Household Finances and Labor Supply: The Role of Public Policies

Inaugural-Dissertation zur Erlangung des akademischen Grades
eines Doktors der Wirtschaftswissenschaft

Fachbereich Wirtschaftswissenschaft
Freie Universität Berlin

vorgelegt von

Cortnie Shupe, M.Sc., M.A.
geboren in Oregon, USA

Berlin, 2019

Erstgutachter

Prof. Dr. Carsten Schröder, Freie Universität Berlin

Zweitgutachter

Prof. David Neumark, Ph.D., University of California, Irvine

Tag der Disputation:

11. Juni 2019

Zusammenarbeit mit Koautoren und Vorveröffentlichungen

Kapitel 1: Declining Teen Employment: Minimum Wages, Returns to Schooling, and Immigration

- Koautor: David Neumark (University of California, Irvine)
- Veröffentlicht als: Neumark, D., and Shupe, C. (2019). Declining Teen Employment: Minimum Wages, Returns to Schooling, and Immigration. *Labour Economics*. doi: https://doi.org/10.1016/j.labeco.2019.03.008.

Kapitel 2: Great Expectations: Reservation Wages and Minimum Wage Reform

- Koautorin: Alexandra Fedorets (DIW Berlin)
- Vorveröffentlichung: SOEPpapers 968

Kapitel 3: Drivers of Participation Elasticities across Europe: Gender or Earner Role within the Household?

- Koautorin: Charlotte Bartels (DIW Berlin)
- Vorveröffentlichung: IZA DP No. 11359

Kapitel 4: Affordability of the Affordable Care Act's Medicaid Expansion: Implications for Household and Public Finances

- Kein Koautor
- Keine Vorveröffentlichung

Acknowledgements

I would like to thank my main supervisor, Carsten Schröder, for excellent supervision, support and valuable feedback throughout my time as a PhD student. I am grateful for the generous time he dedicated to helpful discussions on ongoing research and for incorporating me into the stimulating atmosphere of his research group at the DIW. Further, I am indebted to my second supervisor, David Neumark, who as a coauthor and mentor, has been instrumental to my development as a researcher. I learned a great deal through our joint work and benefited immensely from his comments on my research and from his guidance in my academic pursuits.

I thank my co-authors Charlotte Bartels and Alexandra Fedorets for the fruitful and enjoyable cooperation and for sharing with me skills and insights that have made me a better researcher. I owe a special thanks as well to my colleagues at the DIW Graduate Center and in the SOEP department for thoughtful comments on my work, in particular: Max Bach, Charlotte Bartels, Patrick Burauel, Sarah Dahmann, Khalid Elfayoumi, Alexandra Fedorets, Daniel Graeber, Johannes König, Hannes Kröger, Holger Lüthen, Maria Metzing, Jakob Miethe, Felicitas Schikora, Julia Schmieder, Ulrich Schneider, Fabian Stöckl and Vera Zipperer.

I gratefully acknowledge financial support from the Heinrich-Böll-Foundation and Mercatus Center at George Mason University, which allowed me to concentrate most of my time as a PhD student on my own research and present my work at academic conferences. I thank the DIW Graduate Center for providing an excellent structured PhD program and financial support and colleagues at the SOEP department for their assistance with data-related questions. Finally, the last chapter of my dissertation would not have been possible without access to restricted data at the Federal Agency for Healthcare Research and Quality (AHRQ). In particular, I would like thank Ray Kuntz for his assistance at the Data Center and in running code remotely.

Last, but not least, I thank my mom for her unending support, without which this dissertation would not have been possible.

Contents

Introduction: Household Finances and Labor Supply – The Role of Public Policies . . . 1

1 Declining Teen Employment . . . 4

1.1 Introduction . . . 4
1.2 Prior Research . . . 5
1.3 Descriptive Evidence on Changes in Teen Labor Market and School Enrollment Behavior . . . 9
1.4 Questions Raised by Developments in Teen LFP, Employment, and School Enrollment . . . 11
1.5 Evidence on Sources of Changes in Teen Employment and Enrollment . . . 18
1.5.1 The Model . . . 18
1.5.2 Data . . . 20
1.5.3 Model Estimations . . . 21
1.5.4 Endogenous Immigration? . . . 26
1.5.5 Comparing the effects of minimum wages, returns to schooling, and immigration . . . 28
1.6 Conclusions . . . 33
1.7 Appendix . . . 39

2 Great Expectations: Reservation Wages and Minimum Wage Reform . . . 44

2.1 Introduction . . . 44
2.2 Institutional Background . . . 47
2.3 Data . . . 48
2.4 Estimation Strategy . . . 53
2.4.1 Baseline Specification . . . 55
2.4.2 Heterogeneity and Robustness of Results . . . 56
2.5 Conclusion . . . 59
2.6 Appendix . . . 61

3 Drivers of Participation Elasticities across Europe . . . 69
3.1 Introduction . . . 69
3.2 Empirical Approach . . . 71
3.2.1 Basic Model . . . 71
3.2.2 Measuring Participation Tax Rates . . . 73
3.2.3 Data . . . 76
3.2.4 Estimation Strategy . . . 77
3.3 Participation Tax Rates across Europe . . . 79
3.4 Results . . . 89
3.4.1 Estimates . . . 89
3.4.2 Participation Elasticities . . . 92
3.5 Conclusion . . . 98
3.6 Appendix . . . 100

4 Affordability of the Affordable Care Act's Medicaid Expansion . . . 113
4.1 Introduction . . . 113
4.2 Evidence on the Impact of Public Insurance . . . 116
4.3 Empirical Approach . . . 119
4.3.1 Data and Sample . . . 119
4.3.2 Measuring Policy Provisions of the Affordable Care Act . . . 120
4.3.3 Estimation Strategy . . . 124
4.4 Trends in Household OOP Medical Spending . . . 129
4.5 Results . . . 135
4.5.1 Out-of-Pocket Expenditures . . . 135
4.5.2 Risk Protection . . . 140
4.5.3 Charity Care and Other Sources of Payment . . . 143
4.5.4 Health Care Utilization . . . 146
4.6 Heterogeneous Effects . . . 150
4.7 Cost-Benefit Analysis . . . 153
4.7.1 Fiscal Costs of Medicaid . . . 154
4.7.2 Social Benefits of Medicaid . . . 155
4.8 Conclusion and Discussion . . . 156
4.9 Appendix . . . 159

List of Tables . . . 169

List of Figures . . . 171

Bibliography . . . 171

Deutsche Zusammenfassung 184

Introduction: Household Finances and Labor Supply – The Role of Public Policies

The goal of this dissertation is to contribute to better understanding the role of various public policies in shaping opportunities, incentives and ultimately economic decisions at the individual and household level. The four independent research papers that constitute the thesis employ microeconometric methods to explore causal relationships between policy interventions, household consumption and labor supply, with a particular interest in low-income households. Chapters one and two both focus on the effects of minimum wages among groups exhibiting lower skills than those present in the average population: teens and the non-employed, respectively. Chapter three investigates the work incentives inherent in tax-benefit systems across 12 countries of the European Union and how these incentives influence labor supply decisions on the extensive margin. Chapter four considers the effect of an in-kind benefit, namely the availability of public health insurance, on household medical spending and consumption.

Chapter one presents joint work with David Neumark. In this paper, we explore the drivers of the observed teen substitution away from working while in school and toward exclusively being enrolled, which has substantially contributed to a decline in teen employment in the United States since 2000, in particular among 16-17 year-olds. We consider three main explanatory factors that are often discussed in the media and academic debate: rising minimum wages that could reduce employment opportunities for teens; increasing returns to schooling that could render an intensified focus on academic pursuits more valuable; and competition from immigrants that, like minimum wages, may reduce employment opportunities. We investigate this question using a cross-section of data from the Current Population Survey (CPS) and other sources and estimate a multinomial logit model for the share of teens in each of four mutually exclusive and exhaustive categories: not in school and not employed (NSNE or idle), employed and not in school (ENS), in school and employed (SE), and in school and not employed (SNE).

Among the factors investigated, higher minimum wages prove to be the predominant explanation for changes in the schooling and workforce behavior of 16-17 year-olds since 2000. Immigration from majority Spanish-speaking countries may have played a minor role, while we do not find evidence of higher returns to schooling having made a significant contribution to this trend. The long-term human capital implications of our findings remain somewhat ambiguous. On the one hand, both factors– minimum wages and competition from immigration – are associated with fewer opportunities to gain valuable labor market experience, which may have persistent, negative effects on potential lifetime earnings and employment probabilities of teen cohorts exposed to these factors. On the other hand, both factors could encourage teens to invest more in schooling in order to qualify for jobs with higher productivity standards. The question of the long-run returns to these competing types of labor market and academic experience, remains an important area for future research.

Whereas chapter one uses variation in incremental increases in minimum wages across states and time to identify induced changes in enrollment and employment decisions, chapter two exploits a rather large quasi-natural experiment consisting in the introduction of a highly binding statutory minimum wage in Germany in 2015 and takes issue with changes to wage expectations of non-employed job seekers. This chapter is joint work with Alexandra Fedorets.

While a large literature exists with respect to minimum wage impacts on employment and wages of the general population, chapter two presents the first causal study using quasi-experimental methods to identify the effect on reservation wages of the non-employed. We use exogenous variation in the reform exposure across regions and time in a difference-in-difference framework to identify an 18 percent increase in reservation wages among non-employed job seekers at the low end of the distribution of expected wages following the introduction of the minimum wage. We also document a shift in the observed wages of workers of a similar magnitude. Our findings suggest that minimum wages do not necessarily result in higher labor force participation, as job seekers may adjust their reservation wages accordingly.

Like chapters one and two, chapter three is dedicated to explaining labor supply decisions at the individual level, but with a specific focus on the household context. In collaboration with Charlotte Bartels, I investigate the role of tax-benefit systems across 12 European countries in contributing to the observed divergence in labor force participation rates of low-skilled workers and secondary earners between 2008-2014. Using EUROMOD harmonized data and the accompanying tax-benefit microsimulation model, we compute participation tax rates (PTRs) as a measure of work disincentives for labor market participation in each country.

By exploiting the institutional variation in tax-benefit policies modelled in EUROMOD and a group IV for the PTR, we go beyond comparing levels of work incentives to assess the actual responsiveness of individuals to these extensive margin tax rates. We compute heterogeneous participation elasticities by country, gender and the individual's potential earner role within the household (primary, secondary, sole earner) and find this latter factor dominant in explaining the responsiveness of individuals in their decision to work or not to work. Irrespective of gender, we find negligible responses for primary earners and large responses for secondary earners. The paper contributes to the ongoing debate in the EU regarding policies that incentivize work and moreover offers European evidence corroborating the observed convergence in male and female labor supply elasticities in the US, as traditional divisions of labor break down in some EU countries. Because our findings demonstrate the importance of estimating participation elasticities based on economic concepts like opportunity costs rather than gender, they should be of relevance for researchers working on labor supply models and optimal taxation as well as for policy-makers interested in the labor supply effects of tax-benefit systems.

Finally, chapter four turns to the monetary incentives of the in-kind benefit of public health insurance and its subsequent impact on spending and consumption behavior of low-income households in the United States. While this paper does not treat labor supply directly, understanding the effect of public insurance on the budget constraint is a prerequisite to investigating its potential labor supply effects, which I intend to explore in future research.

Using data from the Medical Expenditures Panel Survey (MEPS), I estimate the short-run impact of Medicaid public insurance expansion under the Patient Protection and Affordable Care Act (ACA), which was implemented in 2014, on medical out-of-pocket spending (OOP). I measure exposure to the reform at the household level according to eligibility rules and apply a DD(D) identification strategy to exploit variation in eligibility across regions, income groups and time. I find that a one standard deviation increase in public insurance expansion significantly reduced household OOP among Medicaid-eligible households by 8.8 percent for medical services and products and by 12.0 percent for insurance premia. It moreover reduced risk exposure to high-cost payments. However, I find no effect on access to urgent care or utilization of preventive care services. Results demonstrate some crowd-out of private insurance in the order of 4.6 percent, but also a reduction in inefficient charity care in favor of more formal public insurance schemes. On net, Medicaid expansion increased the share of total medical costs paid by public sources by 10.9 percent.

1 Declining Teen Employment: Minimum Wages, Returns to Schooling, and Immigration

Published as:
Neumark, D., and Shupe, C. (2019). Declining Teen Employment: Minimum Wages, Returns to Schooling, and Immigration. *Labour Economics*. doi: https://doi.org/10.1016/j.labeco.2019.03.008.

2 Great Expectations: Reservation Wages and Minimum Wage Reform

2.1 Introduction

Neoclassical, monopsonistic, and search theoretical models all predict negative labor demand reactions to the introduction of a binding minimum wage that is set above marginal productivity of labor. On the supply side, however, the same minimum wage could increase the number of people whose reservation wage[1] falls below the available market wage, thus increasing the probability of filling vacancies in low-wage sectors. Yet, this supply-side effect can only mitigate potential employment losses if reservation wages are static. If reservation wages react to minimum wages by adjusting upward, non-employed job seekers do not necessarily increase their search intensity and thus, this supply channel for possible positive employment effects would prove muted at best. The question of how minimum wages affect the wage expectations of non-employed job-seekers is therefore highly relevant for understanding the distributional and labor market effects of this policy tool.

In this paper, we empirically investigate changes to the reservation wages of non-employed job seekers induced by the 2015 statutory minimum wage introduction in Germany. We employ a difference-in-difference strategy using variation in exposure to the reform across regions and time. Defining a treatment intensity 'bite' measure as the degree of each non-worker's exposure to the reform according to his/her county (ROR) of residence, we compare reservation wages of individuals facing different levels of exposure before and after the reform.

Theoretically, the relationship between reservation wages and minimum wages belongs to common model assumptions. Within the neoclassical framework of a competitive market structure, binding minimum wages should increase reservation wages through inflation expectations. In labor markets with search and matching frictions, reservation wages may adjust positively to increases in the observed wages of workers or negatively to a decrease in the job offer arrival rate. In the present case of the introduction of a high-impact statutory minimum wage in Germany, economic theory yields ambiguous predictions about how reservation wages may adjust. First, the market structure is un-

[1]Defined by the threshold at which a potential worker is willing to accept a job offer.

observable. Second, while early evidence suggests a rightward shift of the wage offer distribution on account of the minimum wage introduction (Caliendo et al., 2017; Bossler and Gerner, 2016), a consensus has yet to be reached on the employment impact and thus changes to the job offer arrival rate for job seekers.

Targeting long-run effects, Blömer et al. (2018) estimate an equilibrium search model using a two percent representative sample of German low- and medium-skilled individuals subject to social security contributions and find a large, total increase in unemployment in the order of 13 percent compared to the steady state level. Bossler and Gerner (2016) use the IAB Establishment Panel data with a difference-in-differences identification strategy and find a smaller 1.9 percent decrease in employment among affected German establishments compared to non-affected ones, driven predominantly by a reduction in new hires. Using combined data from the Structure of Earnings Survey and the Socio-Economic Panel (SOEP), Caliendo et al. (2018) find even smaller employment reductions for a sample of prime-aged workers of approximately 0-0.3 percent of regular jobs and 2.4 percent of mini-jobs. Likewise focusing on a sample of prime-aged workers with data from the Federal Employment Agency, Garloff (2016) finds no significant impact of the minimum wage on employment.[2] We return to these early findings of small or insignificant disemployment effects in 2015-2016, which are driven by the reduction in new hires, in our discussion of possible selection effects in our estimation.

Beyond the burgeoning research evaluating the employment effects of the German minimum wage, a large literature has explored the effects of minimum wages on the hourly wages and employment of workers in other countries. Belman and Wolfson (2014) and Neumark and Wascher (2008) provide excellent surveys. With respect to the *non-employed job seekers*, however, the empirical link between minimum wages and reservation wages has largely been neglected in the literature due to lack of information about individual acceptance thresholds. One prominent exception is Falk et al. (2006), who conduct a lab-based experiment and find a positive and significant impact of minimum wages on reservation wages. They demonstrate that minimum wages set a new standard for fair pay and create entitlement effects that persist even after the removal of a wage floor.

While we are unaware of other papers employing reduced form strategies to directly investigate the link between reservation wages and minimum wages, this paper contributes to related structural work on the optimal search behavior and joblessness duration of job seekers (Burdett and Mortensen, 1998; Flinn, 2006). It is well established that higher

[2] Differences in the estimated effects of these studies for Germany can be attributed to two main factors. First, ex-post studies of treatment effects inevitably focus on the short-run impacts while equilibrium search models target longer term trends. Second, effects tend to be smaller when sampling the entire prime-aged population rather than groups likely to be more affected.

reservation wages lead to longer unemployment spells and a higher probability of long-term unemployment (Jones, 1988; Ljungqvist and Sargent, 1998; Addison et al., 2009, 2010, 2013; Brown and Taylor, 2015). In the frictional setting of a sequential search model, the reservation wage is defined implicitly as the optimal stopping rule in the job search behavior of the unemployed and can be expressed as a function of the expected future value of employment. This value depends centrally on the arrival rate of job offers and the observed wage distribution of workers.[3] A minimum wage may affect this optimal reservation strategy in two ways: through a negative impact on the job offer arrival rate or through the rightward (positive) shift of the wage offer distribution. According to this model, a rightward shift of the wage distribution should increase the reservation wage, while a decrease in the job offer rate should decrease the reservation wage.[4]

The current paper furthermore builds upon previous work by Brown and Taylor (2013, 2015), who explore how reservation wages respond to business cycle downturns. They trace out a 'reservation wage curve', documenting that job seekers adjust their acceptance thresholds inversely with the local unemployment rate. Previous papers have also shown that reservation wages tend to decrease over the course of the unemployment spell (Krueger and Mueller, 2016; Burdett and Vishwanath, 1988). Koenig et al. (2016) however argue that reservation wages react little to changes in the job offer arrival rate due to anchoring around previous wages. Against this background, the extent to which the introduction of the minimum wage induced adjustments to the wage acceptance thresholds of job seekers remains an empirical exercise to explore.

To our knowledge, the present paper is the first causal study using quasi-experimental methods to identify the effect of minimum wage reform on reservation wages of non-employed job seekers[5] in a real-world setting. Detailed survey information from the Socio-Economic Panel (SOEP), in combination with a quasi-experiment from the introduction of a highly binding statutory minimum wage in Germany, provide a unique opportunity to document this unexplored relationship. We show that reservation wages adjust upward in

[3] The optimal search strategy for the standard search model can be expressed as follows: $w^{res} = b + \frac{\alpha}{\rho}\int_{w^{res}}^{\overline{w}}[1-F(w)]dF(w)$ where b denotes transfers received when not working, ρ is a time discounting factor, α captures the job arrival rate, and $F(w)$ is the observed wage distribution. More complicated models may include extensions, such as job destruction and job-to-job transitions, but these would not change the relevant predictions for minimum wages that motivate this paper.

[4] These relationships are demonstrated by the sign the first order conditions of the reservation wage equation with respect to α and $[1 - F(w)]$.

[5] Specifically, our population of interest includes individuals officially registered as unemployed as well as all other non-working individuals who answer positively that they intend to take up work in the future (date unspecified in the survey), as the SOEP records reservation wages for both of these groups. We do not have reservation wage information for those simultaneously registered as unemployed and working and thus do not include them in our population of interest. We use an additional survey question on the timing of the job take-up intention in order to make the additional restriction that individual plans to search within the next 12 months.

reaction to minimum wage increases in the short run, suggesting that potential supply-side positive employment effects are mitigated through higher wage expectations. In particular, the introduction of a high-impact minimum wage induces a substantial increase in reservation wages among non-employed job seekers at the low end of the distribution in 2015 in the order of 18.1 percent at the 10th percentile and 12.5 percent at the 25th percentile of the reservation wage distribution compared to the pre-reform distribution. Growth in expectations at the 10th percentile of reservation wages moreover persists two years after the reform, in 2016. Higher percentiles do not exhibit any change.

One limitation of this study is that we are unable to exhaustively exclude the possibility that our results may partially be driven by composition (selection) effects. If dis-employment increases average productivity in the pool of job seekers, this influx of new searchers could mechanically change the distribution of reservation wages. In order to address this issue, we utilize the rich panel data structure of the SOEP to control for observed human capital indicators. We find our results are robust to educational attainment as well as controls for lifetime months of accumulated experience in full-time employment, part-time employment and unemployment spells. Nevertheless, we cannot exclude an impact from other composition changes not observable in our data.

The remainder of the paper is structured as follows. Section 2.2 describes the institutional background of the reform. Section 2.3 discusses our data and sample restrictions. Section 2.4 lays out our estimation strategy, presents the main results, and addresses the question of possible selection and the suitability of our model. Section 2.5 concludes.

2.2 Institutional Background

On January 1, 2015, the German government implemented a nation-wide statutory minimum wage of €8.50 (gross) per hour. This first mandated wage floor in the country's history replaced a long-standing regime of decentralized, voluntary wage bargaining at the sectoral level. The new minimum wage carried a large bite, with 16 percent of non-exempt employees earning below €8.50/hour prior to the reform and thus directly impacted (Amlinger et al., 2016). This measure corresponds to a Kaitz-index of 0.49 (OECD, 2015). Exemptions exist for the self-employed, workers under the age of 18 without vocational training, the long-term unemployed during their first 6 months of employment, trainees and interns working for a period of less than three months in a compulsory internship or in an entry-level internship for the purpose of gaining a qualification. In the transition period that lasted through the end of 2016, additional exceptions encompassed workers already covered by a sector-specific minimum wage.[6] For the calculation of the exposure to the minimum wage reform, we omit from the analysis all individuals exempt from the national minimum wage.

While the national minimum wage was not implemented until January 2015, it was widely discussed in political debate and the media as early as the summer of 2013, albeit without the specific threshold of €8.50. The minimum wage became a central topic of the federal election held on September 22, 2013 and an important component of the Social Democrat campaign platform. Therefore, anticipation of some form of a national minimum wage became more concrete once coalition negotiations began in late September 2013 between that party and the Christian Democratic Union. On December 14, 2013, both parties signed the coalition agreement confirming this intention. The new coalition government then announced the planned minimum wage level at €8.50 on April 2, 2014. In order to avoid distortions in our analysis stemming from possible anticipation effects, we conduct the analysis using 2013 as the pre-reform basis year.

2.3 Data

We use the 2010 to 2016 waves of the Socio-Economic Panel, a representative longitudinal survey that, as of 2016, surveys approximately 15,000 households (doi: 10.5684/soep.v33, Goebel et al., 2018). The survey asks non-working individuals,[7] *"What would your net income (in euros per month) have to be for you to accept a position?"* Subsequently, they are asked, *"How many hours per week would you have to work for this income?"*[8] Using this information, we calculate net hourly reservation wages.

For the period under investigation, between 2010-2016, we begin with a total of roughly 22,000-26,000 unique individual observations of 17-64 year-olds in each year and make several necessary restrictions to the working sample. Appendix Table 2.A1 documents each step of the sample restrictions and how they reduce the working sample. First, we limit the (unbalanced) sample to all non-employed and unemployed, for whom valid information on reservation wages exists and who are willing to take up work either immediately or within one year. As such, our sample comprises the non-working population truly searching for a job. We drop observations missing information about reservation

[6] Examples include the main construction sector, electrician trade, roofing sector, security services, hairdressing, commercial cleaning and others. See Fitzenberger and Doerr (2016) for a thorough overview.

[7] The sample includes respondents in voluntary military service, voluntary social year, or federal volunteer service, but excludes those in any type of employment, in training programs, in apprenticeships, or in partial retirement.

[8] The wording for both questions remains exactly same over the observed time frame.

wages or key socio-demographic characteristics such as gender, age, citizenship, highest educational level, months of part-time, full-time and unemployment experience, marital status, and presence of children below the age of 16 in the household. Further, we exclude observations from four regions that have fewer than 30 observations. Finally, in order to identify the region in which each individual resided (and thus their treatment intensity, or 'bite') prior to the minimum wage reform, we require the individual to be observed in 2012 and meet the working sample criteria listed above in any year between 2010-2016. In total, the working sample contains 8,227 unique observations from 2010-2016, which enter into our regression specification. Table 2.1 provides descriptive statistics for demographic characteristics of the sample and demonstrates that the composition of non-employed job seekers remains relatively stable throughout the period of analysis. In Section 2.4.2 we present regression results that control for these characteristics and corroborate our main findings.

Table 2.1: Sample Description, 2013-2016

	2013	2014	2015	2016	Total
Female share	0.65	0.65	0.62	0.65	0.64
Age, average	38.37	39.58	40.25	41.05	39.57
German share	0.91	0.93	0.92	0.94	0.92
Primary education share	0.42	0.42	0.39	0.37	0.40
Secondary education share	0.45	0.43	0.43	0.45	0.44
Tertiary education share	0.13	0.16	0.18	0.18	0.16
Married share	0.47	0.49	0.48	0.49	0.48
Living in HH with children below 16, share	0.60	0.56	0.55	0.55	0.57
Observations	1,277	956	805	660	3,698

Source: SOEP v33.1, own calculations.

Figure 2.1 shows the density of the distribution of hourly reservation wages and demonstrates that, although the distribution remains almost unchanged in 2013 and 2014, it exhibits a substantial shift to the right in 2015, when the statutory minimum wage was introduced. However, in 2016, the distribution shifts again to the left, implying that the reaction of wage expectations after the introduction of the minimum wage was perhaps temporary for some sections of the wage distribution. Moreover, this back-and-forth movement of the reservation wage distribution underlines the importance of analyzing the extent to which this movement was *caused* by the minimum wage reform rather than simply reflecting secular economic trends or a statistical peculiarity. Last but not least, the reservation wage distribution exhibits clumping in the upper percentiles of the distribution, which may be of importance for the estimation. At the same time, lower percentiles, where the minimum wage reform is likely to kick in, are not prone to clumping.

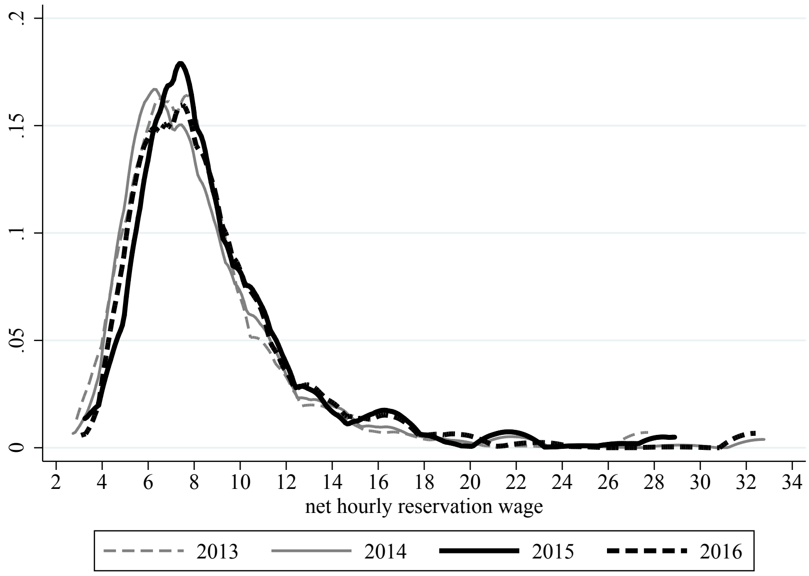


Figure 2.1: Distribution of Hourly Reservation Wages, 2013-2016
Source: SOEP v33.1, own calculations. Net hourly wages are CPI adjusted.

Because the net hourly reservation wage results from dividing net monthly reservation wages by the desired hours worked, it is important to ensure that the observed shifts in the hourly reservation wage distribution over time stem from an increase in expected monthly earnings rather than a decrease in working hours. Appendix Tables 2.A2 and 2.A3 display averages of monthly reservation wages and weekly hours in 20-percent bands around the 10th, 25th, 50th, 75th and 90th percentiles of the annual distribution of hourly reservation wages. The comparison of these averages over time suggests that the shift in the distribution of hourly reservation wages stems from an upward-shift of monthly reservation wages whereas hours, if anything, increase. Therefore, while the positive effect of the minimum wage reform on observed wages found in Caliendo et al. (2017) stems predominantly from reductions in contractual working hours, the main channel of adjustment for hourly reservation wages can be attributed to upward adjustments of desired monthly earnings rather than hours.

It is important to note that the €8.50 German minimum wage reform targeted gross hourly wages while the reservation wage question in the SOEP survey asks about net take-home wages. Appendix Table 2.A4 offers a descriptive comparison of the percentiles of the distribution of gross hourly observed wages, net hourly observed wages and net hourly reported reservation wages between 2013-2016. While the complexity of the German tax system prevents a straightforward comparison between gross and net hourly wages, the table reveals that net hourly observed wages surpass those of the net hourly reservation wages in all percentiles, reflecting, *inter alia*, a lower average skill set of the non-employed compared to workers.

Successful job hunters with reservation wages in the lowest part of the distribution are likely to transition into low-paid jobs directly affected by the minimum wage increase. Table 2.2 shows these transitions in more detail. For example, non-employed job seekers with reservation wages in the lowest quartile of the reservation wage distribution in 2013 obtained a mean gross wage of €8.37 if they took up a job the following year in 2014. In the post-reform period, job seekers stated a reservation threshold of €5.58 and, among those transitioning into employment, the mean realized gross hourly wage after the implementation of the wage floor was €8.67. Although we do not observe in which wage sector respondents look for job, these numbers suggest that low reservation wages tend to, on average, transform into low observed wages. Therefore, minimum wages do not only reshape the lower tail of the distribution of observed wages. They are also likely to affect lower quantiles of the reservation wage distribution. Indeed, the causal analysis results displayed in the following section show that the minimum wage brought about increases in reservation wages only in these lower percentiles of the reservation wage distribution.

Table 2.2: Reservation wages, subsequent employment and observed wages

w^r_{t-1}:	$\mathrm{w}^r_{t-1=2013}$	$\mathrm{u}_{t-1=2013} \rightarrow e_{t=2014}$	$\mathrm{w}^o_{t=2014}$	$\mathrm{w}^r_{t-1=2015}$	$\mathrm{u}_{t-1=2015} \rightarrow e_{t=2016}$	$\mathrm{w}^o_{t=2016}$
Average	8.56	0.23	12.96	9.15	0.23	13.65
1st quartile	5.13	0.17	8.37	5.58	0.14	8.67
2nd quartile	6.92	0.19	9.75	7.34	0.20	12.22
3rd quartile	8.52	0.24	12.77	8.97	0.25	11.18
4th quartile	13.67	0.30	17.40	14.71	0.33	18.91

Source: SOEP v33.1, own calculations.

To establish a causal link between reservation and minimum wages, we use the introduction of a high-impact statutory minimum wage in Germany as a quasi-experimental setting. Despite the nationally uniform introduction of the minimum wage, its impact differs across regions. Figure 2.2 depicts the shares of eligible employees with actual gross hourly wages below €8.50 in 2012 in 92 planning regions in Germany ($Bite_r^{2012}$). The map shows that the shares of eligible employees vary greatly from 4 to 46 percent. Eastern German states exhibit a stronger exposure to the reform than Western Germany, but the variation within these broad regions is also substantial. Due to data limitations, we exclude four regions with fewer than 30 observations. We chose 2013 as the reference pre-reform year in which anticipatory adjustments are unlikely to occur in the SOEP data: the field work of the SOEP survey was finished by September 2013, before the new program of the Grand Coalition announcing the upcoming introduction of minimum wage was published. Accordingly, in order to avoid simultaneity bias, we follow Caliendo et al. (2018) and define the treatment intensity regional bite variable the year before, in this case 2012.

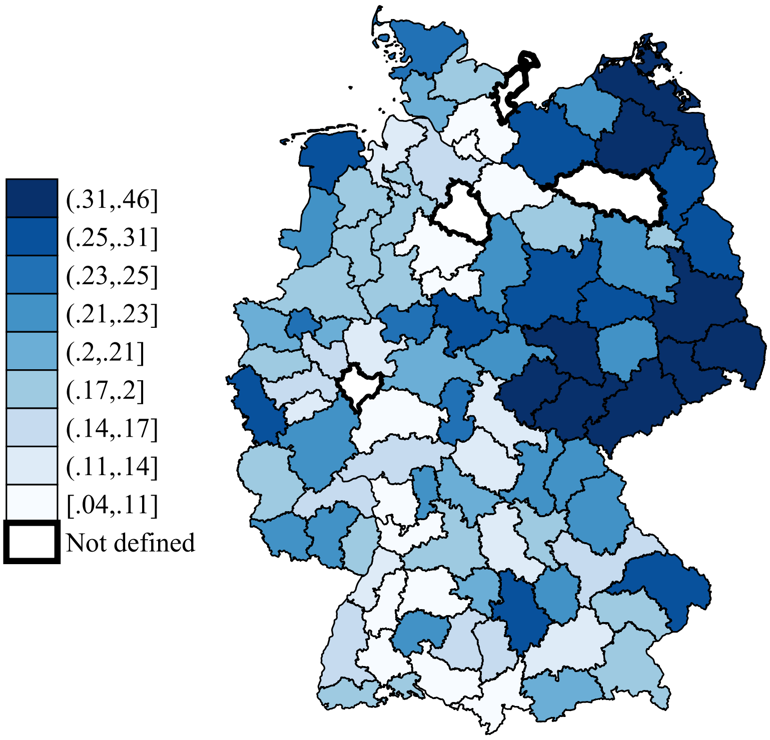


Figure 2.2: Share of Employees with Actual Hourly Wages below the Minimum Wage in 2012

Source: SOEP v33.1, own calculations.

Appendix Table 2.A5 illustrates differences the sample composition between regions with above- and below-median bite. The distribution of the main socio-demographic and human capital characteristics is fairly similar between these two groups. Some differences can be observed in the share of secondary versus tertiary education, with higher bite regions having more secondary rather than tertiary educational attainment, consistent with education being positively correlated with wages. A large difference can also be found in the share of married individuals, which is higher in low-bite regions. This difference can likely be attributed to more traditional family norms in western Germany compared to eastern Germany. We will discuss the inclusion of these controls in more detail when describing different regression specifications.

2.4 Estimation Strategy

The continuous measure of the minimum wage bite enters a difference-in-differences estimation as follows:

$$\log(RW_{irt}) = \alpha + \sum_{t=2010}^{2016} \beta^t \times D^t + \gamma Bite_r^{2012} + \sum_{t=2010}^{2016} \delta^t \left(D^t \times Bite_r^{2012}\right) \\ [+\mu \mathbf{X}_{irt}] + \epsilon_{irt}. \quad (2.1)$$

The dependent variable is the log of the net hourly reservation wage of individual i at time $t \in [2010, 2016]$ residing in region r. We use 2013 as the base year and exclude it and its interaction with the bite from the estimation. Years 2010 to 2012 are pre-reform and pre-announcement years, 2014 is the last pre-reform year in which anticipation effects can be expected, whereas 2015 and 2016 are post-reform years. $Bite_r^{2012}$ denotes treatment intensity as captured by the region-specific shares of eligible employees with actual hourly wages below €8.50 (divided by the average regional bite in 2012, such that the average $Bite_r^{2012} = 1$). The coefficient δ^t on the interaction terms captures the treatment effect of the reform in 2015 and 2016, potential anticipatory effect in 2014 and simultaneously verifies the common pre-trend assumption in the years 2010-2012. In additional specifications, we expand this 'bare bones' specification by the vector $\mathbf{X}_{irt}$, which contains socio-demographic, human-capital related and regional controls. Section 2.4.2 contains these results as well as those for a model with additional regional fixed effects.

Two potential threats exist to the validity of our identification of causal effects: failure of the common trend assumption necessary for unbiasedness in the difference-in-differences estimation and selection bias. The common trend assumption requires that, absent the minimum wage introduction, reservation wages of job seekers in regions with a small share of workers earning below €8.50 would have developed at a similar rate as those in regions with a large share of such workers. Because we estimate an unconditional quantile regression, this assumption must also hold within each quantile.

We offer several forms of evidence that the common trend assumption holds. First, Figure 2.3 shows the evolution of the 10th and 25th percentile of reservation wages since 2010 for high-bite, juxtaposed to low-bite regions and descriptively documents that the high- and low-bite regions display a fairly parallel pre-reform course within the given quantile. We highlight these lower quantiles rather than the mean, as they present the regions of the distribution most impacted by the minimum wage reform and thus are the focus of our descriptive and causal analysis. Appendix Figures 2.A1 and 2.A2 likewise exhibit similar parallel trends for the relevant percentiles of observed hourly wages and employment rates.

With respect to selection (composition) bias, a concern remains that disemployment effects from the minimum wage may have improved the composition of the pool of job seekers if those who lost their job are more productive than the average non-employed job seeker. The increase in average productivity would then mechanically increase reservation wages after the reform. While we cannot exhaustively control for such composition effects, Section 2.4.2 discusses several strategies we employ to account for selection as much as possible, including controlling for human capital indicators such as educational attainment and work experience in full-time, part-time and unemployment. While results prove very robust to the inclusion of these controls, we cannot rule out a possible impact from other composition factors not measured in our data.

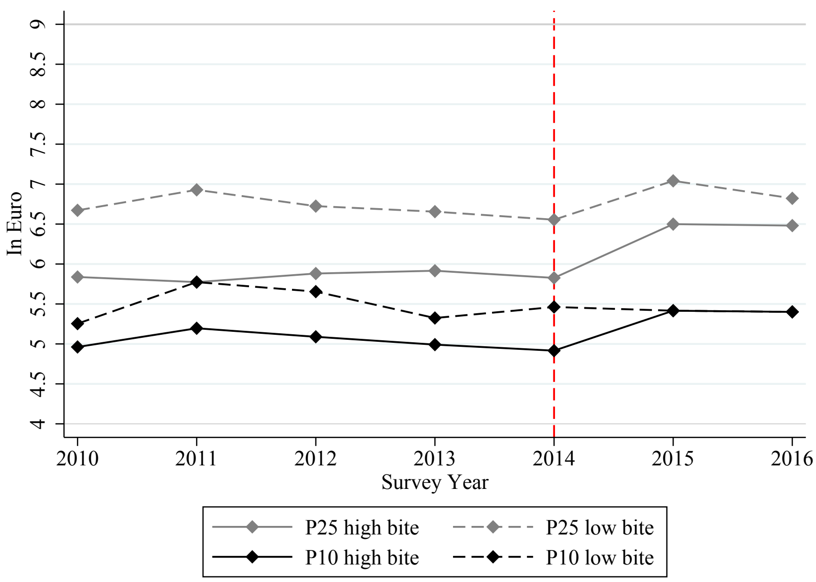


Figure 2.3: 10th and 25th Percentiles of Reservation Wages, by Region Type
Source: SOEP v33.1, own calculations. Net hourly wages are CPI adjusted.

2.4.1 Baseline Specification

We expect the minimum wage to have a differential impact along the distribution of reservation wages. Therefore, we estimate an unconditional quantile regression based on the re-centered influence function (Firpo et al., 2009). Table 2.3 contains the results of the estimation of equation 2.1 in its 'bare bones' specification. It presents estimates of the coefficients δ^t from Equation 2.1 for quintiles of the distribution of log hourly reservation wages. At the 10th percentile, the interaction term documents a growth of 18.1 percent ($exp^{0.166} - 1$) due to the reform in 2015. Given the reservation wage shown in Appendix Table 2.A4 of €4.99 at the 10th percentile in 2013, this wage growth amounts to €0.90/hour. In the 25th percentile, the growth is 12.5 percent ($exp^{0.118} - 1$), or €5.92 $\times$ 0.125 = €0.74/hour. In higher quantiles, the effect in 2015 is insignificant. In 2016, the minimum wage-induced growth at the 10th percentile remains virtually unchanged, at 17.9 percent, while no effect can be detected at the 25th percentile. This result confirms that the introduction of the minimum wage induced an increase in reservation wages exclusively at the bottom of the distribution, where potential low-wage workers are disproportionately located.

Table 2.3 not only depicts the causal impact of the minimum wage in the post-reform years of 2015 and 2016. It also provides a test of the common trend assumption by considering the coefficients on the interaction terms of the bite variable and pre-reform years. As evidenced by the insignificant effects at the 10th, 25th and 50th percentiles, the common trend cannot be rejected at the lower part of the distribution, where we detect an impact from the minimum wage. In contrast, the significant coefficients at the 75th and 90th percentiles in 2011 warrant cautious interpretation in particular at higher quantiles of the distribution.

To better understand the channel of adjustment observed in Table 2.3, we also investigate the effect of the minimum wage introduction on observed hourly wages of workers. Search theory predicts that job seekers should increase their acceptance threshold if the wage offer distribution observed on the labor market shifts to the right. Table 2.4 provides estimates of Equation 2.1 with log gross hourly wages of eligible employees as the dependent variable.

The impact on reservation wages can be compared to the overall shift in the observed wages of workers. The results in Table 2.4 show that the effect on the 10th percentile of the distribution of observed gross hourly wages is 24.0 percent ($exp^{0.215} - 1$) in 2015, which corresponds to 7.55 $\times$ 0.240 = €1.81. This growth is even stronger (25.9 percent) at the 10th percentile of gross hourly wages in 2016. The effect tapers off in higher quantiles, with 6.7 percent growth at the 25th percentile in 2015 and 9.1 percent growth

Table 2.3: Difference-in-Differences: Growth in Reservation Wages in 2010-2016

	P10	P25	P50	P75	P90
$D^{2010} \times Bite^{2012}$	0.097	-0.019	-0.020	0.095	0.119
	(0.067)	(0.047)	(0.047)	(0.060)	(0.102)
$D^{2011} \times Bite^{2012}$	0.032	0.014	-0.016	0.123**	0.221**
	(0.066)	(0.043)	(0.040)	(0.054)	(0.102)
$D^{2012} \times Bite^{2012}$	0.048	0.014	0.010	0.073	0.107
	(0.059)	(0.044)	(0.042)	(0.047)	(0.100)
$D^{2014} \times Bite^{2012}$	0.013	-0.049	-0.050	0.004	-0.034
	(0.059)	(0.048)	(0.047)	(0.062)	(0.121)
$D^{2015} \times Bite^{2012}$	0.166***	0.118**	-0.005	0.030	-0.120
	(0.053)	(0.047)	(0.050)	(0.064)	(0.116)
$D^{2016} \times Bite^{2012}$	0.165***	0.063	-0.051	0.020	-0.183
	(0.055)	(0.057)	(0.053)	(0.068)	(0.136)
Observations	8,227	8,227	8,227	8,227	8,227

Source: SOEP v33.1, own calculations. Robust standard errors in parentheses, calculated using bootstrapping with 200 repetitions (built-in option `bootstrap` in the `rifreg` command).

Reservation wages are in net terms and adjusted to inflation (CPI 2010).

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

in 2016, both compared to the baseline year of 2013. Individuals at the 75th and 90th percentiles not only do not experience wage growth, but actually witness a decline in their earned wages, of about 4.3 and 5.1 percent at the 75th percentile in 2015 and 2016, respectively. In 2015, the decline at the 90th percentile of observed gross wages reached roughly 6.5 percent, pointing to a compression of observed hourly wages. Compared to the impact on hourly reservation wages, the effect on observed wages is slightly higher, more concentrated at the lower tail of the distribution and also exhibits some indication of wage compression. Table 2.4 moreover provides some evidence of anticipation effects, as the 10th and 90th percentiles of observed wages show significant effects already in 2014.

2.4.2 Heterogeneity and Robustness of Results

Because previous studies have found that the German minimum wage most affected the observed wages of women, low-skilled workers and East Germans, we run our estimation separately for these groups in order to explore possible heterogeneous effects. We find the effect only significant among East Germans (as opposed to West Germans) at a magnitude similar to the main results at the 10th and 25th quantiles. The impact among men at the 10th quantile of the reservation wage distribution is stronger than in the main results: 20

Table 2.4: Difference-in-Differences: Growth in Observed Gross Hourly Wages in 2010-2016

	P10	P25	P50	P75	P90
$D^{2010} \times \text{Bite}^{2012}$	0.005	-0.005	0.037	0.024	0.068**
	(0.045)	(0.039)	(0.027)	(0.024)	(0.027)
$D^{2011} \times \text{Bite}^{2012}$	-0.047	-0.024	0.007	0.003	0.030
	(0.041)	(0.034)	(0.023)	(0.022)	(0.026)
$D^{2012} \times \text{Bite}^{2012}$	0.003	0.023	0.006	-0.008	0.019
	(0.039)	(0.033)	(0.021)	(0.024)	(0.028)
$D^{2014} \times \text{Bite}^{2012}$	0.105**	0.050	-0.005	-0.037	-0.056*
	(0.042)	(0.035)	(0.023)	(0.026)	(0.031)
$D^{2015} \times \text{Bite}^{2012}$	0.215***	0.065*	-0.003	-0.044*	-0.067**
	(0.041)	(0.035)	(0.025)	(0.025)	(0.032)
$D^{2016} \times \text{Bite}^{2012}$	0.230***	0.087**	-0.002	-0.052**	-0.051
	(0.039)	(0.037)	(0.026)	(0.025)	(0.034)
Observations	59,539	59,539	59,539	59,539	59,539

Source: SOEP v33.1, own calculations. Robust standard errors in parentheses, calculated using bootstrapping with 200 repetitions (built-in option `bootstrap` in the `rifreg` command).

