the appendix shows that the results are robust when we add the European non-EU countries: Iceland, Norway and Switzerland. Table A.5.10 shows robust results in our sample of 28 OECD countries, when we exclude Japan and Korea as they have a different Asian system. Further, we find the same positive effects when we only consider the period before the Great Recession (1990-2009), in Table A.4.6. Finally, the results remain robust when we standardize the 3 values with the lowest and highest rigidity of the constitution and when we transform the variable for the rigidity of the constitution in a dichotomous variable, to correct for possible outliers, see Table A.4.7 and Table A.4.8.

We study non-linear effects in Tables A.4.9 and interaction effects with politics in Table A.4.10, see the appendix. In Table A.4.9 we observe significant negative effects of the square of CCSS on total social expenditure, suggesting that the concreteness of CCSS is less important than the statement itself. Regarding interaction effects with politics, no effect is found of left-wing cabinet seats on social expenditure, nor of left-wing cabinet seats interacted with CCSS, see Table A.4.10 appendix. We find some evidence that more right-wing cabinet seats translate in lower total social expenditure but that the interaction between right-wing cabinet seats and CCSS has a positive effect on total social expenditure. This suggests that right-wing politicians reduce total social spending less when there is CCSS. However, more research is required on this result as the effect is not significant in specifications (2) to (4).

Discussion and conclusion 4.6

In this paper, we studied the effect of constitutional commitment to social security (CCSS) on different kinds of social expenditure. We used a pooled cross sectional database for 17 EU-countries from 1990 till 2012.

The main challenge of research on institutions like CCSS is that they are related to many other things like culture, religion, legal origin, geography, political institutions, etc. We deal with this potential endogeneity problem extensively by limiting the sample to more similar EU-countries, control for legal origin and use 2SLS models and the Heckman two step model with the rigidity of the constitution as instrumental variable.

First, we find a positive significant effect of CCSS on total social security expenditure, which increases when we control for endogeneity. This includes positive effects on the categories of social expenditure on old age and survivor, incapacity, unemployment and active labor market policies. This is in accordance with the rights-based approach to development, which supplements the focus on market institutions and property rights with human rights and social policies (Townsend 2007: ILO 2014). This result corresponds with the findings of Ben-Bassat and Dahan (2008, 2016) who find a positive relation between CCSS and transfer payments and

between CCSS and the extent and coverage of measures of social security laws.

Second, the results show that the added value of CCSS is mostly found for expenditure on unemployment and ALMPs. Blekesaune and Quadagno (2003) and Van Oorschot (2006) show that the general public perceives the unemployed as less deserving than the old and disabled, suggesting lower support for spending on the unemployed by the median voter. This could explain why CCSS, rather than the median voter theory alone, can explain the scope of expenditure on unemployment and ALMPs. Hence, the importance of CCSS is mainly to protect people who are perceived as less deserving, which makes CCSS a substitute for political decision making. This is in line with the theory of the interdependence cost calculus, in which Buchanan and Tullock (1962) argue that the role of the constitution is mainly to protect minorities. These is also supported by finding more sizable effects when we control for endogeneity and by a negative rho in the Heckman model. This suggests that there are third factors (e.g. political decision making) that have a positive effect on social expenditure and a negative effect on CCSS.

Thirdly, No positive significant effect is found for expenditure on families and health which are the two social expenditure categories that are not included in CCSS. This indicates that the positive relationship between CCSS and the social security types of social expenditure is really due to CCSS and not due to different social preferences that affect both CCSS and social expenditure.

Supplementary material 4.A

Table A.4.1: The OECD social expenditure categories

Category	Description
Old-age	Pensions, early retirement pensions, home-help and residential services for the elderly.
Survivors	Pensions and funeral payments.
Incapacity	Care services, disability benefits, benefits accruing from occupational injury and accident legislation, employee sickness payments.
Health	Spending on in- and out-patient care, medical goods, prevention.
Family	Child allowances and credits, childcare support, income support during leave and sole parent payments.
ALMPs	Active Labour Market Policies: employment services, training youth measures subsidized employment, employment measures for the disabled.
Unemployment	Unemployment compensation, severance pay and early retirement for labour market reasons.
Housing	Housing allowances and rent subsidies.
Other	Social policy areas, non-categorical cash benefits to low-income households, other social services; i.e. support programs such as food subsidies.

Description of the different categories is taken from OECD (2007)

Table A.4.2: Descriptive statistics: values of constitutional commitment to social scurity (CCSS) and the rigidity of the constitution for the different countries

Country	Year	Total	CCSS	Rigidity const.
EU countries				
Austria	2008	26.40	0	−0.47
Belgium	2008	26.31	1	0.64
Czech Republic	2008	18.21	0	−0.18
Denmark	2008	27.44	1	0.37
Finland	2008	23.34	1	−0.08
France	2008	28.54	1	−0.64
Germany	2008	25.30	0	0.16
Greece	2008	21.41	0	−0.34
Hungary	2008	22.65	1	−0.41
Ireland	2008	18.49	1	−0.43
Italy	2008	26.19	1	−0.16
Netherlands	2008	20.16	1	0.65
Poland	2008	20.23	1	−0.02
Portugal	2008	22.57	1	−0.47
Spain	2008	22.19	1	0.58
Sweden	2008	25.95	1	−1.12
United Kingdom	2008	21.72	0	−2.03
Other European countries				
Iceland	2008	20.24	1	
Norway	2008	20.35	0	0.05
Switzerland	2008	22.48	1	0.36
Other non-European OECD countries				
Anglo-Saxon:				
Australia	2008	18.87	0	0.88
Canada	2008	16.31	0	0.55
New Zealand	2008	19.35	0	−1.91
United States	2008	16.84	0	2.07
Non-Anglo-Saxon:				
Chile	2008	12.18	1	0.44
Israel	2008	15.96	0	
Mexico	2008	6.84	1	
Turkey	2008	11.58	1	
Asian countries				
Japan	2008	20.18	1	1
Korea	2008	8.26	1	0.44

The rigidity of the constitution is not available for Iceland, Israel, Mexico, Turkey

Table A.4.3: Descriptive statistics of all used variables: extention of Table 1.

Variable	Obs	Mean	Std. Dev.	Min	Max
Total social expenditure	382	23.0	4.3	12.4	34.6
Old age and survivor	382	9.3	2.8	3.1	17.5
Incapacity	382	2.9	1.238	0.8	6.4
Unemployment	388	1.3	1.0	0.0	4.6
ALMPs	388	0.8	0.5	0.1	2.7
Health	390	5.7	1.1	3.2	8.5
Family	382	2.2	1.0	0.3	4.5
CCSS (dummy)	391	0.71	0.46	0	1
CCSS (non-dichotomous)	391	0.64	0.72	0	2.14
Rigidity constitution	391	-0.23	0.65	-2.03	0.65
GDP per capita (in thousands)	390	3.1	0.8	0.9	4.8
Old age dependency ratio	391	23.3	3.4	15.5	32.2
Unemployment rate	362	8.6	3.8	1.7	24.8
French civil law	391	0.41	0.49	0	1
English common law	391	0.12	0.32	0	1
German law	391	0.12	0.32	0	1
Socialist law	391	0.18	0.38	0	1
Scandinavian law	391	0.18	0.38	0	1

Sample: EU-countries.

Table A.4.4: Estimation results of constitutional commitment to social security (CCSS) on different kinds of social expenditure: sample of EU-countries plus Norway, Switzerland and Iceland

	(1)	(2)	(3)	(4)	(5)
Total social expenditure	2.134*	1.655***	1.496***	2.385***	2.122**
	(1.058)	(0.501)	(0.491)	(0.903)	(0.837)
Old age and Survivor	0.776	0.067	0.190	3.380***	3.040***
	(0.921)	(0.418)	(0.557)	(0.683)	(0.785)
Incapacity	0.421	0.252	0.165	0.617*	0.328
	(0.409)	(0.180)	(0.107)	(0.318)	(0.329)
Unemployment	0.499*	0.611***	0.479**	1.876***	1.984***
	(0.277)	(0.193)	(0.206)	(0.304)	(0.246)
ALMPs	0.266**	0.336***	0.428***	0.354***	0.350***
	(0.112)	(0.087)	(0.095)	(0.113)	(0.115)
Health	0.175	0.207	0.061	−0.231	−0.063
	(0.417)	(0.215)	(0.276)	(0.235)	(0.152)
Family	−0.058	−0.046	0.052	−2.387***	−1.869***
	(0.388)	(0.165)	(0.141)	(0.399)	(0.089)
Year dummies	No	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	2SLS	Heckman
Standard errors	Robust	PCSE	PCSE	Robust	Robust
AR(1) component	NO	YES	YES	NO	NO
Years	2008	1990-2012	1990-2012	1990-2012	1990-2012
Countries	20	20	20	19	19

Instrument: the rigidity of the constitution. The rigidity of the constitution is not available for Iceland, leaving 19 countries in specification (4) and (5). * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.4.5: Estimation results of constitutional commitment to social security (CCSS) on different kinds of social expenditure: sample of OECD countries minus Japan and Korea

	(1)	(2)	(3)
Total social expenditure	1.989**	1.299**	1.694***
	(0.847)	(0.576)	(0.616)
Old age and Survivor	1.185*	0.641*	0.103
	(0.675)	(0.369)	(0.546)
Incapacity	0.559	0.442***	0.315**
	(0.410)	(0.145)	(0.157)
Unemployment	0.379	0.378**	0.564***
	(0.229)	(0.156)	(0.167)
ALMPs	0.265***	0.332***	0.442***
	(0.094)	(0.066)	(0.084)
Health	−0.379	−0.412***	−0.021
	(0.366)	(0.114)	(0.340)
Family	0.044	−0.026	0.286**
	(0.402)	(0.170)	(0.119)
Year dummies	No	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	Robust	PCSE	PCSE
AR(1) component	NO	YES	YES
Years	2008	1990-2012	1990-2012
Countries	28	28	28

Only OLS models lead to reliable results when considering the OECD, because the rigidity of the constitution has lower explanatory power for CCSS (lower F-statistic) and it is harder to argue that the exclusion restriction still holds as the rigidity of the constitution may be endogeneous due to larger cultural differences when considering the OECD rather than merely the EU countries represented in the OECD. Japan and Korea are excluded from the sample as they have a different Asian system of social security, in which social expenditure is typically much lower. * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.4.6: Estimation results of constitutional commitment to social security (CCSS) on different kinds of social expenditure: period before Great Recession (1990-2008)

	(1)	(2)	(3)	(4)	(5)
Total social expenditure	1.990*	2.226***	2.565***	3.544***	3.949***
	(1.099)	(0.696)	(0.603)	(1.086)	(0.992)
Old age and Survivor	0.698	0.202	0.475	3.598***	2.766***
	(1.154)	(0.295)	(0.502)	(0.782)	(0.853)
Incapacity	0.684	0.701**	0.746***	0.705*	0.560
	(0.433)	(0.275)	(0.200)	(0.386)	(0.400)
Unemployment	0.475	0.672***	0.604***	2.066***	2.049***
	(0.290)	(0.160)	(0.147)	(0.368)	(0.112)
ALMPs	0.282**	0.389***	0.480***	0.464***	0.463***
	(0.100)	(0.0823)	(0.0862)	(0.128)	(0.0713)
Health	−0.0662	0.0778	0.133	0.0280	0.118
	(0.375)	(0.200)	(0.237)	(0.242)	(0.129)
Family	−0.0633	0.141	0.242	−2.043***	−1.772***
	(0.404)	(0.159)	(0.204)	(0.423)	(0.104)
Year dummies	No	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	2SLS	Heckman
Standard errors	Robust	PCSE	PCSE	Robust	Robust
AR(1) component	NO	YES	YES	NO	NO
Years	2008	1990-2008	1990-2008	1990-2008	1990-2008
Countries	17	17	17	17	17

Sample: EU-countries. Instrument: the rigidity of the constitution. * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.4.7: Estimation results of constitutional commitment to social security (CCSS) on different kinds of social expenditure: highest and lowest values of rigidity standardized

	(1)	(2)
Total social expenditure	2.562*	3.362**
	(1.545)	(1.341)
Old age and Survivor	−0.391	0.464
	(0.969)	(0.586)
Incapacity	2.486***	1.695***
	(0.718)	(0.282)
Unemployment	2.687***	2.134***
	(0.608)	(0.084)
ALMPs	0.806***	0.666***
	(0.177)	(0.090)
Health	−0.317	0.026
	(0.367)	(0.150)
Family	−2.397***	−2.026***
	(0.572)	(0.095)
Year dummies	Yes	Yes
Method	2SLS	Heckman
Standard errors	Robust	Robust
AR(1) component	NO	NO
Years	1990-2012	1990-2012
Countries	17	17

Sample: EU-countries. Instrument: the rigidity of the constitution. Highest values of rigidity, for The Netherlands, Belgium and Spain, are standardized to 0.37 and the lowest values of rigidity, for United Kingdom, Sweden and France, are standardized to −0.47. By this standardization we try to be as objective as possible as we choose 3 outliers of both sides of the distribution. The values 0.37 and −0.47 are equal to the values of the fourth observation from both sides of the distribution. This choice is also based on the consideration that the mean of the rigidity of the constitution is slightly negative. For the Heckman model on family expenditure we did not control for the old age dependency ratio as there was a discontinuous region encountered. * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.4.8: Estimation results of constitutional commitment to social security (CCSS) on different kinds of social expenditure: rigidity as a dichotomous variable