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

percent growth in 2015 and 25 percent in 2016 compared to the baseline year 2013. For women, the effect is only significant at the 25th percentile in 2015 at a magnitude similar to the main results. The additional sample partition, however, reduces the sample size and prohibits a deeper consideration of these heterogeneous relationships.[9]

Another possible model specification that could control for time-persistent regional trends would be to estimate a regional fixed effects model in line with Caliendo et al. (2018). Appendix Table 2.A6 displays results from adding regional (ROR) fixed effects to the baseline specification and shows that results are robust to any time-constant differences across regions.

Next, we turn to the robustness of our results to selection (composition) bias, which could arise if the quality of the pool of non-employed job seekers increased differentially across regions after the minimum wage reform. In such a case, our results could simply reflect a spurious effect from the positive correlation between higher reservation wages and higher-skilled job seekers. A related paper by Caliendo et al. (2018) shows a small, negative effect of the minimum wage bite on employment, suggesting that the average productivity in the pool of the non-employed in regions with a high bite might in fact have

[9]For exposition purposes, we do not show these results here, but they are available upon request.

increased. At the same time, duration dependence and scarring associated with longer unemployment spells in these high bite regions may counteract the impact of the higher quality pool of job seekers if they adjust their reservation wages downward accordingly.[10] While it is not possible in our setting to control for unobserved heterogeneity of the non-employed job seekers,[11] we do use our rich panel data to control for observed heterogeneity that is related to the quality of the pool of job seekers.

In a first step, we test the robustness of our results to socio-demographic covariates of the pool of job seekers. Appendix Table 2.A7 presents estimation results from Equation 2.1 after the inclusion of control variables for gender, age, German citizenship, the presence of children below the age of 16 in the household and marital status and shows that the impact of the minimum wage on reservation wages at the bottom quantiles of the distribution remain intact.

In a second step, we add further human capital controls that include indicators for three possible levels of educational attainment (primary, secondary, tertiary) as well as variables that capture experience in full-time employment, part-time employment and unemployment in years. Appendix Table 2.A8 reports these results and shows they are nearly identical to those from the main specification without these controls. As such, this evidence would not comport with a story of selection on educational attainment and work experience of the non-employed pool driving the effects.

Finally, Appendix Table 2.A9 additionally controls for a one year lag in the unemployment rate and GDP per capita in the ROR. These additional regional controls account for the regional potential to adapt to the reform (Dolton et al., 2015). Again, the results prove very robust to this addition. In sum, all of these results with different sets of controls confirm their general robustness to different selection channels. We, however, cannot rule out potential selection along other dimensions and characteristics not measured here.

[10] A long literature exists with regard to the adverse effects of long unemployment spells on the probability of re-entering the labor market. Ljungqvist and Sargent (1998) offer a survey of early studies. For recent evidence of scarring effects from a correspondence study, see Kroft et al. (2013), who document that the probability of receiving a job interview decreases with the increasing length of an unemployment spell that is independent of observed productivity.

[11] Ideally, one would use a selection model in line with Heckman (1979) in order to account for non-random selection into unemployment. However, in contrast to a two stage equation in which the first stage explains selection in to employment and the second stage explains observed hourly wages, the concern of this paper lies in explaining *reservation wages*, which by definition define the lowest acceptable wage offer threshold necessary to enter employment. As such, it is not possible to find valid exclusion restrictions necessary for this model that explain the decision to work, but that are unrelated to the reservation wage. Individual fixed effects likewise would not be suitable for our question of interest because they would confine our sample to the long-term unemployed for whom we have observations between 2013-2016 and who overlap very little with active job seekers.

2.5 Conclusion

In this paper, we explored the impact of minimum wages on the reservation wages of non-employed job seekers. We exploited the unique opportunity to combine survey data on reservation wages with the quasi-experiment given by the variation in regional exposure to the 2015 introduction of a statutory minimum wage in Germany. Unlike many of the previous minimum wage reforms around the world, the level of the German minimum wage introduced in 2015 for the first time in German history on a national level was highly binding, directly affecting 16 percent of workers (Amlinger et al., 2016). Using a regional bite measure that captures the share of the workforce affected in each region, we established causality through a difference-in-differences estimation in which we compared the effects on individuals residing in highly affected local area regions with those living in areas with a small share of affected workers.

We find that the introduction of the minimum wage in 2015 led to an increase in the acceptable wage thresholds of non-workers at the bottom of the reservation wage distribution. The induced growth in reservation wages measured 18.1 percent, or €0.90/hour in 2015, among individuals at the 10 percentile of the reservation wage distribution and 12.5 percent, or €0.74/hour, at the 25th percentile in 2015. In 2016, 17.9 percent growth persisted only at the 10th percentile compared to the pre-reform baseline year. Acceptance thresholds at the median and upper tail of the distribution remained unchanged. These findings offer suggestive evidence of a labor market with search frictions, in which job seekers adjust their reservation wage thresholds in reaction to a rightward shift in the observed wage distribution of workers. The large positive impact of the minimum wage on observed hourly wage growth of workers earning in the lowest 10 percentile of the distribution in both 2015 and 2016 demonstrates the existence of this rightward shift. Descriptive statistics in Table 2 show that, for respondents in the lower quartile of the reservation wage distribution, the probability of finding a job fell after the introduction of the minimum wage, whereas the resulting hourly wage grew. This evidence suggests that the decrease in the job arrival rate could result in the re-adjustment of the wage expectations of job seekers with low reservation wages in the medium run.

Finally, inflation expectations may additionally motivate the adjustment in reservation wages at the bottom of the distribution. Individuals from lower income households have, on average, lower reservation wages and also spend a larger share of their income on consumption. It is also plausible that lower-skilled individuals disproportionately utilize services and goods affected by minimum wages and thus are more affected than individuals with higher reservation wages, who reside on average in higher income households. For instance, lower-income individuals would be more likely to receive a haircut from a hairdresser bound by the minimum wage whereas higher income individuals may utilize services from higher end salons, where workers earn above the minimum.

In this paper, we attempted to outline what we consider the most plausible channels of reservation wage adjustments that follow the introduction of a high-impact minimum wage. Further exploration of these channels would shed more light on the supply-side adjustments from the German minimum wage reform in particular and minimum wages in general. At the same time, it remains a challenging empirical endeavor that requires more detailed and larger data sources than those available to us.

2.6 Appendix

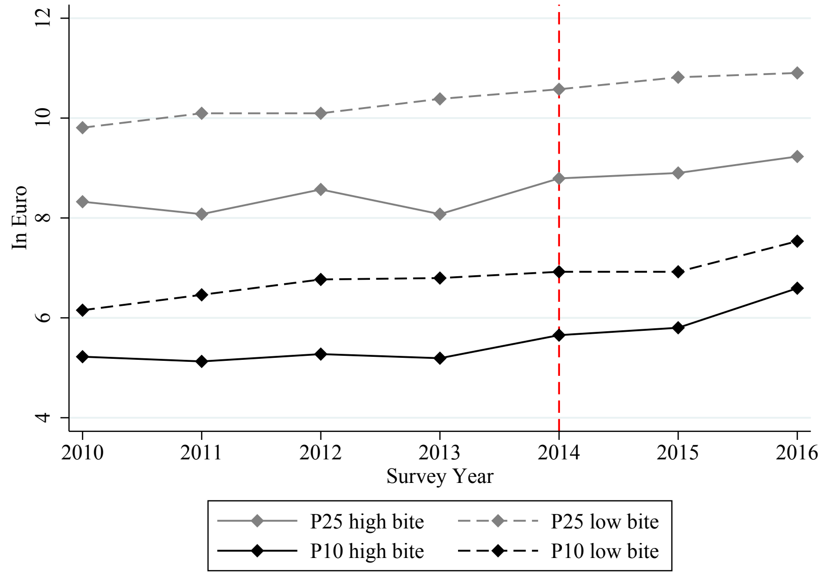


Figure 2.A1: 10th and 25th Percentiles of Observed Hourly Wages, by Region Type
Source: SOEP v33.1, own calculations. Net hourly wages are CPI adjusted.

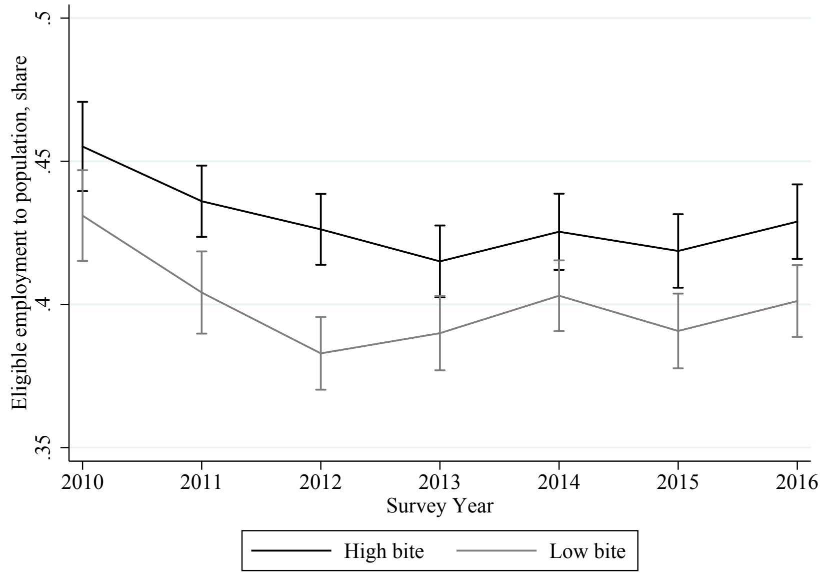


Figure 2.A2: Share of Employment in the Total Population (aged 17 to 64), by Region Type
Source: SOEP v33.1, own calculations.

Table 2.A1: Sample Size after Inclusion of Selection Criteria, by Year

	2010	2011	2012	2013	2014	2015	2016
Total SOEP v33.1 age 17-64	21,984	23,333	22,503	25,589	22,161	21,891	22,575
+ unemployed or non-employed	6,719	6,587	6,099	6,908	5,746	5,712	7,494
+ wants to take up work soon	4,833	4,827	4,584	5,056	4,292	4,084	5,523
+ valid info on res wage or ind characteristics	1,672	1,771	1,743	1,950	1,453	1,413	1,142
+ regions with at least 30 obs	1,671	1,768	1,742	1,947	1,450	1,407	1,141
+ res wage 2010-2016 and present in 2012	1,271	1,516	1,742	1,277	956	805	660

Notes: SOEP v33.1, own calculations.

Table 2.A2: Average of Reservation Monthly Earnings at Percentiles of Hourly Reservation Wage Distribution, 2013-2016

	P10	P25	P50	P75	P90
2013	762.06	914.77	1096.32	1194.43	1627.65
2014	829.44	937.70	1114.80	1353.85	1744.96
2015	954.19	1085.01	1155.28	1243.44	1614.58
2016	891.01	1030.29	1059.15	1220.33	1710.02

Source: SOEP v33.1, own calculations. The table contains averages of monthly earnings in 20-percent bands around the 10th, 25th, 50th, 75th and 90th percentiles of the respective annual distribution of hourly reservation wages.

Table 2.A3: Average Working Hours at Percentiles of Hourly Reservation Wage Distribution, 2013-2016

	P10	P25	P50	P75	P90
2013	33.65	35.24	34.83	32.96	32.75
2014	36.00	36.09	34.76	33.30	33.01
2015	37.15	37.95	34.77	29.00	29.23
2016	37.22	37.02	32.27	28.15	31.33

Source: SOEP v33.1, own calculations. The table contains averages of reservation weekly hours in 20-percent bands around the 10th, 25th, 50th, 75th and 90th percentiles of the respective annual distribution of hourly reservation wages.

Table 2.A4: Percentiles of Hourly Wage Distributions, 2013-2016

	P10	P25	P50	P75	P90
Net hourly reservation wages					
2013	4.99	5.92	7.40	8.87	12.04
2014	5.17	6.01	7.49	9.83	13.11
2015	5.42	6.50	8.12	10.26	13.54
2016	5.40	6.48	8.10	10.23	13.50
Net hourly observed wages					
2013	5.79	7.69	10.26	13.74	18.46
2014	6.23	8.08	10.67	14.34	19.04
2015	6.46	8.24	10.93	14.57	19.53
2016	6.71	8.46	10.99	14.63	19.62
Gross hourly observed wages					
2013	7.55	10.47	15.23	21.10	28.21
2014	7.92	11.08	15.79	21.60	28.96
2015	8.46	11.54	16.15	22.51	30.00
2016	8.65	11.54	16.48	22.69	29.71

Source: SOEP v33.1, own calculations.
The sample for the calculation of net hourly reservation wages contains respondents who were present in the sample in 2012 and were in one of the other survey years 2010-2016 in the status of a non-employed job seeker, i.e. were unemployed or non-employed aged between 17 and 64, who want to take up employment immediately or within a year, with valid information on reservation wages and socio-demographics. The sample excludes regional units with less than 30 observations.
The sample for the calculation of net and gross observed wages includes all employed who are eligible to minimum wages, have valid information on socio-demographics. The sample excludes regional units with less than 30 observations.

Table 2.A5: Sample Description by Region Type, 2014-2016

Regions:	High-Bite	Low-Bite
Female share	0.63	0.65
Age, average	40.07	40.35
German share	0.95	0.90
Primary education share	0.41	0.37
Secondary education share	0.47	0.39
Tertiary education share	0.11	0.24
Married share	0.45	0.53
Living in HH with children below 16, share	0.54	0.56
Observations	1,292	1,129

Source: SOEP v33.1, own calculations. High-Bite: regions with the above-median regional bite in 2012. Low-Bite: regions with the bite lower than the median in 2012.

Table 2.A6: Difference-in-Differences: Growth in Reservation Wages in 2010-2016, with ROR-specific Fixed Effects

	P10	P25	P50	P75	P90
$D^{2010} \times \text{Bite}^{2012}$	0.107	-0.024	-0.020	0.103*	0.134
	(0.066)	(0.046)	(0.044)	(0.062)	(0.100)
$D^{2011} \times \text{Bite}^{2012}$	0.026	0.006	-0.023	0.117**	0.210**
	(0.071)	(0.044)	(0.043)	(0.059)	(0.098)
$D^{2012} \times \text{Bite}^{2012}$	0.049	0.015	0.015	0.080	0.110
	(0.055)	(0.041)	(0.042)	(0.057)	(0.099)
$D^{2014} \times \text{Bite}^{2012}$	0.010	-0.051	-0.046	0.002	-0.037
	(0.063)	(0.051)	(0.048)	(0.063)	(0.106)
$D^{2015} \times \text{Bite}^{2012}$	0.165***	0.112**	-0.008	0.031	-0.110
	(0.053)	(0.049)	(0.047)	(0.072)	(0.113)
$D^{2016} \times \text{Bite}^{2012}$	0.175***	0.061	-0.045	0.029	-0.151
	(0.057)	(0.050)	(0.054)	(0.079)	(0.134)
Observations	8,227	8,227	8,227	8,227	8,227

Source: SOEP v33.1, own calculations. Robust standard errors in parentheses, calculated using bootstrapping with 200 repetitions (built-in option `bootstrap` in the `rifreg` command).

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Table 2.A7: Difference-in-Differences: Growth in Reservation Wages in 2010-2016, with Socio-Demographic Controls

	P10	P25	P50	P75	P90
$D^{2010} \times Bite^{2012}$	0.097	-0.023	-0.024	0.092*	0.118
	(0.068)	(0.053)	(0.042)	(0.054)	(0.104)
$D^{2011} \times Bite^{2012}$	0.035	0.017	-0.017	0.119**	0.218**
	(0.070)	(0.046)	(0.040)	(0.052)	(0.098)
$D^{2012} \times Bite^{2012}$	0.051	0.013	0.005	0.065	0.104
	(0.054)	(0.048)	(0.040)	(0.052)	(0.101)
$D^{2014} \times Bite^{2012}$	0.014	-0.047	-0.046	0.011	-0.024
	(0.061)	(0.053)	(0.047)	(0.058)	(0.119)
$D^{2015} \times Bite^{2012}$	0.159***	0.107**	-0.017	0.018	-0.134
	(0.050)	(0.052)	(0.046)	(0.066)	(0.117)
$D^{2016} \times Bite^{2012}$	0.157***	0.052	-0.060	0.014	-0.180
	(0.054)	(0.055)	(0.054)	(0.075)	(0.139)
Observations	8,227	8,227	8,227	8,227	8,227

Source: SOEP v33.1, own calculations. Robust standard errors in parentheses, calculated using bootstrapping with 200 repetitions (built-in option `bootstrap` in the `rifreg` command).

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

This specification additionally includes *socio-demographic controls* for gender, age, German vs. foreign citizenship, marital status and presence of children aged below 16 in the household.

Table 2.A8: Difference-in-Differences: Growth in Reservation Wages in 2010-2016, with Human Capital Controls

	P10	P25	P50	P75	P90
$D^{2010} \times Bite^{2012}$	0.092	-0.033	-0.039	0.071	0.098
	(0.066)	(0.048)	(0.043)	(0.053)	(0.096)
$D^{2011} \times Bite^{2012}$	0.033	0.011	-0.027	0.104**	0.200**
	(0.076)	(0.047)	(0.044)	(0.045)	(0.093)
$D^{2012} \times Bite^{2012}$	0.042	0.001	-0.010	0.039	0.072
	(0.056)	(0.048)	(0.043)	(0.049)	(0.099)
$D^{2014} \times Bite^{2012}$	0.018	-0.046	-0.045	0.007	-0.039
	(0.063)	(0.051)	(0.050)	(0.057)	(0.118)
$D^{2015} \times Bite^{2012}$	0.167***	0.116**	-0.003	0.042	-0.101
	(0.056)	(0.049)	(0.053)	(0.063)	(0.107)
$D^{2016} \times Bite^{2012}$	0.176***	0.075	-0.030	0.060	-0.128
	(0.058)	(0.051)	(0.054)	(0.058)	(0.135)
Observations	8,227	8,227	8,227	8,227	8,227

Source: SOEP v33.1, own calculations. Robust standard errors in parentheses, calculated using bootstrapping with 200 repetitions (built-in option `bootstrap` in the `rifreg` command).

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

This specification additionally includes *socio-demographic controls* for gender, age, German vs. foreign citizenship, marital status and presence of children aged below 16 in the household as well as *human capital controls* for three categories of the highest achieved educational level (primary, secondary, tertiary) and years of experience in full-time employment, part-time employment and unemployment.

Table 2.A9: Difference-in-Differences: Growth in Reservation Wages in 2010-2016, with Regional Controls

	P10	P25	P50	P75	P90
$D^{2010} \times Bite^{2012}$	0.095	-0.031	-0.037	0.071	0.102
	(0.065)	(0.049)	(0.040)	(0.055)	(0.100)
$D^{2011} \times Bite^{2012}$	0.033	0.010	-0.030	0.097*	0.192*
	(0.060)	(0.045)	(0.043)	(0.058)	(0.100)
$D^{2012} \times Bite^{2012}$	0.045	0.007	-0.002	0.053	0.092
	(0.057)	(0.044)	(0.039)	(0.051)	(0.103)
$D^{2014} \times Bite^{2012}$	0.017	-0.047	-0.047	0.006	-0.041
	(0.062)	(0.047)	(0.049)	(0.062)	(0.118)
$D^{2015} \times Bite^{2012}$	0.165***	0.116**	-0.003	0.044	-0.099
	(0.053)	(0.049)	(0.044)	(0.064)	(0.116)
$D^{2016} \times Bite^{2012}$	0.175***	0.077	-0.024	0.072	-0.115
	(0.057)	(0.052)	(0.050)	(0.071)	(0.134)
Observations	8,227	8,227	8,227	8,227	8,227

Source: SOEP v33.1, own calculations. Robust standard errors in parentheses.

Significance levels: * p<0.1, ** p<0.05, *** p<0.01

This specification additionally includes *socio-demographic controls* for gender, age, German vs. foreign citizenship, and marital status, *human capital controls* for three categories of the highest achieved educational level (primary, secondary, tertiary) and years of experience in full-time employment, part-time employment and unemployment, as well as *regional controls* such as one-year lagged unemployment rate and GDP per capital in the respective ROR.

3 Drivers of Participation Elasticities across Europe: Gender or Earner Role within the Household?

3.1 Introduction

Labor market participation rates diverge greatly across countries of the European Union (EU). The extent to which incentives inherent in the various tax-benefit systems drive these differences remains a topic of contention throughout many member states.[1] Of particular concern are low participation rates among low-skilled individuals and secondary earners with weak labor market attachment. At the same time, these groups traditionally exhibit high responsiveness to monetary employment incentives. Thus, tax-benefit distortions at the extensive margin for these types of potential workers may contribute to low participation rates and create high efficiency costs. The resulting, substantial fiscal costs of inactivity include expenses for out-of-work benefits, foregone taxes and social security contributions. These costs render understanding the responsiveness of these groups to tax-benefit incentives highly relevant.

At the extensive margin, the participation tax rate (PTR) measures tax-benefit distortions to work. Since the 1980s, a wide range of empirical studies estimate the participation elasticity at the micro level, measuring the behavioral response to monetary incentives for work at the extensive margin. These studies exploit exogenous shocks to a particular group's work incentives through a tax or benefit reform in a quasi-experimental setting.[2] A general result is that the behavioral response is higher at the extensive margin than at the intensive margin, particularly for low-skilled, secondary earners (married women) or single mothers. However, it is unclear whether results obtained in a very particular quasi-experimental study of a specific reform yield externally valid results for general application (Goolsbee, 1999; Meghir and Phillips, 2010). Bargain et al. (2013)

[1] For an early and a very recent contribution to this debate, see (Prescott, 2004; Bick and Fuchs-Schündeln, 2018).

[2] An early and often cited example is Eissa and Liebman (1996), who exploit the 1986 introduction of the Earned Income Tax Credit (EITC) in the US in to estimate the labor market response of lone mothers at the intensive and extensive margin. Chetty et al. (2013) and Meghir and Phillips (2010) provide overviews on the estimated elasticities of these quasi-experimental studies. The participation elasticities of the studies reviewed by Chetty et al. (2013) average 0.28 and range from 0.13 to 0.43.

estimate a structural discrete choice model using numeric simulation to calculate labor supply elasticities for several European countries and the United States and obtain results in line with the magnitude found in quasi-experimental studies. Much smaller within-period micro-elasticities are found in two reduced form studies that exploit incremental changes in the tax-benefit system.

Jäntti et al. (2015) and Kalíšková (2018) use two different instrumental variables (IV) approaches to estimate participation elasticities across European countries. Building upon these studies, we establish exogeneity through a group IV that instruments the individual-level net-of-PTR earnings with the group average such that common biasing factors in the labor supply equation cancel out. We use the same instrument as Jäntti et al. (2015), but different from Kalíšková (2018), who employs a simulated IV approach for a pooled EU-wide sample of women. In contrast to Jäntti et al. (2015), who use averages from similar household types to approximate PTRs on the basis of the Luxembourg Income Study data, we use the microsimulation model EUROMOD in order to calculate taxes, social security contributions and benefits for every individual in both potential working states, in work and out of work. This strategy enables us to investigate participation elasticities across European countries and sociodemographic groups, such as gender and earner roles within the household.

Our contribution is threefold. First, this paper is the first reduced form analysis of European participation elasticities based on harmonized microsimulation of taxes and benefits according to the earner position of the individual within the household rather than simply according to gender. We exploit both the institutional variation across EU countries and changes in the tax-benefit systems between 2008 and 2014 to identify the causal impact of tax-benefit work incentives for employment across the EU, controlling for country and skill-level heterogeneity, including cultural norms or tastes for work and leisure. Second, we document the size and distribution of work disincentives, as measured by Participation Tax Rates (PTRs), across EU countries between 2008-2014, using EUROMOD data and the accompanying tax and benefit calculator.[3] In doing so, we are able to not only account for how a specific reform in isolation affects a certain target group, but also how different changes in the tax-benefit system interact with each other to influence work incentives throughout the entire distribution. Third, we provide a decomposition

[3]EU cross-country studies estimating PTRs based on the tax-benefit simulation model EUROMOD from earlier time periods include Immervoll et al. (2007), Immervoll et al. (2011) and O'Donoghue (2011). Kalíšková (2018) uses EUROMOD data covering 2005-2010 to estimate PTRs for women. Several country studies evaluate PTRs over time: Dockery et al. (2011) for Australia, Collado (2018) for Belgium, Adam et al. (2006) and Brewer et al. (2008) for UK, Pirttillä and Selin (2011) and Bastani et al. (2017) for Sweden, as well as Bartels and Pestel (2016) for Germany.

of the driving components of labor supply disincentives at the extensive margin and how individuals react differentially to taxes, benefits, and social security contributions.

Our main results are the following. First, disentangling the drivers of the PTRs, we find that the relative importance of taxes, social insurance contributions and benefits largely depends on household composition and the individual's earner role within the household. In line with optimal tax theory which shows negative PTRs can be optimal at the bottom of the earnings distribution (Saez, 2002; Immervoll et al., 2011; Choné and Laroque, 2011; Jacquet et al., 2013; Hansen, 2017), we document negative PTRs in several countries for low-income working families with children. Secondly, we find an average elasticity of 0.08 for men and 0.15 for women, as well as a high degree of heterogeneity across countries. Elasticities in half of the countries in our sample are not statistically different from zero, while relatively high elasticities ranging from 0.1 to 0.3 can be found in Belgium, Germany, Greece, Spain, Italy, and Sweden. Thirdly, however, by comparing elasticities within the same earner type, i.e., primary or secondary earners, this well-established difference between men and women dissipates. Both male and female secondary earners are the most responsive earner groups with elasticities between 0.1 and 0.4. Our results demonstrate the importance of calculating labor supply responses according to earner roles rather than gender, as differences between female and male labor force participation continue to decrease over time (Blau and Kahn, 2006). The finding that other earner types in most countries do not respond to work incentives limits the case for policymakers to further reduce PTRs for these groups, if the motivation is to increase labor force participation.

This paper is structured as follows. In Section 2, we derive our equation of interest from a static household labor supply model. Section 3 provides a description of how we compute PTRs, our estimation strategy, and the data employed. In Section 4, we take a closer look at the variation of PTRs across countries by household and earner types. We discuss in detail, how the varying degrees of work incentives are related to the specific features of the tax-benefit system in a given country. Section 5 presents our regression results and discusses our estimated participation elasticities. Section 6 concludes.

3.2 Empirical Approach

3.2.1 Basic Model

Our analysis is embedded into the economic framework of a static labor supply model, in which an individual i maximizes household utility $u(y_{ht} - T(e_{it}, e_{-it}, z_{ht}), q)$, where y_{ht} denotes household income defined as $y_{ht} = e_{it} + e_{-it} + z_{ht}$. z_{ht} expresses household non-labor income such as asset income in year t, while $e_{-it} + e_{it}$ denote the labor supply choices of each household member in the form of gross income. e_{it} can also be expressed as the product of wages and hours worked. We use the composite term, gross monthly earnings. $T(e_{it}, e_{-it}, z_{ht})$ are taxes and social security contributions paid net of any public transfers (benefits) received.

Following Immervoll et al. (2007), we assume that individual i enters employment if the financial gain from working is positive considering all resulting changes in taxes and transfers that the household faces as a whole. One should note that the changes in household taxes and transfers when taking up a job not only depend on household income as a whole, but on individual earnings, the earner role in the household (e.g., primary vs. secondary earner) and individual as well as household characteristics (e.g., single vs. couple), in particular. At the extensive margin, fix costs such as search costs, additional transportation costs and commuting time, alternative child care, the opportunity cost of home production, or general disutility from work can play a significant role in participation decisions (cf. Piketty and Saez (2013); Cogan (1981)). We therefore denote fixed costs as q and the condition for taking up a job becomes

$$q_{it} \leq e_{it} - [T(e_{it}, e_{-it}, z_{ht}) - T(0, e_{-it}, z_{ht})]$$

From this inequality, we arrive at the definition of the net-of-PTR earnings, which constitutes the measure of extensive margin work incentives in our analysis:

$$q_{it} \leq \left(1 - \underbrace{\frac{T(e_{it}, e_{-it}, z_{ht}) - T(0, e_{-it}, z_{ht})}{e_{it}}}_{PTR_{it}}\right) \cdot e_{it}$$

Net-of-PTR earnings, $(1 - PTR_{it}) \cdot e_{it}$, summarizes the decision of an individual i facing the binary choice between the two labor market states of being employed w or not working nw. Due to the static, one period nature of our model, we do not consider second order effects, such as possible labor supply adjustments from the partner (i.e. added worker effects) as a result of the individual changing her/his work status. Our equation of interest can be formulated as follows:

$$P(w_{it}) = \alpha + \beta(1 - PTR_{it}) \cdot e_{it} + \eta_{it} \tag{3.1}$$

where $P(w_{it})$ represents the participation decision and takes the value of 1 when the individual works and 0 otherwise. We expect a negative effect of the PTR on employment probability, as distortions to work incentives should make work less probable. Accordingly, we expect the effect of (1-PTR) to be positive. We are interested in the parameter β, which, if estimated consistently, permits us to quantify the participation elasticity. We then add a interaction terms to the parameter of interest, expanding this term to $\beta(1 - PTR_{it}) \cdot e_{it} \cdot \lambda_{cse}$ in order to allow for heterogeneous effects in the reaction to tax and benefit incentives across countries. As a result, it is possible to calculate the gender- or earner-type-specific elasticity in each country c based on the definition of Saez (2002) and adjusted to the PTR context:

$$PE_{cse} = (\hat{\beta_{se}} + \lambda_{cse}) \cdot \frac{\overline{(1 - PTR_{cse})}}{\overline{P(w)_{cse}}} \tag{3.2}$$

where $\overline{PTR_{cse}}$ is the average PTR by gender s or earner type e in each country c and $\overline{P(w)_{cse}}$ is the respective sample employment rate in each country. In the above equation of interest, the error term η_{it} is likely correlated with the PTR, thus causing an endogeneity problem which we address in Section 3.2.4.

3.2.2 Measuring Participation Tax Rates

The PTR measures the net difference in household taxes and benefits when an individual works, w, versus when (s)he does not work, nw, as a proportion of individual earnings in labor market state w and can be formulated as follows, suppressing the time index t

$$PTR_i \equiv \frac{T(y_h^w) - T(y_h^{nw})}{e_i} \tag{3.3}$$

where y_h^w is gross household income, $T(y_h^w)$ is household net taxes, and e_i is individual gross monthly labor earnings if the given individual is in the labor market state w. Gross household income can be calculated as the sum of labor earnings, asset income, private transfers, private pensions, and social security pensions of all household members. y_h^{nw} is gross household income and $T(y_h^{nw})$ is household net taxes, if the given individual is in labor market state nw, i.e. when (s)he has no individual labor earnings. We refer to net taxes T paid by the household h as income taxes t_h including social security contributions reduced by benefits b_h.

If household net taxes are equal for both labor market states, then the PTR amounts to zero, indicating that incentives to take up work are not distorted. However, in reality, a welfare state providing income support in the state nw usually leads to $t_h^{nw} < b_h^{nw}$ resulting in $T(y_h^{nw}) < 0$ as social benefits will surpass taxes paid for the reduced household income y_h^{nw}. As such, the change in net taxes when switching from w to nw will be positive in the presence of a welfare state with means-tested social assistance and the PTR will be higher than zero for most individuals. The higher the PTR, the more generous income support programs in the state of nw and/or high income taxes and social insurance contributions in the state of w reduce the financial gain from working. The PTR will equal one if the change in net taxes $T(y_h^w) - T(y_h^{nw})$ (numerator) is equal to individual earnings e_i (denominator). In this case, no financial gain arises from working. *Ceteris paribus*, lower spousal or other household earnings generally lead to higher PTRs due to higher means-tested transfers, and additionally, in countries where spousal tax splitting exists, a higher spousal tax reduction in the labor status nw. Therefore, in many countries the PTR will depend on household type and each potential worker's earner role within the household. Finally, if out-of-work income support exceeds earnings, then the PTR can be even greater than one; if benefits depend on in-work status such as the case with earned income tax credits (EITCs) or negative income taxes, the PTR could be negative for the affected workers.

In order to obtain a PTR for all individuals in the prime working-aged population, independent of their observed labor market status w or nw, we simulate the non-observed state. For this simulation, we abstract from possible secondary effects of labor status changes and concentrate our analysis on the decision of the individual potential worker, holding all other aspects of household composition fixed. As such, we assume that a change in one partner's labor supply behavior, i.e., giving up or taking up a job, does not simultaneously trigger a compensating labor supply reaction by other household members or changes in household income from other non-labor sources. This assumption reflects standard procedure in the PTR literature (see, e.g., Immervoll et al., 2007; Jäntti et al., 2015).

We start by predicting potential individual monthly earnings $\widehat{e_i}$ using a standard two-step Heckman regression (Heckman, 1979) by country, year and gender separately. Exclusion restrictions used to identify the selection term vary according to these groups. Variables include dummies for the presence of children in certain age groups, marital status, household non-labor income, household size, and the presence of an elderly person (older than 65 years) in the household. On average, predicted earnings closely match observed earnings, as can be taken from Table 3.1. We mostly predict slightly lower average incomes than observed for men and slightly higher average incomes than observed for women. [4]

[4] Appendix Figure 3.A15 demonstrates that the difference in the estimated PTR calculated on the basis of predicted rather than observed earnings is negligible. Small deviations remain for Greece.

Table 3.1: Predicted and Observed Mean Monthly Earnings

	2008			2010			2012			2014		
	pred.	obs.	gap	pred.	obs.	gap	pred.	obs.	gap	pred.	obs.	gap
AT												
Women	2281.2	1966.2	0.1	2297.6	2201.9	0.0	2243.3	1597.2	0.3	2490.1	2431.0	0.0
Men	3174.5	4360.7	-0.4	3467.3	3371.8	0.0	3646.5	4241.1	-0.2	3814.2	3668.0	0.0
BE												
Women	2431.2	2109.8	0.1	2662.0	2391.1	0.1	2828.3	2385.2	0.2			
Men	3193.5	3466.5	-0.1	3291.0	3558.9	-0.1	3463.7	3722.7	-0.1			
BG												
Women	512.7	409.9	0.2	625.5	522.1	0.2	603.6	530.4	0.1	710.7	625.4	0.1
Men	671.6	681.2	-0.0	761.9	818.8	-0.1	754.2	786.4	-0.0	904.0	886.4	0.0
CZ												
Women	17663.9	18514.4	-0.0	19512.3	16601.2	0.1	19181.2	17748.4	0.1			
Men	25714.3	25325.4	0.0	27328.3	23395.0	0.1	28093.0	21259.9	0.2			
DE												
Women	2300.0	2035.0	0.1	2408.1	2147.3	0.1	2402.0	2268.3	0.1			
Men	3645.8	4208.4	-0.2	3537.3	4229.8	-0.2	3641.4	4563.9	-0.3			
DK												
Women	24576.7	23616.7	0.0	0.0	0.0	0.0	30381.3	29347.8	0.0			
Men	32581.9	22403.0	0.3	0.0	0.0	0.0	38521.3	44705.3	-0.2			
EL												
Women	1393.1	1110.2	0.2	1416.0	1162.6	0.2	1297.0	940.5	0.3	1129.4	741.2	0.3
Men	1926.2	2109.6	-0.1	1933.0	1988.3	-0.0	1725.9	1474.7	0.1	1480.6	1260.3	0.1
ES												
Women	1493.1	1122.0	0.2	1592.1	1163.1	0.3	1592.5	1124.2	0.3	1619.0	1304.7	0.2
Men	1895.9	2047.0	-0.1	1951.3	1795.4	0.1	1900.9	1672.2	0.1	2000.1	1911.3	0.0
FR												
Women	1827.0	1680.4	0.1	2070.7	1986.6	0.0	2219.6	2110.6	0.0			
Men	2442.5	2350.2	0.0	2793.9	2739.8	0.0	2935.5	2827.8	0.0			
IT												
Women	1926.8	1427.8	0.3	1905.0	1338.1	0.3	1910.1	1347.5	0.3	1939.2	1373.6	0.3
Men	2567.7	2866.6	-0.1	2423.9	2447.7	-0.0	2431.5	2249.0	0.1	2503.8	2187.4	0.1
SE												
Women	21250.4	20663.0	0.0	24572.0	24490.7	0.0	26323.5	30910.8	-0.2			
Men	28515.8	24710.3	0.1	32010.4	33479.4	-0.0	34649.7	31339.6	0.1			
UK												
Women	1798.3	1612.1	0.1	1793.3	1600.5	0.1	1905.1	1654.8	0.1	1914.8	1624.3	0.2
Men	2648.4	2425.8	0.1	2709.5	2497.2	0.1	2721.8	2671.6	0.0	2658.6	2422.3	0.1

Source: EUROMOD data, own calculations. The sample includes individuals aged 25-54 working at least 20 hours per week, excluding the self-employed, students, pensioners, the permanently disabled, those in compulsory military service, and those on parental leave. Rates describe weighted means per country using the EUROMOD sample weights. The sample only includes years for which EUROMOD input data exist. UK 2008 is based on input from 2007, UK 2010 is based on input from 2009 and UK 2014 is based on input from 2013.

We assign individuals observed in w zero labor earnings in the counterfactual situation nw. We then obtain gross household income in both potential labor market states as $y_h = \widehat{e_i} + \sum_{j\neq i}^{N} e_j + z_h$, whereby $\widehat{e_i} = 0$ when the individual is in labor market state nw.[5]

Following the calculation of household gross income described above, we then use EUROMOD to apply the tax-benefit rules of the respective year and country to obtain household taxes t_h and public transfers b_h for both w and nw in a way that ensures consistent assumptions regarding deductions as well as other special tax and transfer rules

[5] Replacing observed earnings with predicted earnings for those observed in w allows us to isolate the identifying variation of interest discussed in further detail in Section 3.2.4.

across countries. For example, household taxes paid in state nw are the sum of income tax assessed on the basis of y_h^{nw} and social security contributions from the partner's earnings e_j if the partner j is working. Household public transfers are the sum of social assistance, housing allowances, and child benefits. A potential increase in benefits when changing from w to nw will mostly occur for social assistance and housing allowances. In contrast, benefits may also increase when changing from nw to w in the case of in-work benefits.

3.2.3 Data

We draw on EUROMOD data from 2008-2014,[6] which is based on EU-SILC cross-sectional data that have been specifically prepared for use in the EUROMOD microsimulation model.[7] EU-SILC provides *ex-post* harmonized and internationally comparable household-level statistics on labor and income variables. To date, the EUROMOD microsimulation model functions exclusively using this cross-sectional input dataset. We refer to this data in the following as EUROMOD data. All simulations are based on EUROMOD version G4.0+.

The EUROMOD data cover a representative sample of private households in all investigated countries.[8] Our sample includes individuals in their prime working age, between 25 and 54 years of age. We restrict the sample to these ages because large groups of individuals younger than 25 likely face a decision between education and work rather than between employment and inactivity, which is the focus of this paper. Likewise, beginning approximately around age 55, individuals in many countries may choose between (early) retirement and employment rather than employment and inactivity. Furthermore, we exclude the self-employed, students, pensioners, permanently disabled persons, those in compulsory military service, and those on parental leave. We trim the earnings distribution by dropping the bottom 1% in order to exclude unreasonably low earnings. Our final sample consists of approximately 350,000 individuals and four years of observations, namely 2008, 2010, 2012 and 2014.[9]

[6] The income reference period for all countries in our sample, except the UK, refers to the previous calendar year. For the UK, income refers to the previous twelve months. Furthermore, yearly income variables and the number of months employed are used to calculate monthly earnings.

[7] The EUROMOD microsimulation model is developed, maintained, and managed by the Institute for Social and Economic Research (ISER) at the University of Essex, in collaboration with national teams from the EU member states (See Sutherland and Figari (2013) for details).

[8] Countries include: Austria (AT), Belgium (BE), Bulgaria (BG), Czech Republic (CZ), Germany (DE), Denmark (DK), Greece (EL), Spain (ES), France (FR), Italy (IT), Sweden (SE) and United Kingdom (UK). In the following, we use the included abbreviations.

[9] We only include years for which EUROMOD provides input data in order to ensure that the determination of the PTR precedes the observed employment choice of the individual. For country-specific input years, refer to Table 3.2.

The EUROMOD micro-simulator currently offers an option to account for non-take-up of benefits as well as tax evasion for some countries. In order to ensure comparability, however, we do not model these for any country. Moreover, due to data limitations, neither contribution-based transitory benefits, such as unemployment insurance, nor in-kind benefits are accounted for. Not accounting for the former will underestimate the PTR level for countries with contribution-based SIC systems such as Austria, Belgium, and Germany. Lack of the latter could attenuate the participation elasticity, for example, in the case of publicly-provided childcare for individuals with small children, as such complementary goods reduce the fix costs of working.

We define the labor market status of employment, w, as having positive earnings and working at least 20 hours per week. We restrict our definition of w for two reasons. Firstly, working at least 20 hours allows workers to be employed either half- or full-time. Because part-time work is prevalent in many EU countries, this definition avoids the restrictive assumption that if non-workers transition into employment, they will always begin with a full-time job. Secondly, in order to avoid distortions in the PTR due to very low monthly earnings driven by workers in a transitional status between labor market attachment and occasional work, we exclude workers with less than 20 hours from our sample. Consequently, transition into employment is defined as taking up a job for at least 20 hours per week.

3.2.4 Estimation Strategy

In our regression analysis, we investigate the responsiveness of individuals to work incentives that are inherent in tax and benefit systems across the EU. We begin with a simple pooled OLS estimation of the structural labor supply equation, Equation 3.1, in the EUROMOD cross-sectional data and add demographic controls as well as country and year fixed effects. The binary outcome variable is one if individual i is employed in period t (w_{it}).

$$P(w_{it}) = \alpha + \beta(1 - PTR_{it}) \cdot e_{it} + X'_{it}\gamma + \lambda_c + \mu_t + \epsilon_{it} \tag{3.4}$$

If uncorrelated with ϵ_{it}, the coefficient β would capture the effect of the net-of-PTR earnings on the likelihood of labor market participation. A vector of controls for each individual is denoted by X_{it} and includes household non-labor income, education, experience, marital status, and the presence of a child in different age groups. Year fixed effects, μ_t, capture business cycle fluctuations affecting labor demand, while country fixed effects, λ_c, control for possible omitted policy variables and cultural preferences for work and leisure. The idiosyncratic error term is denoted by ϵ_{it}. Table 3.5 shows these results with and without controls for the EU sample as a whole.

We expect OLS to yield biased results due to an endogenous regressors problem in which the error term ϵ_{it} is likely correlated with the PTR. Endogeneity may arise through omitted variables, simultaneity or measurement error. The main concern in our setting stems from the omitted variable, which plausibly influences both an individual's probability to work $P(w_{it})$ and his or her net-of-PTR earnings $(1 - PTR_{it}) \cdot e_{it}$. For instance, highly motivated individuals might invest more in their human capital or choose more ambitious career paths, both of which are associated with higher earnings. At the same time, one would expect these same individuals to have a higher willingness to work compared to someone who is not motivated. Social norms present another omitted factor influencing both willingness to work and labor market income that individuals of particular social groups might expect. The correlation of these omitted variables with earnings e_{it} would bias the estimate of β in an upward direction. At the same time, for most individuals in the EU, higher labor market earnings will yield higher PTRs, as the PTR is a function of labor income. This mechanical correlation holds due to the progressive character of most taxation systems[10] and the means-tested nature of benefit receipt. The positive correlation between the omitted variable and the PTR creates a positive bias. Therefore, $1 - PTR_{it}$ yields a negative bias. In sum, the direction of the overall bias for the composite term of net-of-PTR earnings $(1 - PTR_{it}) \cdot e_{it}$ depends on which component dominates.