	(1)	(2)
Total social expenditure	4.357***	3.894***
	(1.074)	(0.870)
Old age and Survivor	2.657***	1.708***
	(0.747)	(0.492)
Incapacity	2.078***	1.621***
	(0.430)	(0.235)
Unemployment	1.676***	2.130***
	(0.300)	(0.097)
ALMPs	0.466***	0.514***
	(0.112)	(0.111)
Health	−0.534**	0.126
	(0.248)	(0.145)
Family	−1.506***	−1.829***
	(0.316)	(0.107)
Year dummies	Yes	Yes
Method	2SLS	Heckman
Standard errors	Robust	Robust
AR(1) component	NO	NO
Years	1990-2012	1990-2012
Countries	17	17

Sample: EU-countries. Instrument: the rigidity of the constitution. The dichotomous variable for the rigidity of the constitution = 1 if the the rigidity of the constitution > -0.17 and 0 otherwise. Countries with a rigid constitution are Belgium, Denmark, Finland, Germany, Italy, The Netherlands, Poland and Spain. Countries with no rigid constitution are Austria, Czech Republic, France, Greece, Hungary, Ireland, Portugal, Sweden and the United Kingdom. For the Heckman model on family expenditure we did not control for the old age dependency ratio as there was a discontinuous region encountered. * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.4.9: Estimation results of constitutional commitment to social security (CCSS) on different kinds of social expenditure: CCSS as non-dichotomous variable

	(1)	(2)	(3)	(4)
Total social expenditure				
CCSS	5.629*	1.621	4.219**	4.542***
	(2.641)	(1.655)	(1.743)	(1.523)
CCSS squared	−2.794**	−1.008	−2.698***	
	(1.230)	(0.763)	(0.763)	
old age and Survivor				
CCSS	1.783	−0.681	1.213	4.810***
	(2.407)	(1.067)	(1.017)	(0.878)
CCSS squared	−0.872	0.382	−0.582	
	(1.069)	(0.452)	(0.448)	
Incapacity				
CCSS	0.749	−0.454	0.659	1.167**
	(1.307)	(0.660)	(0.423)	(0.578)
CCSS squared	−0.275	0.212	−0.447**	
	(0.619)	(0.337)	(0.202)	
Unemployment				
CCSS	1.836	1.707***	1.299***	3.175***
	(1.038)	(0.535)	(0.356)	(0.773)
CCSS squared	−0.779	−0.790***	−0.584***	
	(0.527)	(0.247)	(0.156)	
ALMPs				
CCSS	0.48*	0.506**	0.772***	0.736***
	(0.251)	(0.230)	(0.198)	(0.217)
CCSS squared	−0.216*	−0.258**	−0.451***	
	(0.104)	(0.121)	(0.103)	
Health				
CCSS	0.793	0.580	0.196	−0.0446
	(0.949)	(0.466)	(0.537)	(0.355)
CCSS squared	−0.573	−0.387*	−0.255	
	(0.539)	(0.215)	(0.232)	
Family				
CCSS	0.166	−0.408	0.503	−3.426***
	(1.210)	(0.306)	(0.403)	(0.754)
CCSS squared	−0.099	0.103	−0.359**	
	(0.552)	(0.134)	(0.167)	
Year dummies	No	Yes	Yes	Yes
Method	OLS	OLS	OLS	2SLS
Standard errors	Robust	PCSE	PCSE	Robust
AR(1) component	NO	YES	YES	NO
Years	2008	1990-2012	1990-2012	1990-2012
Countries	17	17	17	17

Sample: EU-countries. Instrument: the rigidity of the constitution. * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.4.10: Estimation results of constitutional commitment to social security (CCSS) on total social expenditure: interaction with politics

	(1)	(2)	(3)	(4)	(5)
CCSS	4.394**	2.399***	2.229***	4.715	3.201***
	(1.655)	(0.595)	(0.819)	(2.982)	(1.081)
Government left	0.030	0.002	−0.000	0.027	0.002
	(0.020)	(0.003)	(0.003)	(0.026)	(0.007)
Government left*CCSS	−0.062*	−0.004	−0.000	−0.029	0.012
	(0.029)	(0.004)	(0.003)	(0.040)	(0.010)
CCSS	−1.105	2.117***	2.370***	1.272	1.997**
	(1.251)	(0.608)	(0.834)	(1.558)	(0.942)
Government right	−0.040**	−0.005	0.002	−0.040***	−0.041***
	(0.0146)	(0.003)	(0.003)	(0.0148)	(0.007)
Government right*CCSS	0.084***	0.004	−0.003	0.031	0.035***
	(0.020)	(0.004)	(0.004)	(0.024)	(0.010)
CCSS	3.564**	2.197***	2.145***	3.006***	3.507***
	(1.159)	(0.668)	(0.825)	(0.926)	(0.782)
Government center	0.034	0.002	−0.007	0.064***	0.077***
	(0.038)	(0.007)	(0.006)	(0.016)	(0.011)
Government center*CCSS	−0.086	0.006	0.012*	−0.068***	−0.084***
	(0.048)	(0.008)	(0.007)	(0.020)	(0.012)
Year dummies	No	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	2SLS	Heckman
Standard errors	Robust	PCSE	PCSE	Robust	Robust
AR(1) component	NO	YES	YES	NO	NO
Years	2008	1990-2012	1990-2012	1990-2012	1990-2012
Countries	17	17	17	17	17
Observations	17	381	359	381	381

Sample: EU-countries. Instrument: the rigidity of the constitution. * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

5 The Relationship between Different Social Expenditure Schemes and Poverty, Inequality and Economic Growth

Abstract

In this paper, we study how different social expenditure types are related to poverty, income inequality and GDP growth. We contribute to the literature on the potential trade-off between redistribution and economic growth as well as to the literature on the targeted versus universal approach to the welfare state. For this purposes, we use a panel data set for 22 EU-countries from 1990 till 2015. We employ OLS and 2SLS regression models in which we deal with endogeneity problems extensively. We find total public social expenditure to be negatively related to poverty and inequality, but not related to GDP growth. The results vary substantially between the different social expenditure schemes on 1) old age and survivor, 2) incapacity, 3) health, 4) family, 5) unemployment and active labour market policies and 6) housing and others.

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5.1 Introduction

Since Piketty (2014), strengthened by the rise of populist movements, there is a resurgence of the public and academic debate on income and wealth inequality. For a long time, policy makers and academics assumed a trade-off between reducing income inequality and increasing GDP growth (Okun 1975; Benabou 2000; Arjona et al. 2003). More recent studies find no evidence for such a trade-off and even find a negative association between income inequality and economic growth (Persson and Tabellini 1994; Alesina and Rodrik 1994; Perotti 1996; Easterly 2007). However, this negative association between inequality and growth does not yet imply that higher levels of redistribution are related to higher economic growth. At least, the latest empirical evidence does not support that redistribution is negatively related to economic growth (Thewissen 2013; Ostry et al. 2014). Nevertheless, redistribution is a broad concept and different kinds of redistribution, translated into different social expenditure types, have different effects on poverty, inequality and economic growth.

In this paper, we study how different social expenditure types are related to poverty, inequality and GDP growth. First, we investigate how social expenditure at the aggregated level is related to poverty, inequality and GDP growth. This analysis gives insights into the potential trade-off between poverty and inequality on the one hand and GDP growth on the other hand. Second, we study how these relationships between social expenditure and poverty, inequality and GDP growth differ for social expenditure on *1) old age and survivor, 2) incapacity, 3) health, 4) family, 5) unemployment and active labour market policies (ALMPs) and 6) housing and others*. This analysis shows the importance of the different expenditure types for reducing poverty and inequality and stimulating GDP growth.

Our first contribution to the literature is studying whether the expenditure types that reduce poverty and inequality the most are also related to economic growth. This gives new evidence for the presence or absence of a trade-off between redistribution and growth. As a result, we identify the expenditure types which are most effective in reducing poverty and inequality while also being positively related to economic growth. Our second contribution is to study how targeted as well as universal expen-

diture types affect poverty, inequality and growth. This contributes to the literature on the targeted versus the universal approach to the welfare state (Korpi and Palme 1998; Jacques and Noël 2018).

We employ OLS and 2SLS regression models in which the lagged values of the different expenditure variables are used as explanatory variables. We use social expenditure in period (t-1) because social expenditure itself is also depending on growth and potentially also on poverty and inequality. In our 2SLS model, we use the social expenditure variables in period (t-2) as instrument. Our preferred model is an OLS model with panel corrected standard errors in which we correct for first order serial correlation and control for country and year fixed effects. We use a panel data set of 22 EU-countries for the years 1990-2015 for our base results and a panel data set of 32 OECD countries in our robustness analysis. The data are taken from several OECD databases.

Our main findings are as follows. First, we find total public social expenditure to be negatively related to poverty and inequality and not significantly related to GDP growth. Hence, there seems to be no trade-off between reducing poverty and inequality on the one hand and higher economic growth on the other hand. Second, the different social expenditure schemes are differently related to poverty, inequality and economic growth, which makes more accurate targeting possible. For poverty, we find negative relations with expenditure on *family, unemployment and ALMPs* and *housing and other*.[1] For inequality, we find a strong negative connection with social expenditure on *old age and survivor* and *family*. Finally, a strong positive relation with GDP growth is found for expenditure on *housing and others*.

The outline of the paper is as follows. We start with describing the literature on the effects (and mechanisms) of social expenditure on poverty, inequality and GDP growth in Section 5.2. The data is described in Section 5.3, the methodology in Section 5.4 and the results in Section 5.5. We conclude with a discussion of the results in Section 5.6.

[1]Social expenditure on "*others*" consists for the largest part of expenditure on social assistance.

5.2 Literature

5.2.1 The effects of social expenditure on poverty and inequality

We expect social expenditure to reduce poverty and inequality (Caminada and Goudswaard 2009; Adema et al. 2014; ILO 2014). Wang et al. (2012) and Caminada et al. (2019) find that public pensions account for the largest reduction in income inequality but also social assistance, disability benefits, family benefits and unemployment benefits have a negative effect on income inequality. Wang et al. (2014) observe that the tax-benefit systems have offset two-thirds of the average increase in primary income inequality, old age benefits accounted for 60% and social assistance for 20% of the increase in redistribution.

We expect social expenditure types that are best targeted at the poor to have the largest negative effects on poverty. In contrast, the largest effects on income inequality, measured by the Gini index, are expected for social expenditure types with a more universal character. We expect universal expenditure types to have a stronger negative effect on the Gini (for income inequality) for the following two reasons. First of all, because universal social expenditure types can count on more public support as a larger share of the population is benefiting, translating in higher levels of social expenditure (Korpi and Palme 1998). Indeed, not only the targeting efficiency but also the budget size is important for reducing income inequality (Caminada et al. 2017). Second, because the Gini coefficient is much more sensitive to the income groups in the middle of the income distribution than to the bottom or the top of the income distribution.

In table 5.1 we present the share of social cash benefits received by the five quintiles of the income distribution, based on 21 EU-SILC countries in 2015. This table gives an indication which social expenditure categories are best targeted at the poor. We find that housing and social exclusion benefits are best targeted at the poor with 52% and 62% of cash benefits being received by the bottom 20% of the income distribution. Afterwards family benefits are best targeted with 48% going to the bottom 40%. Disability benefits and unemployment benefits are about equally distributed over

the five income quintiles. Social expenditure on old age is not targeted at the poor at all, only 28% of old age cash benefits are received by the bottom 40% of the income distribution.

Table 5.1: Share of social benefits received by quintiles of income distribution

	Q1	Q2	Q3	Q4	Q5
Old age benefits	11	17	19	22	30
Survivor benefits	19	21	21	18	20
Disability benefits	20	23	22	19	17
Family benefits	23	25	21	17	14
Unemployment benefits	24	20	17	18	22
Housing benefits	52	23	9	9	7
Social Exclusion benefits	62	17	10	7	5

Notes: Source: Own calculations based on EU Survey on Income and Living Conditions for European countries (EU-SILC). The calculations are based on equialized household income in 2015 for 21 of the 22 EU-countries in our sample, excluding Germany which is not available in EU SILC.

Another expenditure type which we expect to be effective in reducing poverty and inequality is family expenditure. First of all, because families are more often poor because income must be shared with more household members, including children and non-working adult members. In line with this, higher poverty rates are observed among children than among adults. Second, due to economies of scale for larger households, it is relatively cheap to reduce the poverty rate by targeting on families. Also for the Gini, we expect a large negative effect of family spending, because a large share of family spending is received by the second and third quintiles of the income distribution (25% and 21% of family spending). Increasing income for the second and third quintiles is expected to be relatively effective in reducing the Gini for income inequality because the Gini is relatively sensitive to the income groups in the middle of the income distribution.

The effects of social expenditure on economic growth 5.2.2

The literature is divided on the effect of social spending on growth. On the one hand, Barro (1996) shows that government expenditure has a negative effect on economic growth and Arjona et al. (2003) find some evidence

that social expenditure reduces growth. On the other hand, most studies reject the hypothesis that social expenditure has a negative impact on growth (e.g. Atkinson 1995; Singh 1996; Baldacci et al. 2008; Thewissen 2013; Ostry et al. 2014; Bakija et al. 2016). In line with this, Cingano (2014), OECD (2015) and Dabla-Norris et al. (2015) show that inequality reduces economic growth, suggesting that redistribution may increase growth.

Capital accumulation is one of the main mechanisms that can explain GDP growth rates (Solow 1956). The effect on capital accumulation highly depends on the social insurance system in place. In a pay-as-you-go pension system, the expected effect of old age expenditure on savings is negative as fewer savings are needed when retirees receive a pension paid by the working age population (Feldstein 1974). In a capital-based system, premiums for social insurance can be higher than the amount people would have saved otherwise. Hence, a capital-based pension system can increase total savings and investments and thereby economic growth.

Another main determinant of growth is labor supply. The welfare state typically decreases labor supply as the benefit of supplying labor decreases when the outside option becomes more attractive (Krueger and Meyer 2002; French and Song 2014). There are also some studies which find no effect, or even a positive effect, of social protection schemes on labor supply (Krueger and Pischke 1992; Rust and Phelan 1997). All these studies show that the effects of welfare state programs (e.g. retirement schemes) on labor supply can be explained for a large part by the specific features of the social security system. The largest negative effects on labor supply are expected for the expenditure type *unemployment and ALMPs*, as these target the working age population and not children, the old or the disabled. Besides, only unemployed people are eligible for unemployment benefits, which may create a disincentive to work.

In addition to labor supply, the level of productivity is also important. Social expenditure affects the level of productivity by two main mechanisms: it increases risk-taking behavior and it reduces poverty. First, social protection decreases income risks and this may increase risk-taking, investments, productivity and thereby growth.We expect an increase in risk taking, not only for the poor, but also for the middle class who know they can rely on the social safety net when needed. For example, social security

increases investments by employees in their firm and industry-specific skills (Estevez-Abe et al. 2001). As social protection provides security, the willingness to build up dependence on particular employers and hence being more vulnerable to market fluctuations increases. Second, poverty has a negative effect on productivity. Children's health, capabilities and achievements are negatively affected by poverty (Aber et al. 1997; Brooks-Gunn and Duncan 1997). Furthermore, poverty reduces the cognitive capacity of the brain (Banerjee et al. 2006; Mani et al. 2013). Hence, reducing poverty may increase the capacities of poor people which may translate into increases in productivity and GDP growth. Not only poverty, but also inequality can be detrimental to economic growth. Increased income inequality depresses the development of skills among individuals whose parents have a lower education background (Cingano 2014; OECD 2014). The driver of this negative impact of inequality on growth is the gap between low-income households and the rest of the population.

Finally, social expenditure is expected to have a positive effect on aggregate demand, as the lower and middle-income groups consume a larger part of their income (Keynes 1937). For this reason, we expect the largest positive effects on aggregate demend, for the best targeted schemes. The positive effect of an increase in aggregate demand on economic growth is expected to be larger when aggregate demand is low. Blanchard and Leigh (2013) find large fiscal multiplier during the Great Recession. Darby and Melitz (2008) show in an empirical analysis for 21 OECD countries that spending on unemployment, old age and health-related social expenditure, as well as incapacity and sickness benefits, react to the cycle in a stabilizing manner.

All in all, for GDP growth, we expect the largest positive effects of the targeted schemes when the most important mechanisms are: an increase in risk-taking, releasing the potential of the poor and increasing aggregate demand. Furthermore, we expect the potential negative effects caused by lower capital accumulation to be limited for poor people, because of their low levels of physical capital. Potential negative effects of targeted schemes on labor supply may be compensated by higher levels of productivity when more of the potential of poor people is released. Hence, we expect the largest positive effects on GDP growth of spending on *housing*

and others, as these are best targeted at the poor, see Table 5.1. If labor supply is important for economic growth, spending on *unemployment and ALMPs* is expected to reduce GDP growth, as unemployment benefits may disincentivize work. When we consider the size of the different social expenditure types, we expect large effects of spending on *old age and survivor* as this spending category is most sizable. However, the direction of the effect of old age spending on GDP growth is harder to predict, because a lot depends on the institutional settings of countries.

5.3 Data

We use a panel data set for 22 EU-countries that are a member of the OECD covering 26 years from 1990-2015. The countries in our EU sample are: Austria, Belgium, the Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Luxembourg, the Netherlands, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden and the United Kingdom. The data set is limited to EU countries that are a member of the OECD for reasons of data availability, but also because these countries are more similar in their characteristics, making the results more reliable. We employ the same analysis for a sample of 32 OECD countries in our robustness analysis. Selecting the period 1990-2015 allows us to take the post-Soviet states into account and provides us with a more balanced sample as much less data is available for the years before 1990.

Our dependent variables are the poverty rate (poverty after taxes and transfers for a poverty line of 50%), the Gini coefficient for income inequality (Gini for disposable income post taxes and transfers) and average GDP growth rate over 3 years [(growth(t) + growth(t+1) + growth(t+2))/3] (annual growth of GDP per capita, constant prices, in percentage). We use the average annual GDP growth rate over the next three years to reduce the endogeneity problem (Thewissen 2013). The Poverty rates and the Gini coefficients are taken from the Income Distribution Database of the OECD and the GDP growth rates are taken from the Annual National Accounts data of the OECD.

The explanatory variables of interest are social expenditure variables for which we use the Social Expenditure Database (SOCX) of the OECD. We are aware that social expenditure variables have limitations in explaining the degree of social protection and generosity (De Deken 2014; Van Vliet and Wang 2015). First, differences in spending may reflect variation in demographic and socio-economic trends across countries. Second, expenditures neglect some important institutional characteristics of welfare state programmes, such as the extent to which welfare state programmes are means-tested. Third, gross social expenditure does not take the taxation of benefits into account. We deal with these problems by including year and country fixed effects and a large number of economic and demographic controls to control for different demographic and socio-economic trends and different institutional characteristics. We use gross social expenditure variables for our base results because not much data is available on net social expenditure and no data is available on net social expenditure for the different expenditure categories. But we perform the same analysis with the limited available data for net social expenditure in our robustness analysis. All in all, social expenditure variables are the most objective and most used variables for studying the effects of the welfare state.

Another point is if we should include old age expenditure in total public social expenditure when we are interested in the redistributive effects of social expenditure. Most studies (e.g. the OECD studies) are looking at expenditure schemes targeted at the working-age population on poverty and inequality among the working-age. The main question is if pensions are about redistribution over the life cycle or about redistribution between individuals. Also cohort effects may blur the effects of social expenditure. We choose to look at different social expenditure types, among which old age expenditure, separately. Further, we look at the effects on poverty and inequality for the total population as well as for the working-age population. Furthermore, we control for demographics to ensure that the coefficients are not biased by cohort effects.

Our main explanatory variable is total public social expenditure (as % of GDP), as the quality of public social expenditure data is the highest when we consider the different expenditure types, especially for the comparison over time. We also look at the effects of total public and

mandatory private social expenditure and total social expenditure (including public, mandatory private and voluntary private) in the robustness analysis. The reason for this that public and private social expenditure are close substitutes (Goudswaard and Caminada 2010). Our total public social expenditure variable is separated in spending on 1) old age and survivor, 2) incapacity, 3) health, 4) family, 5) unemployment and active labour market policies and 6) housing and others, which are our next explanatory variables. See Table A.5.1 for a more detailed description of these different categories of social expenditure.

Table 5.2 shows the descriptive statistics for poverty, inequality, and GDP growth and the various social expenditure variables for our sample of EU countries during the period 1990-2015. On average 9.2 percent of the population has an income below the poverty line of 50% of the median income. Our indicator for inequality, the Gini coefficient, is on average 0.29 in this period. GDP growth is on average 2.4 percent between 1990 and 2015. Table 5.2 also denotes the mean values and standard deviations for the different social expenditure variables. Total public social expenditure is on average 22.1% of GDP, the largest part is going to *old age and survivor* (9.1% of GDP) and *health spending* (5.7% of GDP). Lower amounts are spent on *incapacity* (2.6% of GDP), *families* (2.2% of GDP), *unemployment and ALMPs* (1.8%) and *housing and others* (0.7%).

Table 5.2: Descriptive statistis: dependent and explanatory variables 1990-2015 for EU-sample

Variable	Mean	Std. Dev.	Min.	Max.	Obs.
Poverty	9.2	3.1	3.6	18.6	317
Gini	0.29	0.04	0.21	0.39	317
GDP growth	2.4	2.6	-7.3	13.0	555
Total public SE	22.1	4.5	11.1	34.7	534
Old age and Survivor SE	9.1	2.7	3.1	17.1	535
Incapacity SE	2.6	1.1	0.8	5.9	535
Health SE	5.7	1.3	2.3	9.3	545
Family SE	2.2	0.9	0.3	4.5	535
Unemp. and ALMPS SE	1.8	1.2	0.1	6.1	533
Housing and Others SE	0.7	0.5	0.0	2.2	521

The control variables we use in our models for poverty and inequality are GDP per capita (measured in thousands of USD, constant prices,

2010 PPPs), unemployment rate (harmonized), population share 15-64 years of age, population share 65 plus and the trade union density, the data are taken from the OECD databases except for the population data which is taken from United Nations database. We control for business cycle fluctuations and demographics as both have an effect on both social expenditure and poverty and inequality. We consider trade union density as a control for labor market institutions, as unions may increase pressure to increase social expenditure and decrease poverty and inequality (Card 2001; Hooghe and Oser 2016).

In our models for GDP growth we use the control variables population share 15-64 years of age, population share 65 plus of age, gross capital formation (annual growth rate), education (share of population attained tertiary education, 25-64 years), export (as % of GDP) and inflation (consumer price all items, annual % change). We add these control variables to our model as we expect them to have an effect on both social expenditure and on GDP growth. These control variables are based on the papers of (Solow 1956; Barro 1996; Bellettini and Ceroni 2000; Barro 2013). All this data are taken from the OECD databases. See Table A.5.2 for the descriptive statistics of the control variables.

Empirical methodology 5.4

Endogeneity issues 5.4.1

We start this section with elaborating on the reverse causality issue. Not only social expenditure can have an effect on poverty, inequality and economic growth, but also the other way around. We expect a positive effect of poverty and inequality on social expenditure (Alesina and Rodrik 1994; Arjona et al. 2003; Milanovic 2000). This positive effect can be explained by the median voter who cares more about redistribution if the possibilities and benefits of redistribution are larger, which is the case when poverty and inequality are more severe. This positive effect of poverty and inequality on social expenditure may cause a positive relation between social expenditure and poverty and inequality, leading to an

underestimation of a negative effect of social expenditure on poverty and inequality. For economic growth, we expect a negative effect on social expenditure as a percentage of GDP, at least in the short term which we are studying. First of all, because the denominator of social expenditure as a percentage of GDP per capita increases, second because social expenditure is negatively related to the business cycle. This negative effect of GDP growth on social expenditure (as % of GDP) could translate in a negative relationship between social expenditure and GDP growth, leading to an underestimation of a potential positive effect of social expenditure on GDP growth. In short, the coefficients we will find are conservative estimates for the potential negative effects on poverty and inequality and potential positive effect on GDP growth.

We reduce the problem of reverse causality by using the social expenditure variables in period (t-1), as we expect that the dependent variables in period t can not have an effect on the explanatory variables in period (t-1). We also check if the results are robust when we consider different time lags, up to a 5-year period lag, as reverse causality becomes less likely with a longer time lag. In line with the literature, we use the average annual GDP growth rate over the next three years [(growth(t) + growth(t+1) + growth(t+2))/3] as dependent variable in the growth models to reduce endogeneity problems even further (Thewissen 2013).

Besides, we use 2SLS regression models to correct for possible endogeneity. In the 2SLS model, we use the social expenditure variables in period (t-2) as instruments because we argue that social expenditure in period (t-2) has an effect on social expenditure in period (t-1) but no direct effect on poverty, inequality and growth two periods later. We indeed find high F-statistics in the first stage indicating that the instrument is relevant. The exclusion restriction is harder to prove statistically, but it is plausible that the dependent variables poverty, inequality and growth are in the first place affected by a change in social expenditure in the same period or the next period and less, or not at all, two periods later. Nevertheless, we prefer to be cautious by considering the 2SLS results jointly with the OLS estimates, as it is impossible to prove that social expenditure in period (t-2) has no direct effect on our outcome variables. The 2SLS estimates

generally give very similar results to the OLS estimates, indicating that the effects are really due to social expenditure.

Our preferred model is an OLS regression model which contains panel corrected standard errors and in which we control for first order serial correlation. In addition, we include year and country fixed effects to control for different demographic and socio-economic trends and different institutions. This model deals most extensively with possible simultaneity problems in which social expenditure and the dependent variables move simultaneously and affect each other over time.

Empirical specification 5.4.2

The model is built step by step to show how the different parts of the model change the results. The first specification shows a correlation coefficient when we do not include controls. In specification 2, we include the economic, demographic and institutional control variables. We add year fixed effects to control for the business cycle and other time effects in specification 3. We include country fixed effects to control for unobserved characteristics (e.g. institutional differences between countries) in specification 4. Afterwards, in specification 5, we run a 2SLS regression model, in which we use the social expenditure variables in period (t-2) as instruments. The regression equation of our 2SLS model is as follows:

$$y_{it} = \alpha_t + \beta_i + X'_{it}\nu_x + \gamma SE_{it-1} + \epsilon_{it}. \tag{5.1}$$

$$SE_{it-1} = \alpha_t + \beta_i + \delta SE_{it-2} + X'_{it}\nu_x + \mu_{it} \tag{5.2}$$

The dependent variables in which we are interested are denoted by y_{it}, standing for poverty, inequality and GDP growth, which vary by country $(i = 1, ..., N)$ and years $(t = 1, ..., T)$. We regress the outcome variables on year fixed effects (α_t) country fixed effects (β_i), economic and demographic controls (X'_{it}) with coefficients ν_x and the explanatory variables of interest

for social expenditure (SE_{it-1}) with coefficient γ. The second lags of the social expenditure variables, our instruments in the first stage, are captured by SE_{it-2} with coefficient δ.