Due to these endogeneity concerns, we apply an instrumental grouping estimator (group IV), where group averages serve as instruments for the individual level net-of-PTR earnings. This instrument must be correlated with the individual level PTR (relevance condition) and exogenous to the observed labor supply choice (exclusion restriction). As discussed at length in Angrist (1991), Blundell et al. (1998), Blau and Kahn (2006) and Heckman and Robb (1985), instrumenting the individual-level endogenous explanatory variable in the labor supply equation with a group average drives the bias from omitted variables and measurement error toward zero as the cell size used to calculate group averages grows large. Specifically, identifying variation comes from cross-sectional differences across groups while common biasing factors are canceled out. Applications in the labor supply literature include Jäntti et al. (2015), Burns and Ziliak (2015) and Blau and Kahn (2006).

Optimal group partition will minimize heterogeneity within a group while allowing for enough variation beyond the group averages for identification. Minimizing hetero-

[10]Bulgaria and the Czech Republic serve as exceptions, with proportional taxation systems.

geneity involves a trade-off in which the group cells must remain sufficiently large for estimation. Since tax-benefit changes differentially affected individuals in different birth cohorts and income groups, we split the sample into 5-year age groups and three educational attainment levels as a proxy for permanent income, resulting in 18 groups. This group definition follows Burns and Ziliak (2015).[11] Adapting the Wald estimator formulated in Blundell et al. (1998) to the extensive labor supply margin, we estimate the following equation by 2SLS:

$$1^{st} stage : (1 - PTR_{it}) \cdot e_{it} = \theta(1 - PTR_{gt}) \cdot e_{gt} + X_{it}'\gamma + \alpha_g + \lambda_c + \mu_t + u_{it} \tag{3.5}$$

$$2^{nd} stage : P(w_{it}) = \beta\widehat{(1 - PTR_{it}) \cdot e_{it}} + X_{it}'\gamma + \alpha_g + \lambda_c + \mu_t + \epsilon_{it} \tag{3.6}$$

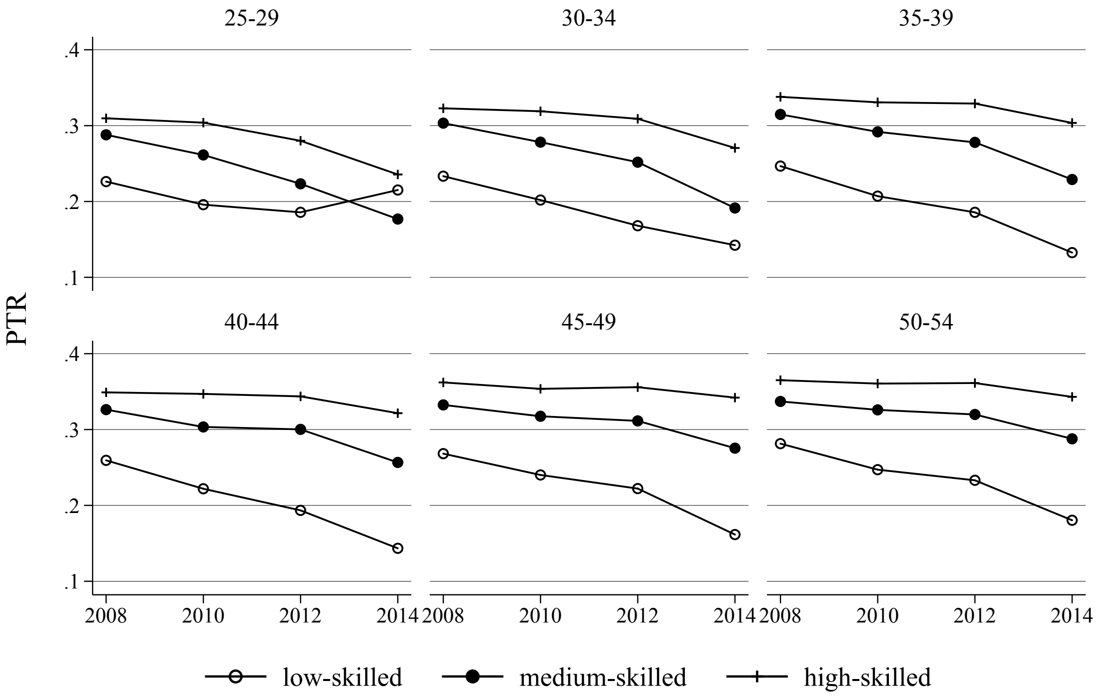
Having replaced the individual net-of-PTR earnings with the predicted value from the first stage, the correlation between the group mean and the idiosyncratic error term ϵ_{it} is assumed negligibly near zero. The necessary exclusion restriction for this instrument is that unobservable differences in net-of-PTR earnings across groups can be captured by permanent group α_g and country effects λ_c and an additive time effect μ_t. The second necessary condition corresponds to the rank condition and requires that, after subtracting the effect of the group, country, and time averages, some identifying variation in the PTR still remains, i.e. net-of-PTR earnings grow differentially across groups. Figure 3.1 displays our grouping estimator, i.e., PTRs by age group and education level over time, for Italy and shows that PTRs decreased at a higher rate for low-skilled than for high-skilled from 2008 to 2014. Further, the reduction is more pronounced for younger age groups. PTRs by age group and education level for the entire set of countries can be found in the Appendix Figures 3.A1 to 3.A12.

3.3 Participation Tax Rates across Europe

In this section, we take a closer look at variation across countries with respect to the dependent variable, employment, and the main explanatory variable of interest, net-of-PTR earnings. Table 3.2 depicts the observed employment rates in our sample across the EU when we define employment as having positive earnings and working at least 20

[11] For our preferred group definition, group sizes range from 29 to 2,046 individual observations. We also provide results according to an alternative group definition according to 10-year age cohorts, three educational attainment groups, and gender for comparison with Jäntti et al. (2015). Our estimates are robust to this alternative definition. These results are presented in Table 3.A1.

Figure 3.1: PTR by Age Group and Education Level, Italy



hours. Employment rates vary substantially between countries from 54 to 94 percent of the prime working-age population of women and between 81 and 97 percent of men.

Juxtaposed to these employment rates, Table 3.3 shows median PTRs for each country by year and gender. It is not only employment rates, but also PTRs that vary greatly across countries, with the highest extensive margin work incentives (lowest PTR) for women in Greece and Bulgaria; and for men in Greece, Italy, and Bulgaria. Several countries share relatively high PTRs and, thus, low work incentives for both women and men; in particular Belgium, Germany, and Denmark. Across all countries in the pooled sample, the average PTR is approximately 32% for women and 36% for men. Men tend to have a higher PTR than women due to higher earnings and, subsequently, higher tax and social security contributions, especially in countries with progressive taxation. As such, the income tax wedge between employment and unemployment is lower for women than for men, yielding a lower PTR. We return to these gender differences in more detail in Figure 3.3 where we decompose the drivers of the median PTR by earner type. Between 2008 and 2014, mean PTR decreased for both women and men in Denmark, Germany, and Italy, while they increased in Belgium, France, and Spain.

Figures 3.2 to 3.4 show the PTR distribution across countries by individual earnings quintile, earner type and household type. The boxplots show the median, interquartile range as well as minimum and maximum PTRs excluding outliers.

Table 3.2: Employment Rates in % and Observations by Country, 2008-2014

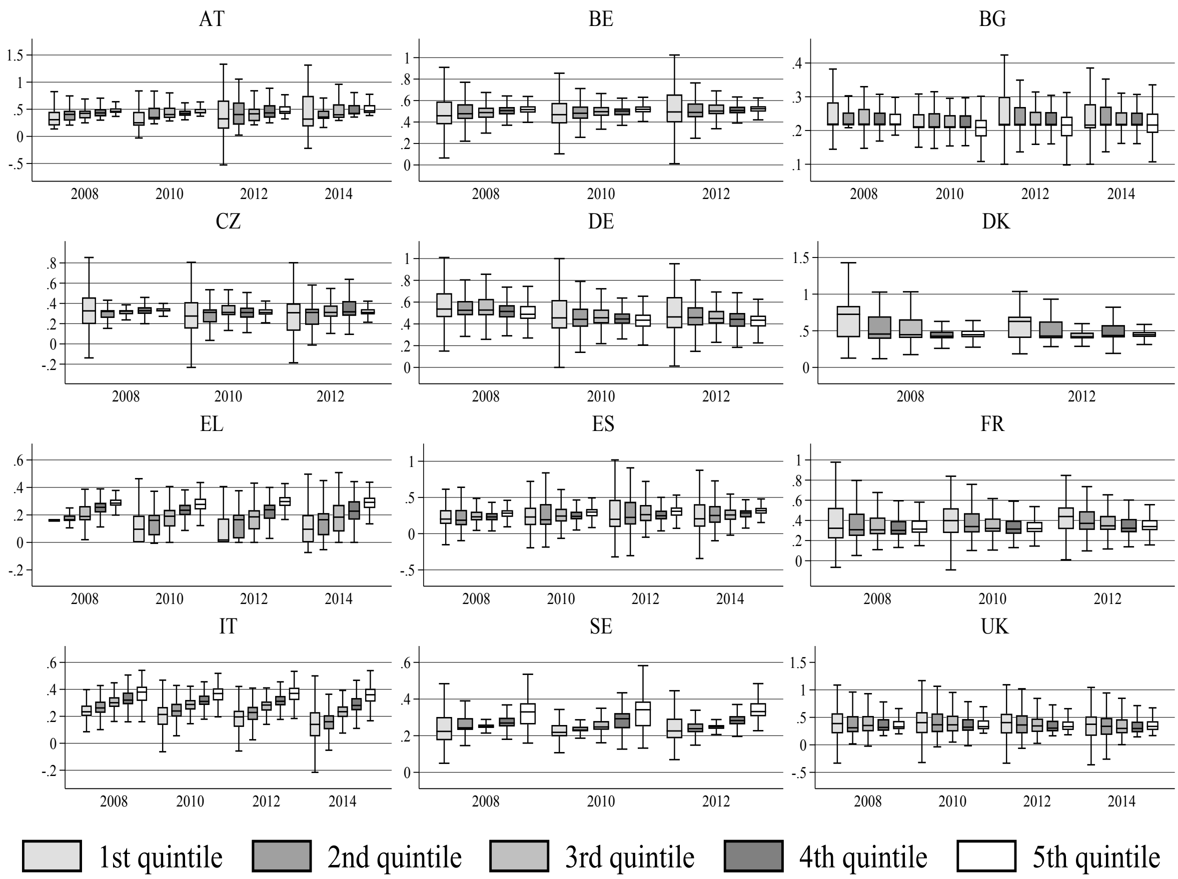
	Employment Rates								Observations			
	Female				Male				All			
	2008	2010	2012	2014	2008	2010	2012	2014	2008	2010	2012	2014
AT	81	84	84	85	97	97	95	97	4990	5232	4997	4511
BE	75	79	79		92	91	90		5415	5052	4723	0
BG	84	86	85	85	92	93	90	89	4222	5683	5032	4254
CZ	81	80	83		97	97	97		9903	7778	7225	0
DE	79	78	82		93	93	94		9608	9424	9180	0
DK	96		97		97		97		5481	0	4520	0
EL	65	65	57	54	94	91	83	78	6253	6354	4835	7550
ES	70	69	64	71	93	84	81	85	13786	14034	12519	11586
FR	85	89	90		94	96	96		9421	9045	9985	0
IT	67	64	64	65	92	89	87	86	20316	18631	18296	17751
SE	95	95	95		97	97	95		6417	5963	5346	0
UK	80	78^b	79	80^c	91	89^b	90	91^c	19319	19167^b	15402	15498^c

Source: EUROMOD data, own calculations. The sample includes individuals aged 25-54 working at least part-time, according to the country-specific part-time norm, excluding the self-employed, students, pensioners, the permanently disabled, those in compulsory military service, and those on parental leave. Rates describe weighted means per country using the EUROMOD sample weights. The sample only includes years for which EUROMOD input data exist. a. based on input 2007 b. based on input 2009 c. based on input 2013.

Figure 3.2 shows varying degrees of dispersion in PTRs across countries by individual earnings quintile. On average, we expect PTRs to increase with earnings in progressive taxation systems as the tax wedge between working and not working increases with potential income. For most of the countries in our sample, we observe increasing median PTRs as we move from the lowest to highest individual potential earnings quintile. This observation lends credence to our concern about an endogenous regressors problem in our structural equation of interest. This effect becomes less pronounced in joint taxation countries like Belgium, France, and Germany because joint assessment of household income lessens the tax burden more on the upper end of the earnings distribution than on the lower end. While Bulgaria has very little variation in median PTRs across the earnings distribution due to a proportional tax rate and relatively insignificant out-of-work benefits, most other country systems show a great deal of dispersion in incentives throughout the earnings distribution.

PTRs are more dispersed in the bottom quintile, reflecting the fact that they consist primarily of single, sole, and secondary earners. While the former may be eligible for means-tested benefits when out of work, the latter most often do not pass the means test for benefit receipt. The highest quintile mostly consists of single and primary earners with high individual labor income, which leads to less dispersed PTRs. Given the significant influence that household structure appears to exert on the size of the individual PTR, in

Figure 3.2: PTR Distributions by Quintile



Source: EUROMOD data, own calculations. Participation tax rates shown on the y-axis.

Table 3.3: Mean Participation Tax Rates by Country and Gender in %, 2008-2014

	Male				Female			
	2008	2010	2012	2014	2008	2010	2012	2014
AT	48	47	52	53	37	39	37	40
BE	51	50	52		50	50	53	
BG	24	23	23	24	25	24	25	25
CZ	33	32	33		33	32	33	
DE	54	47	47		53	47	46	
DK	59		53		54		52	
EL	24	22	21	23	19	15	14	15
ES	25	28	30	30	25	29	31	30
FR	38	38	40		35	36	38	
IT	33	30	28	24	26	25	24	20
SE	33	33	33		30	28	30	
UK	43	43[b]	41	39[c]	34	35[b]	32	30[c]

Source: EUROMOD data, own calculations. Median values weighted using EUROMOD sample weights. a. based on input 2007 b. based on input 2009 c. based on input 2013. The sample includes individuals aged 25-54 working at least 20 hours per week, excluding the self-employed, students, pensioners, the permanently disabled, those in compulsory military service, and those on parental leave.

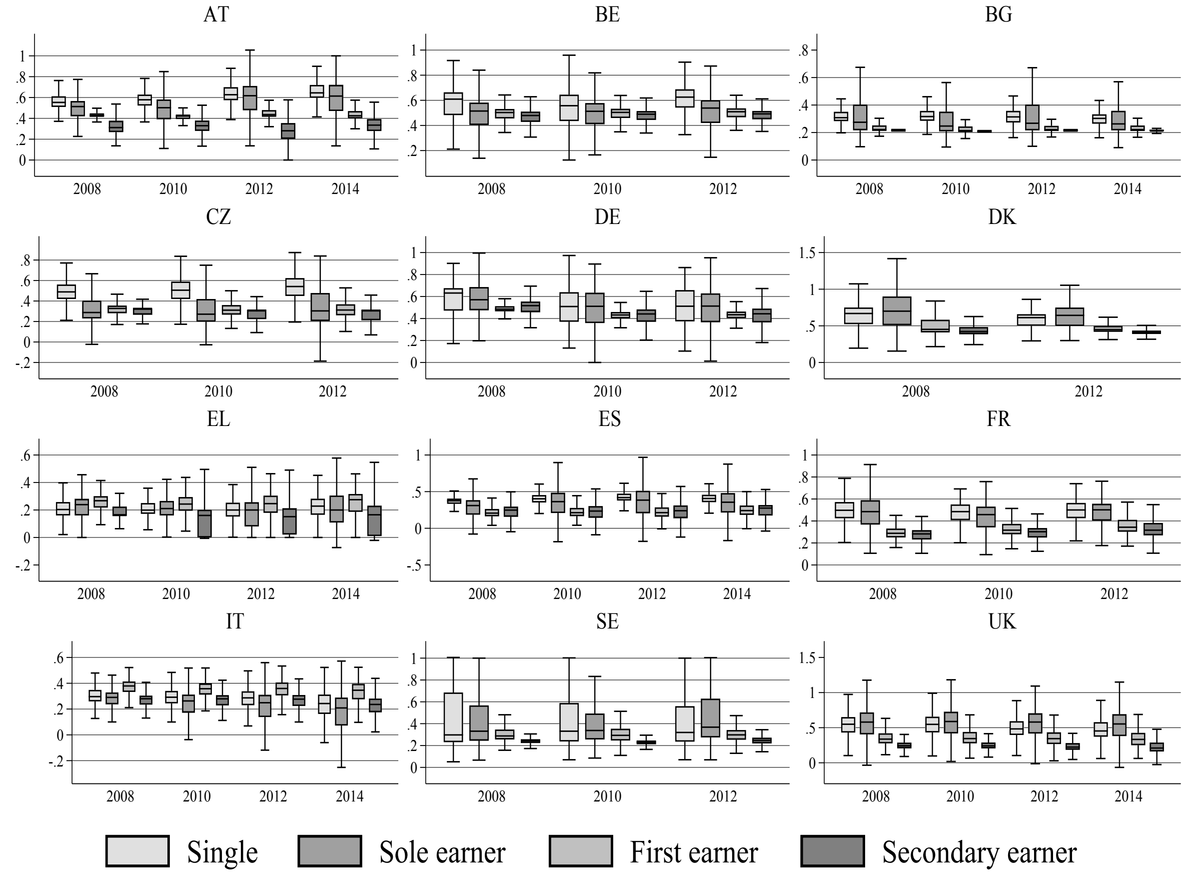
the following we decompose the driving components of the PTR according to household and earner types.

Negative PTRs arise from substantial in-work benefits or earned income tax credits (EITCs) and are especially found at the bottom of the earnings distribution. In most countries, these in-work benefits are either non-existent or small for individuals without children, but generous for working families with children.[12] In Sweden, eligibility for the EITC is independent of the number of children in the household. Belgium, Bulgaria, and Denmark do not have substantial in-work benefits. This finding is of particular interest as some results from optimal tax theory call for a negative PTR at the bottom of the earnings distribution if the extensive elasticity is large (Saez, 2002; Choné and Laroque, 2011; Jacquet et al., 2013; Hansen, 2017).

Figure 3.3 displays the dispersion in PTRs by earner type. We define four stylized earner roles within the household: 1) single earners in a one-person household ("single");

[12] This applies to Austria, the Czech Republic, Germany, Greece, France, Italy, Spain, and the United Kingdom. In Greece, the social dividend was paid in 2014 as a one-time lump-sum payment. In all other years, no substantial in-work credits existed.

Figure 3.3: PTR Distributions by Earner Type

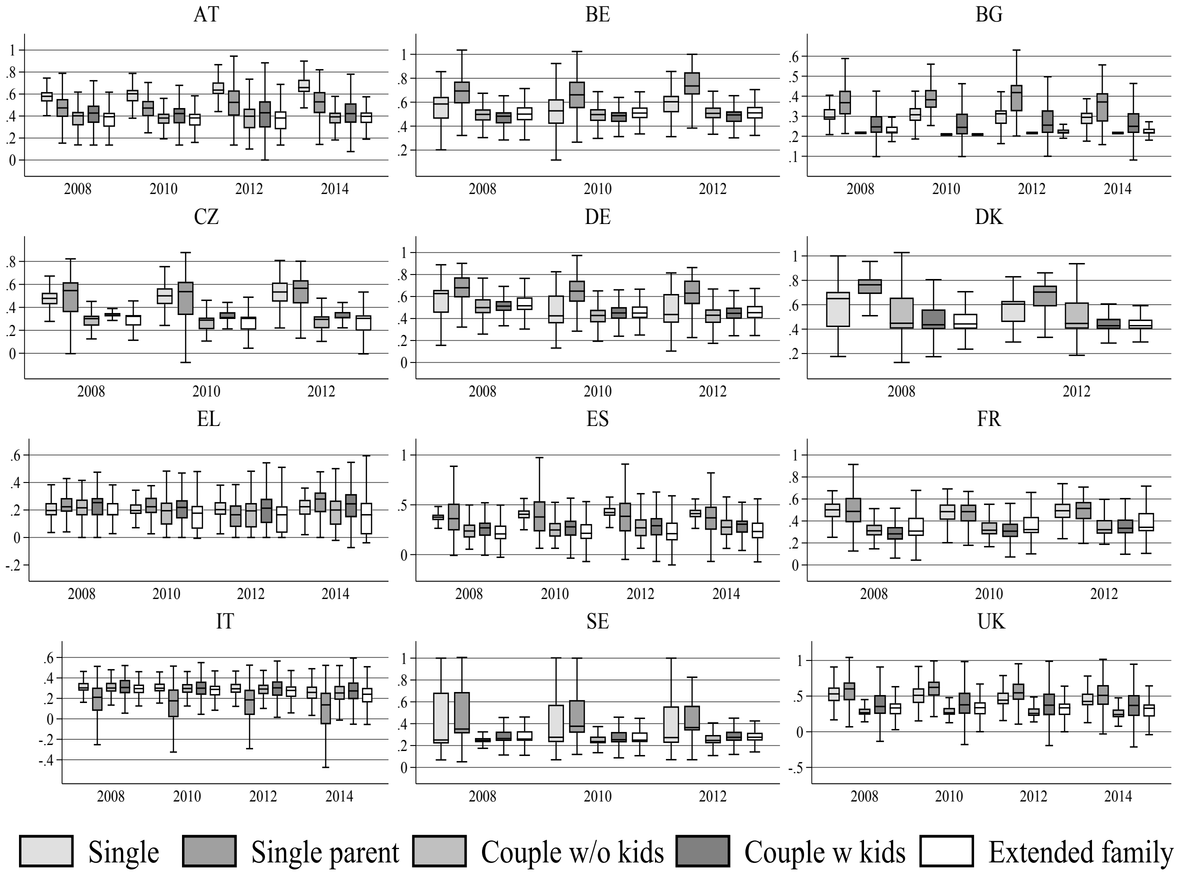


Source: EUROMOD data, own calculations. Participation tax rates shown on the y-axis.

2) sole earners in a multiple-person household (”sole earner”); 3) primary earners in households in which more than one person is employed (”first earner”); and 4) secondary earners in households in which more than one person is employed. The primary earner is the highest earning member of the household. Individual PTRs depend on other household member’s earnings through two channels. First, singles or single earners are more likely to be eligible for means-tested benefits in *nw* than secondary earners. Secondly, single and primary earners face a higher tax wedge between *w* and *nw* than secondary earners. As a result, PTRs are lowest for secondary earners in all countries. We find larger PTRs for primary earners than for secondary earners in all countries, thus corroborating the results of Immervoll et al. (2011). Tax-benefit systems create the highest disincentives for singles in Austria, Bulgaria, the Czech Republic and Spain; for single earners in Belgium, Germany, Denmark, France and Sweden; and for first earners in Greece and Italy.

Figure 3.4 shows that the presence of children in the household has a large effect on the PTR. We distinguish between five stylized household types: 1) single; 2) single parent; 3) couple without children; 4) couple with children; and 5) extended families. Greater

Figure 3.4: PTR Distributions by Household Type



Source: EUROMOD data, own calculations. Participation tax rates shown on the y-axis.

variation can be seen in the PTRs among parents and particularly for single parents in Austria, the Czech Republic, Germany, Spain, France, Italy, and the United Kingdom. This is the effect of two opposing factors. On the one hand, means-tested benefits in *nw* increase with the number of children in the household, which in turn increases the PTR. On the other hand, many countries offer in-work family benefits that increase work incentives and reduce the PTR. The PTR becomes negative for some single parents in the Czech Republic and Italy, as well as for some couples with children in Austria and the United Kingdom. We comment further on these in-work family benefits below.

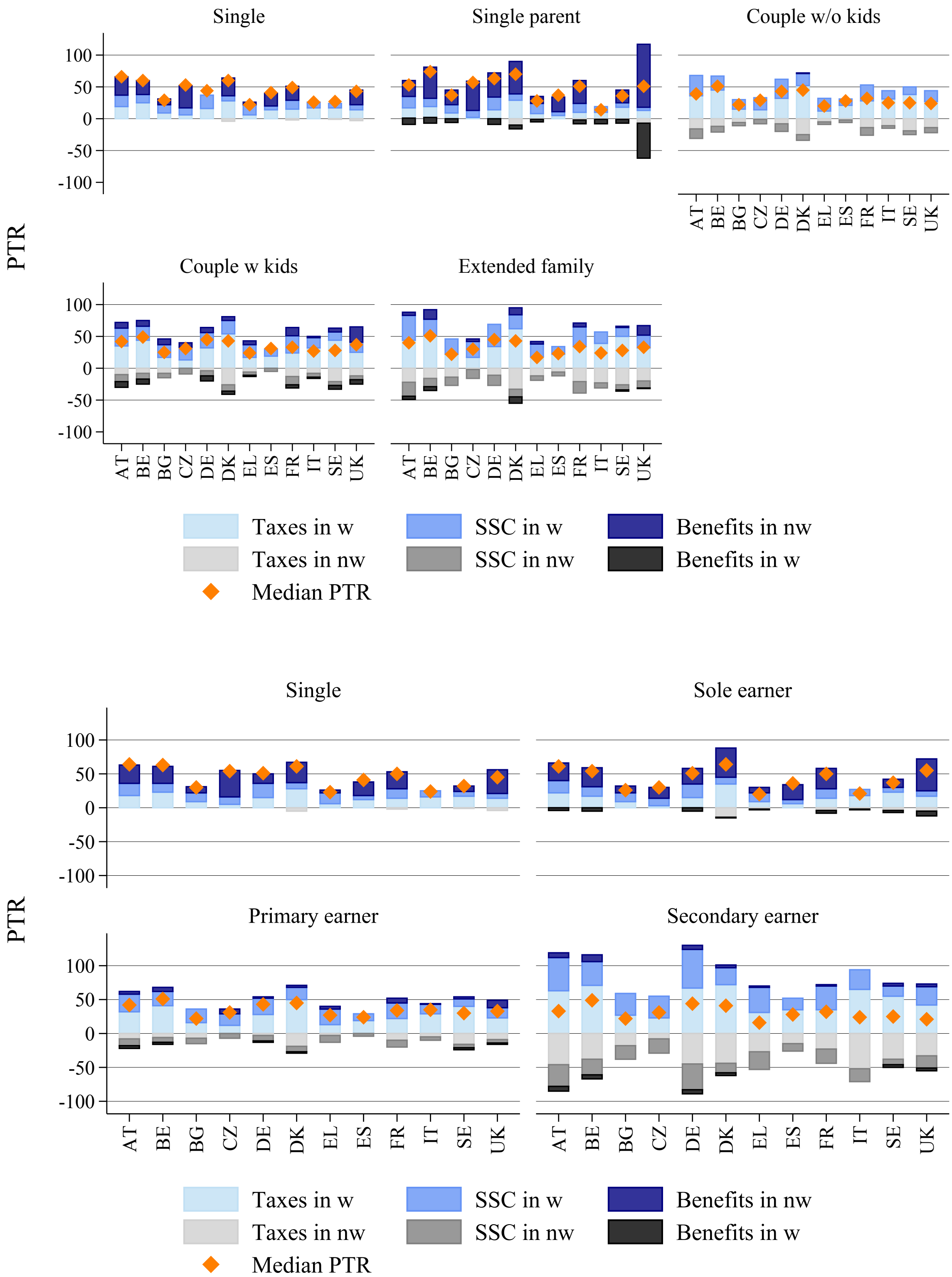
The composition of the PTR by household and earner type for the latest observed year is displayed in Figure 3.3. PTR compositions across all observed country-years are provided in Appendix Figures 3.A13 and 3.A14. The upper part of the figure displays the PTR composition by the five household types and the bottom part displays the PTR composition by the five earner types. The rationale for showing both distinctions lies in the fact that benefits most often depend on household composition and taxes for the household can vary greatly for different earner types across countries. Income taxes as well as social security contributions and benefits are displayed as a share of individual earnings, such that adding up the components results in the individual PTR. Household income taxes and social security contributions when the individual is not working, nw, as well as benefits when employed, w, negatively enter the PTR. Accordingly, this share is denoted below the horizontal axis.

With respect to the household type, three findings are worth discussing. First, out-of-work benefits are high for families, most noticeably for single parents. Second, in-work-benefits are also high for families. Work-related child benefits are granted in Austria (*Kinderbetreuungsgeld*), Belgium (*Basiskinderbijslag*), Italy (*assegni familiari*), Greece (*koinonikó mérisma*), Germany (*Kinderzuschlag*) and Spain (*mínimo por descendientes*), which can create negative PTRs for low-income earners. Similarly, working tax credits and child tax credits that include a partial childcare cost compensation for working parents exist in the United Kingdom and comprise a substantial incentive to work. In France, low-income workers receive in-work payments in addition to the social assistance received by non-workers (*Revenue de solidarité active, RSA*). This benefit is more generous for families than for households without children, as the lump-sum depends on the number of dependent children.[13] EITCs for single earners, on the other hand, while prevalent in some countries, are often negligible compared to the in-work benefits for families. Third, the tax wedge between working and not working is lower for couples than for singles regardless of the presence of children. This tax wedge, however, varies according to the individual's earner role within the household, as demonstrated in the bottom half of Figure 3.3.

In the context of earner types within the household, three findings with regard to individual incentives merit discussion. First, household income taxes and social security contributions as a share of individual earnings are particularly high for secondary earners in Austria, Belgium, Denmark, Germany, and Italy. If the labor income of secondary earners represents only a small portion of overall household income, the household's income tax changes only marginally between working and not working for secondary earner. In contrast, household's income tax changes substantially between working and not working for single earners. Second, only single, sole-earner and no-earner households receive substantial out-of-work transfers, while individuals in two-earner households are mostly not eligible. Third, large differences result from the variation of tax-benefit systems across countries. While countries like Denmark, Germany, France, and the United Kingdom provide generous income support to the unemployed, countries like Bulgaria and Greece only offer small or no benefits.

[13] Additionally in France, the means test for receipt of the family complement benefit (*Complément familial*) is measured against a higher eligibility threshold for households in which two earners, rather than one, are working. In 2016, a separate in-work benefit, *Prime d'activité*, was introduced to replace this system for low income earners.

Figure 3.5: PTR Compositions by Household and Earner Type



Source: EUROMOD data, own calculations.
Note: Latest observed year per country, i.e. 2012, 2013 or 2014.

Beyond the household context, tax-benefit systems differentially affect individual incentives depending on the level of their earnings. Because individuals with a weak attachment to the labor force on average exhibit low potential earnings and high extensive margin responses to incentives, Table 3.4 displays indicators of tax and benefit receipt for individuals in the bottom quintile of the earnings distribution in both possible labor states: 1) the share of benefit receipt and tax payment (% receiving/paying in w/nw); and 2) the level of taxes paid and benefits received, proportional to the bottom quintile's earnings threshold (Ratio in w/nw).

The share of benefit receipt in w and nw varies greatly across countries. In contrast, the ratio of benefits received in w, conditional on receipt, does not exceed one-fifth of the bottom quintile's earnings threshold in most countries. When these individuals do not work, the ratio of benefits to the bottom quintile's earnings threshold increases to 40-50% in Austria, Germany, Spain, and the United Kingdom; to almost 30% in France, Belgium and Denmark; and 20% or less in Bulgaria, Czech Republic, Greece, Italy, and Sweden. The difference in generosity of benefits in w and nw is highest in Austria, Belgium, Germany, Denmark, Spain, and France, indicating lower work incentives for low-income workers in comparison to countries with a small differential.

In most of the countries in our sample, almost all workers in the bottom quintile of the earnings distribution pay taxes. Only in Greece, Spain and the Czech Republic, do high tax allowances lead to roughly 12-22% of low income workers being exempt from paying taxes on their earnings. The ratio of the tax (including SIC) burden to the bottom quintile's earnings threshold, conditional on being positive, is lower than 10% in Spain, the Czech Republic and Italy, slightly higher than 10% in Austria, Belgium and Greece and between ca. 15-30% in Germany, Denmark, France, Sweden and the United Kingdom.

Table 3.4: Tax and Benefit Incentives for Bottom Quintile

	Benefits				Taxes	
	% receiving in w	Ratio in w	% receiving in nw	Ratio in nw	% paying in w	Ratio in w
AT	18.5	23.0	17.0	47.8	98.8	11.6
BE	16.6	10.0	41.1	26.0	98.3	17.4
BG	23.1	19.5	16.0	20.9	100.0	14.9
CZ	23.8	11.6	31.3	20.3	84.5	10.3
DE	17.1	19.5	19.3	41.2	98.8	28.1
DK	63.9	1.9	89.0	41.6	99.5	28.8
EL	8.9	21.9	7.1	20.4	63.4	11.0
ES	20.1	10.9	15.3	44.8	68.1	5.4
FR	68.3	9.2	62.4	28.0	100.0	18.7
IT	13.8	13.4	0.5	19.5	89.5	9.2
SE	28.7	13.9	50.1	28.0	98.6	17.0
UK	31.5	45.6	36.4	62.8	97.7	13.7

Source: EUROMOD data, own calculations. Median values weighted using EU–SILC sample weights. Ratio refers to median benefits or taxes (including social security contributions), respectively, as a share of bottom quintile's earnings. a. based on input 2007 b. based on input 2009 c. based on input. The sample includes individuals aged 25-54 working at least 20 hours per week, excluding students, pensioners, the permanently disabled, those in compulsory military service, and those on parental leave.

3.4 Results

3.4.1 Estimates

Results include data for 12 European countries that represent a variety of welfare state systems. Regression results for Equation 4 are presented in Table 3.5. This table juxtaposes results from the OLS estimation of Equation 3.4 that treats the PTR as exogenous to the probability of employment (columns (1)-(2)) to those of the 2SLS estimation using the group IV of Equation 3.6 (columns (3)-(5)). For the group IV, groups are defined as 5-year age cohorts and three categories of educational attainment. Adding control variables for demographic factors that potentially influence labor supply decisions has a stronger effect in the OLS estimation than in the group IV, as the latter implicitly controls for education and age groups already in the baseline specification. Column (5) displays results including an additional interaction term of net-of-PTR earnings with the female dummy and confirms previous findings in the literature that, on average, women respond more than men to monetary incentives. Further results exhibit the expected signs: higher education and being male are associated with a higher employment probability whereas the presence of children and marital status negatively impact employment probabilities when averaging over men and women. Despite similar results, the group IV with full controls (column (4)) presents our preferred specification, as it carefully controls for the endogeneity of earnings to the labor supply decision as well as the PTR being a function of earnings.

In accordance with economic theory that suggests an increase in work incentives yields an increased probability of gainful employment, we find a strong, positive effect of net-of-PTR earnings on employment probability. The estimates of the group IV yield higher participation responses to changes in the net-of-PTR earnings than the OLS regressions, thus indicating a downward OLS bias. The high first stage Anderson-Rubin statistic, which tests the null-hypothesis of weak instruments, lends credence to the use of the group IV as a strong instrument for individual net-of-PTR earnings.[14]

Results for group IV prove rather robust to the definition of the group both in magnitude and direction of the effect. Appendix Table 3.A1 displays these results from the alternative definition, which includes 10-year age cohorts, three educational attainment levels, and gender.[15] Our preferred specification from Table 3.5 column (3) and (5) (with controls) is robust to alternative clustering strategies, as shown in Appendix Table 3.A2. Further, results prove robust to specifications that allow for country-specific, gender-specific and age-specific time trends, as shown in Appendix Table 3.A3.

[14]We calculate this statistic from Finlay et al. (2016), which allows for cluster robust inference.

[15]Our two alternative group definitions follow the two studies of Burns and Ziliak (2015) and Jäntti et al. (2015), which both applied a group IV in order to estimate hours and participation elasticities.

Table 3.5: Regression Results for Pooled OLS and Group IV

	OLS		Group IV		
	baseline	controls	baseline	controls	
	(1)	(2)	(3)	(4)	(5)
(1-PTR)*e	0.160***	0.039***	0.114***	0.091***	0.053***
	(0.001)	(0.001)	(0.008)	(0.007)	(0.006)
(1-PTR)*e*female					0.090***
					(0.004)
Male		0.075***		0.010**	0.005
		(0.001)		(0.005)	(0.005)
Upper Secondary		0.090***			
		(0.002)			
Tertiary		0.147***			
		(0.002)			
Married		-0.051***		-0.012***	-0.001
		(0.001)		(0.002)	(0.002)
Hh. non-labor income		0.005***		0.003***	0.003***
		(0.001)		(0.001)	(0.001)
Child 1-3		-0.033***		-0.064***	-0.057***
		(0.002)		(0.003)	(0.003)
Child 4-6		-0.013***		-0.035***	-0.031***
		(0.002)		(0.002)	(0.002)
Child 7-17		-0.037***		-0.019***	-0.016***
		(0.001)		(0.002)	(0.002)
Experience		0.036***		0.043***	0.042***
		(0.000)		(0.000)	(0.000)
Experience squared		-0.001***		-0.001***	-0.001***
		(0.000)		(0.000)	(0.000)
Constant	0.836***	0.481***	0.850***	0.568***	0.581***
	(0.001)	(0.004)	(0.009)	(0.011)	(0.011)
Adj. R-squared	0.081	0.239			
First-stage AR-statistic			220.14***	181.13***	480.75***
N	355,793	355,793	355,793	355,793	355,793

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: EUROMOD data, own calculations. The sample includes prime working-aged individuals aged 25-54, excluding students, pensioners, the permanently disabled, and those in compulsory military service. Standard errors are heteroskedasticity robust and are corrected for generated variables bias. All equations include both year and country fixed effects. The omitted education category is lower secondary education. Groups are defined as 5-year age cohorts and three educational attainment groups, following Burns and Ziliak (2015). We define groups within each European country. All regressions are estimated with 2SLS, instrumenting the individual-specific net-of-PTR earnings with the group average in each year and country.

3.4.2 Participation Elasticities

Given the different institutional settings, social norms and tastes for work and leisure across European countries, it is reasonable to expect participation elasticities to vary across countries, gender and earner types. From the marginal effects of the regression in Equation 3.6, we calculate the static, within-period participation elasticity according to Equation 3.2.

Figure 3.6 captures these country-specific elasticities estimated in Equation 3.6 by country for men and women separately. An overall country-specific pattern is observable across gender, in which higher elasticities for men in one country compared to another generally translate into higher relative female elasticities as well. Participation elasticities are high in Belgium, Germany, Greece, Spain, Italy, and Sweden, while they are low and not statistically different from zero in Bulgaria, France, and the United Kingdom. In these countries, especially Bulgaria and France, labor market participation is already high (see Table 3.2), leaving few individuals on the margin between participating and not participating in the labor force. Perhaps even more striking than the difference in the size of the estimates for men and women is the large dispersion in the elasticities among women, indicating substantial heterogeneity that is not captured by gender alone.[16] Our findings indicate an EU-average elasticity of 0.08 for men and of 0.15 for women.[17]

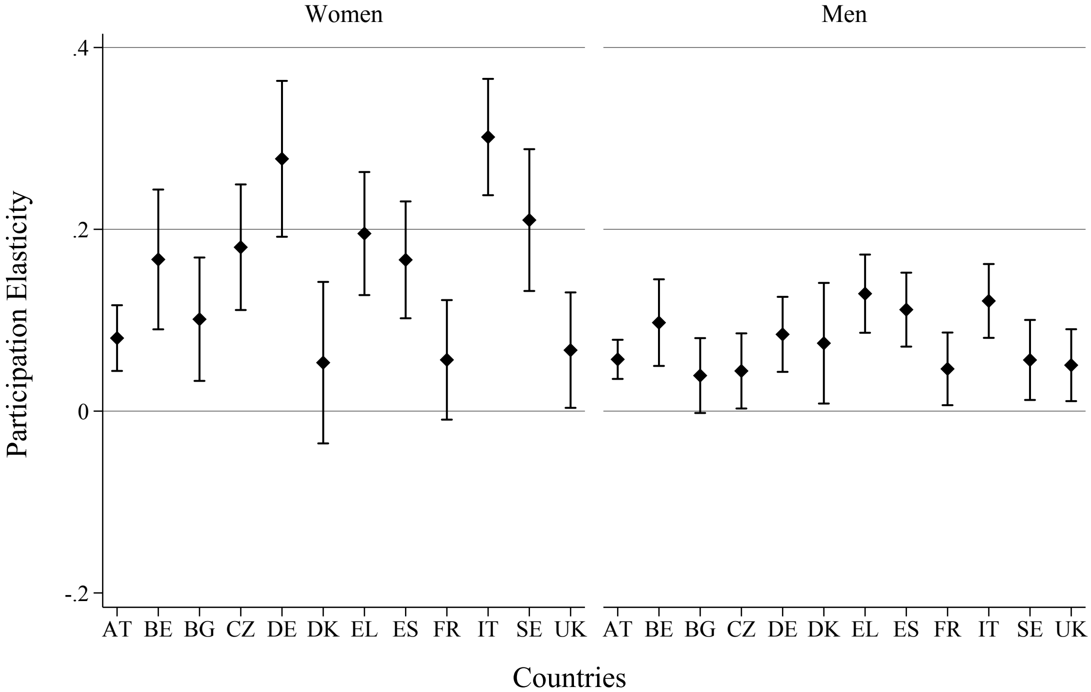
Beyond identifying average male and female responses to work incentives, in the second step of our analysis, we further disaggregate the impact of these disperse PTRs by earner roles within the household. Figures 3.7 to 3.10 display elasticities for men and women, respectively, according to their potential earner role within the household when in labor state w. These figures reveal that men and women respond similarly if compared within the same household earner role. This result corroborates Blau and Kahn (2006) who find that women's labor supply elasticities approached men's in the US from 1980 to 2000 as the traditional division of labor broke down. Our results offer first reduced-form evidence that this closing gap also can be observed in the European context in many but not all countries.

Single and primary earners' elasticities are indistinguishable from zero irrespective of gender. Only primary male earners in Greece and Spain have small, but statistically significant elasticities. Male sole earners in Austria, Greece, Spain and Italy show statistically significant responses to monetary incentives to work in the order of ca. 0.1. In contrast, both male and female secondary earners were the most responsive in terms of

[16] This finding corroborates work by Bastani et al. (2017), who estimate PTRs by skill level and emphasize the importance of providing heterogeneous estimates to be used in the calibration of structural models.

[17] These averages are weighted using the individual EUROMOD sample weights.

Figure 3.6: Participation Elasticities by Gender

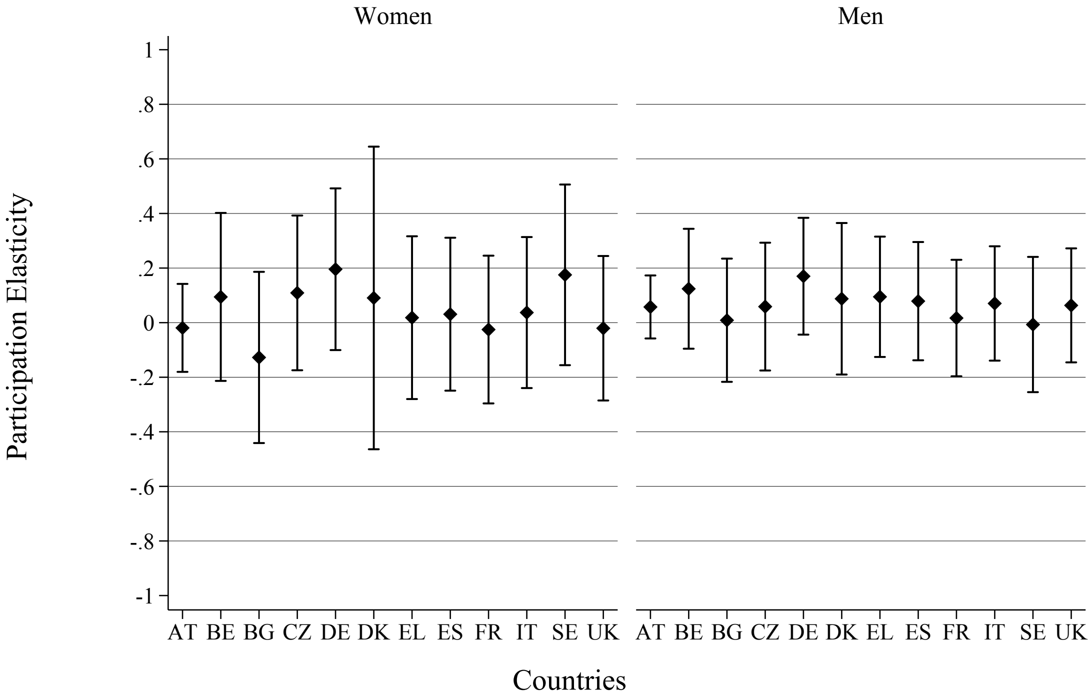


Source: EUROMOD cross-sectional data and microsimulation, own calculations.
Note: Vertical lines show cluster robust confidence intervals at the 95%-level.

size and significance of their respective elasticities, although more variance exists among male secondary earners than for female secondary earners. These results demonstrate that difference between female and male elasticities are much greater on average, than within earner types. A closer consideration of what drives this behavior uncovers the importance of the specific earner role that the individual plays within the household.

In a final disaggregated analysis, we tie our results into work by Aghion et al. (2017) and Abeler and Jäger (2015) on the importance of salience in determining responses to changes in tax-benefit systems. In the following, we consider the extent to which individuals react differentially to the three main components of the PTR: taxes, social insurance contributions, and benefits. Just as we defined the PTR as the household's tax wedge between w and nw, it is possible to break this term down into the wedge for taxes, SIC, and benefits before formulating the net-of-tax earnings from each of these wedges: $(1 - \frac{tax}{e}) * e$ for taxes, $(1 - \frac{SIC}{e}) * e$ for SIC, and $(1 - \frac{ben}{e}) * e$ for benefits. The expected direction of the effect is the same as for the entire net-of-PTR earnings term, but the reaction of individuals to each of these components could vary according to differences in the salience of taxes, SICs or benefits.[18]

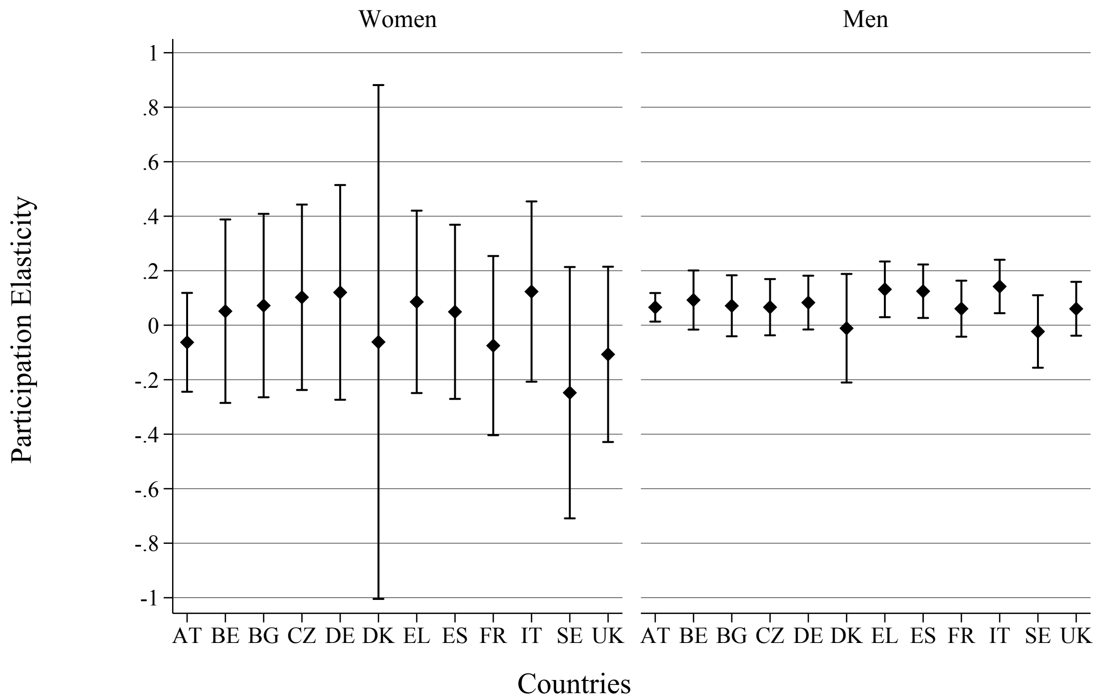
Figure 3.7: Participation Elasticities for Single Households



Source: EUROMOD cross-sectional data and microsimulation, own calculations.
Note: Vertical lines show cluster robust confidence intervals at the 95%-level.

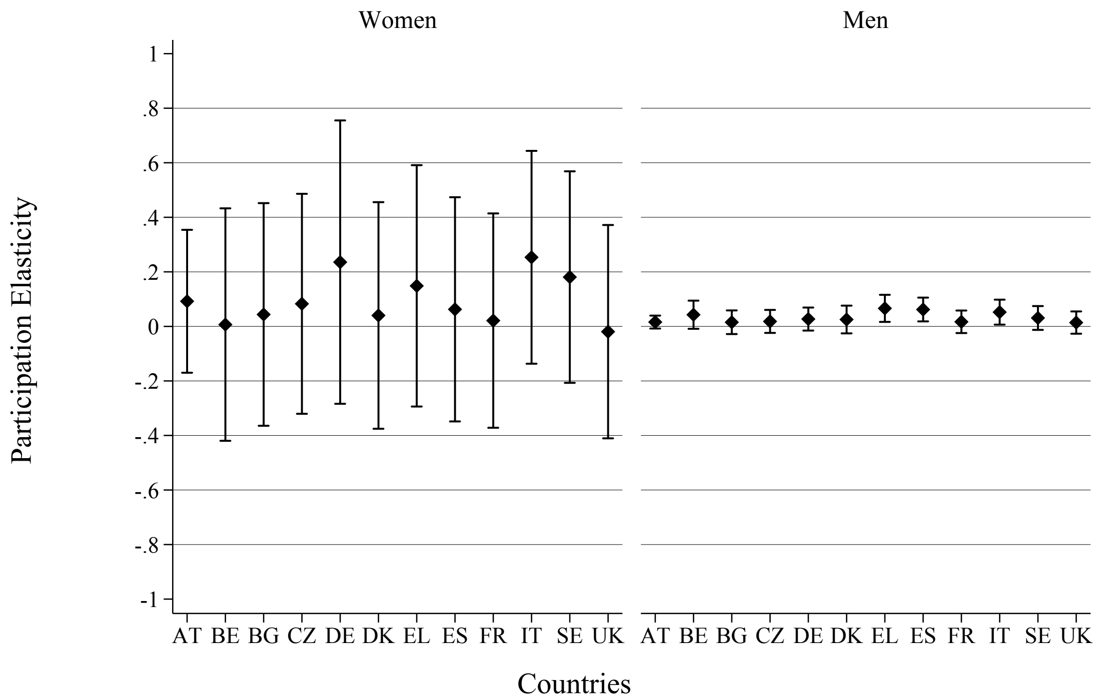
[18]The tax and SIC wedges are defined as $\frac{tax^w - tax^{nw}}{e}$ and $\frac{SIC^w - SIC^{nw}}{e}$, respectively, whereas the benefit wedge, generally larger in the state of nw than in w, is defined as $\frac{ben^{nw} - ben^w}{e}$.

Figure 3.8: Participation Elasticities for Sole Earners



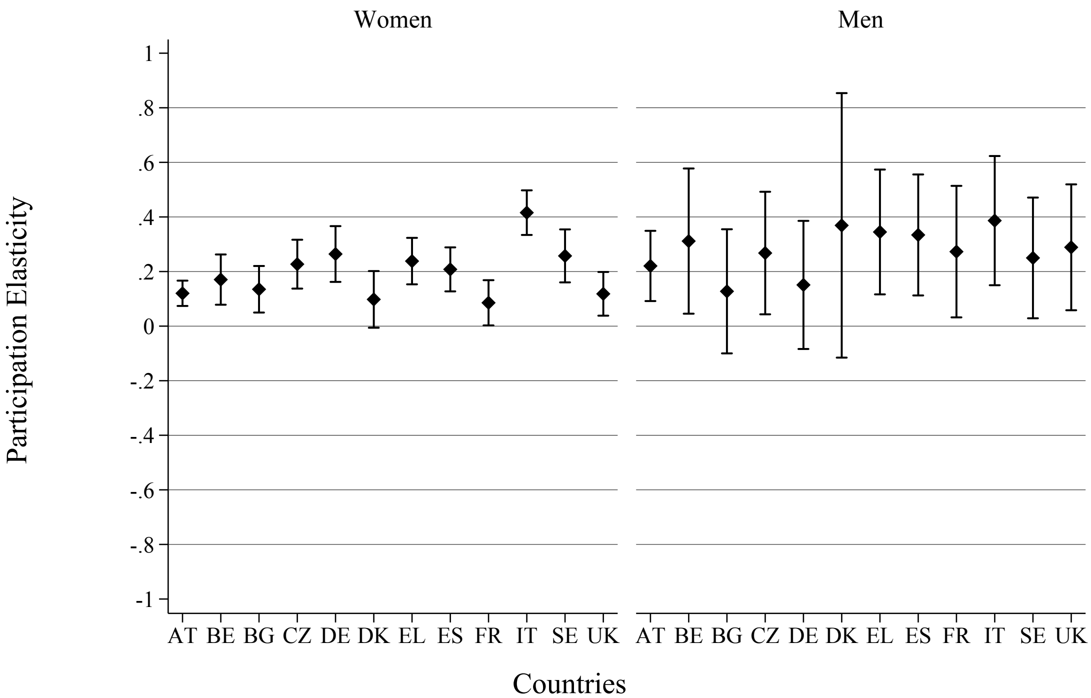
Source: EUROMOD data, own calculations.
Note: Vertical lines show cluster robust confidence intervals at the 95%-level.

Figure 3.9: Participation Elasticities for Primary Earners



Source: EUROMOD data, own calculations.
Note: Vertical lines show cluster robust confidence intervals at the 95%-level.

Figure 3.10: Participation Elasticities for Secondary Earners



Source: EUROMOD data, own calculations.
Note: Vertical lines show cluster robust confidence intervals at the 95%-level.

Following results from the previous section, which demonstrate significant elasticities almost exclusively for secondary earners, Table 3.6 displays average elasticities for this earner type in each country with respect to these separate components. Social insurance contributions systematically generate the highest response for this group, while benefits comparably exert the smallest effect across countries, despite the common focus on benefits in the public debate.

Table 3.6: Participation elasticity by PTR component

	Income tax		Soc. ins. contributions		Benefits	
	Elasticity	SE	Elasticity	SE	Elasticity	SE
AT	0.135	0.038	0.170	0.044	0.107	0.030
BE	0.193	0.081	0.215	0.087	0.197	0.076
BG	0.131	0.065	0.138	0.073	0.122	0.071
CZ	0.177	0.061	0.191	0.070	0.164	0.057
DE	0.260	0.077	0.319	0.082	0.278	0.067
DK	0.123	0.081	0.134	0.088	0.104	0.083
EL	0.211	0.059	0.229	0.068	0.206	0.052
ES	0.216	0.065	0.233	0.075	0.204	0.054
FR	0.108	0.064	0.120	0.072	0.100	0.060
IT	0.336	0.067	0.399	0.075	0.363	0.055
SE	0.182	0.104	0.216	0.105	0.195	0.097
UK	0.153	0.073	0.165	0.077	0.136	0.069

Source: EUROMOD data, own calculations. The sample includes prime working-aged individuals aged 25-54, excluding the self-employed, students, pensioners, the permanently disabled, and those in compulsory military service. Standard errors are clustered at the group level.

3.5 Conclusion

In this paper, we compute Participation Tax Rates (PTRs) across the EU as a comprehensive measure of work disincentives inherent in tax-benefit systems. We find varying degrees of disincentives that were larger on average for men and that increase with gross individual earnings, which is related to the progressivity of most European tax-benefit systems. Throughout the period under investigation, large disparities between countries persisted, but remained relatively constant across time despite several individual reforms.

Disentangling the drivers of the PTRs, we find that the relative importance of taxes, social insurance contributions and benefits largely depends on household composition and the individual's earner role within the household. Tax-benefit systems create the highest disincentives for singles in Austria, Bulgaria, the Czech Republic, and Spain; for sole earners in Belgium, Germany, Denmark, France and Sweden; as well as for first earners in Greece and Italy. Across European countries, PTRs are lowest for secondary earners. High PTRs for singles, sole earners, and those observed not working are the result of substantial out-of-work benefits in Denmark, France, Germany, and the United Kingdom, while out-of-work benefits are very small or even non-existent in Bulgaria, Greece, and Italy. Comparably higher PTRs for secondary earners in Austria, Belgium, Denmark, Germany, and Italy are the result of a high tax and social insurance contribution wedge between participation and non-participation.

Negative PTRs arise in several countries for working families with children at the bottom of the earnings distribution from substantial in-work benefits or earned income tax credits (EITCs). More precisely, work incentives are upwardly distorted for single parents and single earners in the Czech Republic and Italy as well as for couples with children (single earner or first earner) in Austria and the United Kingdom. This finding is of particular interest as optimal tax theory shows negative PTRs can be optimal at the bottom of the earnings distribution for one-earner households as well as for families if the social weight placed on this group is sufficiently high (Saez, 2002; Immervoll et al., 2011; Choné and Laroque, 2011; Jacquet et al., 2013; Hansen, 2017). While two-earner households benefit from economies of scale, childcare costs for parents of small children create higher fixed costs associated with working, which may suggest a lower optimal PTR in comparison to childless households. The present paper empirically documents the widespread existence of negative PTRs as a result of means-tested in-work benefits for some countries and earner types. In contrast, in-work benefits for individuals without children are either non-existent or small in most European countries. Only in Sweden is eligibility for the EITC independent of the number of children in the household, which could be rendered unnecessary due to the general availability of publicly provided childcare. Belgium, Bulgaria, and Denmark do not have substantial in-work benefits.

A reform reducing the PTR of a particular group only increases efficiency if participation elasticities of this group are sufficiently high. In the second step of our analysis, we identify the impact of the disperse PTRs on labor supply and estimated marginal effects on an aggregate level as well as by country, gender and earner roles within the household. We find an average participation elasticity of 0.08 for men and of 0.15 for women, as well as a high degree of heterogeneity across countries. Countries with high extensive margin responses include: Belgium, Germany, Greece, Italy, Spain, and Sweden.

Gender turns out not to be the characteristic that best predicts individual responses to monetary incentives for work. A further analysis reveals that men and women respond similarly if compared within the same household earner role. Typically, both male and female primary earners, sole earners, and singles show elasticities indistinguishable from zero. In contrast, both male and female secondary earners were the most responsive in terms of size and significance of their respective elasticities. Participation elasticities of male and female secondary earners are mostly between 0.1 and 0.4. In a final step, we show that among the three components of the PTR – taxes, social security contributions, and benefits – social security contributions elicit the strongest response among secondary earners.

Our average estimates corroborate the smaller participation elasticities found by other studies that likewise compute reduced-form participation elasticities across countries based on incremental changes to tax-benefit incentives, namely Jäntti et al. (2015) and Kalíšková (2018). Jäntti et al. (2015) find a range of elasticities, mostly between 0-0.2, with statistically insignificant results in many countries. Kalíšková (2018) estimates an average female participation elasticity of 0.08 between 2005-2010 for an EU-wide sample of women from 26 countries. Our results – estimated on the basis of cross-country data, the full prime working-aged population and the tax-benefit system as a whole – demonstrate smaller participation elasticities when compared to existing studies using quasi-experimental settings, mainly using US and UK data. On average, quasi-experimental studies reviewed by Chetty et al. (2013) find a participation elasticity of 0.28 and estimates range from 0.13 to 0.43, which corresponds to the magnitude of our estimates found only for the most responsive group of secondary earners. This discrepancy could be explained by the use of large and intensively discussed reforms such as the introduction of the EITC in the US, which cause disproportionately high reactions in the target group. Smaller behavioral responses imply that government policies may have a less distortionary effect on labor supply in the short run than existing studies suggest.

Taken together, our findings demonstrate the importance of using more heterogeneous participation elasticities when calibrating structural labor supply models and/or predicting welfare effects from simulating tax-benefit reforms. Elasticities calculated on the basis of country-specific case studies may not broadly apply across socioeconomic groups and the entire working-aged population. In particular, our analysis shows the central role of the individual's earner position within the household context. Secondly, larger estimates of quasi-experimental studies are likely more relevant for large, salient reforms, while smaller estimates, such as those found in this study, prove more accurate for incremental changes to the tax-benefit system in the short run.

3.6 Appendix

Figure 3.A1: PTR by Age Group and Education Level, AT

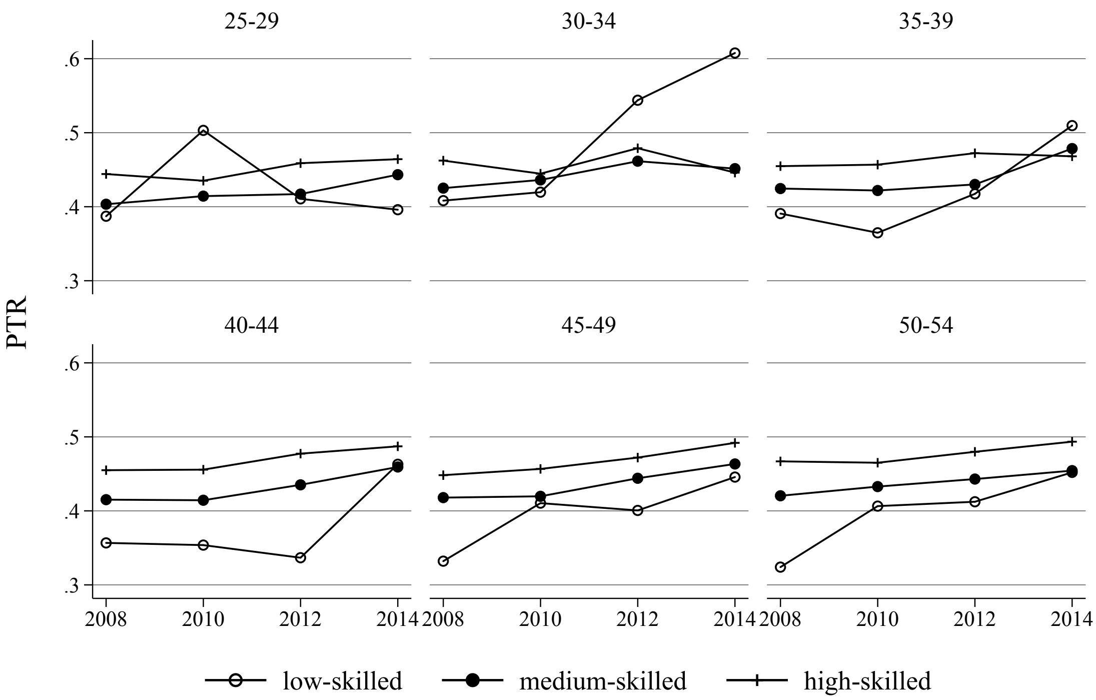


Figure 3.A2: PTR by Age Group and Education Level, BE

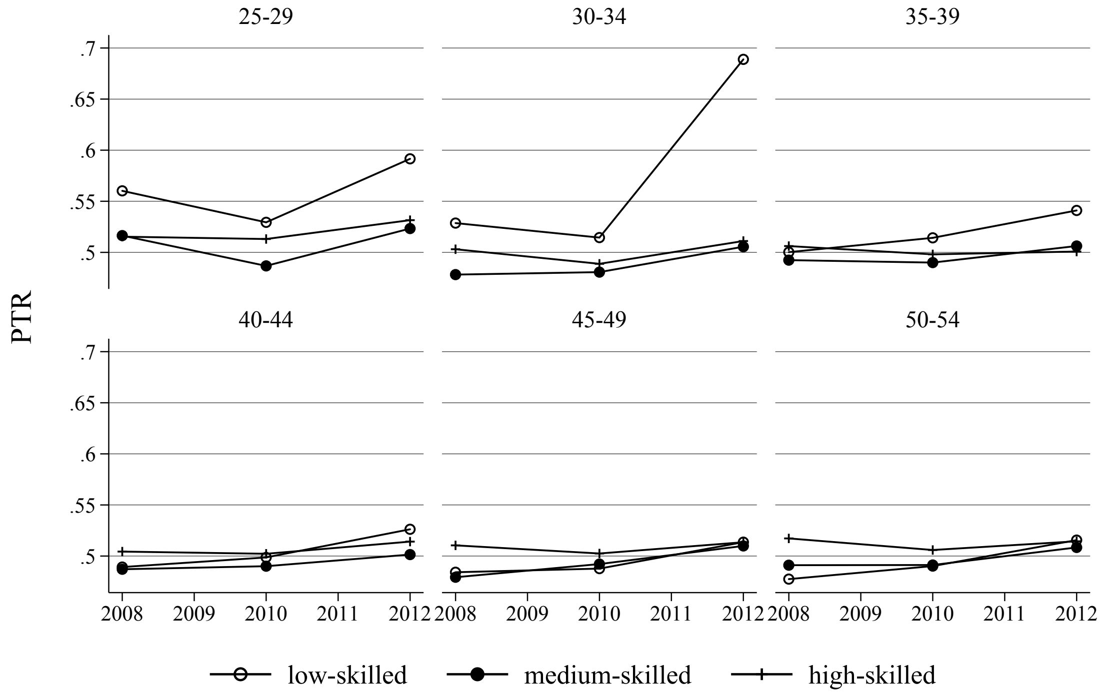


Figure 3.A3: PTR by Age Group and Education Level, BG

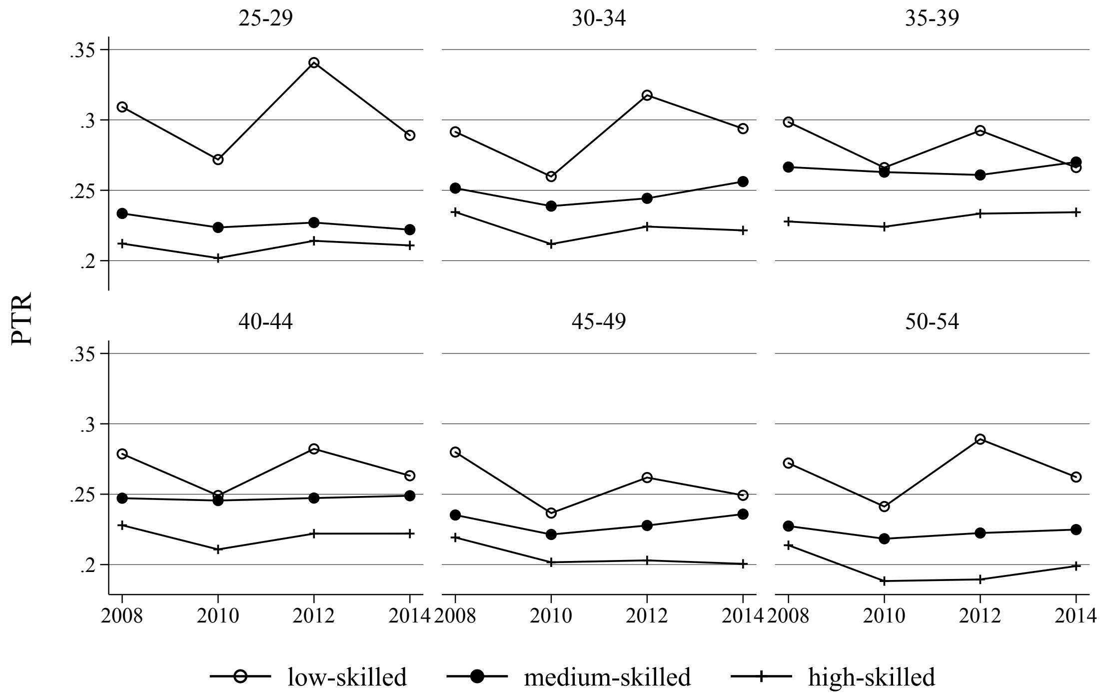


Figure 3.A4: PTR by Age Group and Education Level, CZ

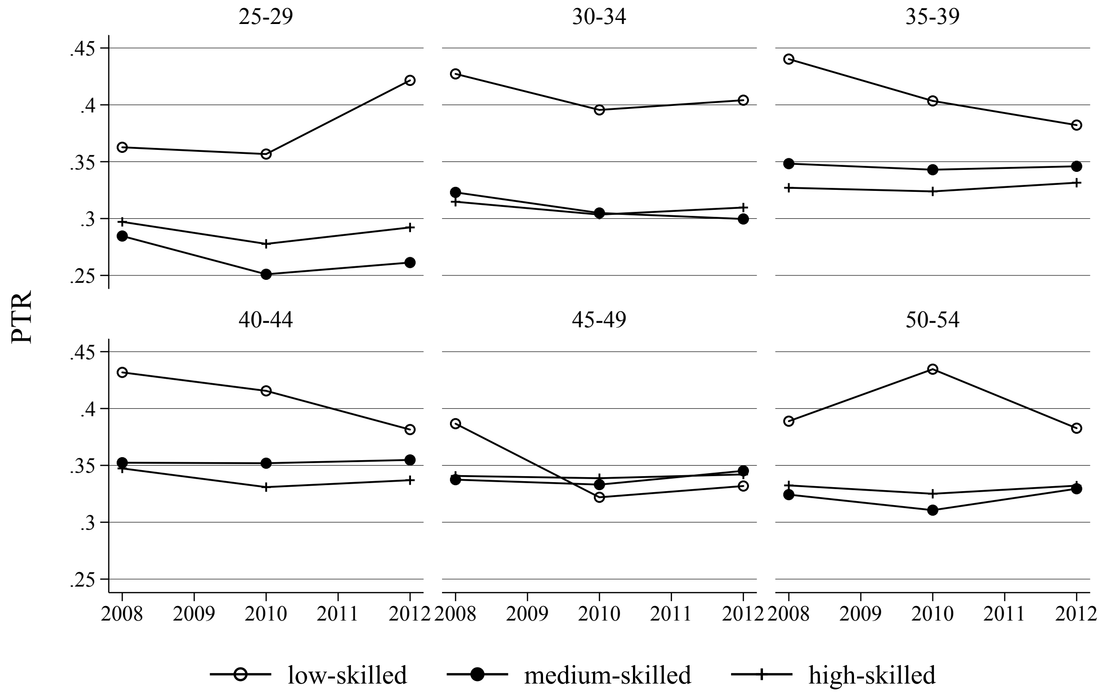


Figure 3.A5: PTR by Age Group and Education Level, DE

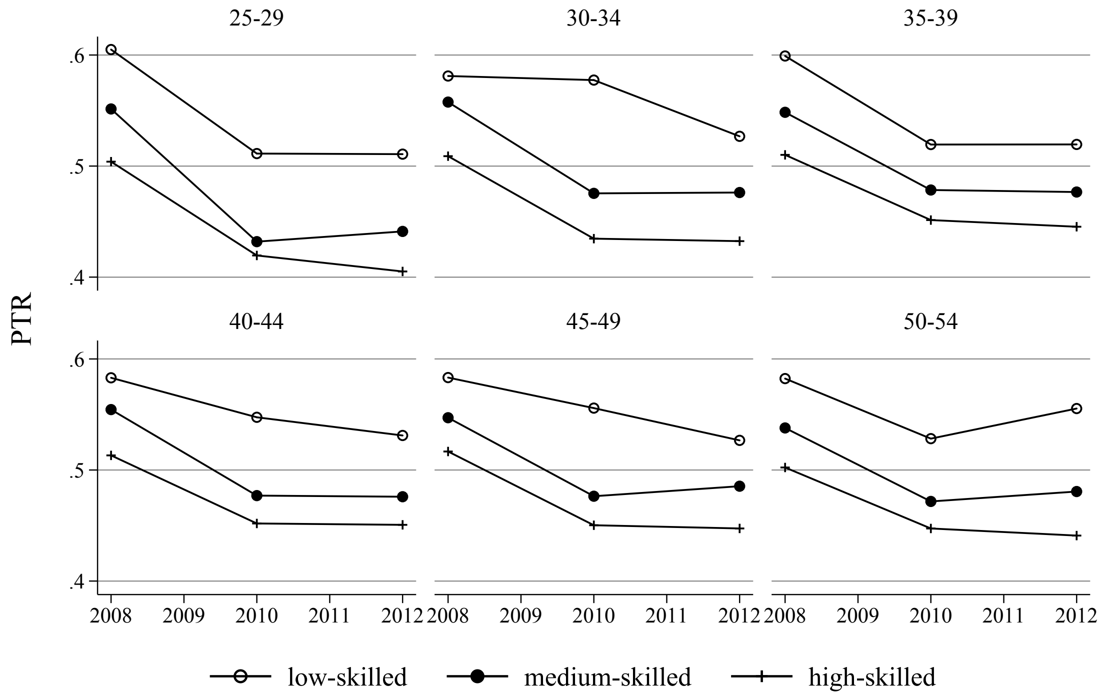


Figure 3.A6: PTR by Age Group and Education Level, DK

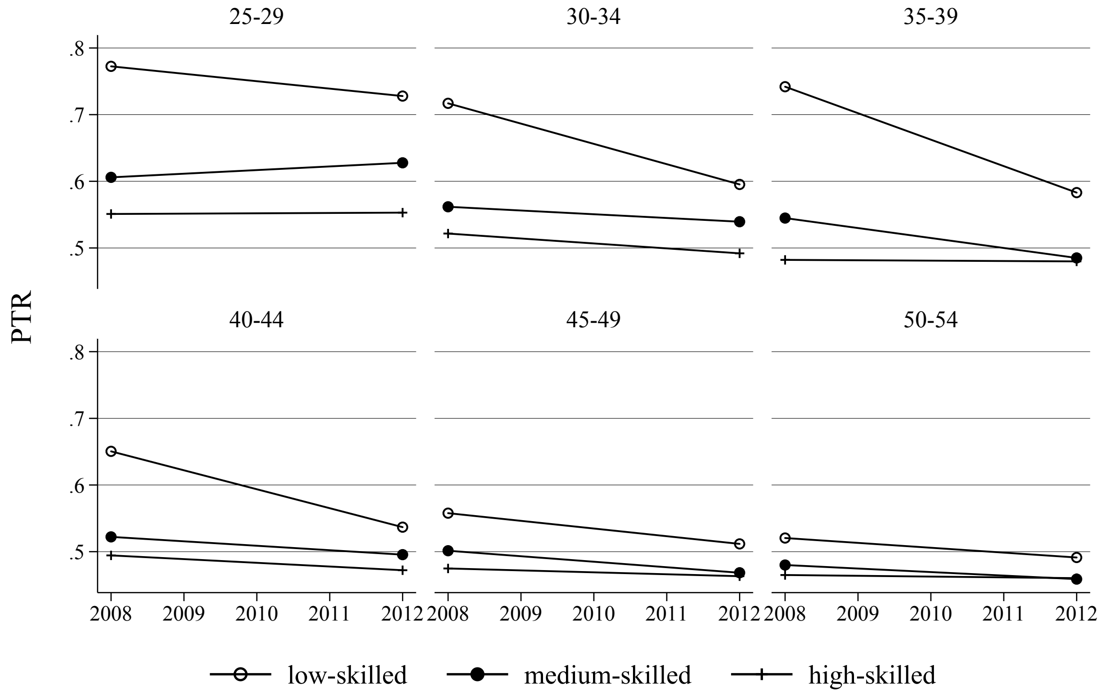


Figure 3.A7: PTR by Age Group and Education Level, EL

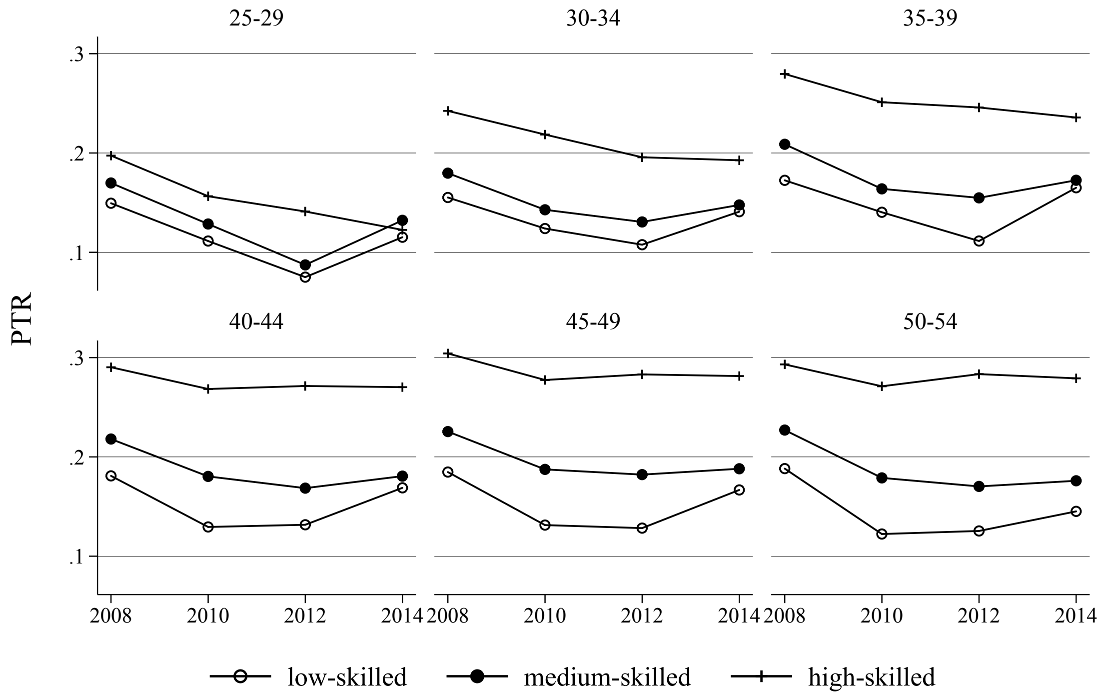


Figure 3.A8: PTR by Age Group and Education Level, ES

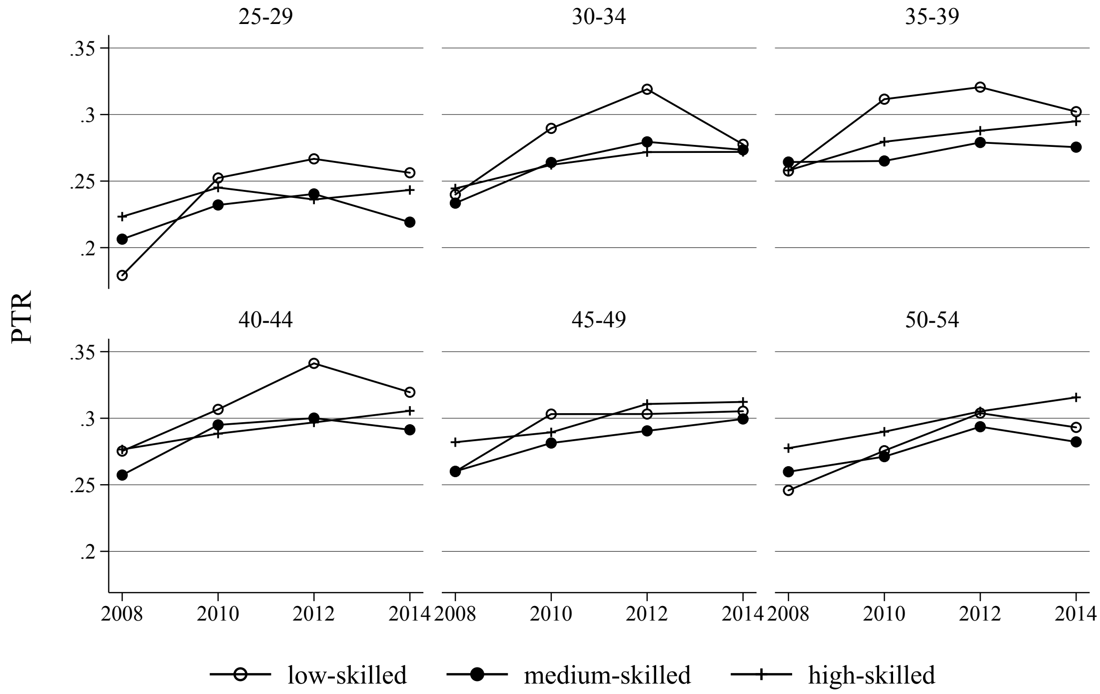


Figure 3.A9: PTR by Age Group and Education Level, FR

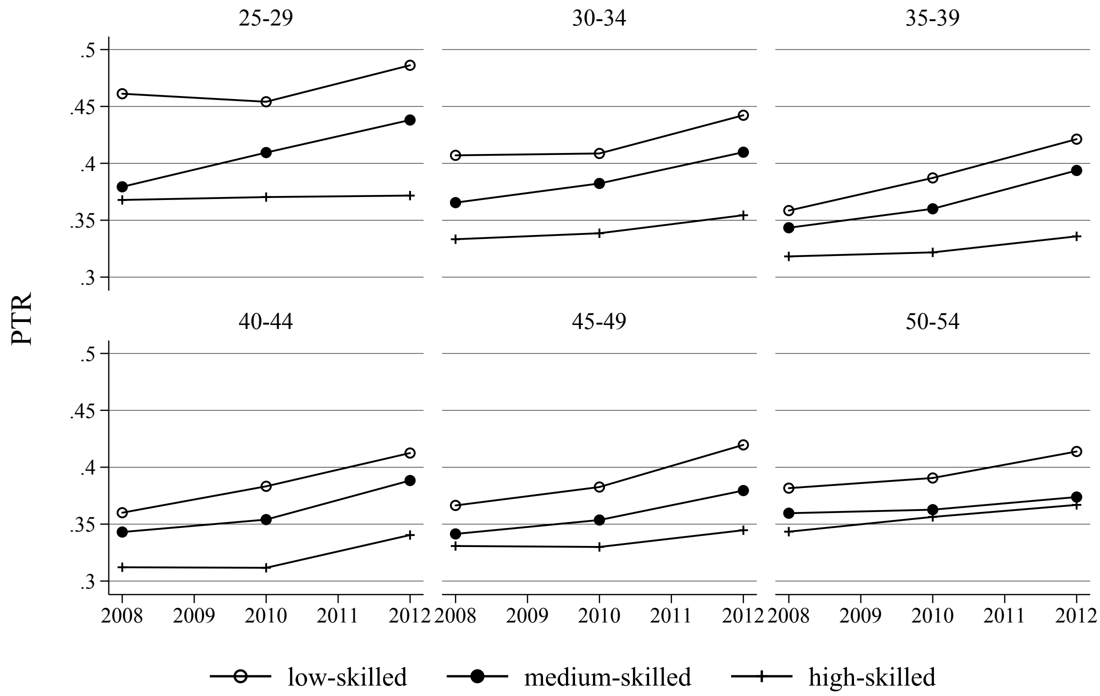


Figure 3.A10: PTR by Age Group and Education Level, IT

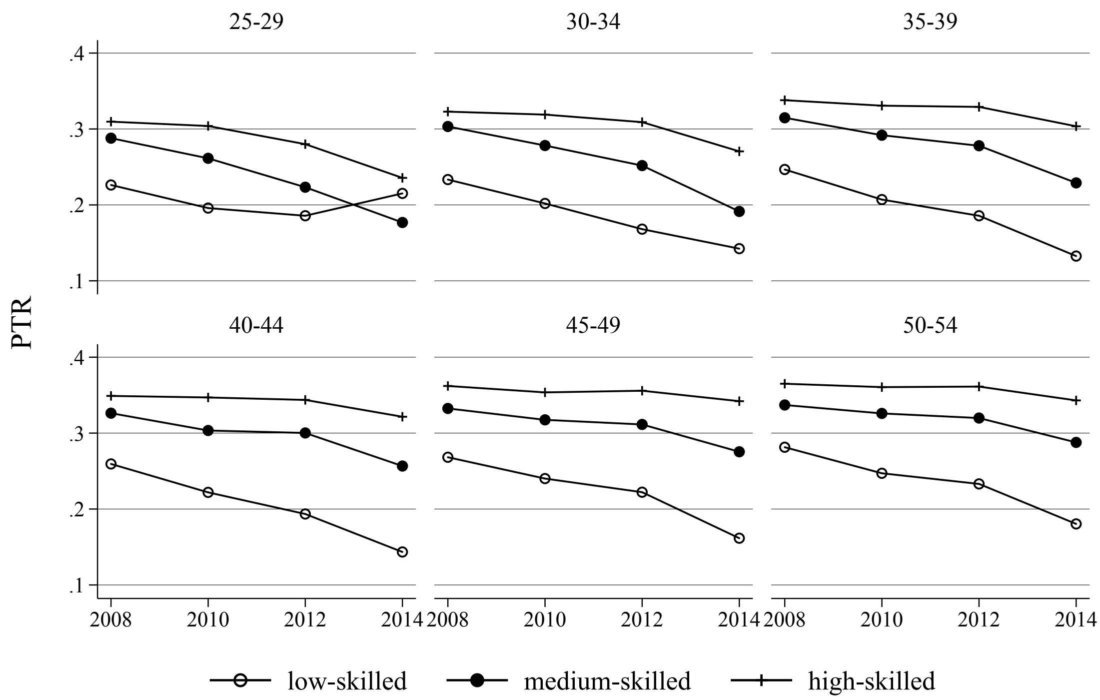


Figure 3.A11: PTR by Age Group and Education Level, SE

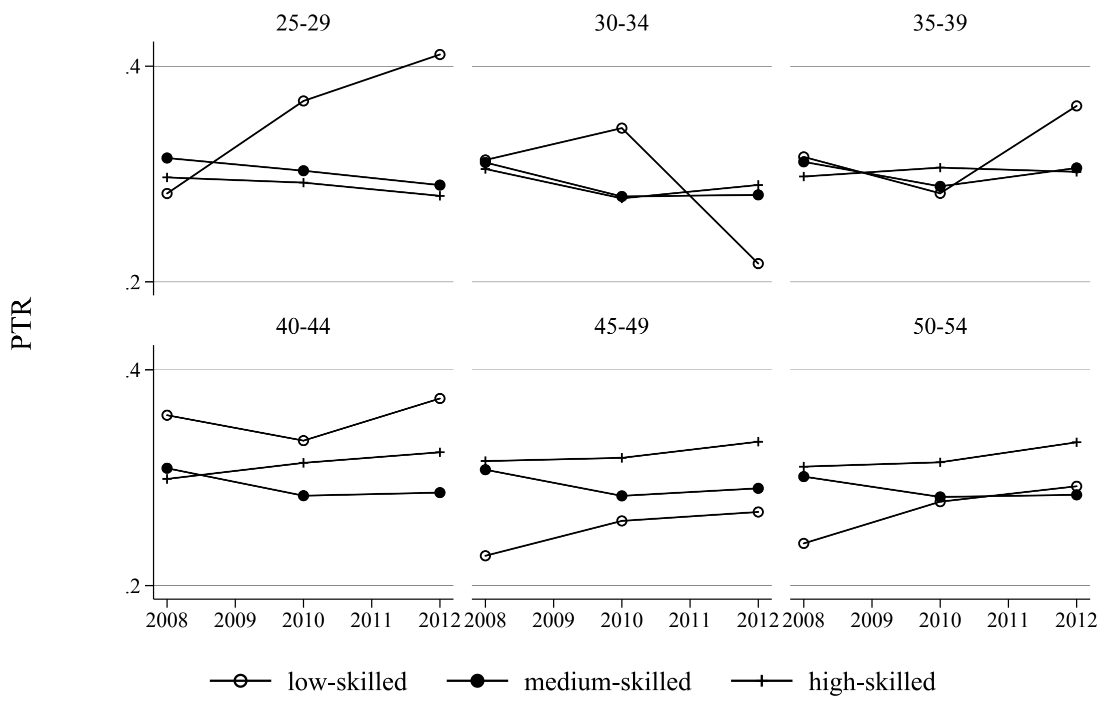


Figure 3.A12: PTR by Age Group and Education Level, UK

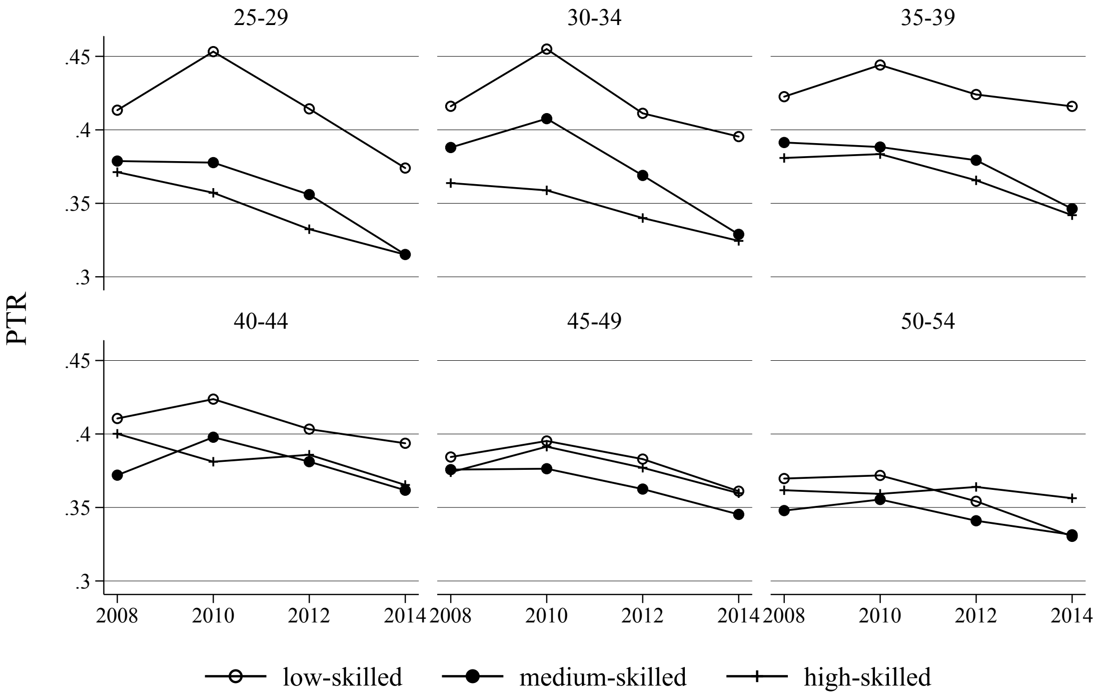


Table 3.A1: Regression Results for Group IV with Alternative Group Definition

	Blundell et al. 1998		Jäntti et al. 2015	
	baseline	controls	baseline	controls
	(1)	(2)	(3)	(4)
(1-PTR)*earnings	0.112***	0.090***	0.147***	0.092***
	(0.008)	(0.007)	(0.005)	(0.005)
	(0.003)	(0.003)	(0.003)	(0.003)
Male		0.017***		
		(0.005)		
Married		-0.023***		-0.021***
		(0.002)		(0.002)
Hh. non-labor income		0.004***		0.003***
		(0.001)		(0.001)
Child aged 1-3		-0.059***		-0.057***
		(0.003)		(0.002)
Child agend 4-6		-0.032***		-0.030***
		(0.002)		(0.002)
Child aged 7-17		-0.022***		-0.020***
		(0.002)		(0.002)
Experience		0.041***		0.040***
		(0.000)		(0.000)
Experience squared		-0.001***		-0.001***
		(0.000)		(0.000)
Constant	0.849***	0.533***	0.775***	0.487***
	(0.008)	(0.010)	(0.009)	(0.009)
First stage AR-statistic	163.26***	520.22***	838.59***	372.70***
N	355,793	355,793	355,793	355,793

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Source: EUROMOD data, own calculations. The sample includes prime working-aged individuals aged 25-54, excluding students, pensioners, the permanently disabled, and those in compulsory military service. Standard errors are heteroskedasticity robust and are corrected for generated variables bias. All equations include both year and country fixed effects. The omitted education category is lower secondary education. Groups are defined as 10-year age cohorts, three educational attainment groups and gender, following Jäntti et al. (2015). We define groups within each European country. All regressions are estimated with 2SLS, instrumenting the individual-specific net-of-PTR earnings with the group average in each year and country. Standard errors are clustered at the group level.