Finally, specification 5 gives our most preferred model, given by regression equations (3) and (4):

$$y_{it} = \alpha_t + \beta_i + X'_{it}\nu_x + \gamma SE_{it-1} + \mu_{it} \tag{5.3}$$

$$\mu_{it} = \rho\mu_{it-1} + \epsilon_{it} \tag{5.4}$$

We prefer this OLS model over the 2SLS model as we can not prove that the exclusion restriction holds, making OLS estimates with panel corrected standard errors in which we control for first order autocorrelation most reliable. This model is the same as the second stage of the 2SLS model, but now we control for autocorrelation in the error term. We use robust standard errors in the first four empirical specifications and panel corrected standard errors in specification 5.

5.5 Results

5.5.1 Main results

Table 5.3 presents the results for the relationship between total public social expenditure and poverty. The first column shows the correlation coefficient in the model when we only control for economic, demographic and institutional control variables. We find a negative significant coefficient of -0.237. Adding year fixed effects in column 2 increases the negative coefficient to -0.409. The coefficient decreases slightly when we include country fixed effects in column 3, but increases again to 0.431 in our 2SLS model in column 4. In our preferred specification, column 5, we run an OLS model with panel corrected standard errors in which we control for serial correlation. The coefficient of total public social expenditure on

poverty has a statistically significant coefficient of −0.337. This coefficient indicates that a one percentage point increase in total social expenditure is associated with a 0.337 percentage point lower poverty level one year later. Also increases in GDP per capita, the population share 15-64 years of age, the population share 65 plus and the union density rate are associated with lower poverty rates. However, these coefficients are smaller than the coefficient for total public social expenditure.

Table 5.3: Estimation results of total public social expenditure on poverty

	(1) Poverty	(2) Poverty	(3) Poverty	(4) Poverty	(5) Poverty
Total pub. SE (t-1)	−0.237***	−0.409***	−0.372***	−0.431***	−0.337***
	(0.053)	(0.080)	(0.107)	(0.077)	(0.074)
GDP per. c. (t-1)	0.020	0.027	−0.162	−0.180**	−0.147 **
	(0.038)	(0.027)	(0.117)	(0.086)	(0.059)
Unemp. rate (t-1)	0.085	0.303***	0.032	0.030	0.037
	(0.059)	(0.083)	(0.072)	(0.049)	(0.040)
Pop. 15-64 (t-1)	−0.130	−0.369	−0.275	−0.371**	−0.286*
	(0.246)	(0.246)	(0.272)	(0.172)	(0.170)
Pop. 65+ (t-1)	0.246	0.583**	−0.209	−0.269**	−0.223*
	(0.171)	(0.249)	(0.160)	(0.126)	(0.133)
U. density (t-1)	−0.058***	−0.032**	−0.102***	−0.092***	−0.097***
	(0.020)	(0.014)	(0.038)	(0.023)	(0.016)
Control variables	Yes	Yes	Yes	Yes	Yes
Year dummies	No	Yes	Yes	Yes	Yes
Country FE	No	No	Yes	Yes	Yes
Ar1 component	No	No	No	No	Yes
Method	OLS	OLS	OLS	2SLS	OLS
Standard errors	Robust	Robust	Robust	Robust	PCSE
Observations	275	275	275	270	275
R-squared	0.468	0.628	0.926	0.327	0.923
Countries	22	22	22	21	22

Notes * denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table 5.4 shows the relation between total public social expenditure (t-1) and poverty, Gini and GDP growth in our preferred model. Tables A.5.3 and A.5.4 in the appendix show the six different regression models for inequality and growth. In Table 5.4, we find a negative significant coefficient of total public social expenditure on inequality of −0.0038, which is 9% of the standard deviation of Gini. This coefficient seems small but is large compared to the coefficients of GDP per capita (−0.0018), unemployment rate (−0.0008) and the union density (−0.0006), which are the controls that are significantly related to the Gini, see Table A.5.3. In column 3, we find a positive but statistically insignificant coefficient for

Table 5.4: Estimation results of total public social expenditure on poverty, inequality and GDP growth

	(1) Poverty	(2) Gini	(3) GDP growth
Total pubic SE (t-1)	−0.337***	−0.0038***	0.142
	(0.074)	(0.0005)	(0.102
GDP per capita (t-1)	−0.147**	−0.0018***	
	(0.059)	(0.0004)	
Unemployment rate (t-1)	0.037	0.0008***	
	(0.040)	(0.0002)	
Union density (t-1)	−0.097***	−0.0006**	
	(0.016)	(0.0002)	
Population 15-64 (t-1)	−0.286*	−0.0019	−0.118
	(0.170)	(0.0019)	(0.261)
Population 65 plus (t-1)	−0.223*	−0.0012	−0.222
	(0.133)	(0.0013)	(0.293)
Capital form. growth (t-1)			−0.001
			(0.013)
Education (t-1)			−0.006
			(0.056)
Export (t-1)			0.056**
			(0.028)
Inflation (t-1)			−0.096*
			(0.057)
Control variables	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	275	275	406
R-squared	0.923	0.9634	0.610
Number of countries	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

total public social expenditure on GDP growth. Finding a statistically insignificant coefficient may explain why the effect of social protection on GDP growth is still disputed in the academic literature. In Table A.5.4 we present the other models for growth and we find a negative significant relation in specifications 1-3, but the coefficient becomes positive and statistically insignificant when we include country fixed effects. This suggests that countries with lower social spending have grown faster, but that no effect remains when we merely consider the within countries variation over time by controlling for (unobserved) differences between countries. Adding fixed effects is needed to make sure that there are no other differences between countries that explain both social expenditure and GDP growth, for example different phases of development.

Table 5.5: Estimation results of different kinds of social expenditure on poverty, inequality and GDP growth

	(1) Poverty	(2) Gini	(3) GDP growth
Old age & Survivor SE (t-1)	−0.197	−0.0058***	0.275
	(0.152)	(0.0009)	(0.254)
Incapacity SE (t-1)	−0.061	−0.0009	0.023
	(0.324)	(0.0027)	(0.279)
Health SE (t-1)	−0.021	−0.0015	−0.033
	(0.154)	(0.0009)	(0.222)
Family SE (t-1)	−1.156***	−0.0108***	0.576
	(0.215)	(0.0035)	(0.466)
Unempl. & ALMPS SE (t-1)	−0.429***	−0.0021	−0.332
	(0.141)	(0.0018)	(0.291)
Housing & Others SE (t-1)	−0.794**	0.0037	1.211*
	(0.367)	(0.0028)	(0.644)
GDP per capita (t-1)	−0.146**	−0.0021***	
	(0.063)	(0.0005)	
Unemployment rate (t-1)	0.029	0.0010***	
	(0.037)	(0.0002)	
Union density (t-1)	−0.078***	−0.0007**	
	(0.016)	(0.0003)	
Population 15-64 (t-1)	−0.183	−0.0004	−0.187
	(0.186)	(0.0018)	(0.264)
Population 65 plus (t-1)	−0.114	−0.0003	−0.341
	(0.165)	(0.0012)	(0.322)
Capital form. growth (t-1)			−0.001
			(0.013)
Education (t-1)			−0.014
			(0.058)
Export (t-1)			0.061**
			(0.029)
Inflation (t-1)			−0.106*
			(0.059)
Control variables	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes
Country FE	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	275	275	400
R-squared	0.927	0.9676	0.619
Number of countries	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

We use the same preferred models to examine the relationships between the different social expenditure types and poverty, inequality and GDP growth, see Table 5.5. Column 1 gives the relation between the different social expenditure schemes and poverty. Social expenditure on *family, unemployment and ALMPs* and *housing and others* are negatively and significantly related to poverty. The largest coefficients are found for *family* (−1.156) and *housing and others* (−0.794). This indicates that a one

percentage point increase in social spending on families as a percentage of GDP is associated with a 1.156 percentage point lower poverty rate in the next year. Column 2 shows the connection between the different kinds of social expenditure and the Gini coefficient for income inequality. We find that spending on *old age and survivor* (−0.0058) and *family* (−0.0108) are negatively and significantly related to the Gini coefficient. In column 3, we find that only expenditure on *housing and others* (1.211) is significantly related to GDP growth. A one percentage point increase in public social expenditure on *housing and others* is associated with a 1.211 percentage point increase in GDP growth over the next three years. Although, countries spend on average only 0.7% of GDP on *housing and others* indicating that this spending category still plays only a small role as determinant of GDP growth. The results in Table 5.5 suggest large differences in effects between the different social expenditure schemes, providing policy makers with the possibility to target more accurately when picking social expenditure schemes for the policy goals of reducing poverty and inequality without detrimental effects on GDP growth.

The two largest categories of social expenditure, *old age and survivor* and *health*, are particularly interesting to look at. *Old age and survivor* expenditure is negatively and significantly related to inequality but there is no statistically significant relation to poverty nor to GDP growth. Finding a strong negative relation with the Gini but no statistically significant effect on poverty indicates that the groups in the middle of the income distribution benefit most from spending on *old age and survivor*. The large positive coefficient for *old age and survivor* on GDP growth indicates that there is at least no large negative association between spending on *old age and survivor* and GDP growth. For *health* expenditure, we find no significant relationship with any of the outcome variables.

In Table A.5.5, we run separate regression models for the different social expenditure variables including only one social expenditure variable in our model at a time. We do this because inclusion of all could lead to multicollinearity issues. This additional analysis shows that the only difference is that the negative coefficients of social expenditure on *incapacity* and *unemployment and ALMPs* on the Gini become statistically significant.

Sensitivity analysis 5.5.2

Finally, we run a large number of additional robustness checks. The results are almost the same for the effects of 'total public and private mandatory social expenditure' (Table A.5.6) and 'total social expenditure' (including voluntary private social expenditure) (Table A.5.7). We also study the relation with net total public social expenditure and net total public and private social expenditure, see Table A.5.8 and Table A.5.9.[2] These indicators capture the amount of actually received social benefits much better, but much less data is available for the net indicator. Furthermore, the quality of the data is limited. Also for net social expenditure we find results that are similar to our results for gross total social expenditure.

In table A.5.10 we look at the effects for a sample of 32 OECD countries for our poverty and inequality models and 33 OECD countries for our growth models.[3] The coefficient size of total public social expenditure on poverty slightly decreases to -0.239 and the coefficient size of total public social expenditure on the Gini decreases substantially to -0.0017, both coefficients remain highly significant. For GDP growth, our positive coefficient of total public social expenditure is statistically significant when we consider the sample of OECD countries, 1 percentage point increase in public social expenditure is associated with a 0.134 percentage point increase in GDP growth in the next three years.

For the different expenditure categories, presented in table A.5.11, we find very similar results using the OECD sample compared to the EU sample. The only two differences are that for the OECD sample the negative coefficient of *unemployment and ALMPs* on the Gini becomes statistically significant and the positive coefficient of expenditure on *housing and others* on GDP growth turns statistically insignificant. Table A.5.12 shows again

[2]Because of missing data, the sample for the analysis of net social expenditure is excluding Greece, Hungary, Latvia, Luxembourg and Portugal for the poverty and inequality models and excludes Latvia in the growth model.

[3]Countries in OECD sample: Australia, Austria, Belgium, Canada, Chile, the Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Korea, Latvia, Luxembourg, Mexico, the Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, Turkey, the United Kingdom, United States. Excluding Israel, Mexico and Australia in our poverty and inequality models and excluding New Zealand and Turkey in our GDP growth models, for reasons of data availability.

similar results when we include only one social expenditure category in the model at a time. The negative coefficients of *incapacity* spending on poverty as well as on the Gini turn significant now. When we separate *housing and other*, in Table A.5.12, we still find a positive significant coefficient for '*other*' (mostly social assistance) on GDP growth.

In Table A.5.13, we show the results for the years 2008-2015. We find a smaller negative coefficient for total public social expenditure on poverty and inequality and a larger positive coefficient, but statistically insignificant, for public social expenditure on GDP growth. Table A.5.14 shows the results for the years 1990-2007 and confirms that our results are not driven by the Great Recession, as the differences between the results in Table A.5.13 and A.5.14 are not statistically significant for poverty and GDP growth.[4] Although not statistically significant, the coefficient size of total public social expenditure on GDP growth is more than two times larger for the years 2008-2015 than for the years 1990-2007. Hence, if the business cycle has any impact on the effect of total public expenditure on GDP growth it would probably be positive. Also, it is possible that the effect of social expenditure on GDP growth has become more positive over time, independent of the business cycle.[5]

We also study if our results are robust for the working age population. In Table A.5.15 we find that the coefficient of total public social expenditure on the working poor is small and insignificant. However, we do find a negative association between total public social expenditure and the poverty rate and Gini coefficient for the age group 18–65. We find that spending on *family, unemployment and ALMPs* and *housing and others* do

[4]Table A.5.14 suggests a stronger negative relationship between total public social expenditure and the Gini in the period 1990-2007 than in the period 2008-2015.

[5]A change in this relationship over time would be in line with our finding of a positive coefficient of the interaction between social expenditure and a timetrend on GDP growth (results are available on request). This would also be in line with finding a negative relationship between social expenditure and growth in some older studies, whereas most recent studies find a positive relationship. This change over time in the relationship between social expenditure and GDP growth may partly be explained by ageing populations as we also find a positive coefficient for the interaction between social expenditure and the population share 65 plus on GDP growth (results are available on request). However, this interaction term with population share 65 plus is much smaller than the interaction with a timetrend and is not statistically significant. Therefore, ageing is unlikely to be the only reason why the relationship between social expenditure and GDP growth may have become more positive over time.

also have the strongest negative relation with poverty for the working age population. A counter-intuitive statistically significant positive association is found between spending on *incapacity* and poverty among the working age population. Spending on *old age and survivor* and *family* are again negatively related to inequality when we consider the working age population. For GDP growth, our findings are robust when we study the effect of total public social expenditure on growth rates for GDP per member of the working age population (results are available on request). This is in line what we expected as we have already controlled for demographic structure in our preferred models.