Table 3.A2: Regression Results for Group IV and Alternative Clustering Strategies

Cluster	group	group year	country	group	group year	country
	(1)	(2)	(3)	(4)	(5)	(6)
(1-PTR)*earnings	0.114***	0.114***	0.114**	0.091**	0.091**	0.091**
	(0.034)	(0.034)	(0.050)	(0.040)	(0.040)	(0.040)
Male				0.010	0.010	0.010
				(0.025)	(0.025)	(0.023)
Married				-0.012**	-0.012**	-0.012
				(0.005)	(0.005)	(0.009)
Hh. non-labor income				0.003	0.003	0.003
				(0.003)	(0.003)	(0.003)
Child 1-3				-0.064***	-0.064***	-0.064***
				(0.010)	(0.010)	(0.016)
Child 4-6				-0.035***	-0.035***	-0.035***
				(0.007)	(0.007)	(0.007)
Child 7-17				-0.019***	-0.019***	-0.019***
				(0.006)	(0.006)	(0.003)
Experience				0.043***	0.043***	0.043***
				(0.003)	(0.003)	(0.005)
Experience squared				-0.001***	-0.001***	-0.001***
				(0.000)	(0.000)	(0.000)
Constant	0.850***	0.850***	0.850***	0.568***	0.568***	0.568***
	(0.026)	(0.026)	(0.064)	(0.056)	(0.056)	(0.060)
N	355,793	355,793	355,793	355,793	355,793	355,793

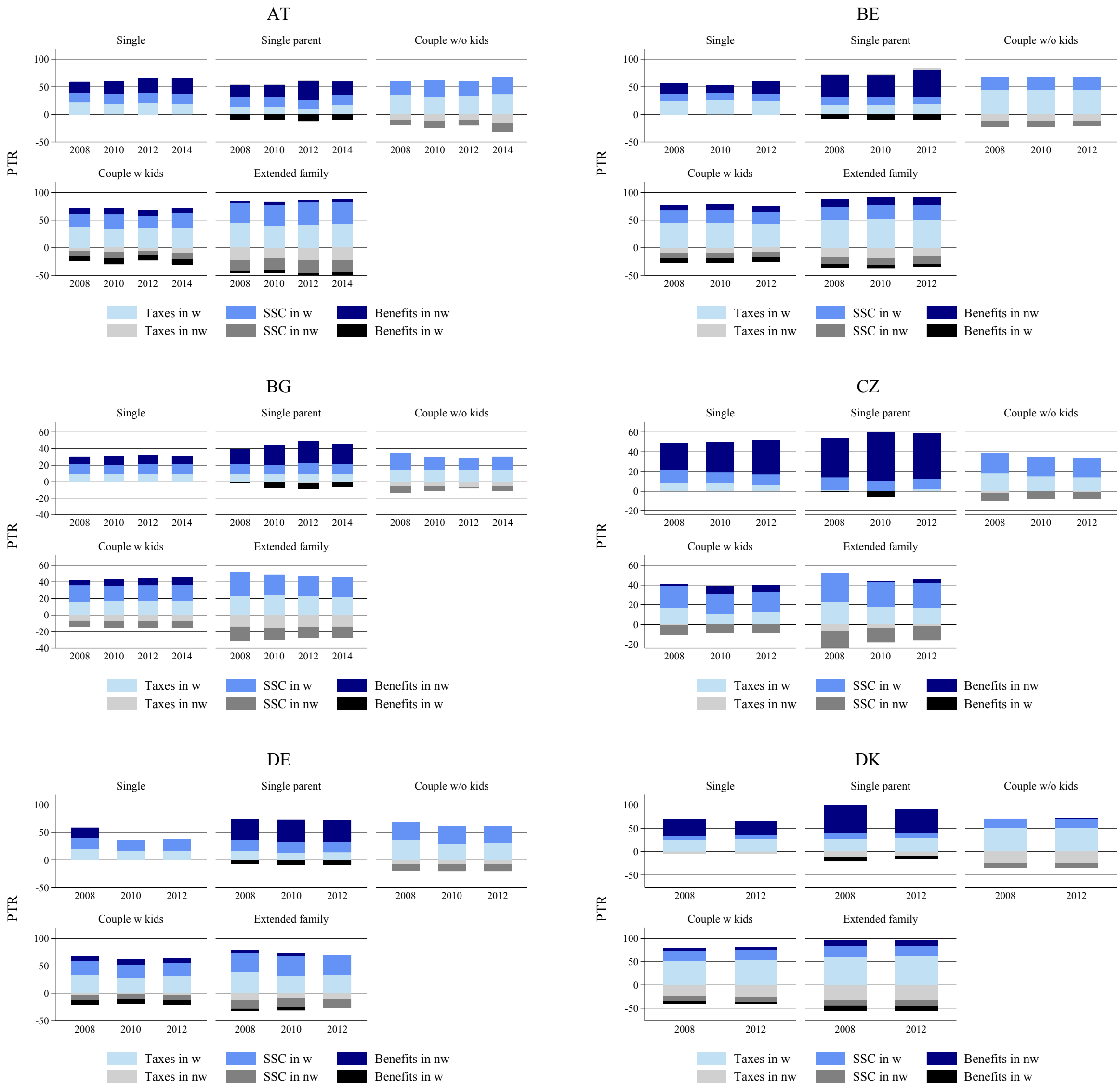
Source: EUROMOD data, own calculations. This table corresponds to Table 3.5 with alternative clustering strategies including clustering by group, clustering by group and year, and clustering by country. Columns (1)-(3) show baseline results, columns (4)-(6) add demographic controls.

Table 3.A3: Robustness of Group IV to Time Trends

	Group IV			
	(1)	(2)	(3)	(4)
(1-PTR)*earnings	0.071***	0.085***	0.091***	0.063***
	(0.007)	(0.007)	(0.007)	(0.007)
Country × Year	✓			✓
Age × Year		✓		✓
Gender × Year			✓	✓
Demographic controls	✓	✓	✓	✓
N	355,793	355,793	355,793	355,793

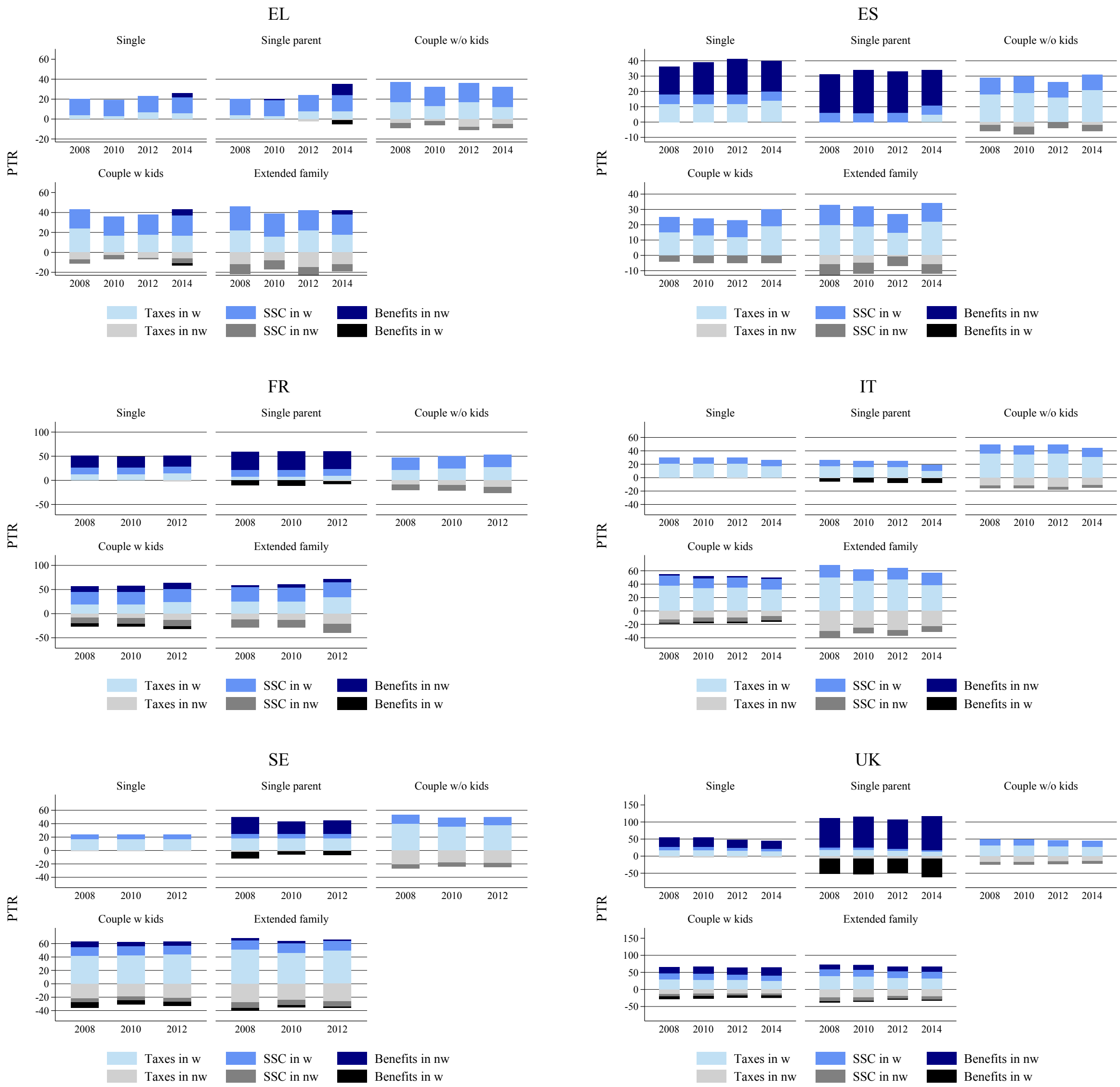
Source: EUROMOD data, own calculations.

Figure 3.A13: PTR Composition by Country and Year



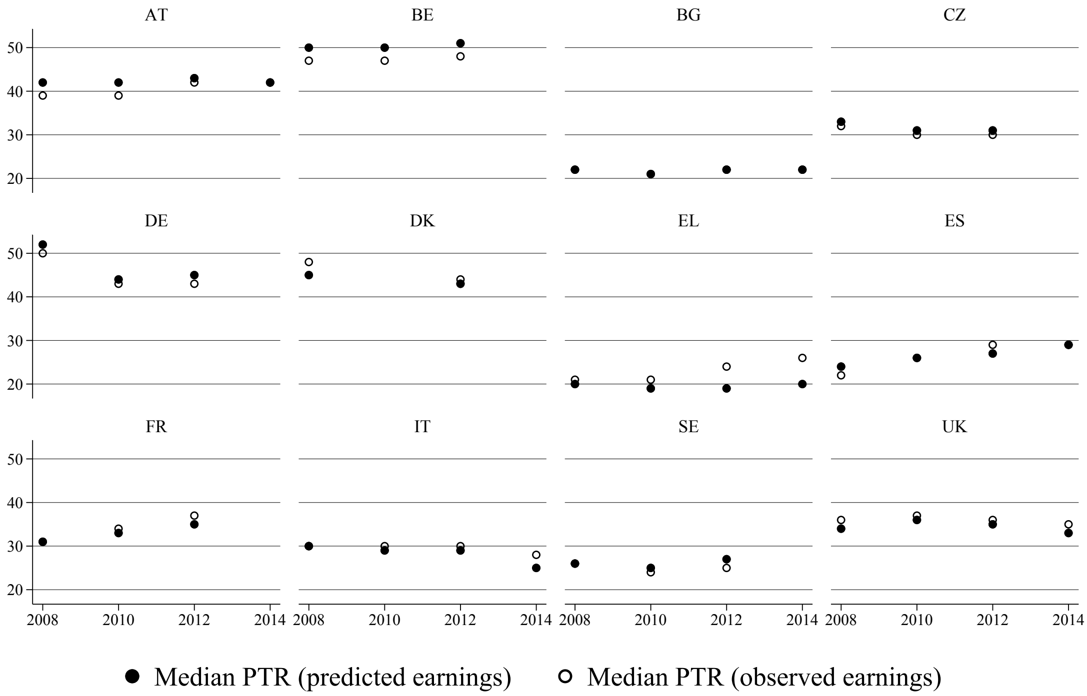
Source: EUROMOD data, own calculations.

Figure 3.A14: PTR Composition by Country and Year



Source: EUROMOD data, own calculations.

Figure 3.A15: Median PTR Based on Predicted vs. Observed Earnings



Source: EUROMOD data, own calculations.

4 Affordability of the Affordable Care Act's Medicaid Expansion: Implications for Household and Public Finances

4.1 Introduction

Health economists and policy-makers around the world continue to debate the merits of public health insurance as an in-kind benefit to low-income households. Beyond the context of the United States, where public insurance (Medicaid) expansion under the Patient Protection and Affordable Care Act (ACA) remains highly contentious, the World Health Organization is leading a campaign to achieve universal health insurance coverage (World Health Organization, 2011, 2013). Across many OECD countries, which have provided state-sponsored health care for decades, moral hazard and adverse selection prevent a thorough understanding of the *causal* link between public insurance, the household budget constraint and medical consumption behavior. The ACA Medicaid expansion presents a unique quasi-experiment through which it becomes possible to causally examine the impact of this policy tool on household medical consumption and spending.

Previous work has established that the ACA Medicaid expansion led to an increase in public insurance coverage ((Sommers et al., 2014; Courtemanche et al., 2016; Duggan et al., 2017; Frean et al., 2017)). However, still very little empirical evidence exists regarding how this increased insurance coverage actually altered affordability of health care goods and services for targeted households and changed the burden of out-of-pocket (OOP) medical expenditures for taxpayers. This paper seeks to fill that gap.

A priori, economic theory yields ambiguous predictions regarding the relationship between subsidized public insurance and OOP. Holding health care consumption (utilization) fixed, subsidizing costs through Medicaid should unambiguously reduce average OOP by 75%, the mean share of health care expenditures covered by Medicaid in the United States. Because health care represents a normal good, however, subsidies likely lead to increased utilization, as the relative price decreases by 75% (substitution effect). Furthermore, income effects may increase the OOP of households with pre-reform positive health expenditures; stark reductions in OOP may free up income for non-health consumption or savings, and some of this increased income could be spent on additional medical services or products.

Differentiation in the health care services market further complicates predictions about the effect on OOP. Common avenues of pre-reform health care access for low-income households such as public hospitals and emergency rooms offer an arguably inferior good compared to regular physician treatment. Post-reform substitution across health care providers may actually increase OOP if households previously received pro-bono treatment through charity care.[1] Determining which of these channels dominates remains the empirical exercise at issue in this paper.

Beyond effects to mean expenditures, insurance should decrease the risk especially of high-cost medical events and thus reduce the mean-preserving risk spread (Feldstein and Gruber, 1995; Finkelstein and McKnight, 2008; Shigeoka, 2014). The potential shock to the budgets of low income households in the form of free or subsidized insurance proves particularly large because OOP medical expenditures comprise a substantial portion of the household budget. In extreme cases, an unlucky medical event could lead to bankruptcy, accompanied by additional negative effects on the long-term financial health of the household.[2] The analysis provides estimates of changes to OOP across the distribution of expenditures and quantifies the reduction in risk exposure.

As a comprehensive reform, the ACA combined several additional policy measures beyond Medicaid public insurance expansion, including the introduction of private insurance exchanges and subsidies, an individual insurance mandate and wide-reaching regulations of the health insurance market. This paper will assess the separate effects of Medicaid, private insurance subsidies and individual mandate penalties on affordability of health care, measured in household out-of-pocket medical expenditures (OOP) including insurance premia. The primary concern lies in identifying the impact of Medicaid expansion while controlling for exposure to these other two policy provisions. At the same time, any causal impacts must be interpreted as being in combination with underlying changes to regulations, which, as a simple time-series change are not separately identifiable.

In order to establish a causal relationship between OOP medical expenditures and exposure to Medicaid, exchange subsidies and mandate penalties, I exploit variation in eligibility rules for each of these policies across regions, income groups and time in an

[1] Charity care refers to an informal network of emergency rooms, public hospitals, community centers and private physicians that offer low-income individuals pro-bono care in exchange for reimbursements from state and federal government programs. For a detailed discussion of charity care, refer to section 4.5.3.

[2] Himmelstein et al. (2009) attribute as much as 62.1% of consumer bankruptcies prior to the onset of the Great Recession in 2007 to medical bills. Gross and Notowidigdo (2011) estimate this share to be much lower, around 26%. Even based on the lower bound, however, this figure remains economically relevant.

enriched difference-in-differences (DD) strategy. I also provide results for a full difference-in-difference-in-differences (DDD) analysis across these dimensions. Furthermore, I instrument observed eligibility with a simulated eligibility measure in the spirit of Cutler and Gruber (1996) and Currie and Gruber (1996) that isolates the exogenous variation in policy generosity from individual-level endogeneity. Rich information in the Medical Expenditure Panel Survey (MEPS) allow me to explore how changes to OOP of recipient households shift the burden of payment between recipient households, private insurance and public insurance (including charity care). It moreover permits a detailed analysis of the possible channels of OOP adjustment, including changes to health care utilization.

The main contribution of this paper is that it causally identifies and quantifies the cost savings to Medicaid-eligible households as well as the cost-shifting from beneficiaries to other taxpayers. To my knowledge, it presents the first analysis of the impact of the ACA public insurance expansion on medical spending and consumption that uses individual-level data to define eligibility status for Medicaid, private insurance subsidies and individual mandate penalties for each household. This focus allows for the interpretation of effects as an intention to treat (ITT), which is the parameter of interest for the policy-maker, who can offer eligibility, but not receipt of health insurance. In addition to the effect on mean reductions in OOP, the paper provides the first analysis of the impact on risk exposure to high out-of-pocket medical payments. Finally, it contributes to the debate on the value of in-kind benefits, which has been growing in fiscal importance in recent decades (Besley and Coate, 1991; Bearse et al., 2000; Currie and Gahvari, 2008; Hoynes and Schanzenbach, 2009; Cunha et al., 2018; Lieber and Lockwood, 2019).

I find that the ACA Medicaid expansion reduced household out-of-pocket expenditures for medical services and products by 8.8% (among households with positive expenditures) and for insurance premia by 12.0%, for each standard deviation increase in Medicaid eligibility. Quantile regressions reveal that mean improvements in affordability for low-income households are driven by reductions in large OOP payments in the upper percentiles of the OOP distribution and by households with at least one pre-existing condition. Medicaid expansion moreover reduces the variance of medical spending. The value of this risk protection to eligible households amounts to $126 annually at the mean, with the utility of insurance increasing with risk aversion, total expected expenditures and the number of newly insured individuals in the household. Despite reductions in OOP expenditures among households that would become eligible for the ACA Medicaid expansion, total expenditures paid on their behalf increased. Reductions in the share of total medical expenditures paid by private insurance (4.6%) and OOP (7.2%) were compensated by a 10.9% increase in the share paid by the taxpayer through public insurance. Despite improvements in affordability for low-income families, the analysis does not detect substantial improvements with respect to access to urgent care or utilization of preventive care services.

The short-run cost-benefit-analysis (CBA) quantifies the net fiscal cost of expanding Medicaid public insurance to low-income households. Total costs include a mechanical cost (or mere transfer component) of the reform and efficiency costs from the increased spending associated with the moral hazard of obtaining health insurance. Benefits of the reform include decreased costs for charity care, the transfer value of reduced OOP to households and the insurance value of risk protection, defined as the willingness to pay of the average household to insure itself against all OOP risk. Considering all of these contributions to the costs and benefits of the reform, the CBA yields an estimate of net costs amounting to approx. $2,432 for each additionally eligible person annually, with almost half of this cost attributed to (an upper bound on) moral hazard. At the same time, this estimate falls well below the Congressional Budget Office estimate of per capita costs because it measures the effect of *offering* insurance rather than holding it.

The paper is structured as follows. Section 4.2 discusses relevant findings from related research. Section 4.3 outlines the identification strategy. Section 4.4 describes trends in OOP as well as observed and simulated eligibility rates for the three ACA provisions investigated. Section 4.5 presents results from the analysis and Section 4.6 explores heterogeneous effects of the reform. Section 4.7 offers a short-run cost-benefit-analysis of Medicaid expansion from the perspective of recipients and other taxpayers and Section 4.8 concludes.

4.2 Evidence on the Impact of Public Insurance

Early studies of the effects of the ACA Medicaid expansion on related outcomes have motivated the questions this paper seeks to answer. Frean et al. (2017) use data from the American Community Survey (ACS) to explore how eligibility for Medicaid expansion, private insurance subsidies and individual mandate penalties influenced increases in insurance coverage in 2014 and 2015. Among these three policy provisions, they find the largest coverage from increases in Medicaid eligibility.[3] The current paper follows a similar identification strategy by applying the same measures of exposure to the Medicaid expansion, private insurance exchange subsidies and individual mandate penalties using

[3] Additional studies using different identification strategies confirm that increased eligibility for Medicaid substantially increased take-up (Courtemanche et al., 2016; Sommers et al., 2015, 2014).

the Medical Expenditure Panel Survey (MEPS) data, which allow me to investigate the financial impact of increased coverage.[4]

Further research has investigated the impact of Medicaid expansion – either through the ACA or previous expansions – on household finances and medical debt. Brevoort et al. (2017), Gross and Notowidigdo (2011), Hu et al. (2016) and Dillender (2017) all find improved measures of financial health as a result of becoming eligible for Medicaid. Of these studies, the present paper builds most closely on Dillender (2017), who focuses on the potential crowd-out of Medicaid expansion and uses data from the Consumer Expenditure Survey (CEX) to show that families switching from private health insurance to Medicaid reduce their probability of having any health insurance spending, while average OOP medical spending remains unchanged for households at the low end of the income distribution. However, his period of investigation spans 2000-2014, including several years of relatively small incremental legislative changes at the state level and the immediate year following ACA Medicaid expansion. In this analysis, in contrast, I focus exclusively on the large changes beginning in 2014, which primarily affected households with adults. This difference likely explains the larger reduction I find for the most recent expansion. I further contribute to this line of research by using the MEPS data to investigate detailed OOP medical expenditures and types of utilization as well as sources of payment in order to explore channels of household adjustment to Medicaid.

One recent paper by Blavin et al. (2018) uses ACS and CPS data to compare differences in changes in coverage and OOP medical expenditures between expansion and non-expansion states and finds that OOP decreased on average by $344 more in expansion states. By restricting their sample to households between 100-138% of FPL[5], their measure of interest is how much more Medicaid expansion states reduce household OOP compared to states that only have access to exchange subsidies in non-expansion states. A further paper from Buchmueller et al. (2018) likewise employs a DD strategy between expansion and non-expansion states on the basis of CEX data to examine the impact of the ACA Medicaid expansion on the medical and non-medical consumption patterns of low-income households. They find small reductions in health spending and little effect on non-health consumption in expansion states, which they attribute to low health spending in general. While my results confirm low overall OOP spending among low-income households, the evidence points to intensive margin growth in health care utilization and a stark reduction in OOP payments in particular among households with at least one

[4] A minor difference is that I use summary measures for the post-reform years of 2014-2016 rather than treat each year separately.

[5] It should be noted that the authors use gross income rather than MAGI for this cut-off, which may bias the results.

pre-existing condition as well as among single households. By defining eligibility at the household level, I am not only able to explore heterogeneous effects and the possible channels of adjustment, but can more precisely identify the level of treatment exposure to this reform.[6]

Very little information exists to date on the effect of private insurance exchange subsidies and penalties on OOP expenditures. Long (2008) and Long and Masi (2009) provide some evidence from the Massachusetts health reform from 2006, which, to some extent inspired the design of some of the ACA provisions discussed in this paper. The authors show that the combination of public insurance expansion and private insurance exchange subsidies to households below 300% of FPL increased the affordability of and access to care. However, this study did not attempt to disentangle the impact from both policies, but rather presented a pre-post comparison of these outcomes in Massachusetts.

Beyond these papers focusing on the ACA Medicaid expansion, some important insights emerge from evaluations of the dependent coverage mandate. The dependent coverage mandate was one of the first provisions of the ACA to be implemented, on September 23, 2010. It required insurance companies to include children of policy holders on their parents' plan until the age of 26 without extra cost. Depew and Bailey (2015) use the MEPS data to show that this provision caused an increase in prices (insurance premiums) for eligible households. Chen et al. (2017) and Chua and Sommers (2014) likewise draw upon the MEPS data to examine the effect of the dependent mandate on OOP medical expenditures excluding premium costs of young adults under the age of 26 and find an overall reduction. Because they suspect higher OOP and utilization for smaller amounts of expenditures, but a reduction in large expenditures, for instance for emergency room (ER) visits, Chen et al. (2017) conduct a quantile regression analysis and find a decrease in OOP only in the 90th and 95th percentile of expenditures. For Chua and Sommers (2014), the negative effect also persists at the mean. However, the dependent mandate affected a very specific section of the population: they were younger, healthier and from lower-than-average-income households.[7] Therefore, it is unclear from the onset whether to expect similar effects from the provisions implemented in 2014 that targeted the entire population. My quantile estimates along the distribution of OOP

[6]Medicaid expansion states differ fundamentally from non-expansion states on several dimensions that are correlated with OOP medical expenditure as well as unobserved tastes for insurance or health care, which might cause concern with endogeneity bias from difference-in-differences (DD) results. For example, expansion states tended to have more generous pre-reform Medicaid eligibility criteria, wealthier populations, more public poverty intervention programs and more widely available access to charity care than did non-expansion states prior to the reform.

[7]Prior to the dependent mandate, students were already covered on their parents' plan. Therefore, the young dependents who newly became eligible through the ACA were neither students nor employed in a job offering health care.

medical expenditures confirm a similar reaction of the ACA expansion population, in that large reductions at and above the 95th percentile drive the positive and significant effect at the mean.

4.3 Empirical Approach

4.3.1 Data and Sample

A pooled cross-section of the Household Component of the Medical Expenditures Panel Survey (MEPS-HC) forms the main dataset used in the analysis. It is the most detailed source of nationally representative data for the United States regarding medical conditions, health care utilization, insurance coverage, and expenditures by source of payment. Furthermore, it contains demographic and income information for each household, which enables me to identify Medicaid eligibility status at the individual level. Commensurate with eligibility rules, the unit of analysis is defined at the household insurance unit (HIU). A HIU may consist in single households, couples or families with children and some dwelling units may contain two or more HIUs. While all outcome variables of interest stem from this dataset, additional sources are necessary to measure household eligibility for each respective policy provision of the ACA. Section 4.3.2 describes these datasets in detail.

The analysis focuses on non-elderly households with individuals below the age of 65. I restrict the sample to households earning below 400% of the poverty line (FPL), in accordance with the income cut-off for the target population for ACA subsidies.[8] The final sample encompasses approximately 73,000 households from 2010-2016. While most information in the MEPS is available before 2010, I restrict the sample to these years in order to maintain comparability of the main results with those from the heterogeneous effects according to the presence of a pre-existing condition, as some chronic conditions appear in the dataset in 2010. Appendix Table 4.A2 does however demonstrate that results of the main analysis in Table 4.4 are very robust to including years 2007-2016, with the longer time period yielding marginal effects of similar magnitude and higher significance due to the increased power with roughly 101,000 household observations. Table 4.1 displays sample characteristics of sample households in the treatment and control groups.

[8] Appendix Table 4.A1 shows that the main results (Table 4.4) are robust to including all income groups. Results are somewhat stronger given the larger sample size.

4.3.2 Measuring Policy Provisions of the Affordable Care Act

In order to isolate the effect of Medicaid expansion from that of other policy instruments that comprise the ACA, it is important to control for exposure of households to possible alternative treatments. Some substantial aspects of the reform, such as changes to regulation requiring community rating and guaranteed issue cannot be disentangled from Medicaid expansion, as the former present a time series change without exogenous variation in exposure among the population.[9] For this reason, results must be interpreted as being *in addition to* any impact from underlying changes to the regulatory environment.

In addition to Medicaid eligibility, I measure exposure to two possible alternative treatments for which exogenous variation and data are available: the individual mandate penalties and insurance exchange subsidies. Using this variation, I construct treatment and control groups according to treatment intensity, defined by the share of the HIU eligible for each policy provision. While the main concern lies in the impact specifically of the Medicaid expansion provision, controlling for exposure of households to subsidies and penalties allows for the interpretation of policy interest, namely the effect of Medicaid eligibility versus non-eligibility rather than Medicaid eligibility versus exposure to these other policies. For example, an individual eligible for Medicaid does not qualify for private insurance exchange subsidies, but a household with the same income level residing in a state that did not expand Medicaid, will not qualify for Medicaid and instead would be eligible for a private exchange subsidy. This section provides some institutional background for the three provisions and describes the measurement of eligibility for each.

All relevant income thresholds for ACA eligibility refer to the concept of modified gross adjusted income (MAGI). Using income information available in the MEPS-HC, MAGI amounts to the sum of the following family unit income components: gross wages and salaries from employment, business and farm income, taxable interest income, rent income, trust fund income, alimony received less alimony paid, annuities, dividends[10], taxable pensions, and unemployment benefits.[11] This gross amount is then adjusted by subtracting deductions, including large medical expenses, to arrive at adjusted gross income (AGI). I apply NBER's TAXSIM program, version 9, to account for deductions based on household gross income, expenses and composition. Commensurate with con-

[9] Guaranteed issue refers to the prohibition of insurance companies from denying coverage to eligible individuals, regardless of pre-existing conditions. Community rating obliges insurance companies to offer one price for individuals of the same age and location, regardless of sex or pre-existing conditions. The ACA further mandates that insurers offer coverage for 10 health benefits deemed essential and required all policies sold in the US to provide an annual maximum cap for out of pocket payments.

[10] Dividends are treated as other property income in the TAXSIM model because the MEPS does not contain information about whether dividends are qualified.

[11] Capital gains are set to zero due to lack of information in MEPS.

vention in the United States, I treat married couples as filing jointly for the purpose of calculating AGI. Finally, MAGI results from adding untaxed foreign income, non-taxable Social Security benefits and tax-exempt interest to the AGI. Due to lack on information on these last components, I use AGI rather MAGI. The importance of this restriction, however, is limited because AGI and MAGI are equivalent in the vast majority of households and in particular for the low-income groups targeted by the reform.

4.3.2.1 Medicaid Expansion

Medicaid public insurance coverage was introduced in the United States in 1965. However, coverage was restricted to protected groups such as pregnant women, the disabled, children and parents of eligible children with very low incomes. The ACA expanded eligibility to childless adults with MAGI below 138% of the federal poverty line (FPL).[12] Simultaneously, some states also increased means-tested thresholds for parents and children. However, before its planned implementation on January 1, 2014, a 2012 landmark Supreme Court ruling in the case *National Federation of Independent Business v. Sebelius* declared the Medicaid provision of the ACA coercive and permitted states to decide whether or not to expand it. As a result, only 26 states, roughly half, implemented the expansion in 2014 and 5 more by 2016.[13] I will use this expansion decision as quasi-exogenous variation from the perspective of the individual household. At the same time, expansion and non-expansion states differ in important dimensions such as income distributions, family types, pre-reform insurance levels and anti-poverty government interventions. I detail how I account for these differences in section 4.3.3.

Eligibility for ACA Medicaid expansion depends on residing in an expansion state and having household income below a category-specific (pregnant women, children of certain age ranges, disabled individuals, parents or other adults) means-tested threshold. I use the state expansion status from 2014-2016 and apply the Medicaid/CHIP eligibility thresholds by state, year, age and status from the Kaiser Family Foundation.[14] I apply these thresholds to the MEPS households and determine individual eligibility according to the demographic and income information in the main public-use files of the MEPS-HC as well as the restricted geographic information available at the Agency for Healthcare

[12]In 2016, the last year included in this analysis, the federal poverty line is $11,880 in annual taxable income for a single household or $24,300 for a family of four.

[13]These states include: AR, AZ, CA, CO, CT, DC, DE, HI, IA, IL, KY, MA, MD, MI, MN, NH, NJ, NM, ND, NV, NY, OH, OR, RI, WA and WV. By 2015, PA, IN and AK also expanded Medicaid and MT and LA followed suit in 2016. For the period under investigation, which extends through 2016, non-expansion states in the sample include: AL, FL, GA, ID, KS, IN, ME, LA, MS, MO, NC, NE, OK, SC, SD, TN, TX, UT, VA, WI and WY.

[14]These can be found on the website of the Kaiser Family Foundation, under `https://www.kff.org/state-category/medicaid-chip/medicaidchip-eligibility-limits/` (accessed June 10, 2018).

Quality and Research in Rockville, Maryland. Because means-tested thresholds depend on status category, one household may have some eligible and some ineligible members such the eligibility measure applied in the regression analysis consists in the share of the HIU eligible.

4.3.2.2 Minimum Essential Coverage Requirement ("Individual Mandate")

The "individual mandate" refers to the requirement of every non-exempted individual to purchase health insurance or pay a penalty. Effective January 1, 2014[15], it sought to prevent a possible downward spiral in the private insurance market, induced by adverse selection.[16] Penalty amounts rise with household income and were gradually increased from 2014 to 2016. For the 2014 tax year, the penalty amounts to the higher of a flat rate of $95 per uninsured adult and half of that per uninsured child or 1% of modified adjusted gross income (MAGI). In 2015, penalties increased to the greater of $325 per uninsured adult and half of that per uninsured child or 2% of MAGI. In 2016 and all subsequent years, the penalty reaches $695 per adult and half of that per uninsured child or 2.5% of MAGI. These penalties are capped at the national average price of a bronze level insurance plan. Between 2014-2016, roughly 33-31% of households are exempted from penalties irrespective of insurance status. Exempted groups include: households earning below the tax filing threshold, those earning below 138% of FPL in non-expansion states, and those without access to affordable health care, defined as access to an insurance plan that costs no more than 8% of MAGI. For all of these groups, the potential penalty is equal to $0. While about 67-69% of households are eligible for a penalty for foregoing insurance coverage, penalties should in practice only affect those previously uninsured, increasing transitions into private, non-group insurance. For these groups, a higher penalty amount should increase average OOP.

The calculation of penalty amounts for non-exempted households is straightforward. Determining exemption status, however, requires calculating the price of the lowest-cost bronze plan for the given household size and rating area, which serves as the government benchmark for the affordability of accessible insurance.[17] For this calculation, I incorporate price information from the Robert Wood Johnson Foundation for the lowest

[15] The Tax Cuts and Jobs Act of 2017 eliminated the enforcement mechanism for the individual mandate beginning in 2019.

[16] Such a situation could occur if young and healthy individuals chose not to purchase insurance and exited the pool of the insured, leaving relatively unhealthy and older individuals in the market. The subsequent increase in the risk pool would likely raise prices, further pushing the marginally healthier individuals out of the market until it finally collapses.

[17] Rating areas are equivalent to counties with the exception of AK, CA, ID, MA and NE, which use 3-digit ZIP codes. For these 5 states, I use the average price in the rating area.

cost bronze plan on the federal insurance exchange in each county. I supplement federal exchange prices with those from state exchanges with the help of the Kaiser Family Foundation's Marketplace Calculator.[18] Next, I apply the age adjustment curves documented by the Center for Medicare and Medicaid Services in order to account for age-based price setting. Finally, the price of the benchmark plan equates to the sum of costs for the individual plans in the HIU, including up to 3 children.[19] For HIUs with the lowest-cost bronze insurance premium above 8% of MAGI, the potential penalty is zero.

4.3.2.3 Health Insurance Exchange Subsidies

Health insurance exchanges and exchange subsidies offer Americans without Medicaid or employer-sponsored-insurance (ESI) an online marketplace for purchasing private health insurance. The creation of the insurance exchanges on the federal and state level increased the transparency and comparability of private plans. Additionally, plans are categorized into three metal tiers according to their actuarial value, reflecting the percentage of total costs the insurance covers for the average policy holder. However, in addition to these regulations that sought to lower costs and increase transparency for the consumer, the ACA simultaneously increased the risk pool for non-group private insurance.

Households with income between 100–400% of the FPL are eligible for exchange subsidies that decrease with income up to this threshold. The subsidy amount is equal to zero for households eligible for Medicaid as well as those earning at least 400% of FPL or with access to affordable ESI. The value of this subsidy depends on household income (MAGI) as well as the cost of the second-lowest premium for single coverage in the household's rating area.[20] For eligible households, the amount results from the difference between a progressive affordability cap and the second-lowest cost silver plan in the household's rating area.[21] Both the sliding scale of affordability caps and the market prices of the benchmark silver plans changed in 2015 and 2016. Therefore, similar to the

[18] Beginning in 2015, the Robert Wood Johnson Foundation provides price data for all states. For 2014, however, information is missing for the 14 states that relied on their own state exchanges rather than the federal exchange. I fill in this missing information manually using the Marketplace Calculator.

[19] Federal regulations stipulate that insurance coverage of the fourth and subsequent children must be offered without extra cost.

[20] In 2014 in Medicaid non-expansion states, childless adults below the FPL do not qualify for Medicaid or exchange subsidies. This unforeseen gap emerged because the drafters of the ACA assumed these individuals would qualify for Medicaid. RAND estimates that 5.3 million Americans fell into this category and did not have insurance directly after the implementation of the ACA in 2014. This gap has since been closed.

[21] In 2014, the affordability caps as a percentage of MAGI were: 2% for households earning below 138% of FPL, 4% for those in the range of 138-150, 6.3% for 150-200% of FPL, 8.05% for 200-250% of FPL, 9% for 250-300% of FPL and 9.5% for 300-400% of FPL.

treatment of the mandate penalties, the treatment variable for subsidy eligibility is an average of the potential subsidy for which the household qualified between 2014-2016.

As in the case of individual mandate penalties discussed above, the calculation of eligible subsidy amounts requires price information for benchmark insurance plan costs in the HIU rating area. I compile this information likewise from the Robert Wood Johnson Foundation and Kaiser Family Foundation, but rather than using the lowest price bronze plan, the benchmark for subsidies relates to the second-lowest cost silver plan. I apply these thresholds to the households in the MEPS dataset and calculate the eligible dollar amount of subsidy using the HIU composition, income and geographic information in the MEPS.

4.3.3 Estimation Strategy

In order to establish a causal relationship between the ACA Medicaid expansion and household medical spending, the present paper implements an enriched difference-in-difference (DD) estimation strategy in combination with a simulated instrument for eligibility status. Building on this specification described in detail in section 4.3.3.1, section 4.3.3.2 incorporates additional interaction terms that control for time trends in income-group and state-specific trends in OOP. Finally, section 4.3.3.3 describes the construction and implementation of the simulated instrument.

4.3.3.1 Main Specification: Enriched Difference-in-Differences

In this treatment effects analysis, I define treatment group status, $MCAID_{hst}^{ACA}$, in both pre- and post-reform years as being Medicaid eligible according to the ACA rules from 2014-2016[22], but applied to household income, state and demographic composition in each year. Because the level of analysis is the health insurance unit (HIU), I aggregate $MCAID_{hst}^{ACA}$ to the share of the household newly becoming eligible for Medicaid through the ACA. Eligibility is determined by residency in an ACA expansion state as well as falling below the means-tested household FPL threshold, which depends on each individual's status as a child (in different age ranges), single adult, parent, pregnant or disabled person.

After controlling for the direct, time-invariant influence of income, region and year on the outcome variable of interest, identifying variation stems from differences in these rules across income groups, states and time. Rather than considering all households residing

[22] The measure uses the average eligibility status. For example, if an individual only becomes eligible in 2016, average eligibility of the individual in the post-reform period would be $0.\overline{3}$. In a next step, the average eligibility is calculated over all individuals in the household.

in an expansion state as treated, this strategy allows me to use additional variation at the individual level and reflects the different Medicaid eligibility thresholds for children, pregnant women, parents, the disabled and childless adults. The enriched DD equation can be stated as follows:

$$\begin{aligned} Y_h = \alpha + &\overbrace{\beta MCAID^{ACA}_{hst}}^{Treatment} + \overbrace{\theta(MCAID^{ACA}_{hst} \times Post_t)}^{Treatment \times Post} \\ &+ \mu INCGR_{hst} + \lambda_1 STATE_s + \underbrace{\gamma T_t}_{Post} + \xi X_{hst} + \varepsilon_h \end{aligned} \tag{4.1}$$

In the main analysis, the dependent variable, Y_h, represents the amount of out-of-pocket medical expenditures including premium costs paid by the household in time t. It does not include any subsidies received or payments made by third parties, insurance or otherwise. Results for this su mmary measure of total medical spending are displayed in Table 4.4. I also estimate the equation for the two components separately, namely for OOP excluding premium costs and for premium costs, results of which can be found in Tables 4.5 and 4.6. In the secondary analysis, which serves to explain the channels of OOP reductions and fiscal consequences, Y_h denotes the following outcome variables: the probability of any visit or the number of visits to different types of medical facilities, the share of total household medical expenditures by source of payment and the share of the household delaying necessary treatment for financial reasons.

$INCGR_{hst}$ encompasses the following 8 income bands: below 50% FPL, [50-100% FPL), [100-150% FPL), [150-200% FPL), [200-250% FPL), [250-300% FPL), [300-350% FPL), [350-400% FPL). Breaking points correspond to relevant ACA thresholds for the three provisions investigated, whereby Medicaid eligibility for childless adults requires the household to earn below 138% of the FPL.[23] Income band controls absorb the positive correlation between levels of OOP and income that stem from the nature of health care as a normal good. The matrix X_{hst} contains characteristics of the household such as family type (singles, single parents, couples without children and families with children) as well as the age, sex, race and a dummy for Hispanic origin of the head of the household. $STATE_s$ denotes the household state of residence, which nets out time invariant differences between expansion and non-expansion states. $URATE_{ct}$, the annual county unemployment rate obtained from the Bureau of Labor Statistics, controls for the local labor demand shocks at the county level that could influence income and thus OOP.[24] T_t are year fixed effects.

[23] The means-tested threshold for children and parents varies by state and is at least 138% of FPL or higher, with most expansion states having raised the threshold above 138%.

[24] Local unemployment rates also control for possible early adjustments from the employer mandate that was implemented beginning in 2015, but not enforced until 2018.

The coefficient θ on the interaction term of treatment and the post-reform years 2014-2016 ($Post_t$), captures the reform effect on household OOP, denoted as Y_h.

4.3.3.2 Robustness Strategy: Difference-in-Difference-in-Differences

As a further robustness check to the enriched DD, I estimate the full DDD model formulated below.[25] Because Medicaid eligibility involves an interaction between state expansion status and household income, the coefficient θ in Equation 4.1 can be interpreted as a triple interaction by adding the missing double interactions to line 2 of Equation 4.2.

$$\begin{aligned} Y_h = \alpha &+ \overbrace{\mu INCGR_{hst}}^{TG1} + \overbrace{\lambda STATE_s}^{TG2} + \overbrace{\gamma T_t}^{TT} \\ &+ \overbrace{\beta MCAID^{ACA}_{hst}}^{TG1\times TG2} + \overbrace{(\delta_1 INCGR_{hst} + \delta_2 STATE_s) \times POST_t}^{TG1\times TT,\, TG2\times TT} \\ &+ \underbrace{\theta(MCAID^{ACA}_{hst} \times POST_t)}_{TG1\times TG2\times TT} + \xi X_{hst} + \varepsilon_h \end{aligned} \tag{4.2}$$

Finally, to finish the full specification for the ACA Medicaid expansion provision, I control for eligibility according to the old Medicaid rules, captured by $MCAIDPreACA$. This group control proves necessary in order to interpret the effect of $MCAID^{ACA}$ as that attributed only to the ACA expansion. With this final inclusion, the same identification strategy is then applied analogously to each of the other two policy measures of private insurance exchange subsidies and individual mandate penalties.

Incorporating all three provisions into 4.2 and consolidating terms yields the complete regression equation used in the main specification:

$$\begin{aligned} Y_h = \alpha &+ \beta_1 MCAIDPreACA_{hst} + \beta_2 MCAID^{ACA}_{hst} \\ &+ \beta_4 SUBSIDY^{ACA}_{hst} + \beta_5 PENALTY^{ACA}_{hst} \\ &+ (\theta_1 MCAIDPreACA_{hst} + \theta_2 MCAID^{ACA}_{hst} \\ &+ \theta_3 SUBSIDY^{ACA}_{hst} + \theta_4 PENALTY^{ACA}_{hst}) \times POST_t \\ &+ (\delta_1 INCGR_{hst} + \delta_2 STATE_s) \times POST_t \\ &+ \mu INCGR_{hst} + \lambda_1 STATE_s + \lambda_2 URATE_{ct} + \gamma T_t + \xi X_{hst} + \varepsilon_h \end{aligned} \tag{4.3}$$

where θ_2 on the interaction term for ACA Medicaid expansion and post-reform years denotes the main coefficient of interest. The coefficients θ_3 and θ_4 summarize the effect of insurance exchange subsidies, $SUBSIDY^{ACA}_{hst}$ and mandate penalties, $PENALTY^{ACA}_{hst}$. It is important to include all three provisions in the same equation in order to be able to

[25] Given the number of observations to regressors, the DD strategy is preferred.

interpret these coefficients as the absolute effect of the respective policy provision rather than the relative effect of, for example, becoming eligible for Medicaid rather than for a subsidy.[26]

The corresponding β coefficients capture the effect of different levels of OOP spending among households in each of these treatment groups and in each pre- and post-reform year, according to the rules of the provision between 2014-2016. The coefficient β_2 absorbs the differential spending patterns of families that would later newly qualify for Medicaid in 2014 according their income as a percent of FPL, their household composition and state of residence. $INCGR_{hst}$ additionally controls for the fact that OOP tends to increase with income. $STATE_s$ and T_t are state and year fixed effects that respectively account for local policies such as the availability of charity care and trends in spending over time. Table 4.2 shows descriptive statistics for medical spending of households in the treatment and control groups.

4.3.3.3 Simulated Instrument

Despite the DD(D) strategy, some endogeneity concerns may remain with respect to the relationship between OOP and program eligibility. First, households may adjust their income in response to the ACA Medicaid expansion in order to qualify for benefits. Second, different income distributions across regions may qualify a larger share of the population in poorer states for ACA provisions or otherwise affect demand for and thus prices of health care. In order to address these concerns, I instrument observed household eligibility with simulated eligibility. Simulated instruments are well-established in the health economics literature as a method of isolating variation generated by the generosity of policy rules alone (see for example Cutler and Gruber (1996), Currie and Gruber (1996), Lo Sasso and Buchmueller (2004), Schmidt et al. (2016), Frean et al. (2017), and Dillender (2017)).

To construct the instrument, I take the entire national sample of observations from the MEPS-HC in each year, income group and family type and simulate eligibility of these individuals as if they resided in each county. I then assign each observed household the average eligibility share from its corresponding household type and income group from the national sample according to the eligibility rules in its county of residence. In total, there are 4 household types and 8 income groups for a total of 32 averages in each county. I then use the simulated eligibility measures as instruments for actual eligibility and esti-

[26]The control group for the Medicaid eligible do not receive Medicaid eligibility treatment, but may receive the alternative treatment of subsidy eligibility. While Medicaid and subsidy eligibility are mutually exclusive treatments, all households that choose not to gain either public or private insurance and are not otherwise exempt are subject to an individual mandate penalty.

mate regression equation 4.3 using 2SLS. This procedure isolates the exogenous variation stemming from ACA changes to the policy rules for the three provisions, rather than differential income distributions, family structures or other demographic characteristics in each state. Moreover, even if the household income is endogenous at the individual level, exogeneity should hold at the group level. Because the simulated instrument varies only in policy rules, it offers a plausibly exogenous instrument.

Table 4.4 juxtaposes the DD and DDD marginal effects using OLS with those using the simulated instrument and shows that results are very similar using either estimation strategy. Consistently high first stage F-statistics well above the critical value of 10 for all regression models are displayed in the table and lend credence to the relevance of these instruments. Due to the presence of multiple endogenous variables (4 simulated policy measures[27] and 4 interaction terms containing these measures), I calculate F-statistics as Anderson-Rubin (AR) statistics from Finlay et al. (2016), which allows for cluster robust inference from multiple endogenous variables. Appendix Table 4.3 displays the observed and simulated eligibility values for the years 2010-2016. Nevertheless, because I cannot reject equality of the OLS and simulated IV results, I proceed using the OLS specification, as it renders interpretation of the marginal effects and cost-benefit analysis straightforward.[28]

[27]These measures include: pre-ACA Medicaid eligibility, ACA-Medicaid eligibility, subsidy eligibility and exposure to mandate penalties.

[28]Results tables of all main outcome variables have also been produced using the simulated IV DD specification and show comparable effects. With the exception of the IV counterpart to Table 4.A6, the instrument has a strong first stage. These results are available upon request.

Table 4.1: Sample Characteristics of Treatment and Control Households, 2010-2016

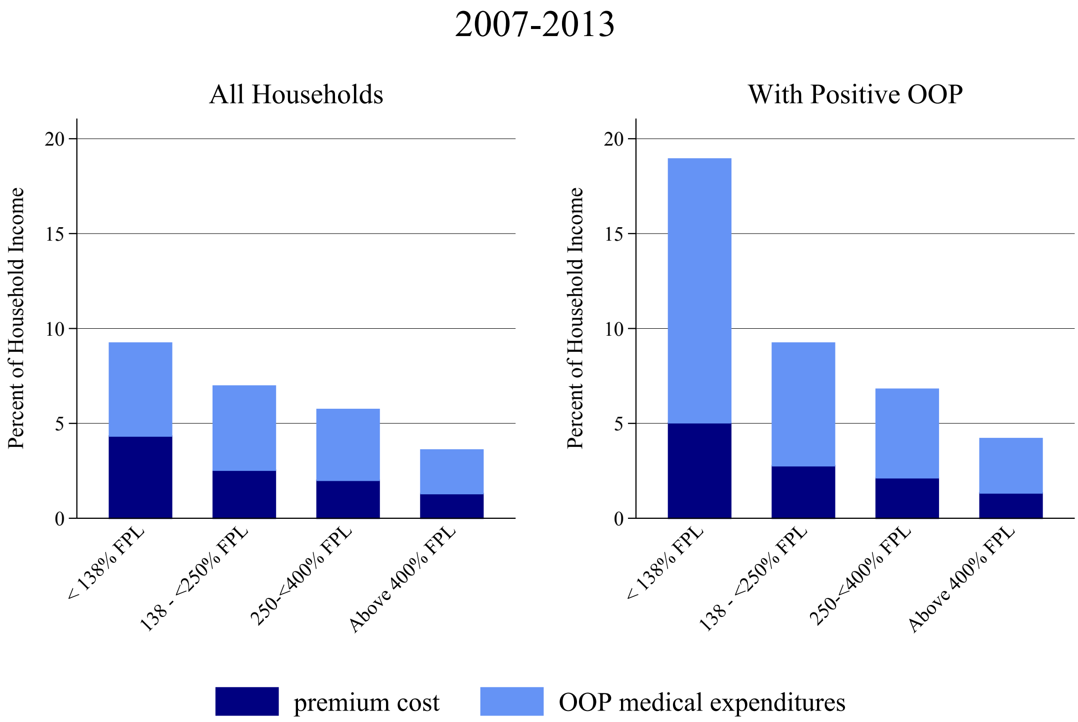
	According to the ACA Medicaid Rules from 2014-2016:			
	Medicaid Eligible 2010-2013	Medicaid Eligible 2014-2016	Not Eligible 2010-2013	Not Eligible 2014-2016
	(1)	(2)	(3)	(4)
Household Head:				
- Age 18-25	0.15	0.17	0.13	0.14
- Age 26-39	0.31	0.30	0.36	0.35
- Age 40-49	0.21	0.18	0.23	0.21
- Age 50-64	0.33	0.35	0.28	0.29
- White	0.79	0.79	0.79	0.77
- Black	0.15	0.16	0.17	0.18
- Hispanic	0.18	0.14	0.20	0.20
Household Context:				
- Singles	0.68	0.70	0.51	0.47
- Single parents	0.13	0.11	0.12	0.11
- Couples w/o children	0.11	0.05	0.22	0.13
- Couples w/ children	0.05	0.10	0.11	0.22
- Larger families	0.02	0.03	0.04	0.07
- Total HH income ($)	21,248.84	20,943.53	40,511.13	43,306.71
	(16,167.54)	(17,751.41)	(24,684.14)	(28,126.70)
- Any private insurance	0.40	0.40	0.56	0.60
- Public only	0.31	0.46	0.21	0.25
- Uninsured	0.29	0.14	0.23	0.15
- Share with chronic disease	0.53	0.57	0.46	0.46
Observations	6,778	3,941	36,096	25,882

Source: MEPS cross-sectional data 2010-2016, calculations based on the working sample residing in households below 400% FPL. Weighted means using MEPS household sample weights. Households with at least one member becoming eligible through the ACA Medicaid expansion in any year between 2014-2016 are categorized as eligible. Column (1) presents the average value for households that would have been eligible according to the ACA rules, had the reform been implemented between 2010-2013. Column (2) presents the average value for the treatment×post group that actually became eligible for Medicaid through the ACA expansion. Columns (3) and (4) show weighted means for households that would not have met eligibility criteria for the ACA Medicaid expansion in any year. Standard deviations in parentheses.

4.4 Trends in Household OOP Medical Spending

Out-of-pocket medical expenditures comprise a significant share of the household budget. Figure 4.1 displays this share in percent according to the household gross income before the implementation of the central reform provisions. Income groups are divided according to their AGI in relation to the federal poverty line (FPL), as the FPL determines eligibility for each of the three reform provisions. The left panel shows the average over all households and the right panel only those households with positive OOP values, as

Figure 4.1: OOP Medical Expenditures as a Share of Household Gross Income



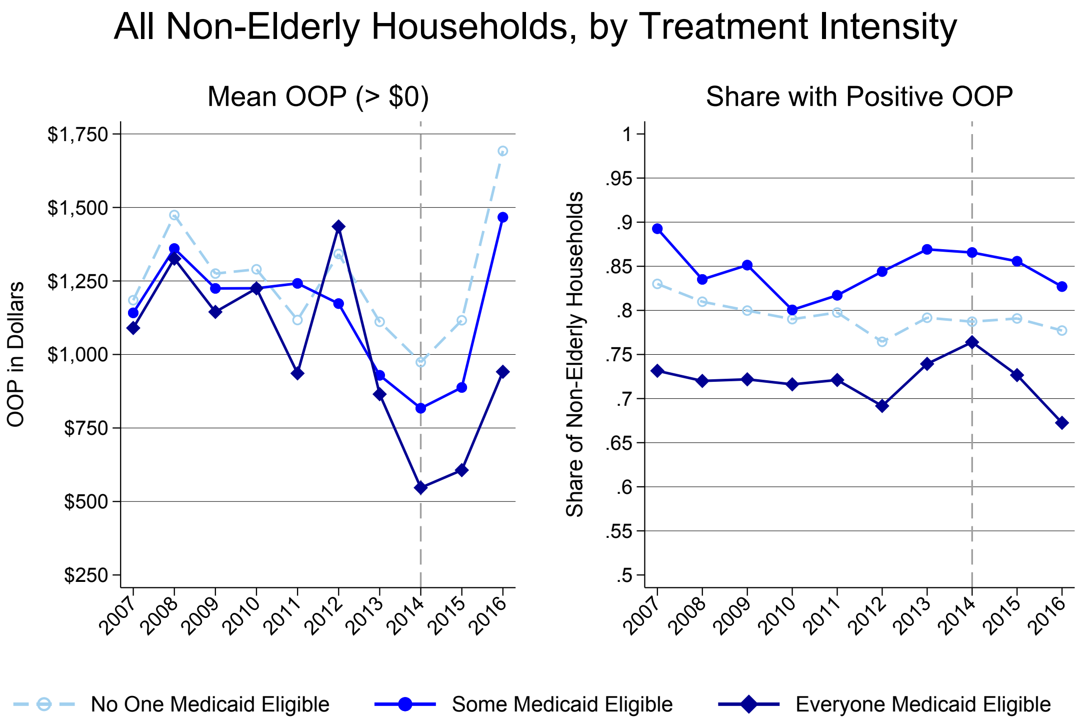
Source: MEPS 2007-2013. Weighted shares for all ages using MEPS household weights.

many households have zero OOP medical expenditures in most years. Zero expenditures may ensue because households do not have any medical events or because public insurance covers both the premium and any medical costs. For the lowest income (per capita) households, total OOP including any insurance premiums comprises over 18 percent of total household gross income in the event that the household has any OOP medical expenses at all. Both the share of gross household income spent on premium costs and that spent on OOP medical expenses (excluding premiums) decreases as income increases. This fact simply reflects that the difference in gross income surpasses the difference in health care expenditures.[29] In absolute terms, however, both OOP medical expenditures and premiums tend to increase with income, as seen in Figures 4.2 and 4.3. As mentioned above, this observation likely reflects higher utilization of health care for higher income households, as health care is a normal good.

Figures 4.2 and 4.3 show the evolution of OOP medical expenditures (excluding premiums) and OOP insurance premium costs, respectively, by household treatment intensity. The left panel includes all households with positive OOP medical expenditures and the right panel plots the share of households with positive payments. Analogously to the causal analysis, the share of the HIU eligible for Medicaid refers to the share eli-

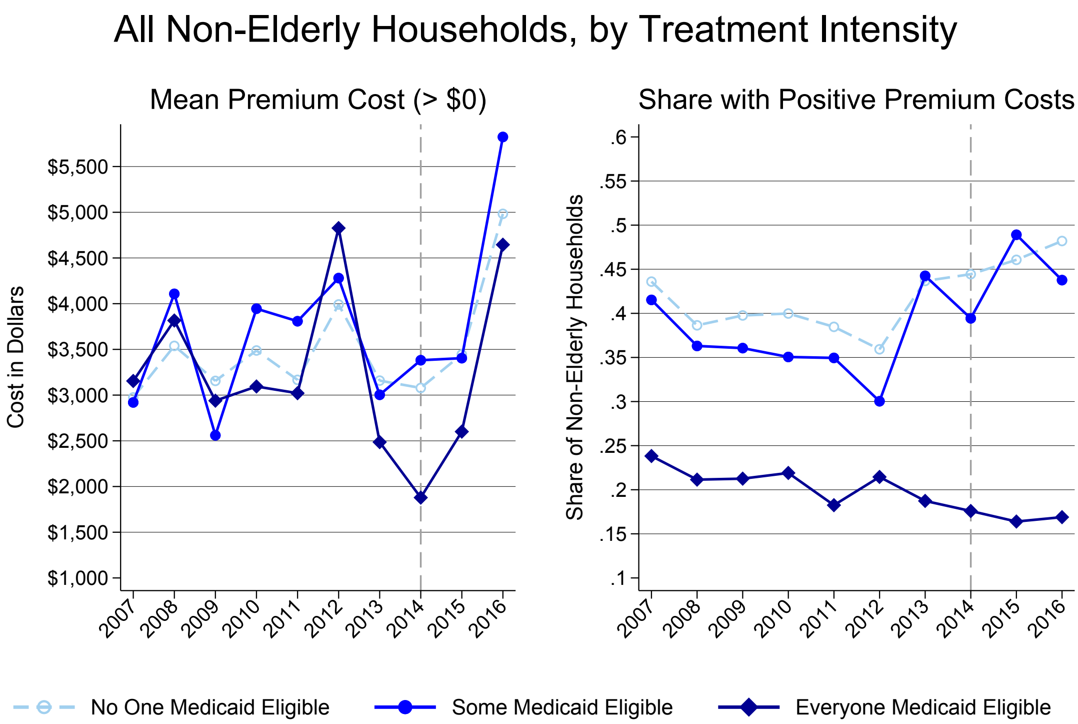
[29]The graph excludes households reporting zero income, but positive expenditures. Imposing an income floor of $1,000 does not change the general message of the composition chart.

Figure 4.2: Household OOP Medical Expenditures (Excluding Premiums)



Source: MEPS 2007-2016. Weighted shares for all ages using MEPS household weights. Dollar amounts have been adjusted to constant 2017 dollars using the CPI-med.

Figure 4.3: Household Insurance Premiums



Notes: See Notes from Figure 4.2.

gible according to the ACA 2014-2016 Medicaid rules in each state and does not include individuals already eligible for Medicaid under rules preceding the ACA reform. These shares are then separated into treatment intensity categories of households in which no one became eligible, households where some but not all members gained eligibility and those where every member of the household became eligible. Despite their volatility, mean OOP medical expenditures remained similar for households in all treatment intensity groups until 2014 when ACA Medicaid expansion took effect, after which these trends began to separate in the expected direction. The right panel of Figure 4.2 shows that the group with the highest treatment intensity had the lowest share of households paying for any medical expenditures out of pocket, both before and after the reform. The most striking observation in Figure 4.3 is that the share of households paying insurance premiums increased for the groups of households with no or low treatment intensity and decreased for households with the highest treatment intensity.

In order to take a closer look at household medical spending patters before and after the reform in treatment and control groups, Table 4.2 displays weighted means for four groups: column (1) presents the average value for households that would have been eligible according to the ACA rules, had the reform been implemented between 2010-2013; column (2) presents the average value for the treatment$\times$post group that actually became eligible for Medicaid through the ACA expansion; columns (3) and (4) show weighted means for households that would not have met eligibility criteria for the ACA Medicaid expansion in any year. As such, column (2) shows the average expenditures and shares with any expenditures for the treatment group.[30]

The first two rows of Table 4.2 show the summary measure of total OOP, including OOP medical expenditures for medical services, care and products as well as costs of any insurance premiums. Rows 3-5 and 6-8 separate the two components of total OOP into OOP expenditures for services, care or products and OOP expenditures for insurance costs, respectively. Households with at least one member meeting the ACA eligibility requirements are slightly less likely to spend any money out of pocket on medical services, care or products, albeit with only a 3-4 percentage point difference compared to households in which no one meets eligibility criteria. 'Would be' Medicaid eligible households (column (1)) with positive OOP medical spending (excluding premiums) spent slightly less than their non-eligible counterparts *before* expansion. However, while mean spending remained remarkably constant for non-eligible households, eligible households experienced a stark

[30] Note that the table shows an average for all treated households, defined as having at least one member become eligible for Medicaid expansion, while the causal analysis additionally uses variation in treatment intensity, as some households experienced a larger portion of their HIU becoming eligible.

reduction in OOP spending after 2014, in line with public insurance covering a large portion of their medical expenses.

In the pre-reform baseline period from 2010-2013, a much lower share of Medicaid-eligible households spent any amount on insurance premiums compared to non-eligible households and this share does not change significantly after the reform for this group. This observation can likely be explained by the fact that previously uninsured individuals transitioned into public insurance in 2014, as depicted in Appendix Figure 4.A1, and neither category would exhibit positive premium spending. In contrast, the share of non-eligible households with positive insurance payments substantially increased by 7 percentage points, as many of these households qualify subsidies on the private insurance exchange market and are subject to the individual mandate penalty in the case of remaining uninsured. These descriptive trends are reflected in the causal analysis, which finds the largest reductions for Medicaid-eligible households on the intensive margin of OOP medical spending on care, services and products as well as the extensive margin of insurance premium costs.

Table 4.3 displays the means of the main policy variables of interest that measure exposure to Medicaid, private insurance exchange subsidies and mandate penalties from 2010-2016. The first two columns show means of the variables calculated on the basis of household composition, income and state as observed in the MEPS data for each household while the third and fourth columns reflect simulated eligibility as described in detail in section 4.3.3.3. The table shows that, on average, 17-18% of the household is eligible for Medicaid according to pre-ACA rules.[31] The causal analysis controls for the share of the household eligible under previous Medicaid rules, but is not primarily concerned with possible impacts on this group.[32] The average share of the household becoming newly eligible for ACA Medicaid expansion is 13-17%. Average unsubsidized insurance premiums are included in the table for reference and are simply average observed benchmark premiums for the lowest cost silver plan. The ratio of the subsidy to the unsubsidized benchmark plan yields the average share of the premium that would be reimbursed by the government according to the rules of 2014-2016. The share of the unsubsidized premium covered is rather substantial, ranging from 30-42% in 2014 and 2015 for the 6-12% of the population eligible to receive a subsidy.

[31] Note that this share is different from the share of households in the population eligible for Medicaid. The average share of the household eligible is preferred to the share of households because it is the variable used in the causal analysis and captures treatment intensity rather than using a binary variable for treatment.

[32] While this group did not experience any change in eligibility, they plausibly received other types of treatments, such as increased information regarding application procedures due to the salience of the reform or a decrease in stigmatization due to the larger share of the population enrolling in Medicaid.

Table 4.2: OOP Medical Expenditures of Treatment and Control Households, 2010-2016

	According to the ACA Medicaid Rules from 2014-2016:			
	Medicaid Eligible 2010-2013	Medicaid Eligible 2014-2016	Not Eligible 2010-2013	Not Eligible 2014-2016
	(1)	(2)	(3)	(4)
- Household total OOP ($)	1,561.32	1,347.76	2,187.78	2,521.78
	(3,255.66)	(2,710.79)	(3,530.69)	(3,877.09)
- Household total OOP if > $0	2,008.42	1,705.68	2,635.87	3,014.85
	(3,568.91)	(2,947.86)	(3,719.94)	(4,060.11)
- Share with pos. OOP (excluding premiums)	0.75	0.76	0.79	0.79
- Household OOP ($) (excluding premiums)	790.10	553.65	896.59	896.98
	(2,168.24)	(1,232.58)	(2,076.29)	(2,078.18)
- Household OOP if > $0 (excluding premiums)	1,052.75	728.78	1,139.33	1,140.31
	(2,447.02)	(1,368.33)	(2,280.71)	(2,283.21)
- Share with pos. ins. premium cost	0.24	0.25	0.39	0.46
- Household OOP premium costs if > $0	3,189.36	3,199.60	3,271.87	3,498.69
	(3276.48)	(3188.94)	(3226.52)	(3349.30)
- Household OOP ($) premium costs	771.21	794.11	1,291.20	1,624.81
	(2,111.79)	(2,105.37)	(2,581.78)	(2,872.99)
Observations	6,778	3,941	36,096	25,882

Notes: See notes from Table 4.1. OOP = out-of-pocket medical expenditures. Values have been adjusted to the CPI-med and are presented in constant 2017 dollars.

Finally, the individual mandate penalty represents the provision that theoretically applies to the largest portion of the population, with 51-60% of the sample subject to a penalty. Nevertheless, as discussed in section 4.3.1, the penalty is only binding for individuals without ESI. Appendix Figure 4.A1 demonstrates, however, that roughly 55% of the population obtains insurance coverage through their employer, their partner's or parent's employer and this share did not change after 2014. De facto, the penalty likely proves binding only for uninsured individuals and those with non-group private insurance.

Table 4.3: Observed and Simulated Eligibility for Policy Variables

	Observed		Simulated	
	2010-2013	2014-2016	2010-2013	2014-2016
Medicaid Eligibility:				
- Pre-ACA eligible	0.17	0.18	0.17	0.18
- ACA newly eligible	0.15	0.13	0.17	0.15
Exchange Subsidies:				
- Unsub. premium ($)	3,659.99	3,624.99	3,659.99	3,624.99
	(38.92)	(22.22)	(38.92)	(22.22)
- Share subsidy (if >0)	0.40	0.42	0.30	0.32
- Share of households eligible	0.06	0.06	0.12	0.11
Mandate Penalties				
- Share subject to penalty	0.51	0.52	0.60	0.60
-Avg. penalty (if >0) ($)	736.08	848.17	624.61	735.09
	(329.72)	(408.07)	(349.26)	(416.98)

Source: MEPS-HC and MEPS-IC 2010-2016. Standard deviations in parentheses. The average unsubsidized premium does not represent the amount actually paid, but rather the second lowest cost premium plan among the Silver tier category for the household, given the age and composition of household members, as this benchmark plan determines the eligible subsidy amount.

4.5 Results

4.5.1 Out-of-Pocket Expenditures

Table 4.4 displays the regression results of all policy interaction terms of interest ($\theta_2 - \theta_4$) from the DD and DDD equations for the composite measure of total OOP medical expenditures, which includes both expenditures for medical care and for insurance policy premium costs. Tables 4.5 and 4.6 provide results, respectively, for household OOP medical expenditures (excluding premium costs) and OOP insurance premium costs. In addition to presenting results for the log-transformed dependent variable that captures intensive margin effects on OOP, results tables include the inverse hyperbolic sine (IHS) of OOP in order to retain valuable information contained in zero expenditure observations, as the descriptive statistics showed the largest changes on the intensive margin of OOP medical expenditures for care, services and products, but on the extensive margin for insurance premium costs. The IHS is defined as $IHS(OOP) = log(OOP + (OOP^2 + 1)^{1/2})$ and has the advantage that it, like the logarithmic function can be used to approximate percent changes while not excluding values of zero (Burbidge et al., 1988; Pence, 2006; Ravallion, 2017; Barcellos and Jacobson, 2015). It therefore captures a combined response at the intensive and extensive margins.

The impact of the main explanatory variable of interest, the share of the household eligible for ACA Medicaid expansion, can be found in row 1 for all results tables, followed by the impact of subsidy eligibility and exposure to individual mandate penalties. Columns (1)-(2) present results from the DD OLS regression and columns (3)-(4) the same for the DD simulated instrumental regression. Columns (5)-(6) show robustness to a full DDD OLS regression and columns (7) - (8) the DDD simulated instrument specification. All results contain state and year fixed effects, local county unemployment rate controls, fixed effects for the 8 income groups and 4 household types as well as a full set of demographic controls including age, sex, race and ethnic background of the head of household. Columns (5)-(8) add interaction terms for $INCGR \times POST$ and $STATE \times POST$ fixed effects.

For the group targeted by the ACA expansion, denoted $MCAID^{ACA} \times POST$, induces a large reduction in total OOP, which is greatest on the intensive margin. Column (2) shows that one standard deviation increase in the share of the household becoming eligible for Medicaid (roughly 0.32, from Table 4.3), or roughly the equivalent of one additional person in a family of three, amounts to a 11.2% decrease in OOP $(0.32 \times (\exp^{-0.429} - 1))$. With an average total OOP expenditure of \$2,008.42 in the pre-reform period, this savings amounts to roughly \$224 annually for a Medicaid-eligible household with positive medical expenditures. Results for OLS and simulated instrument regressions are similar and we cannot reject the equality of coefficients across these specifications. I therefore interpret the OLS results as the preferred specification and focus on the OLS DD and DDD results in the following tables.

Table 4.4: Results for Total OOP: Pooled OLS vs Simulated IV

	Difference-in-Difference (DD)				Difference-in-Difference-in-Difference (DDD)			
	OLS		Simulated IV		OLS		Simulated IV	
	(IHS)	(log)	(IHS)	(log)	(IHS)	(log)	(IHS)	(log)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medicaid×post	-0.242***	-0.429***	-0.178*	-0.372***	-0.064	-0.264***	-0.174	-0.304***
	(0.089)	(0.070)	(0.092)	(0.074)	(0.135)	(0.091)	(0.148)	(0.106)
Subsidy×post	0.050***	0.030***	0.058***	0.039***	0.030**	0.026**	0.067***	0.061***
	(0.011)	(0.010)	(0.013)	(0.012)	(0.015)	(0.012)	(0.025)	(0.018)
Penalty×post	0.037***	0.029***	0.049***	0.038***	0.022	0.016	0.115**	0.081**
	(0.010)	(0.008)	(0.012)	(0.009)	(0.017)	(0.016)	(0.051)	(0.036)
State×post controls					✓	✓	✓	✓
Income group×post controls					✓	✓	✓	✓
$\bar{R}^2$	0.260	0.257			0.260	0.256		
AR-Statistic (H_0=weak IVs)			21.86**	27.22***			22.82**	28.05***
Observations	72,697	56,261	72,697	56,261	72,697	56,261	72,697	56,261

Source: MEPS cross-sectional data 2010-2016. Post = years 2014-2016. Weighted regression results using household sample weights. All regressions contain controls for year and state fixed effects, local county unemployment rate, household income group, household type (single, couple without children, family with children) and the race/ethnic origin (non-exclusive dummies for black, white, Hispanic) and age of the head of household. Columns (1)-(2) present results from the difference-in-difference (DD) OLS specification and columns (3)-(4) show results from the DD simulated IV. Results for the difference-in-difference-in-differences specifications can be found in columns (5)-(8): the OLS version in columns (5) and (6) and the simulated IV in columns (7) and (8). The dependent variable for results in columns (1),(3),(5) and (7) is transformed by inverse hyperbolic sine (IHS) to capture intensive and extensive margin changes including zeros while remaining columns show results from a log-transformed dependent variable. Standard errors are clustered at the state level. AR-Statistic = Anderson-Rubin first stage statistic testing for weak instruments.

Table 4.5: Results for OOP Excluding Premium Costs

	Difference-in-Difference (DD)			Difference-in-Difference-in-Difference (DDD)		
	(IHS)	(log)	(log)	(IHS)	(log)	(log)
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid×post	-0.151	-0.320***	-0.340***	-0.198	-0.278***	-0.269***
	(0.095)	(0.066)	(0.065)	(0.129)	(0.089)	(0.093)
Subsidy×post	0.017	0.012	0.016**	0.011	0.011	0.012
	(0.010)	(0.008)	(0.007)	(0.014)	(0.009)	(0.008)
Penalty×post	0.022*	0.022**	0.023***	0.031	0.014	0.011
	(0.011)	(0.008)	(0.008)	(0.019)	(0.012)	(0.012)
Conditional on utilization			✓			✓
State×post controls				✓	✓	✓
Income group×post controls				✓	✓	✓
$\bar{R}^2$	0.230	0.183	0.234	0.230	0.183	0.233
Observations	72,697	53,122	53,122	72,697	53,122	53,122

Source: MEPS cross-sectional data 2010-2016. Post = years 2014-2016. Weighted regression results using household sample weights. All regressions contain controls for year and state fixed effects, local county unemployment rate, household income group, household type (single, couple without children, family with children) and the race/ethnic origin (non-exclusive dummies for black, white, Hispanic) and age of the head of household. All columns refer to OLS estimations. Columns (1)-(3) present results from the difference-in-difference (DD) specification, columns (3)-(6) show results from the DDD specification. The dependent variable for results in columns (1) and (4) is transformed by inverse hyperbolic sine (IHS) to capture intensive and extensive margin changes including zeros while remaining columns show results from a log-transformed dependent variable. Columns (3) and (6) add controls for the number of household visits to hospitals and doctors' offices. Standard errors are clustered at the state level.

A comparison of this composite measure of total OOP with Tables 4.5 and 4.6 demonstrates that the effect is driven by OOP for medical care and services as well as for insurance premium costs. Reductions in OOP spending on medical goods and services, as displayed in Table 4.5, stem from decreases in OOP among households with any positive spending, which can be seen comparing the log transformed dependent variable in columns (2)-(3) and (5)-(6) to the IHS results in columns (1) and (4). One standard deviation increase in the share of the household becoming eligible for Medicaid yields a mean reduction of 8.8% of OOP expenditures for medical goods and services ($0.32\times(\exp^{-0.320}-1)$). Among households with positive expenditures, Table 4.2 displays the pre-reform mean for positive OOP excluding premiums as \$1,052.75. As such, average OOP savings amount to approximately \$92 annually for Medicaid-eligible households. Results are robust to adding state×post and income group×post controls as well as to conditioning on health care utilization.

With respect to affordability of premium costs, results from the main DD specification in Table 4.6, columns (1)-(3) show that Medicaid eligible households experienced a reduction in OOP for insurance premia in the order of 12.0% for each additional standard deviation increase in the share eligible. Because the intensive margin results in columns (2) and (3) are small and insignificant while the IHS formulation yields a large negative and statistically highly significant effect, results suggest that the reduction in premia for Medicaid eligible households is driven by extensive margin adjustments, namely by households switching from non-zero private insurance premia to Medicaid coverage that does not charge a premium.

While Medicaid targets the poorest households and subsidies a broader range of low-middle income households, the penalty is the most binding for households at higher positions in the taxable income distribution. In contrast to Medicaid eligibility, exposure to exchange subsidies and mandate penalties actually increased household total OOP, which is driven by extensive margin changes in insurance premium costs. Table 4.6 demonstrates that a 10 percent increase in the potential subsidy amount increases OOP premium costs by 1.4% ($0.10\times(\exp^{0.127}-1)$), or roughly \$43 annually, for the mean household OOP premium for Medicaid-eligible households shown in column (1) of Table 4.2. Likewise, a 10% increase in the potential penalty amount increases premium costs by 0.4%. The small effect of the penalty could be attributed to the fact that the lowest income households tend to be exempt from the penalty or privy to Medicaid coverage. The progressive nature of the mandate penalty makes it most binding for higher-income households, which are not part of this analysis.

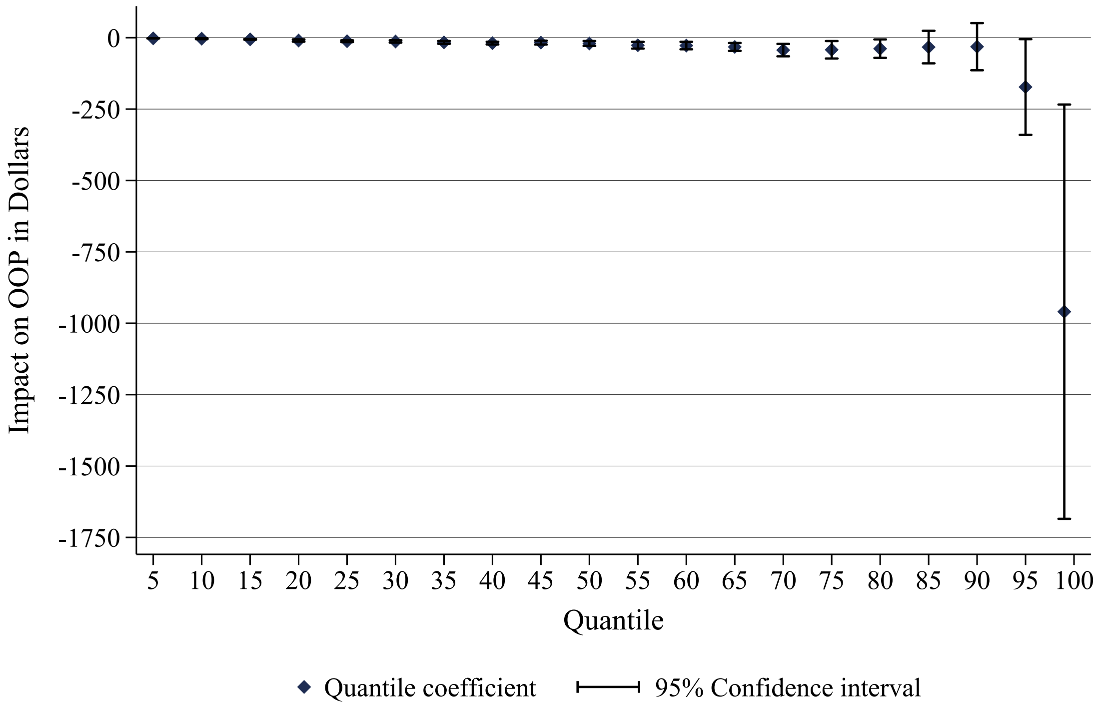
Table 4.6: Results for Premium Costs

	Difference-in-Difference (DD)			Difference-in-Difference-in-Difference (DDD)		
	(IHS)	(log)	(log)	(IHS)	(log)	(log)
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid×post	-0.471***	-0.073	-0.077	0.154	-0.040	-0.042
	(0.126)	(0.068)	(0.069)	(0.167)	(0.069)	(0.070)
Subsidy×post	0.127***	0.001	0.001	0.108***	0.001	0.001
	(0.024)	(0.006)	(0.007)	(0.027)	(0.007)	(0.007)
Penalty×post	0.042***	0.015**	0.015**	-0.016	0.014	0.014
	(0.011)	(0.006)	(0.006)	(0.027)	(0.014)	(0.013)
Conditional on utilization			✓			✓
State×post controls				✓	✓	✓
Income group×post controls				✓	✓	✓
$\bar{R}^2$	0.246	0.164	0.165	0.247	0.166	0.167
Observations	72,697	23,537	23,537	72,697	23,537	23,537

See notes for Table 4.5.

Figure 4.4 shows results from quantile regressions of out-of-pocket expenditures. For exposition purposes, these regressions use the level of OOP in dollars rather than the IHS or log specification on the left-hand side in order to ease interpretation of the magnitude of

Figure 4.4: Quantile Regressions



Source: MEPS 2010-2016. Regression results from OLS DD quantile regressions of total OOP (including care and policy payments) on the full set of regressors listed in equation 4.3 and conditional on the number of visits to doctors and hospitals. Point estimates display the effect of Medicaid expansion, θ_1. Confidence intervals are based on robust standard errors.

effects. The quantile point estimates show the effect of ACA Medicaid eligibility on total household OOP at each ventile of the total OOP distribution. The figure demonstrates that the mean OOP reduction is driven by high-cost medical expenditures in the 95th and 99th percentiles of the distribution. For the median household, no effect on OOP can be detected. Results of the quantile regressions thus demonstrate the relevance of high-cost medical events for the Medicaid eligible households.

4.5.2 Risk Protection

Given the large impact of medical costs in the upper quantiles of the distribution, this section evaluates the contribution of Medicaid expansion on risk protection from large medical payments and attempts to quantify willingness to pay for this additional risk protection as a measure of welfare gain. I employ a similar approach to that used in previous work evaluating the welfare gains from Medicare and other pension insurance reforms (Feldstein and Gruber (1995); Finkelstein and McKnight (2008); Engelhardt and

Gruber (2011);Shigeoka (2014); Barcellos and Jacobson (2015)). The basic model involves a one-period utility maximization problem subject to the household budget constraint:

$$c = y - m^{oop} \tag{4.4}$$

where non-health consumption, c, is defined as household income, y, less OOP medical expenditures, m^{oop}. m^{oop} is a random variable with a probability density function $f(m^{oop})$ along the support of $[0,\bar{m}^{oop}]$. The household's expected utility can then be stated as:

$$\int_0^{\bar{m}^{oop}} u(y - m^{oop}) f(m^{oop}) dm^{oop} \tag{4.5}$$

The value a household places on risk protection from Medicaid insurance is captured by the risk premium, π, which places an amount on the household's willingness to pay in order to completely insure itself against the random variable m^{oop}. In other words, it is the difference between the certainty equivalence (CE) of non-health consumption and expected consumption and can be defined for two possible states of the world: one in which the household is eligible for Medicaid, $s = 1$ and one in which no one if the household is eligible $s = 0$. The risk premium for each household π_h is then implicitly defined by the following equation:

$$u(CE_s) = u(y - E[m_s^{oop}] - \pi_s) = \int_0^{\bar{m}_s^{oop}} u(y - m_s^{oop}) f(m_s^{oop}) dm_s^{oop};\ s = 0, 1 \tag{4.6}$$

By incorporating the causal treatment effects from the quantile regressions, it becomes possible to measure the value of risk protection under Medicaid insurance as the difference in the CE under Medicaid eligibility, CE_1, and under a counterfactual situation without it, CE_0:

$$\Delta CE = (\pi_0 - \pi_1) + (E[m_0^{oop}] - E[m_1^{oop}]) \tag{4.7}$$

In order to approximate ΔCE, I first predict the out-of-pocket distribution of expenditures with Medicaid eligibility, $\hat{m}_{1,h}^{oop}$, and without, $\hat{m}_{0,h}^{oop}$ for each household and percentile j in the sample, conditional on observable characteristics. For exposition purposes, I consolidate the notation for the control variables from equation 4.3 and make a linear prediction of $\hat{m}^{oop}_{0,h}$ using the coefficients from the quantile regressions in each $j = 1/99$ quantiles:[33]

$$\hat{m}^{oop,j}_{0,h} = \hat{\alpha}^j + \hat{\beta}^j MCAID^{ACA}_h + \boldsymbol{r}_c\hat{\lambda}^j + \hat{\gamma}^j T + \boldsymbol{X}_h\hat{\xi}^j \tag{4.8}$$

where, as in equation 4.3, $MCAID^{ACA}_h$ defines the average share of the household that would be eligible for Medicaid according to rules from 2014-2016, $\boldsymbol{X}_h$ is a matrix containing the observable household characteristics, $\boldsymbol{r}_c$ are regional controls, including the local county unemployment rate and state fixed effects, and T are year fixed effects from 2010-2016.[34] Predicted out-of-pocket expenditures *with* Medicaid, $\hat{m}^{oop,j}_{1,h}$ then equate to:

$$\hat{m}^{oop,j}_{1,h} = \hat{\alpha}^j + \hat{\beta}^j MCAID^{ACA}_h + \hat{\theta}^j MCAID^{ACA}_h \times POST + \boldsymbol{r}_c\hat{\lambda}^j + \hat{\gamma}^j T + \boldsymbol{X}_h\hat{\xi}^j, \tag{4.9}$$

in which $\hat{\theta}^j$ captures the quantile treatment effects of the policy interaction term for the ACA Medicaid expansion.