Table A.5.16 gives the results for a poverty rate of 60% instead of the 50% poverty rate. On average 16.1% of the people in the EU had an income of less than 60% of the median income (Table A.5.2), whereas only 9.2% of the people had an income of less than 50% of the median income (Table5.2). Studying the relationship between total public social expenditure and the 60% poverty rate, in Table A.5.16 Column 1, gives a coefficient of 0.570. In column 2, we study how the different social expenditure types are related to the 60% poverty rate. We find again substantial negative significant coefficients for social expenditure on *family* and *unemployment and ALMPS*, but the large negative coefficient of *Housing and Others* turns statistically insignificant. Most interesting is the negative relation between *Old age and Survivor* which becomes stronger and statistically significant, with a coefficient of -0.504. When we compare the differences in results between the 50% and 60% poverty rates we can infer the following: *Housing and Others* are most effective in reducing poverty among the poorest decentile, whereas *Old age and Survivor* spending is reducing poverty among the second decentile of the income distribution.

In a final robustness test we consider different time lags for our explanatory variables. Table A.5.17 shows that the negative relation between total public social expenditure and poverty is almost exactly the same when we use different time lags. Table A.5.18 presents that total public social expenditure remains negatively and significant related to the Gini, but the coefficient size decreases to -0.0019 when we consider a 5-year time lag. For GDP growth, we find that our results are robust for different time lags, except for a larger positive and statistically significant coefficient when

we consider a 5-year time lag, see Table A.5.19. This result provides some evidence that the relationship between social expenditure and growth is not only a short run relation caused by an aggregated demand effect, but that the positive effect can be considered to be a long-run effect.

5.6 Conclusion

In this paper, we studied how different social expenditure schemes are related to poverty, inequality and economic growth. First, we find that total public social expenditure is negatively related to poverty and inequality, but not related to GDP growth. Hence, the results do not support a trade-off between reducing poverty and inequality on the one hand and increasing GDP growth on the other. The negative effect on poverty and inequality corresponds with the literature (Caminada and Goudswaard 2009; Wang et al. 2012; Wang et al. 2014). Finding no significant relation with GDP growth is in line with Bellettini and Ceroni 2000; Thewissen 2013; Ostry et al. 2014, who find no negative effect of social expenditure on economic growth.

Second, we find substantial differences in the effects of various types of social expenditure. These differences allow policy makers to achieve better targeting and thereby increase the effectiveness of reducing poverty and inequality, without detrimental effects on GDP growth. Studying which expenditure categories are most effective answers the call of Ostry et al. (2014) for more research on the mechanisms at play to make redistribution as efficient as possible.

Our results suggest that social expenditure types targeted at *families* are most strongly negatively related to both poverty and inequality. Social expenditure on *unemployment and ALMPs* and *housing and others* (mostly social assistance) are also effective ways of reducing poverty but are not reducing the Gini (for income inequality). Social expenditure on *old age and survivor* is negatively related to Gini for income inequality but the negative relation with poverty is not statistically significant. Hence, social expenditure on *family*, *unemployment and ALMPs* and *housing and others* are on average better targeted, while social expenditure on *old age and*

survivor has a more universal character to the benefit of a larger group of people. However, the budget size of the targeted expenditure schemes is relatively small and therefore the effect of old age expenditure is still relatively important as much larger amounts are spent on the *old age and survivor* category.

For GDP growth, finding a strong positive relationship with social expenditure on *housing and others* indicates that the best targeted social expenditure schemes are positively associated with GDP growth. This is in line with Cingano (2014) and OECD (2014) who show that the negative impact of inequality on growth can mainly be explained by the gap between the bottom and the middle of the income distribution. This positive association between spending on *housing and others* and GDP growth could be explained by the positive effects of the safety net on the potential of the poor, the development of skills, levels of risk-taking and aggregate demand. Potential negative effects on labor supply may be compensated by higher levels of productivity when more of the potential of poor people is released. Furthermore, expenditure on housing may have a large fiscal multiplier as there are non-negligible spillover effects from the housing market to the broader economy (Iacoviello and Neri 2010), causing a relatively large positive effect on GDP growth.

All in all, we can conclude that the expenditure types most effective in reducing poverty are also positively related to economic growth, indicating that there is no evidence for a trade-off between redistribution and economic growth. Second, the more universal expenditure types (*old age and survivor* and *family*) are most effective in reducing the Gini for income inequality, which is in line with Korpi and Palme (1998).

5.A Supplementary material

Table A.5.1: The OECD social expenditure categories

Category	Description
Old-age	Pensions, early retirement pensions, home-help and residential services for the elderly.
Survivors	Pensions and funeral payments.
Incapacity	Care services, disability benefits, benefits accruing from occupational injury and accident legislation, employee sickness payments.
Health	Spending on in- and out-patient care, medical goods, prevention.
Family	Child allowances and credits, childcare support, income support during leave and sole parent payments.
ALMPS	Active labour market policies: employment services, training youth measures subsidized employment, employment measures for the disabled.
Unemployment	Unemployment compensation, severance pay and early retirement for labour market reasons.
Housing	Housing allowances and rent subsidies.
Other	Social policy areas, non-categorical cash benefits to low-income households, other social services; i.e. support programs such as food subsidies.

Description of the different categories is taken from OECD (2007)

Table A.5.2: Descriptive statistics: control variables 1990-2015 for EU-sample

Variable	Mean	Std. Dev.	Min	Max	Obs
Total public and mandatory private SE	22.4	4.7	11.1	35.2	534
Total public and private SE	24.1	5.4	11.2	37.6	534
Net public SE	20.0	3.3	12.1	28.0	159
Net total SE	21.7	3.7	13.0	31.2	159
GDP per capita	31.6	13.5	8.0	91.4	562
Unemployment rate	8.9	4.3	1.0	27.5	528
Population 15-65	67.1	1.8	61.4	72.2	572
Population 65 plus	15.5	2.4	10.0	22.4	572
Union density	35.3	21.6	2.4	93.9	486
Capital formation growth	2.9	10.6	-41.7	49.8	554
Education	24.3	8.7	6.1	45.9	434
Export share	50.2	30.7	14.0	222.7	563
Inflation	8.5	53.5	-4.5	951.7	558
Poverty working	7.0	3.1	2.8	15.1	236
Poverty 1865	8.6	2.8	3.6	16.5	315
Gini 1865	0.29	0.4	0.21	0.39	317
Poverty 60%	16.1	3.6	7.9	26.2	290

Table A.5.3: Total public social expenditure on Gini: base results

	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini
Total pubic SE (t-1)	−0.0006	−0.0029***	−0.0060***	−0.0041***	−0.0049***	−0.0038***
	(0.0009)	(0.0009)	(0.0014)	(0.0008)	(0.0009)	(0.0005)
GDP per capita (t-1)		0.0008	0.0003	−0.0021**	−0.0023***	−0.0018***
		(0.0005)	(0.0003)	(0.0009)	(0.0008)	(0.0004)
Unemployment rate (t-1)		0.0015**	0.0029***	0.0008	0.0009*	0.0008***
		(0.0007)	(0.0008)	(0.0007)	(0.0005)	(0.0002)
Population 15-64 (t-1)		−0.0025	−0.0132***	−0.0024	−0.0035*	−0.0019
		(0.0035)	(0.0038)	(0.0024)	(0.0021)	(0.0019)
Population 65 plus (t-1)		0.0021	0.0022	−0.0015	−0.0030**	−0.0012
		(0.0028)	(0.0027)	(0.0015)	(0.0015)	(0.0013)
Union density (t-1)		−0.0007**	−0.0008***	−0.0005	−0.0005*	−0.0006
		(0.0003)	(0.0002)	(0.0004)	(0.0003)	(0.0002)
Control variables	No	Yes	Yes	Yes	Yes	Yes
Year dummies	No	No	Yes	Yes	Yes	Yes
Country fixed effects	No	No	No	Yes	Yes	Yes
Ar1 component	No	No	No	No	No	Yes
Method	OLS	OLS	OLS	OLS	2SLS	OLS
Standard errors	Robust	Robust	Robust	Robust	Robust	PCSE
Observations	303	275	275	275	270	275
R-squared	0.161	0.537	0.706	0.952	0.4657	0.9634
Number of countries	22	22	22	22	21	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.4: Total public social expenditure on GDP growth: base results

	(1) GDP growth	(2) GDP growth	(3) GDP growth	(4) GDP growth	(5) GDP growth	(6) GDP growth
Total pubic SE (t-1)	−0.120**	−0.126**	−0.088**	0.019	0.072	0.142
	(0.051)	(0.056)	(0.043)	(0.109)	(0.071)	(0.102)
Cap. for. gr. (t-1)		0.044***	0.058***	0.055***	0.056***	−0.001
		(0.016)	(0.017)	(0.017)	(0.014)	(0.013)
Education (t-1)		−0.032	−0.019	−0.015	−0.027	−0.006
		(0.023)	(0.024)	(0.087)	(0.049)	(0.056)
Export (t-1)		0.003	0.011	0.062**	0.066***	0.056**
		(0.006)	(0.009)	(0.031)	(0.021)	(0.028)
Inflation (t-1)		−0.219***	−0.148**	−0.143**	−0.140***	−0.096*
		(0.079)	(0.064)	(0.071)	(0.049)	(0.057)
Pop. 15-64 (t-1)		−0.290**	−0.171	−0.272	−0.236*	−0.118
		(0.113)	(0.161)	(0.276)	(0.137)	(0.261)
Pop. 65 plus (t-1)		−0.474***	−0.349***	−0.300	−0.300*	−0.222
		(0.073)	(0.124)	(0.332)	(0.170)	(0.293)
Control variables	No	Yes	Yes	Yes	Yes	Yes
Year dummies	No	No	Yes	Yes	Yes	Yes
C. fixed effects	No	No	No	Yes	Yes	Yes
Ar1 component	No	No	No	No	No	Yes
Method	OLS	OLS	OLS	OLS	2SLS	OLS
Standard errors	Robust	Robust	Robust	Robust	Robust	PCSE
Observations	513	406	406	406	401	406
R-squared	0.063	0.265	0.576	0.655	0.564	0.610
Numb. of countries	22	22	22	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.5: Estimation results of different categories of social expenditure on poverty, inequality and GDP growth: One social expenditure category in the model at a time

	(1) Poverty	(2) Gini	(3) GDP growth
Old age and Survivor SE (t-1)	−0.226	−0.0060***	0.280
	(0.151)	(0.0010)	(0.232)
Incapacity SE (t-1)	−0.366	−0.0055***	−0.015
	(0.324)	(0.0018)	(0.308)
Health SE (t-1)	−0.181	−0.0009	0.105
	(0.177)	(0.0016)	(0.221)
Family SE (t-1)	−1.430***	−0.0124***	0.740
	(0.205)	(0.0031)	(0.471)
Unemployment and ALMPS SE (t-1)	−0.808***	−0.0063***	0.014
	(0.133)	(0.0014)	(0.241)
Housing and Others SE (t-1)	−0.992**	0.0032	1.243**
	(0.400)	(0.0036)	(0.576)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	275	275-277	403-409
Number of countries	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.6: Estimation results of total public and mandatory private social expenditure on poverty, inequality and GDP growth

	(1) Poverty	(2) Gini	(3) GDP growth
Total public and mandatory private SE (t-1)	−0.308***	−0.0030***	0.138
	(0.068)	(0.0004)	(0.099)
GDP per capita (t-1)	−0.134**	−0.0015***	
	(0.056)	(0.0004)	
Unemployment rate (t-1)	0.033	0.0007***	
	(0.040)	(0.0002)	
Union density (t-1)	−0.101***	−0.0006***	
	(0.016)	(0.0002)	
Population 15-64 (t-1)	−0.297*	−0.0017	−0.116
	(0.172)	(0.0018)	(0.261)
Population 65 plus (t-1)	−0.282*	−0.0016	−0.202
	(0.147)	(0.0013)	(0.290)
Capital formation growth (t-1)			−0.001
			(0.013)
Education (t-1)			−0.005
			(0.056)
Export (t-1)			0.056**
			(0.028)
Inflation (t-1)			−0.097*
			(0.057)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	275	275	406
R-squared	0.922	0.9643	0.610
Number of countries	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.7: Estimation results of total public and private social expenditure on poverty, inequality and GDP growth

	(1) Poverty	(2) Gini	(3) GDP growth
Total public and private SE (t-1)	−0.320***	−0.0037***	0.163
	(0.074)	(0.0005)	(0.106
GDP per capita (t-1)	−0.132**	−0.0017***	
	(0.057)	(0.0004)	
Unemployment rate (t-1)	0.046	0.0010***	
	(0.040)	(0.0002)	
Union density (t-1)	−0.100***	−0.0006**	
	(0.017)	(0.0003)	
Population 15-64 (t-1)	−0.251	−0.0015	−0.108
	(0.165)	(0.0020)	(0.256)
Population 65 plus (t-1)	−0.236*	−0.0013	−0.201
	(0.135)	(0.0014)	(0.290)
Capital formation growth (t-1)			−0.001
			(0.013)
Education (t-1)			−0.007
			(0.056)
Export (t-1)			0.057**
			(0.028)
Inflation (t-1)			−0.095*
			(0.057)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	275	275	406
R-squared	0.921	0.9643	0.612
Number of countries	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.8: Estimation results of NET public social expenditure on poverty, inequality and GDP growth

	(1) Poverty	(2) Gini	(3) GDP growth
Net public social expenditure (t-1)	−0.298***	−0.0038***	−0.065
	(0.107)	(0.0003)	(0.147)
GDP per capita (t-1)	−0.182***	−0.0039***	
	(0.049)	(0.0007)	
Unemployment rate (t-1)	0.007	0.0009*	
	(0.034)	(0.0004)	
Union density (t-1)	−0.110**	−0.0013***	
	(0.051)	(0.0003)	
Population 15-64 (t-1)	−0.041	0.0001	−1.095*
	(0.211)	(0.0012)	(0.578)
Population 65 plus (t-1)	−0.063	−0.0011	−0.262
	(0.243)	(0.0014)	(0.363)
Capital formation growth (t-1)			−0.045
			(0.030)
Education (t-1)			0.184
			(0.127)
Export (t-1)			0.041
			(0.049)
Inflation (t-1)			0.037
			(0.187)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	105	105	136
R-squared	0.932	0.9728	0.760
Number of countries	17	17	21

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.9: Estimation results of NET total social expenditure on poverty, inequality and GDP growth

	(1) Poverty	(2) Gini	(3) GDP growth
Net total social expenditure	−0.271***	−0.0032***	−0.002
	(0.102)	(0.0003)	(0.154)
GDP per capita (t-1)	−0.191***	−0.0038***	
	(0.050)	(0.0007)	
Unemployment rate (t-1)	−0.005	0.0007	
	(0.037)	(0.0005)	
Union density (t-1)	−0.122**	−0.0015***	
	(0.055)	(0.0004)	
Population 15-64 (t-1)	−0.118	−0.0010	−1.032*
	(0.228)	(0.0017)	(0.594)
Population 65 plus (t-1)	−0.134	−0.0019	−0.226
	(0.270)	(0.0017)	(0.380)
Capital formation growth (t-1)			−0.043
			(0.030)
Education (t-1)			0.174
			(0.126)
Export (t-1)			0.043
			(0.049)
Inflation (t-1)			0.053
			(0.181)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	105	105	136
R-squared	0.929	0.9643	0.759
Number of countries	17	17	21

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.10: Estimation results of total pubic social expenditure on poverty, inequality and GDP growth: OECD countries

	(1) Poverty	(2) Gini	(3) GDP growth
Total pubic SE (t-1)	−0.239***	−0.0017***	0.134**
	(0.043)	(0.0005)	(0.060)
GDP per capita (t-1)	−0.098**	0.0003	
	(0.049)	(0.0007)	
Unemployment rate (t-1)	0.044	0.0009***	
	(0.034)	(0.0002)	
Union density (t-1)	−0.073***	0.0001	
	(0.018)	(0.0002)	
Population 15-64 (t-1)	−0.177	−0.0024**	−0.053
	(0.119)	(0.0011)	(0.152)
Population 65 plus (t-1)	0.001	−0.0004	−0.052
	(0.101)	(0.0009)	(0.185)
Capital formation growth (t-1)			0.000
			(0.007)
Education (t-1)			0.013
			(0.034)
Export (t-1)			0.054***
			(0.013)
Inflation (t-1)			−0.072**
			(0.032)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	382	382	595
R-squared	0.953	0.9794	0.589
Number of countries	32	32	33

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.11: Estimation results of different social expenditure categories on poverty, inequality and GDP growth: OECD countries

	(1)	(2)	(3)
	Poverty	Gini	GDP growth
Old age and Survivor SE (t-1)	−0.165	−0.0023***	0.238
	(0.103)	(0.0008)	(0.214)
Incapacity SE (t-1)	−0.004	−0.0008	0.118
	(0.213)	(0.0022)	(0.174)
Health SE (t-1)	−0.031	0.0015	0.025
	(0.066)	(0.0010)	(0.120)
Family SE (t-1)	−1.044***	−0.0115***	0.235
	(0.156)	(0.0032)	(0.285)
Unemployment and ALMPS SE (t-1)	−0.428***	−0.0023*	−0.239
	(0.142)	(0.0013)	(0.212)
Housing and Others SE (t-1)	−0.528***	−0.0021	0.486
	(0.181)	(0.0034)	(0.466)
GDP per capita (t-1)	−0.152***	−0.0003	
	(0.046)	(0.0006)	
Unemployment rate (t-1)	0.039	0.0010***	
	(0.029)	(0.0002)	
Union density (t-1)	−0.078***	0.0000	
	(0.017)	(0.0002)	
Population 15-64 (t-1)	−0.125	−0.0013	−0.150
	(0.099)	(0.0014)	(0.171)
Population 65 plus (t-1)	0.005	0.0007	−0.152
	(0.093)	(0.0011)	(0.236)
Capital formation growth (t-1)			0.004
			(0.008)
Education (t-1)			0.027
			(0.032)
Export (t-1)			0.051***
			(0.014)
Inflation (t-1)			−0.102***
			(0.036)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	372	372	552
R-squared	0.956	0.9793	0.601
Number of countries	31	31	31

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.12: Estimation results of total pubic social expenditure on poverty, inequality and GDP growth: OECD countries : One social expenditure category in the model at a time

	(1) Poverty	(2) Gini	(3) GDP growth
Old age and Survivor SE (t-1)	−0.147	−0.0022***	0.320
	(0.112)	(0.0008)	(0.196)
Incapacity SE (t-1)	−0.421**	−0.0043**	0.175
	(0.201)	(0.0018)	(0.146)
Health SE (t-1)	−0.143*	0.0004	0.108
	(0.080)	(0.0011)	(0.113)
Family SE (t-1)	−1.222***	−0.0119***	0.300
	(0.147)	(0.0030)	(0.264)
Unemployment and ALMPS SE (t-1)	−0.729***	−0.0053***	0.043
	(0.117)	(0.0011)	(0.170)
Housing SE	−1.690***	−0.0074	0.205
	(0.263)	(0.0079)	(0.922)
Others SE	−0.485**	−0.0003	0.824**
	(0.246)	(0.0032)	(0.407)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	372-384	372-382	573-598
Number of countries	31-32	31-32	32-33

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.13: Estimation results of total pubic social expenditure on poverty, inequality and GDP growth: Years 2008-2015

	(1)	(2)	(3)
	Poverty	Gini	GDP growth
Total pubic SE (t-1)	−0.210***	−0.0011**	0.235
	(0.073)	(0.0005)	(0.194)
GDP per capita (t-1)	−0.176***	−0.0018***	
	(0.044)	(0.0006)	
Unemployment rate (t-1)	−0.014	−0.0000	
	(0.030)	(0.0001)	
Union density (t-1)	0.035	0.0008	
	(0.034)	(0.0006)	
Population 15-64 (t-1)	−0.467***	0.0000	−1.602***
	(0.062)	(0.0015)	(0.433)
Population 65 plus (t-1)	−0.566***	0.0008	−0.988
	(0.143)	(0.0033)	(0.610)
Capital formation growth (t-1)			0.016
			(0.016)
Education (t-1)			0.182*
			(0.095)
Export (t-1)			0.033
			(0.034)
Inflation (t-1)			−0.211**
			(0.105)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE
Observations	133	133	154
R-squared	0.967	0.9800	0.697
Number of countries	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.14: Estimation results of total pubic social expenditure on poverty, inequality and GDP growth: Years 1990-2007

	(1)	(2)	(3)
	Poverty	Gini	GDP growth
Total pubic SE (t-1)	−0.187	−0.0046***	0.090
	(0.130)	(0.0012)	(0.147)
GDP per capita (t-1)	0.229	0.0010	
	(0.353)	(0.0030)	
Unemployment rate (t-1)	0.131	0.0020**	
	(0.139)	(0.0010)	
Union density (t-1)	−0.114**	−0.0001	
	(0.054)	(0.0007)	
Population 15-64 (t-1)	−0.602	−0.0057	0.003
	(0.506)	(0.0051)	(0.284)
Population 65 plus (t-1)	−0.019	0.0020	0.147
	(0.304)	(0.0029)	(0.394)
Capital formation growth (t-1)			−0.012
			(0.023)
Education (t-1)			0.023
			(0.070)
Export (t-1)			0.085*
			(0.049)
Inflation (t-1)			−0.097*
			(0.054)
Year dummies	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes
Method	OLS	OLS	OLS
Standard errors	Robust	Robust	PCSE
Observations	124	124	230
R-squared			0.709
Number of countries	21	21	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.15: Different categories of social expenditure on poverty among working population and poverty and inequality among working age population

	(1) Poverty Working	(2) Poverty Working	(3) Poverty 18-65	(4) Poverty 18-65	(5) Gini 18-65	(6) Gini 18-65
Total pubic SE (t-1)	−0.068		−0.237***		−0.0032***	
	(0.055)		(0.035)		(0.0008)	
Old & Surv. SE (t-1)		−0.231		−0.110*		−0.0054***
		(0.196)		(0.058)		(0.0009)
Incapacity SE (t-1)		1.056***		0.276**		−0.0001
		(0.188)		(0.113)		(0.0023)
Health SE (t-1)		0.078		0.067		−0.0005
		(0.186)		(0.046)		(0.0009)
Family SE (t-1)		−0.439		−0.681***		−0.0092***
		(0.372)		(0.147)		(0.0036)
Un. & almps SE (t-1)		−0.456**		−0.685***		−0.0022
		(0.182)		(0.133)		(0.0022)
Hous. & oth. SE (t-1)		−1.657***		−0.752***		0.0016
		(0.632)		(0.205)		(0.0026)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE	PCSE	PCSE	PCSE
Observations	213	213	273	273	275	275
R-squared	0.803	0.817	0.930	0.939	0.965	0.969
Number of countries	21	21	22	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.16: Different categories of social expenditure on the poverty rate 60%

	(1) Poverty 60% line	(2) Poverty 60% line
Total pubic SE (t-1)	−0.570***	
	(0.080)	
Old age and Survivor SE (t-1)		−0.504***
		(0.168)
Incapacity SE (t-1)		0.180
		(0.336)
Health SE (t-1)		−0.208
		(0.150)
Family SE (t-1)		−1.851***
		(0.213)
Unemployment and ALMPS SE (t-1)		−0.676***
		(0.146)
Housing and Others SE (t-1)		−0.670
		(0.482)
Year dummies	Yes	Yes
Controls	Yes	Yes
Fixed effects	Yes	Yes
Ar1 component	Yes	Yes
Method	OLS	OLS
Standard errors	PCSE	PCSE
Observations	258	258
R-squared	0.938	0.943
Number of countries	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.17: Total public social expenditure on poverty: different lags

	(1) Poverty	(2) Poverty	(3) Poverty	(4) Poverty	(5) Poverty	(6) Poverty
Total public SE	−0.370***	−0.372***	−0.369***	−0.311***	−0.327***	−0.339***
	(0.118)	(0.107)	(0.105)	(0.102)	(0.099)	(0.101)
GDP per capita	−0.178**	−0.162	−0.197	0.031	−0.017	−0.146
	(0.087)	(0.117)	(0.124)	(0.071)	(0.101)	(0.120)
Unemployment rate	0.048	0.032	0.030	0.108***	0.130**	0.078
	(0.073)	(0.072)	(0.053)	(0.042)	(0.052)	(0.066)
Population 15-64	−0.070	−0.275	−0.476	−0.608*	−0.591*	−0.611*
	(0.250)	(0.272)	(0.301)	(0.337)	(0.343)	(0.325)
Population 65 plus	−0.018	−0.209	−0.393	−0.424	−0.538*	−0.618*
	(0.181)	(0.160)	(0.247)	(0.270)	(0.289)	(0.320)
Union density	−0.090*	−0.102***	−0.116**	−0.081	−0.078*	−0.072
	(0.047)	(0.038)	(0.053)	(0.049)	(0.042)	(0.048)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	OLS	OLS	OLS
Standard errors	Robust	Robust	Robust	Robust	Robust	Robust
Lag	0	1	2	3	4	5
Observations	282	275	273	265	264	250
R-squared	0.916	0.926	0.926	0.930	0.922	0.922
Number of countries	22	22	22	21	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.18: Total public social expenditure on Gini: different lags

	(1) Gini	(2) Gini	(3) Gini	(4) Gini	(5) Gini	(6) Gini
Total public SE	−0.0034***	−0.0041***	−0.0034***	−0.0028**	−0.0027***	−0.0019*
	−0,001)	(0.0008)	(0.0010)	(0.0011)	(0.0010)	(0.0010)
GDP per capita	−0.0019***	−0.0021**	−0.0022**	−0.0007	−0.0005	−0.0017
	(0.0006)	(0.0009)	(0.0010)	(0.0005)	(0.0010)	(0.0011)
Unemployment rate	0.0006	0.0008	0.0007	0.0013**	0.0012**	0.0005
	(0.0006)	(0.0007)	(0.0006)	(0.0005)	(0.0006)	(0.0006)
Population 15-64	−0.0013	−0.0024	−0.0047*	−0.0051*	−0.0070**	−0.0075**
	(0.0026)	(0.0024)	(0.0028)	(0.0030)	(0.0032)	(0.0032)
Population 65 plus	0.0006	−0.0015	−0.0033	−0.0037	−0.0064**	−0.0067**
	(0.0023)	(0.0015)	(0.0024)	(0.0026)	(0.0030)	(0.0032)
Union density	−0.0004	−0.0005	−0.0008	−0.0006	−0.0006	−0.0011**
	(0.0006)	(0.0004)	(0.0006)	(0.0005)	(0.0005)	(0.0005)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	OLS	OLS	OLS
Standard errors	Robust	Robust	Robust	Robust	Robust	Robust
Lag	0	1	2	3	4	5
Observations	282	275	273	265	264	250
R-squared	0.950	0.952	0.951	0.953	0.95	0.952
Number of countries	22	22	22	21	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

Table A.5.19: Total public social expenditure on GDP growth: different lags

	(1)	(2)	(3)	(4)	(5)	(6)
	GDP growth	GDP growth	GDP growth	GDP growth	GDP growth	GDP growth
Total public SE	−0.123	0.142	0.104	0.060	0.133	0.277***
	(0.086)	(0.102)	(0.116)	(0.104)	(0.089)	(0.089)
Cap. for. gr.	0.047***	−0.001	−0.010	−0.011	0.009	−0.004
	(0.014)	(0.013)	(0.014)	(0.013)	(0.012)	(0.011)
Education	0.007	−0.006	0.020	0.013	0.011	−0.011
	(0.050)	(0.056)	(0.070)	(0.063)	(0.054)	(0.051)
Export	0.048**	0.056**	0.026	0.014	0.039	0.030
	(0.023)	(0.028)	(0.033)	(0.029)	(0.025)	(0.030)
Inflation	−0.129**	−0.096*	0.028	0.013	−0.055	−0.043
	(0.053)	(0.057)	(0.064)	(0.056)	(0.052)	(0.053)
Pop. 15-64	−0.231	−0.118	0.016	−0.134	−0.360	−0.297
	(0.211)	(0.261)	(0.303)	(0.274)	(0.229)	(0.249)
Pop. 65 plus	−0.172	−0.222	−0.035	−0.247	−0.481	−0.622**
	(0.236)	(0.293)	(0.358)	(0.329)	(0.316)	(0.303)
1-13 Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Ar1 component	Yes	Yes	Yes	Yes	Yes	Yes
Method	OLS	OLS	OLS	OLS	OLS	OLS
Standard errors	PCSE	PCSE	PCSE	PCSE	PCSE	PCSE
Lags	0	1	2	3	4	5
Observations	427	406	384	362	340	318
R-squared	0.681	0.610	0.585	0.597	0.618	0.633
Num. of countries	22	22	22	22	22	22

* denotes significant at the 10% level, ** at the 5% level and *** at the 1% level.