Using the predicted distributions in the treated and counterfactual situations, we can now formulate equation 4.6 for π_0 and π_1.

$$u(y - \bar{\hat{m}}_{s,h} - \hat{\pi}_s) = \frac{1}{99}\sum_{j=1}^{99} u(y - \hat{m}^j_s);\ s = 0, 1 \tag{4.10}$$

where $\bar{\hat{m}}_{s,h}$ is the expected value of OOP based on 99 predictions (one from each quantile) for each household and each state s. Assuming a CRRA utility function with risk aversion parameters of 1,3,5 allows us to solve this equation for $\pi_{s,h}$. Equipped with this last parameter, the willingness to pay of each household for risk protection of Medicaid can be calculated as the sum of $\Delta\pi_h$ and $\Delta\bar{\hat{m}}_h$. Averaging over the entire sample of households yields the parameter of interest, the average welfare gain from Medicaid risk protection.

I calculate the change in CE at the mean of the sample of all households as well as at higher quantiles and present results in Table 4.7. Table 4.7 demonstrates that, much like the case of the mean benefits of Medicaid on reducing expected OOP expenditures, the value of risk protection is highly right-skewed, indicating that very large expenditures in the upper tail of the distribution drive the positive value of risk protection, which ACA Medicaid eligible families value at \$420.26 in the 95th percentile and \$695.11 in the 99th percentile of OOP expenditures per additionally eligible family member. The value of risk

[33]In order to make the benefit calculation comparable to the cost, measured per person, the quantile regressions for the benefit analysis define Medicaid treatment as the number of people in the household eligible rather the share of people in the household. This adjustment enables the interpretation of the treatment effect coefficient as the OOP expenditure effect of one additional person in the household becoming eligible for Medicaid.

[34]While all estimations at the mean contain county fixed effects, computational constraints only allow me to include state fixed effects for the quantile regressions.

protection in this calculation, however, is likely understated because the OOP medical expenditures include the behavioral response of increased utilization.

Table 4.7: Welfare Gain from Medicaid Risk Protection

		Using Quantile Estimates
Mean Effects		
Risk aversion:	1	\$57.34
	3	\$125.85
	5	\$256.36
Distribution		
risk aversion = 3		
	25th percentile	\$46.56
	Median	\$92.76
	75th percentile	\$141.46
	90th percentile	\$268.44
	95th percentile	\$420.26
	99th percentile	\$695.11

Source: MEPS cross-sectional data 2010-2016. Values listed in 2017 CPI-med-adjusted dollars. Calculations are based on quantile regressions at each percentile of the conditional OOP medical expenditure distribution (excluding premiums). Control variables include household income band and type, the number of people in the household with a chronic disease as well as the age, race (dummy for black) and Hispanic background of the household head. Regressions also include year and state fixed effects.

4.5.3 Charity Care and Other Sources of Payment

Prior to the reform, its proponents argued that insuring low-income households would reduce the formidably expensive and inefficient costs of charity care to the taxpayer through several channels. First, different state and federal programs either mandate care to patients without the ability to pay for treatment or offer strong monetary incentives to do so by reimbursing facilities for uncompensated care or offering them tax benefits or subsides, which government finances through taxes. Second, hospitals often charge paying customers a surcharge to recover part of their losses from uncompensated care (Qin and Liu, 2013). Many uninsured Americans have relied on this unofficial safety net for decades, beginning with the Hill-Burton Act in 1946, when non-profit hospitals became legally required to provide a certain amount of uncompensated care to those unable to pay in exchange for government funding. In 1986, the Federal Emergency Medical Treatment and

Active Labor Act (EMTALA) further strengthened the role of charity care by mandating all ERs to treat patients regardless of their ability to pay. Herring (2005) and Gruber and Rodríguez (2007) estimate national pre-reform charity care costs at somewhere between $3.2 and $27 billion annually. Against this background, I investigate whether the reform reduced charity care expenditures.

I define charity care in the MEPS data as the sum of total medical expenditures not covered by the patient or the patient's family (OOP expenditures), any type of insurance, Medicaid or Medicare. Furthermore, I consider changes to the share of total household expenditures for medical care and services (excluding premiums) paid by one of three mutually exclusive categories: household OOP, private insurance and any public source, including charity care, Medicaid, and Medicare. Table 4.8 shows conditional means of these variables in the pre- and post-reform periods. Total medical expenditures paid on behalf of households from any source have increased in both treatment and control groups and average expenditures are higher among Medicaid-eligible (according to ACA rules from 2014-2016) compared to non-eligible households both before and after the reform. Eligible households moreover cover a larger fraction of their total medical expenses through public sources and a smaller fraction through private insurance compared to non-eligible households.

Table 4.9 displays the marginal effects of exposure to Medicaid expansion on the proportion of total expenditures paid by or on behalf of households covered by each source of payment discussed above. The left panel focuses on the amount paid by charity sources and shows a stark reduction in charity care expenditures on behalf of households eligible for the ACA Medicaid expansion. One standard deviation increase in Medicaid eligibility decreases the amount of charity care coverage by 9.3% $(0.32\times(\exp^{-0.343}-1))$ in the IHS specification and by 10.4% in the log specification. While the ACA Medicare expansion decreased the amount of charity care paid on behalf of Medicaid-eligible households, it also increased the share of total expenditures paid by public sources overall, including charity care. Column (5) documents a 10.9% increase for one standard deviation increase in eligibility $(0.32\times(\exp^{0.294}-1))$. It likewise reduced the share paid OOP by 7.2% and the share covered through private insurance by 4.6%, indicating some crowd-out of private insurance by public sources. As such, the reform did reduce the tax-payer burden for charity care, but increased the total tax-payer burden on net, due to increases in the shares formally covered by Medicaid. In section 4.7, I calculate the magnitude of the costs to taxpayers of expanding Medicaid public insurance, highlighting many of the cost components discussed above.

Table 4.8: Conditional Means of Sources of Payment Variables

	According to the ACA Medicaid Rules from 2014-2016:			
	Medicaid Eligible 2010-2013	Medicaid Eligible 2014-2016	Not Eligible 2010-2013	Not Eligible 2014-2016
	(1)	(2)	(3)	(4)
- Total household med. expenditures ($)	5,721.76	6,323.11	3,827.64	4,292.11
	(17,690.26)	(17,724.79)	(11,705.94)	(15,647.64)
- Amt. paid by charity care ($)	331.67	375.48	199.47	152.39
	(2,688.35)	(5,570.74)	(2,957.45)	(1,668.90)
- Share paid by charity care	0.07	0.04	0.05	0.04
	(0.20)	(0.14)	(0.17)	(0.14)
- Share paid by public source	0.39	0.49	0.28	0.30
	(0.43)	(0.44)	(0.39)	(0.40)
- Share paid by private ins.	0.32	0.31	0.42	0.45
	(0.37)	(0.37)	(0.38)	(0.38)
- Share paid out of pocket	0.29	0.20	0.30	0.25
	(0.34)	(0.28)	(0.32)	(0.28)
Observations	6,778	3,941	36,096	25,882

Source: MEPS cross-sectional data 2010-2016. See notes from Table 4.A3.

Table 4.9: Sources of Payment for Household Medical Expenditures: Difference-in-Difference OLS Results

	Total amount paid by charity care		*Share of total expenditures paid by:*			
			charity care	OOP	public sources	private ins.
	(IHS)	(log)	(log)	(log)	(log)	(log)
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid×post	-0.343***	-0.391*	-0.339	-0.253***	0.294***	-0.157**
	(0.100)	(0.207)	(0.211)	(0.081)	(0.080)	(0.071)
Subsidy×post	-0.008	-0.019	-0.008	0.021***	-0.001	-0.001
	(0.016)	(0.013)	(0.015)	(0.007)	(0.009)	(0.007)
Penalty×post	0.021**	0.004	0.005	0.028***	-0.007	-0.004
	(0.008)	(0.013)	(0.014)	(0.007)	(0.011)	(0.007)
$\bar{R}^2$	0.055	0.032	0.098	0.136	0.214	0.123
Observations	72,697	13,817	13,817	53,122	36,448	36,306

Source: MEPS cross-sectional data 2010-2016.

4.5.4 Health Care Utilization

Due to the possible substitution effects discussed in section 4.1, which could increase utilization of health care goods and services, this section examines to what extent the reform in fact altered patterns of utilization, which in turn affect OOP. Rich information regarding health care utilization and the number of visits to different types of service providers allows me to distinguish between types of care such as emergency room visits, inpatient hospital stays, outpatient facility visits and visits to regular doctors' offices. Table 4.10 provides conditional means for medical service utilization variables according to treatment and control groups and Table 4.11 shows causal effects from the OLS DD analysis of Medicaid eligibility on the probability of any family member having at least one visit to any of these types of providers as well as the total number of annual visits. In

Table 4.10: Medical Service Utilization: Conditional Means

	According to the ACA Medicaid Rules from 2014-2016:			
	Medicaid Eligible 2010-2013	Medicaid Eligible 2014-2016	Not Eligible 2010-2013	Not Eligible 2014-2016
	(1)	(2)	(3)	(4)
- Share with any ER visit	0.24	0.28	0.23	0.24
	(0.43)	(0.45)	(0.42)	(0.43)
- Number of ER visits	1.68	1.79	1.64	1.68
(if> 0)	(1.31)	(1.62)	(1.25)	(1.28)
- Share with any inpatient visit	0.10	0.11	0.10	0.10
- Number of hospital	1.51	1.54	1.31	1.38
inpatient visits	(1.09)	(1.06)	(0.73)	(0.84)
- Share with any outpatient visit	0.20	0.26	0.17	0.21
- Number of outpatient	3.39	4.34	2.74	3.35
hospital visits	(6.27)	(9.57)	(4.92)	(6.87)
- Share with any office visit	0.72	0.75	0.76	0.78
- Number of physician	10.10	13.20	9.30	11.13
office visits	(16.39)	(25.15)	(14.08)	(17.03)
Observations	6,778	3,941	36,096	25,882

Notes: See notes from Table 4.1. ER = emergency room.

general, both treatment and control groups increased their utilization of medical services in the post-reform years. The Medicaid-eligible population tends to utilize medical services slightly more than non-eligible households, both before and after the reform took effect.

In the causal analysis, I do not detect any change in utilization of hospitals, including emergency rooms, inpatient and outpatient facilities. Lawmakers intended for Medicaid expansion to decrease inefficiently costly utilization of emergency rooms for non-urgent care and I do not find evidence that this reduction took place at a statistically significant level. However, Siddiqui et al. (2015) investigate the response from Medicaid recipients to a previous reform that introduced co-payments for non-urgent ER visits and find that a lack of available physicians in poor neighborhoods prevented Medicaid recipients from switching from the ER to regular physicians, which may also explain why I did not find a stronger reduction in ER visits for the ACA Medicaid expansion: insurance provision grants recipients access, but access does not materialize without adequate supply of providers.[35]

Despite finding no effect on hospital utilization, column (7) of Table 4.11 documents a statistically significant but economically negligible increase in the probability of having any doctor's visit among individuals newly eligible for the ACA Medicaid expansion. A standard deviation (0.32) increase in the share of the household newly eligible for Medicaid results in a 0.011 increase in the probability of a visit. Given the 0.72 baseline share of households with any visit to a doctor's office in the past 12 months, shown in column (1) of Table 4.10, this increase on account of Medicaid expansion is very small. These findings suggest that individuals did not respond to public insurance eligibility by utilizing more health care goods and services (increasing consumption), but rather simply shifted the burden of payment for these goods and services when they did utilize them. They moreover offer further evidence to support the results for OOP medical expenditures in Section 4.5.1, which found a reduction for goods and services only on the intensive margin.

While Medicaid-eligible households did not appear to increase visits to health care providers, it is possible that the nature of utilization changed in the intended direction of more preventive care. In order to investigate this question, I consider changes to two different types of utilization: 1) access to care for acute medical needs and 2) preventive care services. Beginning with access to health care for acute needs, Appendix Table 4.A3 summarizes the share of households delaying or forgoing necessary medical care for financial reasons.[36] All items in the table stem from direct questions in the MEPS. The bottom panel of Appendix Table 4.A3 further shows the share of households reporting

[35]In the long-run, giving low-income households purchasing power in the form of health insurance may encourage more doctors to move into low-income neighborhoods. To what extent this occurs in the future remains to be seen.

[36]MEPS asks each respondent two questions: 1) whether they delayed or forwent care (for each type of care listed in the table) and 2) the reason for delaying or forgoing such care. The dummy variable at the individual level is coded as 1 if the respondent answered yes to both of these questions and zero otherwise. The table shows the share of households with at least one person answering positively to these two questions.

that they must travel more than 30 minutes in order to reach their usual care provider and the share reporting they lack access to a usual care provider. Access for acute care seems to have improved for both treatment and control groups during the post-reform period. Notably, only 18% of Medicaid-eligible households in the baseline period report forgoing or delaying acute treatment of any kind or not purchasing necessary drugs due to financial constraints.

Despite an upward trend in access to care for acute medical needs, the causal analysis indicates that these improvements cannot be attributed to Medicaid eligibility. Results displayed in Appendix Table 4.A4 fail to find any significant impact on these measures of access to care, with exception of a statistically significant but economically very minor decrease in the probability of reporting access to a usual care provider; a standard deviation increase in Medicaid eligibility (0.32) only decreases this share by 0.005.

Next, I investigate whether it is possible to detect an increase specifically in preventive care, which could yield long-run reductions in costs for the tax-payer through improved health outcomes. Appendix Table 4.A5 reports conditional means of the share of treatment and control households that utilized central preventive care services during the previous 12 months. While a general upward trend is visible for both treatment and control households, no notable difference exists between the Medicaid-eligible and non-eligible groups with respect to preventive care behavior, with the exception of dental checkups and blood pressure checks. Perhaps not surprising given the descriptive statistics, the causal analysis provided in Appendix Table 4.A6 does not find that ACA Medicaid eligibility elicited more preventive care behavior.

In sum, the results of the medical utilization analysis indicate neither increased access to urgent care nor increased utilization of preventive care services for the Medicaid-eligible population, both of which were central goals of the ACA expansion. With respect to urgent care, the absence of an effect can be explained in one of two ways: either Medicaid-eligible households lacked access to urgent care prior to the ACA and public insurance provision did not succeed in granting it to them or; low-income households previously had access to this type of care already prior to the reform. The relatively low share of the sample population reporting lacking access to care (Appendix Table 4.A3) may simply render finding significant average marginal effects difficult. The analysis in section 4.5.3 suggests that the availability of charity care may also play a partial role in explaining this outcome.

Table 4.11: Difference-in-Difference OLS Results: Medical Service Utilization

	Emergency Room		Inpatient Stay		Outpatient Facility		Physician Office		Share with usual care provider
	Any visit	Log Nr. of visits	Any visit	Log Nr. of visits	Any visit	Log Nr. of visits	Any visit	Log Nr. of visits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Medicaid×post	0.010	0.020	-0.008	-0.050	0.025	-0.026	0.035**	0.061	0.024
	(0.011)	(0.037)	(0.009)	(0.063)	(0.020)	(0.084)	(0.016)	(0.065)	(0.015)
Subsidy×post	0.001	-0.006	-0.004***	-0.012**	0.001	-0.013	0.002	0.000	0.001
	(0.003)	(0.004)	(0.001)	(0.005)	(0.003)	(0.008)	(0.002)	(0.006)	(0.002)
Penalty×post	-0.002	0.003	-0.001	-0.002	0.001	-0.001	0.003	-0.002	0.001
	(0.002)	(0.004)	(0.001)	(0.005)	(0.002)	(0.008)	(0.002)	(0.005)	(0.001)
$\bar{R}^2$	0.068	0.044	0.040	0.026	0.089	0.039	0.168	0.147	0.124
Observations	72,697	18,011	72,697	7,438	72,697	12,594	72,697	53,222	72,697

Source: MEPS cross-sectional data 2010-2016. Post = years 2014-2016. Weighted regression results using household sample weights. All regressions contain controls for year and state fixed effects, local county unemployment rate, household income group, household type (single, couple without children, family with children) and the race/ethnic origin (non-exclusive dummies for black, white, Hispanic) and age of the head of household. All columns refer to OLS estimations. Standard errors are clustered at the state level.

4.6 Heterogeneous Effects

Beyond the average treatment effects discussed in the preceding sections of this paper, this section highlights heterogeneous effects of the ACA Medicaid expansion for households with and without a pre-existing condition for the main outcomes of interest. These households not only had less access to insurance prior to the reform, but that they also paid more for insurance premiums when they were covered and have particularly high out of pocket payments for medical needs. Therefore, one would expect the average reductions shown in section 4.5.1 to be even larger for these households.

The MEPS data allows me to identify many of the most common chronic conditions used by insurance companies prior to the ACA in order to price discriminate among costumers or to deny coverage altogether.[37] These conditions include: heart attack, coronary heart disease, angina, other heart disease condition, stroke, emphysema, diabetes, arthritis, high blood pressure, asthma, high cholesterol, pregnancy, and extreme obesity (BMI$\geq$40). Table 4.12 displays mean OOP expenditures for medical goods and services as well as for insurance premium costs of households with at least one pre-existing condition (Panel A) juxtaposed to those without any pre-existing condition (Panel B). In line with moral hazard expectations, households in both treatment and control groups are more likely to purchase private insurance if they have a chronic condition. Likewise, premium costs and OOP expenditures for medical goods and services are higher for these households.

Figure 4.5 confirms that in fact households without chronic conditions are driving the mean OOP reductions observed in the population average among all Medicaid-eligible households. The top panel of the figure corresponds to Tables 4.4 and 4.5 and shows that the marginal effects for households without a pre-existing condition are insignificant while those for households with such a condition are slightly larger and more significant than in the population average. The most substantial difference between all Medicaid-eligible households and those with a pre-existing conditions can be seen on the extensive margin of OOP insurance premium costs. Whereas the average reduction amounted to 12.0%, the savings for households with a chronic condition reach 15.1% $(0.32\times(\exp^{-0.636}-1))$, or, given the much higher average premium cost shown in Table 4.12, the equivalent of roughly \$522 annually. While this effect can be interpreted as the additional impact from Medicaid expansion, above and beyond the underlying regulation changes of the ACA,

[37] For a more complete discussion and list of conditions see, for example, Fehr et al. (2018).

it should be noted that without changes to guaranteed access and community rating regulations, there may not have been any reduction for this subpopulation.[38]

The bottom panel of Figure 4.5 documents heterogeneous effects for the sources of payment outcomes by the presence of a chronic condition, which can be compared to those for the entire sample in Table 4.9. The reduction in the amount paid by charity care on behalf of Medicaid-eligible households is larger and more significant for those with a pre-existing condition. The increase in the share of total expenditures covered by public sources is also slightly larger for households with chronic conditions while the reduction in the fraction paid by private insurance becomes insignificant for this subgroup.

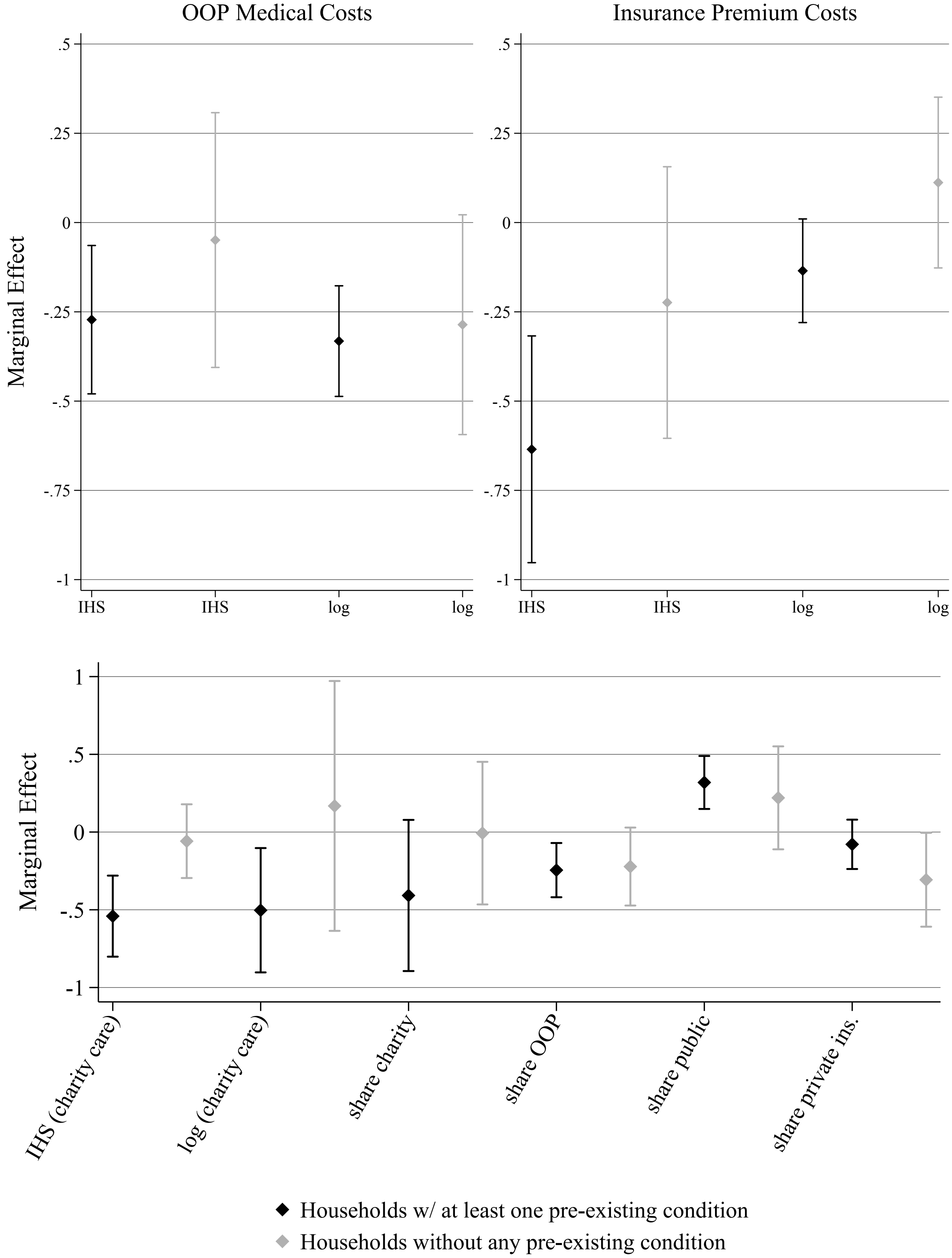
Table 4.12: Out of Pocket Expenditures by Pre-Existing Condition Status: Conditional Means

	According to the ACA Medicaid Rules from 2014-2016:			
	Medicaid Eligible 2010-2013	Medicaid Eligible 2014-2016	Not Eligible 2010-2013	Not Eligible 2014-2016
	(1)	(2)	(3)	(4)
	Panel A: Households with at least one pre-existing condition:			
- Share with pos. OOP (excluding premiums)	0.86	0.86	0.88	0.87
- Household OOP excluding premiums if > \$0	1,213.58	809.38	1,317.92	1,290.82
	(2,745.32)	(1,456.17)	(2,489.73)	(2,455.21)
- Share with pos. premium cost	0.27	0.27	0.43	0.50
- Household OOP premium costs (if > 0\$)	3,470.96	3,461.08	3,501.59	3,747.10
	(3,404.07)	(3,452.39)	(3,366.65)	(3,421.73)
Observations	4,219	2,652	21,841	16,671
	Panel B: Households without any pre-existing condition:			
- Share with pos. OOP (excluding premiums)	0.57	0.57	0.64	0.62
- Household OOP excluding premiums if > \$0	654.47	482.25	752.61	746.56
	(1,389.99)	(1,017.22)	(1,680.56)	(1,693.30)
- Share with pos. premium cost	0.20	0.20	0.34	0.39
- Household OOP premium costs (if > 0\$)	2,559.99	2,492.86	2,812.67	2,902.06
	(2,877.00)	(2,186.84)	(2,872.21)	(3,088.56)
Observations	2,559	1,289	14,255	9,211

Notes: See notes from Table 4.1.

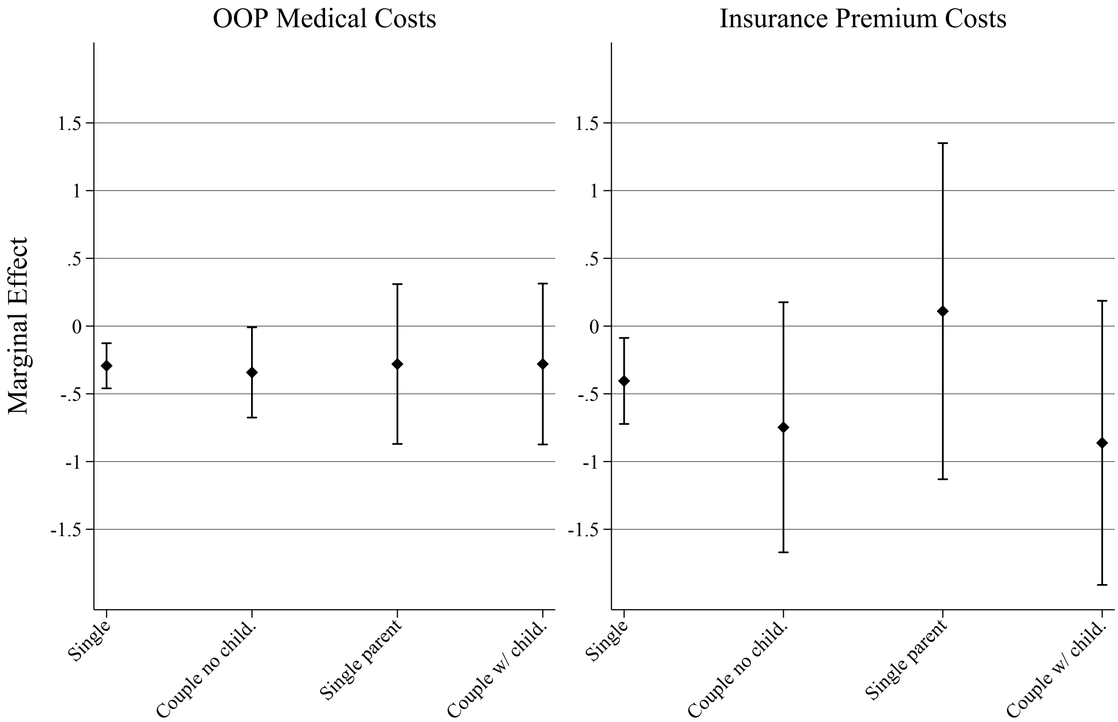
[38] Although this statement applies to the average marginal effects for the entire Medicaid-eligible population, it is particularly relevant to households with chronic conditions.

Figure 4.5: Marginal Effects of ACA Medicaid Eligibility for Households with and without a Pre-Existing Condition



Source: MEPS 2010-2016. Regression results from DD OLS regressions for households with and without at least one pre-existing condition. Pre-existing conditions include: heart attack, coronary heart disease, angina, other heart disease condition, stroke, emphysema, diabetes, arthritis, high blood pressure, asthma, high cholesterol, pregnancy, and extreme obesity (BMI≥40). Confidence intervals based on 95% significance with standard errors clustered at the state level. Regressions are run separately for households with and without a pre-existing condition.

Figure 4.6: Marginal Effects by Household Type



Source: MEPS 2010-2016. The left panel shows the log specification of household OOP expenditures excluding insurance premium costs by household type. Notes from Table 4.5, column (2) apply. The right panel displays marginal effects from the IHS specification of insurance premium costs by household type. Notes from Table 4.6, column (1) apply.

A subanalysis by household type (singles, couples without children, single parents and couples with children) reveals that reductions in OOP medical spending on goods and services are concentrated among single households and couples without children, corresponding to the groups most impacted by the ACA Medicaid expansion. Reductions are roughly equivalent to those found in the causal analysis in Table 4.5, column (2). With respect to insurance premium costs, single households are driving the overall reduction in OOP for coverage, albeit likewise at a similar level as that found for the entire sample and displayed in Table 4.6, column (1).[39]

4.7 Cost-Benefit Analysis

Policy-makers did not intend the ACA Medicaid expansion to be a budget-neutral reform: providing free and heavily subsidized health insurance is expensive and many of the intended benefits non-monetary in nature, such as improved physical, mental and financial

[39] Results are shown for the two specifications for which the main analysis found the largest effects: log OOP medical expenditures for goods and services and the IHS of insurance premium costs.

health. The goal of this short-run cost-benefit analysis of the ACA Medicaid expansion is to highlight the different components of overall costs and benefits of the ACA Medicaid expansion to eligible households and non-eligible taxpayers and to quantify a ballpark figure of net social costs and benefits in the three years after the reform. It should not be seen as exhaustive, but rather a summary measure of the aspects that are measurable during the first three years following implementation with the data available. Furthermore, the short-run impact may well differ from the medium-term or a long-run analysis, in which individuals have more time to adjust their behavior and potential health effects may become apparent. For the short run, net costs of the Medicaid expansion can be formulated as follows:

$$\begin{aligned} Net\ Costs &= (Total\ Cost) - (Total\ Benefit) \\ &= (1 + MCF) \times (mechanical\ cost + moral\ hazard) \\ &- (mechanical\ gain + risk\ protection + health\ improvement) \\ &= MCF \times mechanical\ cost + (1 + MCF) \times moral\ hazard \\ &- risk\ protection - health\ improvement \end{aligned} \tag{4.11}$$

where MCF stands for the marginal costs of (raising public) funds to finance the reform and can be considered deadweight loss. I apply the consensus value in the literature of 0.3 (Poterba (1996); Finkelstein and McKnight (2008); Shigeoka (2014); Hendren (2017); Finkelstein (2018)). Note that the mechanical gain and mechanical cost of the reform cancel out, as this portion represents a mere transfer value. The following sections discuss each of these cost and benefit components and provide an overall estimate of the net social costs of the benefits provided by the ACA expansion of Medicaid eligibility.

4.7.1 Fiscal Costs of Medicaid

I follow Shigeoka (2014) and Hendren (2016) in distinguishing between two types of program costs for Medicaid: a 'mechanical cost' and a 'fiscal externality', or 'efficiency cost'. Mechanical costs encompass increases in government spending necessary to extend the benefit of Medicaid to new recipients, holding the spending behavior of these recipients constant. Estimating this cost requires a counterfactual thought experiment that answers the question of how much government would have paid, had Medicaid covered the expenditures of individuals who became eligible for Medicaid in 2014 *prior* to the reform. To measure this counterfactual, I define 'would be' Medicaid eligibility status at the individual level according to the rules of 2014-2016 for individuals observed in all years prior to

2014[40] and calculate the average pre-reform total health care expenditure per individual, which amounts to about \$4,269 annually. Because Medicaid covered an average of 75% of total expenditures for covered individuals prior to implementation of the ACA in 2014, I estimate a mechanical cost equal to \$3,207/per person annually. Furthermore, a minor downward adjustment by \$14.82 to account for the reduction in public funds spent on this group for informal public insurance in the form of charity care yields a final mechanical cost of \$3,192.18 that is adjusted to 2017 dollars according to the CPI-med.

As the previous analysis demonstrated, increased health insurance coverage increased total expenditures despite decreasing the fraction paid out of pocket. Part of this increase stems from the moral hazard efficiency cost of providing insurance. Because health care prices become relatively less expensive for those receiving heavily subsidized insurance, people consume more of the good. While it is not possible to exactly distinguish how much of this increase should be deemed socially inefficient, it is possible to approximate an upper bound on the moral hazard as the difference between actual post-reform expenditures and the mechanical cost. Average post-reform expenditures for individuals in the treatment group amount to \$4,335.37. The (upper bound) of the moral hazard cost is then \$1,143.19 annually per person between 2014-2016. The lower bound, if all additional expenditure is due to previously inefficent lack of access, is zero. However, given the results from sections 4.5.4 and 4.5.3, it is likely that the true amount is non-zero. The calculation yields a total per person cost of \$5,635.98 ($\$4,335.37 \times 1.3$) using the upper bound of moral hazard cost.

4.7.2 Social Benefits of Medicaid

Because the mechanical cost of the reform is nothing more than a transfer from taxpayers to benefit recipients, the mechanical cost is equal to the mechanical gain. Additional benefits may exist if Medicaid expansion prevents bankruptcy due to a catastrophic medical event. Because I do not observe bankruptcy in the MEPS data, I leave this aspect of the reform to future research. Omitting this potential benefit would lead to an underestimation of the social benefits from Medicaid. Another additional benefit could stem from improved health status or increased preventive health behavior that could decrease the probability of developing a chronic condition and even lead to higher productivity. The MEPS data is ideal to investigate possible improvements to preventive health behavior and health status. However, Appendix Table 4.A7 documents that I fail to detect any

[40] This is the same measure used for the treatment definition of $MCAID_{hst}^{ACA}$ in regression equation 4.3 with the exception that here, it is defined at the individual rather than household level for ease of interpretation on a per person basis.

short-run improvements in health status on account of Medicaid expansion.[41] Given the absence of any impact on mental or physical health found in this study, I do not include any value for improved health in the benefit calculation.

The second part of the total benefit from Medicaid stems from its nature as an insurance reform that affects the risk of high out of pocket expenditures, as analyzed in section 4.5.2. At the mean willingness to pay to insure against the risk of any OOP spending and at a moderate risk aversion parameter of 3, risk protection from Medicaid is estimated to be valued at roughly \$126 annually, as shown in Table 4.7 above.

Inserting the total costs and total benefits calculated above into Equation 4.11 yields the net social cost of the ACA Medicaid expansion of \$2,318/person annually. As discussed above, this calculation should be interpreted as a rough summary measure of the costs and benefits of ACA Medicaid expansion, in particular with respect to OOP medical spending as it influences the household budget of Medicaid-eligible households and non-eligible taxpayers. It does not include the value of potential improvements to financial health - not investigated in this paper - or to physical and mental health, which may arise as a medium-run benefit. Including either of these factors could possibly decrease the net cost of the reform. Furthermore, the CBA cannot fully account for general equilibrium effects, in which prices may respond to increasing demand for medical goods and services.

4.8 Conclusion and Discussion

This paper examined the short-run impact of the expansion of Medicaid public insurance under the Affordable Care Act on the out-of-pocket medical spending of low-income eligible households. I find that Medicaid expansion improved affordability of care for eligible households by reducing household out-of-pocket expenditures for medical services and products by 8.8% (among households with positive expenditures) and for insurance premia by 12.0%. Reductions in OOP premium costs are driven by households switching from positive insurance payments to free or nearly free public insurance, indicating some crowd-out of private from public coverage. Reductions at the mean of total OOP medical spending (including insurance premia) can be attributed to smaller OOP in particular in the upper five percent of the OOP distribution, suggesting the largest effect for high-cost medical events.

In line with the impact of high OOP expenditures in the upper quantiles of the distribution, Medicaid expansion also reduced the variance of medical payments, which is

[41] This finding is perhaps unsurprising, as health improvements may not emerge immediately in the short run. Moreover, the present study finds no improvements in preventive behavior or access to care, which would be a likely precursor to such improvements.

assumed to carry additional value for risk-averse households. The risk protection analysis documents a corresponding value of insuring against the uncertain variance of OOP medical expenditures that is economically small at the mean, but moderately large, roughly \$310 annually, in the upper-most tail of the expected OOP distribution. The presence of charity care may have dampened the impact of Medicaid on risk protection. Alternatively, the lack in substantial improvements to access to care may have contributed to a muted effect.

Whereas Medicaid-eligible households experienced a stark reduction in their own OOP payments, the total expenditures paid by any source on their behalf increased. For one standard deviation increase in the share of a household eligible for the ACA expansion, the reduction in the share paid OOP and the share covered through private insurance decreased by 7.2% and 4.6%, respectively, and was compensated by a 10.9% increase in the share paid by the taxpayer through formal and informal public sources, including charity care. As such, the burden of payment for medical goods and services shifted from low-income Medicaid-eligible households toward non-eligible taxpayers in a non-negligible magnitude.

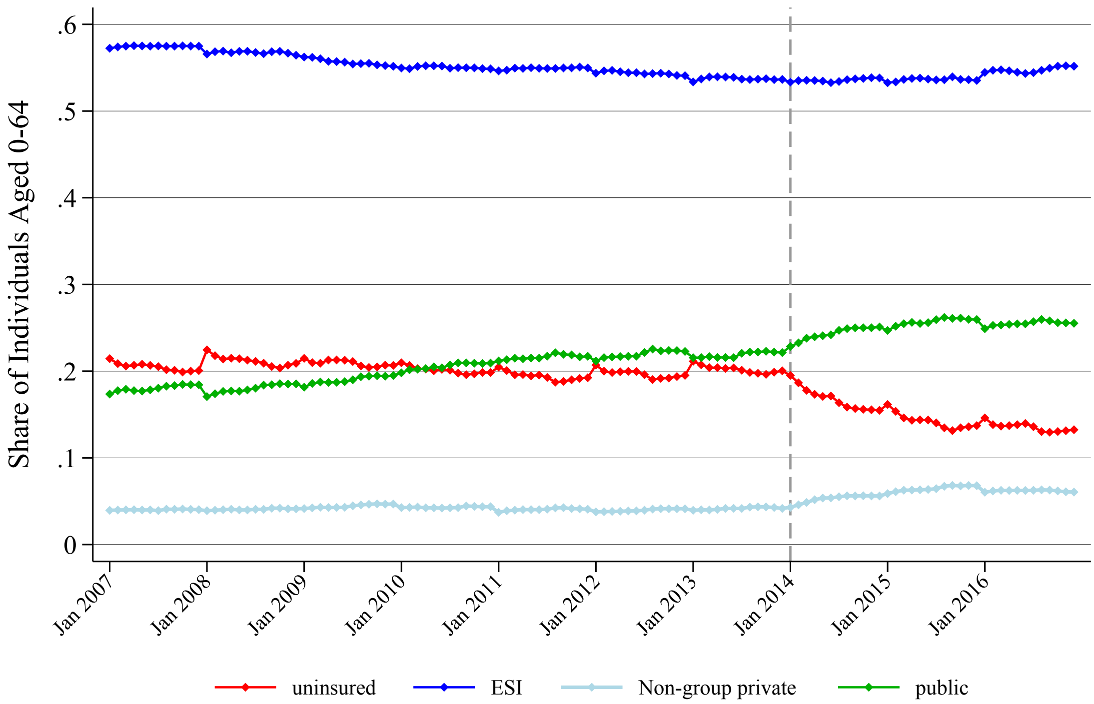
The heterogeneity analysis reveals that households with at least one pre-existing chronic condition are driving the average reductions in OOP payments among Medicaid-eligible households, while reductions among those without any illness are statistically insignificant. The most substantial difference between average effects and those among chronic households can be seen with respect to insurance premium costs, which lead to annual savings of \$383 in the overall Medicaid-eligible population and \$522 among those with a chronic condition, for each additional standard deviation increase in eligibility.

Despite improvements in affordability for low-income families, the analysis does not detect substantial improvements with respect to access to urgent care or utilization of preventive care services and the improvement in access to a regular physician is economically negligible. I do not find any change in delays to necessary treatment due to the financial burden of health care costs or changes in self-assessed mental or physical health. These findings could help to explain why the reduction in spending proved stronger among households with positive expenditures for medical services and goods, as households with positive spending already had access to some form of care. However, these results do not imply that persistent barriers to care are necessarily driving the lack of substantial changes to access to care. In fact, given the low share of Medicaid-eligible households reporting lack of access to a usual care provider both before and after the reform, it is likely that many already in fact did have some form of informal insurance through charity care. Coupled with the evidence regarding sources of payment, results are in line with the hypothesis that pre-reform access to charity care offered informal insurance in particular against urgent and acute medical needs of low-income households.

The cost-benefit analysis summarizes short-run changes in the incidence of medical expenditures for taxpayers and recipients induced by the Medicaid expansion component of the ACA. It quantifies an annual, per person mechanical cost (and subsequent gain) of $3,207 in transfers from the pool of taxpayers to recipients (some of whom are also taxpayers), a $14.82 reduction in charity care expenses, a moral hazard cost of $1,143.19 (upper bound) from increased utilization and a moderate average gain of $125.85 in risk protection, with the moderate latter result likely at least partially attributable to the role of charity care in previously insuring high-cost expenditures. The net social cost of the ACA Medicaid expansion in the order of $2,318/person annually serves as a benchmark going forward, in which potential medium-run financial, physical or mental health benefits can be monetized and compared. Because previous research has found cost reductions from health improvements in the context of other reforms to show strongest effects in the medium or even long run, the current paper leaves for future research the question of whether these improvements amount to more than the social cost of the reform.

4.9 Appendix

Figure 4.A1: Insurance Status of Individuals



Source: MEPS 2007-2016. Weighted shares for individuals aged 0-64 using MEPS individual weights.

Table 4.A1: Results for Total OOP Including Households with >400% FPL

	Difference-in-Difference (DD)				Difference-in-Difference-in-Difference (DDD)			
	OLS		Simulated IV		OLS		Simulated IV	
	(IHS)	(log)	(IHS)	(log)	(IHS)	(log)	(IHS)	(log)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medicaid×post	-0.209**	-0.431***	-0.143	-0.364***	-0.217*	-0.357***	-0.361***	-0.418***
	(0.096)	(0.071)	(0.098)	(0.075)	(0.115)	(0.083)	(0.135)	(0.098)
Subsidy×post	0.059***	0.030***	0.078***	0.040***	0.042***	0.031**	0.088***	0.070***
	(0.011)	(0.010)	(0.014)	(0.014)	(0.013)	(0.012)	(0.026)	(0.019)
Penalty×post	0.018*	0.031***	0.025**	0.039**	0.045**	0.025	0.165***	0.104***
	(0.009)	(0.006)	(0.010)	(0.008)	(0.017)	(0.015)	(0.052)	(0.036)
State×post controls					✓	✓	✓	✓
Income group×post controls					✓	✓	✓	✓
$\bar{R}^2$	0.269	0.269			0.269	0.269		
AR-Statistic (H_0=weak IVs)			22.67**	24.73***			24.42***	29.07***
Observations	104,962	83,134	104,962	83,134	104,962	83,134	104,962	83,134

*Source:*MEPS 2010-2016. Post = years 2014-2016. Standard errors are clustered at the state level.

Table 4.A2: Results for Total OOP Including Households, Including Years 2007-2016

	Difference-in-Difference (DD)				Difference-in-Difference-in-Difference (DDD)			
	OLS		Simulated IV		OLS		Simulated IV	
	(IHS)	(log)	(IHS)	(log)	(IHS)	(log)	(IHS)	(log)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Medicaid×post	-0.260***	-0.421***	-0.211**	-0.374***	-0.076	-0.259***	-0.221*	-0.342***
	(0.087)	(0.059)	(0.091)	(0.063)	(0.119)	(0.085)	(0.126)	(0.097)
Subsidy×post	0.052***	0.034***	0.070***	0.045***	0.038***	0.027**	0.091***	0.069***
	(0.009)	(0.009)	(0.011)	(0.011)	(0.013)	(0.011)	(0.021)	(0.017)
Penalty×post	0.043***	0.043***	0.054***	0.053***	0.036**	0.025*	0.139***	0.100***
	(0.008)	(0.006)	(0.011)	(0.007)	(0.014)	(0.013)	(0.040)	(0.032)
State×post controls					✓	✓	✓	✓
Income group×post controls					✓	✓	✓	✓
$\bar{R}^2$	0.247	0.237			0.247	0.238		
AR-Statistic (H_0=weak IVs)			23.16**	28.42***			23.52***	28.12***
Observations	101,313	79,437	101,313	79,437	101,313	79,437	101,313	79,437

*Source:*MEPS 2007-2016. Post = years 2014-2016. Standard errors are clustered at the state level.