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Nederlandse samenvatting (Dutch summary)

Onderzoek naar economische effecten van sociale zekerheid

In ontwikkelde economieën wordt tussen de 20% en 30% van het bruto binnenlands product (bbp) uitgegeven aan pensioenen, gezondheidszorg en uitkeringen gericht op ouderen, werklozen, gehandicapten, zieken, gezinnen en armen. Om deze uitgaven op een doelgerichte en kostenefficiënte manier te doen is het nodig om de economische effecten van sociale zekerheid in kaart te brengen. Aan de ene kant is het relevant of de beoogde doelen van sociale zekerheid worden bereikt, zoals het beschermen van mensen tegen inkomensschokken. Aan de andere kant kan sociale zekerheid gepaard gaan met onwenselijke gedragseffecten, bijvoorbeeld dat werklozen door een uitkering mogelijk minder hard zoeken naar een nieuwe baan.

Dit proefschrift draagt bij aan de literatuur over sociale zekerheid door bestudering van enkele beoogde effecten en enkele mogelijke onwenselijke effecten. We beginnen in hoofdstuk 1 met een kort overzicht van de doelstellingen van sociale zekerheid, waarna we bespreken welke negatieve gedragseffecten sociale zekerheid kan hebben. We bestuderen vervolgens een aantal van deze positieve en negatieve effecten van sociale zekerheid in de hoofdstukken 2 t/m 5.

Sociale zekerheid is bedoeld om mensen te beschermen tegen negatieve inkomensschokken veroorzaakt door werkloosheid, arbeidsongeschiktheid, ziekte en ouderdom (Barr 2012). In principe is dit welzijn verhogend omdat mensen risico avers zijn en er onzekerheid bestaat over het toekomstige inkomen. Risico-aversie houdt in dat een verhoging van

het inkomensrisico het verwachte nut verlaagt vanwege afnemend marginaal nut van consumptie.[6] Sociale verzekeringen helpen om het inkomen over de levenscyclus en over 'goede' en 'slechte' periodes te spreiden. Daarmee vermindert het risico's en verhoogt het totale nut dat over het hele leven wordt afgeleid. Dit geldt in het bijzonder voor mensen met liquiditeitsbeperkingen, die minder mogelijkheden hebben om hun consumptiepatroon te laten afwijken van hun inkomstenpatroon (Chetty 2008) en voor mensen met inconsistente voorkeuren die het heden bevoordelen ten opzichte van de toekomst en daardoor te weinig sparen (Thaler and Shefrin 1981). Bovendien kunnen sociale verzekeringen de welvaart verhogen omdat mensen verlies-avers zijn (bv. Kahneman et al. (1991)) en inkomensverliezen worden verminderd door sociale verzekeringen.[7]

In een breder perspectief is sociale zekerheid gericht op het verminderen van armoede en ongelijkheid en in sommige gevallen op het laten stijgen van de werkgelegenheid (Barr 2012). Het terugdringen van armoede en ongelijkheid kan de welvaart en productiviteit onder arme mensen vergroten (Baldacci et al. 2008; Cingano 2014; OECD 2014). Bovendien zal naar verwachting het verkleinen van de ongelijkheid het aantal *rent-seeking* activiteiten doen afnemen (Stiglitz 2012).[8] Verder kunnen uitgaven aan sociale zekerheid de ongelijkheid verkleinen en zorgen voor meer macro-economische stabiliteit. Dit kan ook leiden tot een toename van de politieke en sociale stabiliteit (Rodrik 1999; Kumhof et al. 2015). Tenslotte kan sociale zekerheid positieve gedragseffecten veroorzaken. Inkomenszekerheid kan het nemen van risico's, investeringen en daarmee de productiviteit verhogen (Acemoglu and Shimer 2000; Estevez-Abe et al. 2001).

Sociale zekerheid kan echter ook ongewenste gedragseffecten veroorzaken. Sociale zekerheid verschuift een deel van de kosten die gepaard gaan

[6] Afnemend marginaal nut van consumptie betekent dat mensen een hoger marginaal nut halen uit een extra eenheid consumptie bij een lager niveau van consumptie.

[7] Verlies-avers verwijst naar de neiging van mensen om het voorkomen van een verlies te verkiezen boven het verwerven van een gelijkwaardig voordeel.

[8] Rent-seeking verwijst naar middelen die (door de rijken) worden besteed aan het vergroten van hun aandeel in de welvaart zonder welvaart te creëren. *Rent-seeking* resulteert in minder economische efficiëntie door een slechte toewijzing van middelen, verminderde feitelijke welvaartscreatie, verloren overheidsinkomsten en verhoogde inkomensongelijkheid (Stiglitz 2012).

met bepaald gedrag (bijvoorbeeld het nemen van risico's) naar anderen. Dit genereert een discrepantie tussen de individuele kosten en baten en de maatschappelijke kosten en baten. Mensen hebben de neiging om vanuit maarschappelijk perspectief te veel vrije tijd (of consumptie) te kiezen wanneer een deel van de kosten van vrije tijd (of consumptie) door anderen worden gedragen (Chetty 2008; Chetty and Finkelstein 2013). Dit inefficiënte gedrag wordt ook wel moreel risico genoemd. Voorbeelden van moreel risico veroorzaakt door sociale zekerheid zijn: verminderingen in het zoeken naar werk vanwege werkloosheidsuitkeringen (bijv. Krueger and Meyer (2002)) en vroegpensioen vanwege voordelen bij vervroegde uittreding (bv. Staubli and Zweimüller (2013)). Een ander verstorend effect ontstaat uit belastingen die nodig zijn om sociale zekerheid te financieren. Deze belastingen kunnen het arbeidsaanbod en particuliere investeringen verminderen, wat kan leiden tot een lagere welvaart.

In de hoofdstukken 2 t/m 5 bestuderen we zowel het behalen van een aantal van de beoogde doelstellingen van sociale zekerheid, evenals mogelijke negatieve gedragseffecten die door sociale zekerheid kunnen worden veroorzaakt. Kortom, we beschouwen de verzorgingsstaat vanuit verschillende perspectieven, met een focus op zowel het herverdelende effect van sociale zekerheid als de werkgelegenheidseffecten van sociale zekerheid.

Het tweede hoofdstuk gaat over het voorkomen van NEET's, jongeren die niet werken en geen onderwijs of training volgen (NEET= Not in Employment, Education or Training). Het voorkomen van NEET's heeft prioriteit bij beleidsmakers, met name tijdens recessies. NEET's zijn bijvoorbeeld een belangrijk punt van zorg voor de Europese Commissie (Carcillo et al. 2015). Deze aandacht voor het voorkomen en terugdringen van het aantal NEET's gaat samen met een andere beleidstrend, namelijk die van strengere voorwaarden voor het recht op bijstand, bijvoorbeeld door een strengere baanzoekplicht of het verplicht verrichten van tegenprestaties.

We bestuderen de effecten van verplichte activeringsprogramma's voor jongeren in de bijstand op het percentage NEET's tijdens een diepe economische recessie. Om precies te zijn bestuderen we de Wet Investeren in Jongeren (WIJ) hervorming. Deze hervorming is in 2009 ingevoerd

in Nederland, net na het begin van de Grote Recessie. Het doel van de WIJ hervorming was om het aantal jonge NEET's te verminderen. Voor dit doel werd de ontvangst van de bijstand voor jongeren tot 27 jaar oud afhankelijk gemaakt van deelname in zogenaamde 'werk-leertrajecten'.

Dit onderzoek beantwoordt de volgende vraag: "*Wat is het effect van verplichte activeringsprogramma's voor jongeren in de bijstand op de NEET's tijdens de grote economische recessie?*"We beschouwen de effecten van de WIJ-hervorming op de belangrijkste uitkomst variabelen: NEET's met bijstand, NEET's zonder bijstand, totale aandeel NEET's, de werkgelegenheid en scholing.

We gebruiken de methodes *differences-in-differences* en *regression discontinuity* om de causale effecten van de WIJ-hervorming te schatten. We maken hiervoor gebruik van het 'Arbeidsmarktpanel' van het CBS, dat een steekproef van 1,2 miljoen personen volgt over de periode 1999-2012. Voor deze personen beschikken we over arbeidsmarktuitkomsten en over een groot aantal individuele en huishoudens kenmerken. We bestuderen de effecten voor drie verschillende leeftijdsgroepen, 20-22, 23-24 en 25-26 jaar, terwijl onze basis controlegroep bestaat uit personen van 27-28 jaar. Een belangrijke uitdaging in de empirische analyse is om te controleren voor mogelijke verschillende tijdseffecten tussen de behandel- en controlegroep, als gevolg van bijvoorbeeld verschillende trends of verschillende conjunctuurreacties (Bell en Blanchflower 2011). In ons voorkeursmodel controleren we voor verschillen in conjunctuurpatronen door interactie tussen leeftijd en het werkloosheidspercentage en daarnaast voor jaareffecten, leeftijdseffecten, specifieke trends voor leeftijd en voor de demografische kenmerken. We presenteren ook een uitgebreide placebo-analyse, inclusief placebo effecten voor de jaren voorafgaand aan de hervorming en placebo effecten voor de eerdere economische crisis in 2002-2004.

Onze belangrijkste bevindingen zijn als volgt. De WIJ-hervorming heeft niet het beoogde effect gehad om het totale aantal NEET's te verminderen. Wel nam het aantal NEET's met een bijstandsuitkering door de WIJ af met maar liefst 24% in de leeftijdscategorie 25-26 jaar, de enige behandelingsgroep die alle placebotesten doorstaat. Daar stond echter een in omvang vergelijkbare toename tegenover van NEET's zonder bijstandsuitkering. De WIJ had geen effect op de werkgelegenheid of het

volgen van onderwijs. Een plausibele verklaring hiervoor is dat de WIJ-hervorming werd ingevoerd toen de Grote Recessie net was begonnen, een periode waarin het vooral voor jongeren moeilijk was om werk te vinden. Verder laat onze analyse zien dat het belangrijk kan zijn in een *differences-in-differences* analyse om te controleren voor verschillen in trends bij het bestuderen van een hervorming die zich richt op jonge mensen en die wat oudere personen gebruikt als controlegroep. Dit is belangrijk om het effect te vinden op bepaalde uitkomstvariabelen zoals deelname aan onderwijs. Tot slot laten we zien dat standaard placebo dummies voor de jaren vóór de hervorming niet altijd in staat zijn om de aanname van de gelijke tijdseffecten te verwerpen.

In hoofdstuk 3 hebben we onderzoek gedaan naar het *Added Worker Effect*. Sinds het begin van de Grote Recessie is de belangstelling van beleidsmakers en academici toegenomen voor het effect van werkloosheidsschokken op het arbeidsaanbod van de partners van werklozen, ook wel bekend als het *Added Worker Effect* (hierna: AWE). De empirische literatuur vindt in het algemeen een klein AWE, zie bijvoorbeeld Hardoy and Schøne (2014), Halla et al. (2018) and Bredtmann et al. (2018) voor recente bijdragen. Twee openstaande vragen zijn of het AWE belangrijker is geworden sinds het begin van de Grote Recessie en of het AWE is afgenomen over de tijd doordat de participatiegraad van vrouwen is toegenomen. Door een hogere participatiegraad blijft er immers minder ruimte over voor de toename van het arbeidsaanbod na het werkloos raken van de partner.

We focussen in dit hoofdstuk op de volgende vraag: "*Hoe veranderde het Added Worker Effect over de jaren en over de conjunctuurcyclus in Nederland tijdens de periode 2003-2015?*"Om meer inzicht te krijgen in de relatie tussen het AWE en de conjunctuurcyclus bestuderen we het AWE voor vrouwen van wie de mannelijke partner werkloos raakte in de jaren voor en tijdens de Grote Recessie.

We bestuderen het AWE met de nieuwe versie van het 'Arbeidsmarktpanel' van het Centraal Bureau voor de Statistiek (2017). Deze nieuwe versie van het Arbeidsmarktpanel volgt de arbeidsmarktuitkomsten van 1,8 miljoen Nederlanders voor de periode 1999-2015, evenals het gebruik van sociale zekerheid. Onze onderzoeksstrategie vergelijkt huishoudens

met mannelijke partners die werkloos raakten met huishoudens met mannelijke partners die niet werkloos raakten in een gegeven jaar. Met een *differences-indifferences* analyse met individuele *fixed effects*, schatten we de impact van een werkloosheidsschok van mannelijke partners in een bepaald jaar op het inkomen uit werk en de verkregen winst uit zelfstandigheid van beide partners, als ook de inkomsten uit de werkloosheidsverzekering (WW) en andere sociale uitkeringen. Dit wordt allemaal gemeten over een tijdsperiode van 2 jaar vóór het jaar dat de behandelgroep in de WW terecht komt tot 3 jaar na het jaar dat de behandelgroep in de WW terecht kwam. Door verschillende referentiejaren te nemen voor de werkloosheidsschokken van cohorten in onze steekproef kunnen we bestuderen hoe de effecten variëren over de conjunctuur en hoe deze veranderen over de tijd.

Onze belangrijkste bevindingen zijn als volgt. Ten eerste vinden we dat de werkloosheidsschok van een mannelijke partner, waardoor het bruto-inkomen 20 tot 30 duizend euro daalt, een klein positief statistisch significant AWE teweegbrengt van 2-5% (500-1.000 euro). Dit AWE is klein en niet statistisch significant tijdens de eerste jaren van de Grote Recessie (2008-2009). We vinden zowel intensieve (uitbreiding uren) als extensieve (toename van aantal werkenden) marge effecten voor het AWE. De afname van het AWE wordt in het begin van de Grote Recessie vooral gedreven door een afname van het intensieve marge effect, terwijl het extensieve marge effect is afgenomen over de tijd. Verder is er een AWE van ongeveer 2% (500 euro) uit winst uit zelfstandigheid. Tot slot compenseren werkloos geraakte mannen een steeds groter deel van het inkomensverlies met winst uit zelfstandigheid. Dit neemt toe van ongeveer 2000 euro 3 jaar na werkloos raken in 2004 tot ongeveer 4500 euro 3 jaar na het werkloos raken in 2012.

Hoofdstuk 4 gaat over de effecten van een grondwettelijk recht op sociale zekerheid (Constitutional Commitment to Social Security, hierna: CCSS). In de afgelopen decennia hebben politici en academici het belang van sociale grondrechten benadrukt voor sociale en economische ontwikkeling (Townsend 2007; ILO 2014). Het belangrijkste argument voor een op rechten gebaseerde benadering van sociale zekerheid is dat het een recht geeft dat kan worden afgedwongen bij de rechter. Zonder een der-

gelijk recht zijn mensen afhankelijk van de 'goede wil' van de huidige regering voor goed onderwijs, gezondheidszorg en sociale zekerheid. In theorie kan de grondwet een belangrijke rol spelen voor sociale rechten, aangezien de grondwet voor iedereen geldt en bescherming biedt aan minderheden. Echter, het aantal empirische studies over het effect van sociale grondrechten zijn beperkt.

Wij dragen bij aan deze literatuur door het beantwoorden van volgende vraag: "*Wat is het effect van een grondwettelijk recht op sociale zekerheid op verschillende soorten uitgaven aan sociale zekerheid?*". Allereerst zijn we geïnteresseerd in het effect van een grondwettelijk recht op sociale zekerheid op de totale sociale uitgaven, waaruit blijkt dat CCSS een positief effect heeft op uitgaven aan sociale zekerheid. Ten tweede onderzoeken we of het effect van CCSS het grootst is voor sociale uitgavenregelingen gericht op groepen die door de publieke opinie gezien worden als groepen die minder steun verdienen. We verwachten een groter effect van CCSS op deze sociale-uitgavenregelingen als de mediane stemmer minder belang hecht aan deze sociale-uitgavenregelingen, waardoor een grotere rol voor de grondwet is weggelegd.

We gebruiken een paneldataset voor 17 EU-landen van 1990 tot 2012. De gegevens over uitgaven aan sociale zekerheid als percentage van het bbp zijn afkomstig van de Social Expenditure Database (SOCX) van de OESO. Voor CCSS gebruiken we de indicator ontwikkeld door Ben Bassat en Dahan (2018), die we als 0 of 1 definiëren, afhankelijk van de aanwezigheid van een grondwettelijke bepaling over de steun aan ouderen, nabestaanden, gehandicapten, werklozen, zieken of armen. We gebruiken OLS-modellen, 2SLS-regressiemodellen en het Heckman tweestappenmodel met de rigiditeit van de grondwet als een instrumentele variabele, om te corrigeren voor mogelijke endogeniteit en om het effect te schatten van CCSS op de verschillende uitgaven aan sociale zekerheid. In lijn met onze verwachtingen, heeft de rigiditeit van de grondwet een positief effect op CCSS, wat aangeeft dat ons instrument relevant is.

Onze belangrijkste bevindingen zijn als volgt. Ten eerste vinden we een positief significant effect van CCSS op de totale uitgaven aan sociale zekerheid van 3,8 procentpunten. CCSS heeft een positief effect op de uitgaven aan ouderen, nabestaanden, arbeidsongeschikten, werklozen en

op actief arbeidsmarktbeleid. Ten tweede vinden we de meest omvangrijke effecten, uitgedrukt als een percentage van de gemiddelde bestedingen, voor uitgaven voor aan werklozen en actief arbeidsmarktbeleid. Dit zijn de uitgavenregelingen gericht op mensen die door de publieke opinie gezien worden als een groep die minder steun verdient dan bijvoorbeeld ouderen en gehandicapten (Blekesaune en Quadagno 2003; Van Oorschot 2006). Ten derde is er geen positief effect op sociale uitgaven aan gezondheidszorg en gezinnen. Deze uitgaven worden niet gedekt door de definitie van CCSS. Dit suggereert dat het positieve effect op uitgaven aan sociale zekerheid daadwerkelijk wordt verklaard door CCSS en niet wordt veroorzaakt door een positieve houding tegenover herverdeling.

Hoofdstuk 5 gaat over de relatie tussen verschillende sociale uitgavenregelingen en armoede, ongelijkheid en bbp-groei. Sinds Piketty (2014) zijn werk over inkomensongelijkheid heeft gepubliceerd is er een heropleving van het publieke en academische debat over inkomens- en vermogensongelijkheid. Dit debat wordt versterkt door de opkomst van populistische bewegingen. Beleidsmakers en academici hebben lang gedacht dat er een afruil bestaat tussen het verkleinen van inkomensongelijkheid en het vergroten van economische groei (Kaldor 1957; Okun 1975; Lazear and Rosen 1981; Benabou 2000; Arjona et al. 2003). Meer recente studies betwisten dit en vinden een negatieve associatie tussen inkomensongelijkheid en economische groei (Persson and Tabellini 1994; Alesina and Rodrik 1994; Perotti 1996; Easterly 2007; OECD 2014; Dabla-Norris et al. 2015). Bovendien laat recent empirische onderzoek zien dat herverdeling niet negatief samenhangt met economische groei (Thewissen 2013; Ostry et al. 2014). Niettemin is herverdeling een breed concept en verschillende soorten herverdeling, vertaald in verschillende soorten sociale uitgaven, hebben verschillende gevolgen voor armoede, ongelijkheid en economische groei.

Daarom concentreren we ons in dit hoofdstuk op de vraag: "*Hoe hangen verschillende soorten uitgaven aan sociale zekerheid samen met armoede, ongelijkheid en economische groei?*". Allereerst onderzoeken we hoe sociale uitgaven op geaggregeerd niveau samenhangen met armoede, ongelijkheid en de groei van het bbp. Deze analyse biedt inzichten in de potentiële afruil tussen het verkleinen van de armoede en ongelijkheid aan de ene kant en BBP-groei aan de andere kant. Ten tweede bestuderen we hoe deze

relaties verschillen voor sociale uitgaven aan 1) ouderen en nabestaandenpensioenen, 2) arbeidsongeschiktheid, 3) gezondheidszorg, 4) gezinnen, 5) werklozen en actief arbeidsmarktbeleid en 6) huisvesting en overige. Deze analyse toont het belang van de verschillende soorten uitgaven voor het verkleinen van armoede en ongelijkheid en hoe de verschillende soorten uitgaven samenhangen met de economische groei.

We gebruiken een paneldataset van 22 EU-landen voor de jaren 1990-2015 voor onze basisresultaten en een paneldataset van 32 OESO-landen in onze robuustheidsanalyse. De gegevens zijn afkomstig uit verschillende OESO-databases. We maken gebruik van OLS en 2SLS regressiemodellen waarin we de vertraagde waarden van de verschillende variabelen voor sociale uitgaven gebruiken als verklarende variabelen. We nemen sociale uitgaven in periode (t-1) omdat de sociale uitgaven zelf ook kunnen worden beïnvloed door economische groei en mogelijk ook door armoede en ongelijkheid.

In ons 2SLS-model gebruiken we de variabelen voor sociale uitgaven in periode (t-2) als instrumentele variabele. Ons voorkeursmodel is een OLS-model waarin we corrigeren voor seriële correlatie door het toevoegen van een autoregressieve error component en controleren voor demografische, economische en institutionele verschillen, als ook voor *fixed effects* voor landen en jaren. Daarnaast hanteren we panel gecorrigeerde standaardfouten.

Onze belangrijkste bevindingen zijn als volgt. Ten eerste vinden we dat hogere uitgaven aan sociale zekerheid samenhangen met minder armoede en een kleinere ongelijkheid en dat uitgaven aan sociale zekerheid niet gerelateerd zijn aan bbp-groei. Daarom lijkt er geen afruil te bestaan tussen het verminderen van armoede en ongelijkheid enerzijds en hogere economische groei aan de andere kant. Ten tweede, de relaties met armoede, ongelijkheid en economische groei verschillen aanzienlijk tussen de verschillende sociale uitgavenregelingen, waardoor beleidsmakers verschillende sociale uitgavenregelingen kunnen gebruiken voor verschillende doelen. Voor armoede vinden we de sterkste negatieve relaties met gezinsuitgaven, werkloosheidsuitkeringen, actief arbeidsmarktbeleid, huisvesting en overige (vooral sociale bijstand). Voor ongelijkheid vinden we een sterk negatief verband met sociale uitgaven aan ouderen, nabestaan-

denpensioenen en gezinnen. Tenslotte is er een sterke positieve relatie tussen sociale uitgaven aan 'huisvesting en overige' en economische groei.

Curriculum Vitae

Emile Cammeraat (born in Ede, the Netherlands on August 2, 1990) starts in October as a Junior Policy Analyst at the OECD Directorate for Science, Technology and Innovation. For his work at the OECD he will contribute to both measurement and analytical work to shed light on human capital and organizations in the era of Artificial Intelligence (AI). Prior to starting at the OECD, Emile participated in the Fund Internship Program (FIP) 2019 at the IMF.

Emile wrote his PhD thesis in Economics at the Department of Economics at the Leiden Law School, Leiden University. His PhD thesis contains four papers on economic effects of social protection. He received the Meijers Prize for his paper on the effect of a constitutional right to social security. He presented his research at many international conferences, such as FISS, EALE and the IIPF. Furthermore, he presented policy relevant work at the Dutch Economist Day, the CPB Netherlands Bureau for Economic Policy Analysis and the Ministry of Social Affairs. Emile was also selected to participate in the IIPF Doctoral School 2016 and he took courses at the Tinbergen Institute. Besides doing research, Emile taught Principles of Economics, Law and Economics and supervised bachelor theses.

During his PhD, Emile performed research projects at the OECD and the CPB. He studied the relationship between different social expenditure schemes and poverty, inequality and economic growth at the Social Policy Division within the OECD Directorate for Employment, Labour and Social Affairs. At the CPB he studied the added worker effect in the Netherlands during the Great Recession.

Prior to his PhD, Emile pursued two bachelors and two masters, in both Law and Economics. He did a master in International Economics at the Erasmus University Rotterdam. For The European Master in Law and Economics (EMLE) he studied in Bologna, Hamburg and Mumbai and received the Best Thesis Award as well as the Award for the highest average grade EMLE 2014/2015. During his studies, Emile was the chairman of a political party in the district city council of Rotterdam Centrum.

In the range of books published by the Meijers Research Institute and Graduate School of Leiden Law School, Leiden University, the following titles were published in 2018 and 2019:

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Social protection aims to protect individuals against negative income shocks and to reduce poverty and inequality. In developed economies, no less than 20-30% of GDP is spent on social protection, such as pensions, public expenditure on health and benefits targeted at the elderly, unemployed, disabled, sick, families and the poor. In order to ensure that these expenditures are well-targeted and cost-efficient, we need to know the economic effects of social protection.

This thesis studies a number of the intended effects and potential adverse effects that social protection may have. It contains four empirical studies that answer the following questions. Are mandatory activation programs for young welfare recipients reducing the number of individuals Not in Employment, Education or Training (NEETs) during a severe economic recession? To what extent are income losses caused by unemployment shocks compensated by increases in earnings from the spouse? Does a right to social security in the constitution have an impact on social expenditure? How are different social expenditure types related to poverty, inequality and GDP growth? Altogether, this book considers the welfare state from different perspectives, with a focus on both the redistributive effects and the employment effects of social protection.

This is a volume in the series of the Meijers Research Institute and Graduate School of the Leiden Law School of Leiden University. This study is part of the Law School's research program on 'reform of Social Legislation'.