Table 4.A3: Access to Care: Conditional Means

	According to the ACA Medicaid Rules from 2014-2016:			
	Medicaid Eligible 2010-2013	Medicaid Eligible 2014-2016	Not Eligible 2010-2013	Not Eligible 2014-2016
	(1)	(2)	(3)	(4)
Due to cost, delayed/forwent:				
- Medical care	0.07	0.05	0.05	0.03
- Dental care	0.12	0.09	0.08	0.06
- Prescription drugs	0.06	0.05	0.04	0.03
- Any care or drugs	0.18	0.14	0.11	0.09
Access to usual care provider:				
- Must travel > 30 min. to USC provider	0.09	0.09	0.07	0.07
- Lacks access	0.11	0.07	0.08	0.06
Observations	6,778	3,941	36,096	25,882

Source: MEPS cross-sectional data 2010-2016. Weighted means at the household level using household sample weights. Households with at least one member becoming eligible through the ACA Medicaid expansion in any year between 2014-2016 are categorized as eligible. Column (1) presents the average value for households that would have been eligible according to the ACA rules, had the reform been implemented between 2010-2013. Column (2) presents the average value for the treatment×post group that actually became eligible for Medicaid through the ACA expansion. Columns (3) and (4) show weighted means for households that would not have met eligibility criteria for the ACA Medicaid expansion in any year.

Table 4.A4: Access to Care: Difference-in-Difference OLS Results

	Delayed or forewent the following due to cost:				*Access to USC Provider:*	
	medical care	dental care	prescription drugs	any care	Must travel >30 min.	lacks access
	(1)	(2)	(3)	(4)	(5)	(6)
Medicaid×post	0.001	-0.016	0.004	-0.020	-0.003	-0.017**
	(0.008)	(0.011)	(0.008)	(0.013)	(0.010)	(0.008)
Subsidy×post	0.001**	0.001	0.001***	0.002	-0.001	0.001*
	(0.0004)	(0.001)	(0.0004)	(0.001)	(0.001)	(0.001)
Penalty×post	0.002***	0.003***	0.003***	0.004***	0.0005	0.001
	(0.001)	(0.001)	(0.0005)	(0.001)	(0.001)	(0.001)
$\bar{R}^2$	0.030	0.042	0.029	0.059	0.016	0.045
Observations	72,697	72,697	72,697	72,697	72,697	72,697

Notes: See notes to Table 4.11.

Table 4.A5: Preventive Care Service Utilization: Conditional Means

	According to the ACA Medicaid Rules from 2014-2016:			
	Medicaid Eligible 2010-2013	Medicaid Eligible 2014-2016	Not Eligible 2010-2013	Not Eligible 2014-2016
	(1)	(2)	(3)	(4)
- Any preventive checks	0.80	0.83	0.82	0.84
- Physical exam	0.47	0.51	0.44	0.46
- Dental checkup	0.44	0.47	0.55	0.58
- Checked blood pressure	0.64	0.68	0.58	0.60
- Checked cholesterol level	0.40	0.46	0.38	0.41
- Received flu shot	0.27	0.31	0.23	0.26
- Prostate exam	0.05	0.05	0.05	0.05
- Pap smear	0.20	0.19	0.20	0.19
- Breast exam	0.22	0.21	0.22	0.21
- Mamogram	0.12	0.12	0.10	0.10
- Stool test	0.03	0.04	0.02	0.03
- Colonoscopy	0.04	0.05	0.03	0.03
Observations	6,778	3,941	36,096	25,882

*Source:*MEPS 2010-2016. Weighted shares using household sample weights. The share is taken with respect to the number of household members asked about each preventive measure in the MEPS, according to the age and sex for which each is recommended. These target groups are as follows: any preventive measure (all ages, both sexes); routine physical, dental check-up, blood pressure check, cholesterol check, flu shot (age>17, both sexes); prostate exam (age >39, males); pap smear test, breast exam (age>17, females); mammogram (age>29, females); blood stool test and colonoscopy (age >39, both sexes).

Table 4.A6: Difference-in-Difference OLS Results: Changes in Preventive Care

	Share of household utilizing the following preventive services in the past 12 months:											
	Any preventive measure	Routine physical	Dental check-up	blood pressure check	cholesterol check	Flu shot	Prostate exam	Pap smear test	Breast exam	Mamo-gram	blood stool test	colon-oscopy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Medicaid×post	0.018	-0.006	0.022	0.002	0.027	0.010	0.020	-0.005	-0.037	-0.021	0.012	0.016
	(0.015)	(0.022)	(0.024)	(0.019)	(0.021)	(0.019)	(0.027)	(0.025)	(0.026)	(0.026)	(0.012)	(0.010)
Subsidy×post	-0.001	-0.001	-0.001	0.004***	0.004**	0.0005	-0.003	0.002	0.004	-0.0004	0.0003	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)
Penalty×post	-0.001	-0.002	-0.002	0.0004	0.0005	0.0004	0.001	-0.001	0.0001	-0.002	-0.001	-0.002*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.001)	(0.001)
$\bar{R}^2$	0.030	0.065	0.106	0.050	0.117	0.039	0.096	0.042	0.027	0.122	0.036	0.025
Observations	72,697	72,697	72,697	72,697	72,697	72,697	20,854	49,300	49,300	37,685	37,802	37,802

*Source:*MEPS 2010-2016. Post = years 2014-2016. Weighted OLS DD results using household sample weights. All regressions contain controls for year and state fixed effects, local county unemployment rate, household income group, household type (single, couple without children, family with children) and the race/ethnic origin (non-exclusive dummies for black, white, Hispanic) and age of the head of household. The share is taken with respect to the number of household members asked about each preventive measure in the MEPS, according to the age and sex for which each is recommended. These target groups are as follows: any preventive measure (all ages, both sexes); routine physical, dental check-up, blood pressure check, cholesterol check, flu shot (age>17, both sexes); prostate exam (age >39, males); pap smear test, breast exam (age>17, females); mammogram (age>29, females); blood stool test and colonoscopy (age >39, both sexes). Standard errors are clustered at the state level.

Table 4.A7: Self-Assessed Mental and Physical Health Status: Difference-in-Difference OLS Results

	Average MCS score (1)	Average PCS score (2)	poor physical health (3)	poor mental health (4)	Share with Depression (5)
Medicaid×post	-0.288	-0.685	-0.004	0.004	0.012
	(0.479)	(0.418)	(0.004)	(0.003)	(0.011)
Subsidy×post	-0.071	-0.043	0.0003**	-0.0001	0.002***
	(0.056)	(0.036)	(0.0002)	(0.0002)	(0.001)
Penalty×post	-0.004	-0.044	0.001	0.00001	0.002*
	(0.033)	(0.028)	(0.0004)	(0.0003)	(0.001)
$\bar{R}^2$	0.064	0.182	0.027	0.007	0.065
Observations	66,399	66,363	72,697	72,697	72,697

*Source:*MEPS Self-Assessed Questionnaire (SAQ), cross-sectional data 2010-2016. Weighted OLS DD results using household sample weights. Post = years 2014-2016; MCS score = mental component summary score; PCS = physical component summary score. MCS and PCS measures are generated MEPS variables based on the trademark algorithm of Ware et al. (2002), which computes a weighted average of 12 questions assessing current mental and physical well-being indicators from the self-assessed MEPS questionnaire. Columns (3) and (4) show results for the share of the household reporting poor physical and/or mental health on a 5 point ordinal scale (excellent, very good, good, fair, poor). Column (5) reports the share of the household with depression, defined as having a Kessler Index score of at least 3/6 (see Kessler et al. (2002); Kroenke et al. (2003)). All regressions contain controls for year and state fixed effects, local county unemployment rate, household income group, household type (single, couple without children, family with children) and the race/ethnic origin (non-exclusive dummies for black, white, Hispanic) and age of the head of household. Standard errors are clustered at the state level.

Table 4.A8: Medicaid Eligibility vs. Receipt

	Difference-in-Difference (DD)		Difference-in-Difference-in-Difference (DDD)	
	OLS	Simulated IV	OLS	Simulated IV
	(1)	(2)	(3)	(4)
Medicaid×post	0.157***	0.163***	0.110***	0.145***
	(0.018)	(0.020)	(0.020)	(0.021)
Subsidy×post	-0.001	0.001	0.001	-0.001
	(0.002)	(0.003)	(0.002)	(0.003)
Penalty×post	-0.001	-0.001	-0.001	-0.012**
	(0.001)	(0.001)	(0.003)	(0.005)
$\bar{R}^2$	0.309	0.308	0.308	0.307
Observations	72682	72682	72682	72682

Source: MEPS cross-sectional data 2010-2016.

List of Tables

1.1 Share of Employed Teens Earning At or Below the Minimum Wage 6
1.2 Share of Teens and Immigrants in Top 10 Industries Employing 16-17 Year-Olds, 2000 17
1.3 Share of Teens and Immigrants in Top 10 Industries Employing 18-19 Year-Olds, 2000 17
1.4 Descriptive Statistics for Explanatory Variables for Sample of 16-19 Year-Olds (1986-2015) 22
1.5 SUR Grouped Data Multinomial Logit Model Estimates of Marginal Effects, 16-17 Year-Olds (1986-2015) 36
1.6 SUR Grouped Data Multinomial Logit Model Estimates of Marginal Effects, 18-19 Year-Olds (1986-2015) 37
1.7 SUR Grouped Data Multinomial Logit Model Estimates of Marginal Effects, 16-19 Year-Olds (1986-2015) 38
1.A1 Characteristics of Teens Age 16-19 in First and Last Sample Years 39
1.A2 Multinomial Logit Model Estimates of Marginal Effects, using Microdata, 1986-2015 41
1.A3 SUR Grouped Multinomial Model Estimates of Marginal Effects for Multinomial Grouped Data (1986-2015), with Leads 42
1.A4 3SLS Grouped Data Multinomial Model Estimates of Marginal Effects 16-19 Year-Olds (1986-2015), Treating Immigrant Share as Endogenous . 43

2.1 Sample Description, 2013-2016 49
2.2 Reservation wages, subsequent employment and observed wages 51
2.3 Difference-in-Differences: Growth in Reservation Wages in 2010-2016 . . . 56
2.4 Difference-in-Differences: Growth in Observed Gross Hourly Wages in 2010-2016 57
2.A1 Sample Size after Inclusion of Selection Criteria, by Year 62
2.A2 Average of Reservation Monthly Earnings at Percentiles of Hourly Reservation Wage Distribution, 2013-2016 63
2.A3 Average Working Hours at Percentiles of Hourly Reservation Wage Distribution, 2013-2016 63
2.A4 Percentiles of Hourly Wage Distributions, 2013-2016 64
2.A5 Sample Description by Region Type, 2014-2016 65

2.A6 Difference-in-Differences: Growth in Reservation Wages in 2010-2016, with ROR-specific Fixed Effects . . . 65
2.A7 Difference-in-Differences: Growth in Reservation Wages in 2010-2016, with Socio-Demographic Controls . . . 66
2.A8 Difference-in-Differences: Growth in Reservation Wages in 2010-2016, with Human Capital Controls . . . 67
2.A9 Difference-in-Differences: Growth in Reservation Wages in 2010-2016, with Regional Controls . . . 68

3.1 Predicted and Observed Mean Monthly Earnings . . . 75
3.2 Employment Rates in % and Observations by Country, 2008-2014 . . . 81
3.3 Mean Participation Tax Rates by Country and Gender in %, 2008-2014 . . . 83
3.4 Tax and Benefit Incentives for Bottom Quintile . . . 89
3.5 Regression Results for Pooled OLS and Group IV . . . 91
3.6 Participation elasticity by PTR component . . . 97
3.A1 Regression Results for Group IV with Alternative Group Definition . . . 107
3.A2 Regression Results for Group IV and Alternative Clustering Strategies . . . 108
3.A3 Robustness of Group IV to Time Trends . . . 109

4.1 Sample Characteristics of Treatment and Control Households, 2010-2016 . . . 129
4.2 OOP Medical Expenditures of Treatment and Control Households, 2010-2016 . . . 134
4.3 Observed and Simulated Eligibility for Policy Variables . . . 135
4.4 Results for Total OOP: Pooled OLS vs Simulated IV . . . 137
4.5 Results for OOP Excluding Premium Costs . . . 138
4.6 Results for Premium Costs . . . 139
4.7 Welfare Gain from Medicaid Risk Protection . . . 143
4.8 Conditional Means of Sources of Payment Variables . . . 145
4.9 Sources of Payment for Household Medical Expenditures: Difference-in-Difference OLS Results . . . 145
4.10 Medical Service Utilization: Conditional Means . . . 146
4.11 Difference-in-Difference OLS Results: Medical Service Utilization . . . 149
4.12 Out of Pocket Expenditures by Pre-Existing Condition Status: Conditional Means . . . 151
4.A1 Results for Total OOP Including Households with >400% FPL . . . 160
4.A2 Results for Total OOP Including Households, Including Years 2007-2016 . . . 161
4.A3 Access to Care: Conditional Means . . . 162
4.A4 Access to Care: Difference-in-Difference OLS Results . . . 162

4.A5 Preventive Care Service Utilization: Conditional Means 163
4.A6 Difference-in-Difference OLS Results: Changes in Preventive Care 164
4.A7 Self-Assessed Mental and Physical Health Status: Difference-in-Difference OLS Results 165
4.A8 Medicaid Eligibility vs. Receipt 166

List of Figures

1.1 Labor Force Participation Rates (%) by Age Group, 1980-2016 10

1.2 Employment and School Enrollment Status (%) by Age Group, 1986-2016 11

1.3 Minimum Wages by State and Nationally (Population Weighted), 1979-2016 12

1.4 Increases in the Returns to a High School Degree, Ages 25-40, 1980-2016 14

1.5 Increases in the Spanish-Speaking Immigrant Share Relative to Native Working-Age Population, 1986-2015 16

1.6 Effects of Minimum Wage Changes on 16-17 Year-Olds' Employment and School Enrollment Status: Actual before 2000 and Simulated since 2000 29

1.7 Effects of Changes in Returns to High School Degree on 16-17 Year-Olds' Employment and School Enrollment Status: Actual before 2000 and Simulated since 2000 30

1.8 Effects of Spanish-Speaking Immigrant Share Changes on 16-17 Year-Olds' Employment and School Enrollment Status: Actual before 2000 and Simulated since 2000 31

1.9 Effects of Minimum Wage, Returns to a High School Degree, and Spanish-Speaking Immigrant Share Changes on 16-17 Year-Olds' Employment (SE + ENS) Status: Actual before 2000 and Simulated since 32

1.A1 Effects of Minimum Wage, Returns to a High School Degree, and Spanish-Speaking Immigrant Share Changes on 16-17 Year-Olds' Employment (SE + ENS) Status, Based on 3SLS Grouped Multinomial Model Estimates Treating Immigrant Share as Endogenous: Actual before 2000 and Simulated since 2000 40

2.1 Distribution of Hourly Reservation Wages, 2013-2016 50

2.2 Share of Employees with Actual Hourly Wages below the Minimum Wage in 2012 52

2.3 10th and 25th Percentiles of Reservation Wages, by Region Type 54

2.A1 10th and 25th Percentiles of Observed Hourly Wages, by Region Type . . 61

2.A2 Share of Employment in the Total Population (aged 17 to 64), by Region Type 61

3.1 PTR by Age Group and Education Level, Italy 80

3.2 PTR Distributions by Quintile . . . 82
3.3 PTR Distributions by Earner Type . . . 84
3.4 PTR Distributions by Household Type . . . 85
3.5 PTR Compositions by Household and Earner Type . . . 87
3.6 Participation Elasticities by Gender . . . 93
3.7 Participation Elasticities for Single Households . . . 94
3.8 Participation Elasticities for Sole Earners . . . 95
3.9 Participation Elasticities for Primary Earners . . . 95
3.10 Participation Elasticities for Secondary Earners . . . 96
3.A1 PTR by Age Group and Education Level, AT . . . 100
3.A2 PTR by Age Group and Education Level, BE . . . 101
3.A3 PTR by Age Group and Education Level, BG . . . 101
3.A4 PTR by Age Group and Education Level, CZ . . . 102
3.A5 PTR by Age Group and Education Level, DE . . . 102
3.A6 PTR by Age Group and Education Level, DK . . . 103
3.A7 PTR by Age Group and Education Level, EL . . . 103
3.A8 PTR by Age Group and Education Level, ES . . . 104
3.A9 PTR by Age Group and Education Level, FR . . . 104
3.A10 PTR by Age Group and Education Level, IT . . . 105
3.A11 PTR by Age Group and Education Level, SE . . . 105
3.A12 PTR by Age Group and Education Level, UK . . . 106
3.A13 PTR Composition by Country and Year . . . 110
3.A14 PTR Composition by Country and Year . . . 111
3.A15 Median PTR Based on Predicted vs. Observed Earnings . . . 112

4.1 OOP Medical Expenditures as a Share of Household Gross Income . . . 130
4.2 Household OOP Medical Expenditures (Excluding Premiums) . . . 131
4.3 Household Insurance Premiums . . . 131
4.4 Quantile Regressions . . . 140
4.5 Marginal Effects of ACA Medicaid Eligibility for Households with and without a Pre-Existing Condition . . . 152
4.6 Marginal Effects by Household Type . . . 153
4.A1 Insurance Status of Individuals . . . 159

Bibliography

Aaronson, D., K.-H. Park, and D. Sullivan (2006). The Decline in Teen Labor Force Participation. *Economic Perspectives 10*, 2 – 18.

Abeler, J. and S. Jäger (2015). Complex Tax Incentives. *American Economic Journal: Economic Policy 7*, 1–28.

Adam, S., M. Brewer, and A. Shephard (2006). *The Poverty Trade-Off. Work Incentives and Income Redistribution in Britain*. Policy Press.

Addison, J., M. Centeno, and P. Portugal (2009). Do Reservation Wages Really Decline? Some International Evidence on the Determinants of Reservation Wages. *Journal of Labor Research 30*(1), 1–8.

Addison, J., M. Centeno, and P. Portugal (2010). Unemployment Benefits and Reservation Wages: Key Elasticities from a Stripped-Down Job Search Approach. *Economica 77*(305), 46–59.

Addison, J., M. Centeno, and P. Portugal (2013). The Reservation Wage Unemployment Duration Nexus. *Oxford Bulletin of Economics and Statistics 75*(6), 980–987.

Aghion, P., U. Akcigit, M. Lequien, and S. Stantcheva (2017). Tax Simplicity and Heterogeneous Learning. *NBER Working Paper No. 24049*.

Aguiar, M., M. Bils, K. K. Charles, and E. Hurst (2017). Leisure Luxuries and the Labor Supply of Young Men. Technical report, National Bureau of Economic Research.

Allegretto, S., A. Dube, M. Reich, and B. Zipperer (2017). Credible Research Designs for Minimum Wage Studies: A Response to Neumark, Salas, and Wascher. *ILR Review 70*(3), 559–592.

Allegretto, S. A., A. Dube, and M. Reich (2011). Do Minimum Wages Really Reduce Teen Employment? Accounting for Heterogeneity and Selectivity in State Panel Data. *Industrial Relations: A Journal of Economy and Society 50*(2), 205–240.

Altonji, J. G. and D. Card (1991). The Effects of Immigration on the Labor Market Outcomes of Less-Skilled Natives. In *Immigration, Trade, and the Labor Market*, pp. 201–234.

Amlinger, M., R. Bispinck, and T. Schulten (2016). Ein Jahr Mindestlohn in Deutschland – Erfahrungen und Perspektiven. *WSI-Report 28*.

Angrist, J. D. (1991). Grouped-data estimation and testing in simple labor-supply models. *Journal of Econometrics 47*(2), 243 – 266.

Autor, D. H., L. F. Katz, and M. S. Kearney (2008). Trends in U.S. Wage Inequality: Revising the Revisionists. *The Review of Economics and Statistics 90*(2), 300–323.

Barcellos, S. H. and M. Jacobson (2015). The Effects of Medicare on Medical Expenditure Risk and Financial Strain. *American Economic Journal: Economic Policy 7*(4), 41 – 70.

Bargain, O., K. Orsini, and A. Peichl (2013). Comparing Labor Supply Elasticities in Europe and the United States. *Journal of Human Resources 49*(3), 723–838.

Bartels, C. and N. Pestel (2016). Short- and Long-Term Participation Tax Rates and Their Impact on Labor Supply. *International Tax and Public Finance 23*(6), 1126–1159.

Baskaya, Y. S. and Y. Rubinstein (2015). Using Federal Minimum Wages to Identify the Impact of Minimum Wages on Employment and Earnings across the U.S. States.

Bastani, S., Y. Moberg, and H. Selin (2017). The Anatomy of the Extensive Margin Labor Supply Response.

Bean, F. D. and M. Tienda (1988). *The Hispanic Population of the United States*. Russell Sage Foundation.

Bearse, P., G. Glomm, and E. Janeba (2000). Why Poor Countries Rely Mostly on Redistribution In-kind. *Journal of public Economics 75*(3), 463–481.

Bell, B., R. Costa, and S. J. Machin (2018). Why Does Education Reduce Crime?

Belman, D. and P. J. Wolfson (2014). *What Does the Minimum Wage Do?* Kalamazoo, MI: Upjohn Institute for Employment Research.

Bengali, L., M. Daly, R. Valletta, et al. (2013). Will Labor Force Participation Bounce Back? *FRBSF Economic Letter 14*.

Besley, T. and S. Coate (1991). Public Provision of Private Goods and the Redistribution of Income. *American Economic Review 81*(4), 979–984.

Bick, A. and N. Fuchs-Schündeln (2018). Taxation and Labour Supply of Married Couples across Countries: A Macroeconomic Analysis. *Review of Economic Studies 85*, 1543 –1576.

Blackburn, M. L. and D. Neumark (1993). Omitted-Ability Bias and the Increase in the Return to Schooling. *Journal of Labor Economics 11*(3), 521–544.

Blau, F. D. and L. M. Kahn (2006). The U.S. Gender Pay Gap in the 1990s: Slowing Convergence. *Industrial and Labor Relations Review 60*(1), 45–66.

Blavin, F., M. Karpman, G. M. Kenney, and B. D. Sommers (2018). Medicaid Versus Marketplace Coverage for Near-Poor Adults: Effects on Out-of-Pocket Spending and Coverage. *Health Affairs 37*(2), 299 – 307.

Blömer, M. J., N. Guertzgen, L. Pohlan, H. Stichnoth, and G. J. van den Berg (2018). Unemployment Effects of the German Minimum Wage in an Equilibrium Search Model. *ZEW Discussion Paper No. 18-032*.

Blundell, A., A. Duncan, and C. Meghir (1998). Estimating Labor Supply Responses Using Tax Reforms. *Econometrica 66*(4), 827–861.

Bossler, M. and H.-D. Gerner (2016). Employment Effects of the New German Minimum Wage: Evidence from Establishment-Level Micro Data. *IAB Discussion Paper No. 10/2016*.

Brevoort, K., D. Grodzicki, and M. B. Hackmann (2017). Medicaid and Financial Health. *NBER Working Paper No. 24002*.

Brewer, M., E. Saez, and A. Shephard (2008). Means-Testing and Tax Rates on Earnings. Prepared for the Report of a Commission on Reforming the Tax System for the 21st Century, Chaired by Sir James Mirrlees, Institute for Fiscal Studies.

Brown, S. and K. Taylor (2013). Reservation Wages, Expected Wages and Unemployment. *Economics Letters 119*(3), 276–279.

Brown, S. and K. Taylor (2015). The Reservation Wage Curve: Evidence from the UK. *Economics Letters 126*, 22–24.

Buchmueller, T., H. Levy, and S. Nikpay (2018). The Impact of Medicaid Expansion on Household Consumption. *Working Paper*.

Burbidge, J. B., L. Magee, and A. L. Robb (1988). Alternative Transformations to Handle Extreme Values of the Dependent Variable. *Journal of the American Statistical Association 83*(401), 123–127.

Burdett, K. and D. T. Mortensen (1998). Wage Differentials, Employer Size, and Unemployment. *International Economic Review 39*(2), 257–273.

Burdett, K. and T. Vishwanath (1988). Declining Reservation Wages and Learning. *Review of Economic Studies 55*(4), 655–665.

Burns, S. K. and J. P. Ziliak (2015). Identifying the Elasticity of Taxable Income. *The Economic Journal 127*, 297–329.

Caliendo, M., A. Fedorets, M. Preuss, C. Schröder, and L. Wittbrodt (2017). The Short-Term Distributional Effect of the German Minimum Wage Reform. *IZA discussion Paper 11246*.

Caliendo, M., A. Fedorets, M. Preuss, C. Schröder, and L. Wittbrodt (2018). The Short-Run Employment Effect of the German Minimum Wage Reform. *Labour Economics 53*(C), 46–62.

Card, D. (1990). The Impact of the Mariel Boatlift on the Miami Labor Market. *ILR Review 43*(2), 245–257.

Card, D. (1999). The Causal Effect of Education on Earnings. In *Handbook of Labor Economics*, Volume 3, pp. 1801–1863.

Chen, J., A. Vargas-Bustamante, and P. Novak (2017). Reducing Young Adults' Health Care Spending through the ACA Expansion of Dependent Coverage. *Health Services Research 52*(5), 1–23.

Chetty, R., A. Guren, D. Manoli, and A. Weber (2013). Does Indivisible Labor Explain the Difference between Micro and Macro Elasticities? A Meta-Analysis of Extensive Margin Elasticities. *NBER Macroeconomics Annual 2012 27*, 1–56.

Choné, P. and G. Laroque (2011). Optimal Taxation in the Extensive Model. *Journal of Economic Theory 146*(2), 425 – 453.

Chua, K.-P. and B. D. Sommers (2014). Changes in Health and Medical Spending Among Young Adults Under Health Reform. *Journal of the American Medical Association 311*(23), 2437–2439.

Clemens, J., L. Khan, and J. Meer (2018). Dropouts Need Not Apply: The Minimum Wage and Skill Upgrading. Technical report, Working Paper.

Cogan, J. F. (1981). Fixed Costs and Labor Supply. *Econometrica 49*(4), 945–963.

Collado, D. (2018). Financial Work Incentives and the Longterm Unemployed: the Case of Belgium. *EUROMOD Working Paper Series Number EM 1/18*.

Courtemanche, C., J. Marton, B. Ukert, A. Yelowitz, and D. Zapata (2016). Early Impacts of the Affordable Care Act on Health Insurance Coverage in Medicaid Expansion and Non-Expansion States. *Journal of Policy Analysis and Management 36*(1), 178–210.

Cunha, J. M., G. De Giorgi, and S. Jayachandran (2018). The Price Effects of Cash versus In-kind Transfers. *The Review of Economic Studies 86*(1), 240–281.

Currie, J. and F. Gahvari (2008). Transfers in Cash and In-kind: Theory Meets the Data. *Journal of Economic Literature 46*(2), 333–83.

Currie, J. and J. Gruber (1996). Health Insurance Eligibility, Utilization of Medical Care and Child Health. *Quarterly Journal of Economics 111*(2), 431–466.

Cutler, D. M. and J. Gruber (1996). Does Public Insurance Crowd Out Private Insurance? *Quarterly Journal of Economics 11*(2), 391–430.

Depew, B. and J. Bailey (2015). Did the Affordable Care Act's Depend Coverage Mandate Increase Premiums? *Journal of Health Economics 41*, 1–14.

Dillender, M. (2017). Medicaid, family spending, and the Financial Implications of Crowd-Out. *Journal of Health Economics 53*, 1–16.

Dockery, A., R. Ong, and G. Wood (2011). Welfare Traps in Australia: Do they bite? *CLMR Discussion Paper Series No. 08/02*.

Dolton, P., C. R. Bondibene, and M. Stops (2015). Identifying the employment effect of invoking and changing the minimum wage: A spatial analysis of the UK. *Labour Economics 37*, 54–76.

Duggan, M., G. S. Goda, and E. Jackson (2017). The Effects of the Affordable Care Act on Health Insurance Coverage and Labor Market Outcomes. NBER Working Paper No. 23607.

Ehrenberg, R. G. and A. J. Marcus (1982). Minimum Wages and Teenagers' Enrollment-Employment Outcomes: A Multinomial Logit Model. *Journal of Human Resources 17*(1), 39–58.

Eissa, N. and J. B. Liebman (1996). Labor Supply Response to the Earned Income Tax Credit. *Quarterly Journal of Economics 111*, 605–637.

Engelhardt, G. V. and J. Gruber (2011). Medicare Part D and the Financial Protection of the Elderly. *American Economic Journal: Economic Policy 3*(November), 77 – 102.

Falk, A., E. Fehr, and C. Zehnder (2006). Fairness Perceptions and Reservation Wages: the Behavioral Effects of Minimum Wage Laws. *The Quarterly Journal of Economics 121*(4), 1347–1381.

Fehr, R., A. Damico, L. Levitt, G. Claxton, C. Cox, and K. Pollitz (2018). Mapping Pre-existing Conditions across the U.S. Kaiser Family Foundation Issue Brief (August). Available at `http://files.kff.org/attachment/Issue-Brief-Mapping-Pre-existing-Conditions-across-the-US` (accessed December 2, 2018).

Feldstein, M. and J. Gruber (1995). A Major Risk Approach to Health Insurance Reform. *Tax Policy and the Economy 9*, 103 – 130.

Finkelstein, A. (2018). Welfare Analysis Meets Causal Inference: A Suggested Interpretation of Hendren (2016, 2017). Cliff Notes.

Finkelstein, A. and R. McKnight (2008). What did Medicare do? The initial impact of Medicare on mortality and out of pocket medical spending. *Journal of Public Economics 92*(7), 1644 – 1668.

Finlay, K., L. Magnusson, and M. E. Schaffer (2016). WEAKIV: Stata Module to Perform Weak-Instrument-Robust Tests and Confidence Intervals for Instrumental Variable (IV) Estimation of Linear, Probit and Tobit Models. Statistical Software Components, Boston College Department of Economics.

Firpo, S., N. M. Fortin, and T. Lemieux (2009). Unconditional Quantile Regressions. *Econometrica 77*(3), 953–973.

Fitzenberger, B. and A. Doerr (2016). Konzeptionelle Lehren aus der ersten Evaluationsrunde der Brancenmindestloehne in Deutschland. *Journal for Labour Market Research 49*(4), 329–347.

Flinn, C. J. (2006). Minimum Wage Effects on Labor Market Outcomes. *Econometrica 74*(4), 1013–1062.

Frean, M., J. Gruber, and B. D. Sommers (2017). Premium subsidies, the mandate, and Medicaid expansion: Coverage effects of the Affordable Care Act. *Journal of Health Economics 53*, 72–86.

Garloff, A. (2016). Side Effects of the New German Minimum Wage on (Un-)Employment: First Evidence from Regional Data. *IAB Discussion Paper No. 31/2016*.

Goebel, J., M. M. Grabka, S. Liebig, M. Kroh, D. Richter, C. Schröder, and J. Schupp (2018). The German Socio-Economic Panel (SOEP). *Jahrbücher für Nationalökonomie und Statistik/Journal of Economics and Statistics 0*(0), online first.

Goolsbee, A. (1999). Evidence on the High-Income Laffer Curve from Six Decades of Tax Reform. *Brookings Papers on Economic Activity 1999*(2), 1–64.

Grady, M. L. (1994). Compulsory Education: A Policy Analysis. ERIC. Available at `https://files.eric.ed.gov/fulltext/ED377556.pdf` (accessed February 23, 2019).

Gross, T. and M. J. Notowidigdo (2011). Health Insurance and the Consumer Bankruptcy Decision: Evidence from Expansions of Medicaid. *Journal of Public Economics 95*, 767–78.

Gruber, J. and Rodríguez (2007). How Much Uncompensated Care do Doctors Provide? *Journal of Health Economics 26*(6), 1151–1169.

Hansen, E. (2017). Optimal Income Taxation with Labor Supply Responses at Two Margins: When is an Earned Income Tax Credit Optimal? *Discussion Paper Series of the Max Planck Institute for Research on Collective Goods* (2017/10).

Heckman, J. (1979). Sample Selection Bias as a Specification Error. *Econometrica 30*(1), 239–267.

Heckman, J. and R. Robb (1985). Alternative Methods for Evaluating the Impact of Interventions: An Overview. *Journal of Econometrics 30*(1-2), 239–267.

Heckman, J. J. and P. A. LaFontaine (2010). The American High School Graduation Rate: Trends and Levels. *The Review of Economics and Statistics 92*(2), 244–262.

Hendren, N. (2016). The Policy Elasticity. *Tax Policy and the Economy 30*(1), 51–89.

Hendren, N. (2017). Efficient Welfare Weights. NBER Working Paper Nr. 20351.

Herring, B. (2005). The Effect of the Availability of Charity Care to the Uninsured on the Demand for Private Health Insurance. *Journal of Health Economics 24*(2), 225–252.

Himmelstein, D. U., D. Thorne, E. Waren, and S. Woolhandler (2009). Medical Bankruptcy in the United States, 2007: Results of a National Study. *The American Journal of Medicine 122*(8), 741–746.

Hoynes, H. W. and D. W. Schanzenbach (2009). Consumption Responses to In-kind Transfers: Evidence from the Introduction of the Food Stamp Program. *American Economic Journal: Applied Economics 1*(4), 109–39.

Hu, L., R. Kaestner, B. Mazumder, S. Miller, and A. Wong (2016). The Effect of the Patient Protection and Affordable Care Act Medicaid Expansions on Financial Wellbeing. *NBER Working Paper No. 22170*.

Immervoll, H., H. Kleven, C. Kreiner, and E. Saez (2007). Welfare reform in European countries: a microsimulation analysis. *The Economic Journal 117*, 1–44.

Immervoll, H., H. Kleven, C. Kreiner, and N. Verdelin (2011). Optimal Tax and Transfer Programs for Couples with Extensive Labor Supply Responses. *Journal of Public Economics 95*, 1485–1500.

Jacquet, L., E. Lehmann, and B. Van der Linden (2013). Optimal Redistributive Taxation with Both Extensive and Intensive Responses. *Journal of Economic Theory 148*(5), 1770–1805.

Jäntti, M., J. Pirttilä, and H. Selin (2015). Estimating Labour Supply Elasticities Based on Cross-Country Micro Data: A Bridge between Micro and Macro Estimates? *Journal of Public Economics 127*, 87–99.

Jones, S. R. G. (1988). The Relationship between Unemployment Spells and Reservation Wages as a Test of Search Theory. *The Quarterly Journal of Economics 103*(1), 741–765.

Kalíšková, K. (2018). Tax and Transfer Policies and the Female Labor Supply in the EU. *Empirical Economics*, 1–27.

Keane, M. P. (1992). A Note on Identification in the Multinomial Probit Model. *Journal of Business & Economic Statistics 10*(2), 193–200.

Kessler, R. C., G. Andrews, L. J. Colpe, E. Hiripi, D. K. Mroczek, S.-L. Normand, E. E. Walters, and A. M. Zaslavsky (2002). Short Screening Scales to Monitor Population Prevalences and Trends in Non-Specific Psychological Distress. *Psychological medicine 32*(6), 959–976.

Koenig, F., A. Manning, and B. Petrongolo (2016). Reservation Wages and the Wage Flexibility Puzzle. *IZA Discussion Paper No. 9717*.

Kroenke, K., R. L. Spitzer, and J. B. Williams (2003). The Patient Health Questionnaire-2: Validity of a Two-Item Depression Screener. *Medical care*, 1284–1292.

Kroft, K., F. Lange, and M. J. Notowidigdo (2013). Duration Dependence and Labor Market Conditions: Evidence from a Field Experiment. *The Quarterly Journal of Economics 128*(3), 1123–1167.

Krueger, A. B. and A. I. Mueller (2016). A Contribution to the Empirics of Reservation Wages. *American Economic Journal: Economic Policy 8*(1), 142–179.

Lieber, E. M. J. and L. M. Lockwood (2019). Targeting with In-Kind Transfers: Evidence from Medicaid Home Care. *American Economic Review 109*(4), 1461–1485.

Liu, S., T. J. Hyclak, and K. Regmi (2016). Impact of the Minimum Wage on Youth Labor Markets. *Labour 30*(1), 18–37.

Ljungqvist, L. and T. J. Sargent (1998). The European Unemployment Dilemma. *Journal of Political Economy 106*(1), 514–550.

Lo Sasso, A. T. and T. C. Buchmueller (2004). The Effect of the State Children's Health Insurance program on health insurance coverage. *Journal of Health Economics 23*, 1059–1082.

Long, S. K. (2008). On the Road to Universal Coverage: Impacts of Reform in Massachusetts at One Year. *Health Affairs 27*(4), w270–w284.

Long, S. K. and P. B. Masi (2009). Access and Affordability: An Update on Health Reform in Massachusetts, Fall 2008. *Health Affairs 28*(4), w578–w587.

Lordan, G. and D. Neumark (2018). People vs. Machines: The Impact of Minimum Wages on Automatable Jobs. *Labour Economics 52*, 40–53.

Mackey, P. E. and T. G. Duncan (2013). Does Raising the State Compulsory School Attendance Age Achieve the Intended Outcomes? REL 2014-005. *Regional Educational Laboratory Mid-Atlantic*.

McFadden, D. (1973). Conditional Logit Analysis of Qualitative Choice Behavior. In P. Zarembka (Ed.), *Frontiers of Econometrics*. New York: Academic Press.

Meghir, C. and D. Phillips (2010). Labor Supply and Taxes, Chapter 3 for Mirrlees Review (2009). In J. Mirrlees, S. Adam, T. Besley, R. Blundell, S. Bond, R. Chote, M. Gammie, P. Johnson, G. Myles, and J. Poterba (Eds.), *Dimensions of Tax Design: the Mirrlees Review*. Oxford University Press.

Mixon, J. W. and E. F. Stephenson (2016). Young and Out of Work: An Analysis of Teenage Summer Employment, 1972-2012. *Cato Journal 36*, 89.

Morisi, T. L. (2008). Youth Enrollment and Employment during the School Year. *Monthly Labor Review 131*, 51.

Morisi, T. L. (2010). The Early 2000s: A Period of Declining Teen Summer Employment Rates. *Monthly Labor Review 133*(5).

National Center for Education Statistics (1990). Digest of Education Statistics. Available at `https://nces.ed.gov/pubs91/91660.pdf` (accessed February 20, 2019).

National Center for Education Statistics (2017). Digest of Education Statistics. Available at `https://nces.ed.gov/programs/digest/d17/tables/dt17_234.10.asp` (accessed February 20, 2019).

Neumark, D., I. Salas, and W. Wascher (2014). Revisiting the Minimum Wage-Employment Debate: Throwing Out the Baby with the Bathwater? *Industrial and Labor Relations Review 67*(7), 608–648.

Neumark, D., I. Salas, and W. Wascher (2017). Reply to 'Credible Research Designs for Minimum Wage Studies'. *Industrial and Labor Relations Review 70*(3), 593–609.

Neumark, D. and C. Shupe (2018). Declining Teen Employment: Minimum Wages, Other Explanations, and Implications for Human Capital Investment. Mercatus Working Paper.

Neumark, D. and W. Wascher (1994). The Effects of Minimum Wages on Teenage Employment and Enrollment: Estimates from Matched CPS Data. *Research in Labor Economics 15*, 25–64.

Neumark, D. and W. Wascher (1995). Minimum Wage Effects on Employment and School Enrollment. *Journal of Business & Economic Statistics 13*(2), 199–206.

Neumark, D. and W. Wascher (2003). Minimum Wages and Skill Acquisition: Another Look at Schooling Effects. *Economics of Education Review 22*(1), 1–10.

Neumark, D. and W. Wascher (2008). *Minimum Wages.* Cambridge, MA: Cambridge University Press.

Neumark, D. and W. Wascher (2011). Does a Higher Minimum Wage Enhance the Effectiveness of the Earned Income Tax Credit? *Industrial and Labor Relations Review 64*(4), 712–746.

O'Donoghue, C. (2011). Do Tax-Benefit Systems Cause High Replacement Rates? A Decompositional Analysis Using EUROMOD. *LABOUR 25*, 126–151.

OECD (2015). *Employment Outlook 2015.* OECD Publishing.

Pence, K. (2006). The Role of Wealth Transformations: An Application to Estimating the Effect of Tax Incentives on Savings. *Journal of Economic Analysis and Policy 5*(1), 1–26.

Piketty, T. and E. Saez (2013). Optimal Labor Income Taxation. In M. F. Alan J. Auerbach, Raj Chetty and E. Saez (Eds.), *Handbook of Public Economics 5*, pp. 391–474.

Pirttillä, J. and H. Selin (2011). Tax Policy and Employment: How does the Swedish system fare. *CESifo Working Paper Series No. 3355*.

Porterfield, S. L. and A. E. Winkler (2007). Teen Time Use and Parental Education: Evidence from the CPS, MTF, and ATUS. *Monthly Lab. Rev. 130*, 37.

Poterba, J. M. (1996). Government Intervention in the Markets for Education and Health Care: How and Why? pp. 277–308.

Prescott, E. C. (2004). Why Do Americans Work So More Than Europeans? *Federal Reserve Bank of Minneapolis Quarterly Review 28*, 2–13.

Qin, X. and G. G. Liu (2013). Does the US Health Care Safety Net Discourage Private Insurance Coverage? *European Journal of Health Economics 14*, 457–469.

Ravallion, M. (2017). A Concave Log-Like Transformation Allowing Non-Positive Values. *Economics Letters 161*, 130–132.

Roodman, D. (2011). Fitting Fully Observed Recursive Mixed-Process Models with cmp. *The Stata Journal 11*(2), 159–206.

Ross, M. and N. P. Svajlenka (2015). Worrying Declines in Teen and Young Adult Employment. *Brookings Institution, December 16*.

Saez, E. (2002). Optimal Income Transfer Programs: Intensive versus Extensive Labor Supply Responses. *Quarterly Journal of Economics 117*, 1039–1073.

Schmidt, L., L. Shore-Sheppard, and T. Watson (2016). The Effect of Safety-Net Programs on Food Insecurity. *Journal of Human Resources 51*(3), 589–614.

Shigeoka, H. (2014). The effect of patient cost sharing on utilization, health, and risk protection. *American Economic Review 104*(7), 2152–84.

Siddiqui, M., E. T. Roberts, and C. E. Pollack (2015). The Effect of Emergency Department Copayments for Medicaid Beneficiaries Following the Deficit Reduction Act of 2005. *Journal of the American Medical Association 175*(3), 393–398.

Smith, C. L. (2011). Polarization, Immigration, Education: What's behind the Dramatic Decline in Youth Employment? Finance and Economics Discussion Series No. 2011-41.

Smith, C. L. (2012). The Impact of Low-Skilled Immigration on the Youth Labor Market. *Journal of Labor Economics 30*(1), 55–89.

Sommers, B. D., M. Z. Gunja, K. Finegold, and T. Musco (2015). Health Reform and Changes in Health Insurance Coverage in 2014. *Journal of the American Medical Association 314*(4), 366–374.

Sommers, B. D., T. Musco, K. Finegold, M. Z. Gunja, A. Burke, and A. M. McDowell (2014). Health Reform and Changes in Health Insurance Coverage in 2014. *New England Journal of Medicine 371*(9), 867–874.

Sutherland, H. and F. Figari (2013). EUROMOD: The European Union Tax-Benefit Microsimulation Model. *International Journal of Microsimulation 6*(1), 4–26.

Ware, J. E., M. Kosinski, D. M. Turner-Bowker, and B. Gandek (2002). How to Score Version 2 of the SF-12 (r) Health Survey. *Quality Metric*.

Whitehurst, G. J. and S. Whitfield (2012). Compulsory School Attendance: What Research Says and What It Means for State Policy. Brown Center on Education Policy at Brookings. Available at `https://www.brookings.edu/wp-content/uploads/2016/06/0801_education_graduation_age_whitehurst_whitfield.pdf` (accessed February 20, 2019).

World Health Organization (2011). The World Health Report – Health Systems Financing: The Path to Universal Coverage. World Health Organization, Geneva.

World Health Organization (2013). The World Health Report – Research for Universal Coverage. World Health Organization, Geneva.

Deutsche Zusammenfassung

Diese Dissertation setzt sich aus vier empirischen Forschungsartikeln zusammen. Das erste Kapitel entstand in Zusammenarbeit mit David Neumark. In diesem Papier untersuchen wir die Treibkräfte sinkender Jugenderwerbstätigkeit in den USA, die sich seit 2000 für 16-17-Jährige am stärksten abzeichnet. Insbesondere berücksichtigen wir drei mögliche Erklärungsfaktoren, die am häufigsten in der Literatur und in der öffentlichen Debatte erörtert werden: erhöhte Mindestlöhne, die die Beschäftigungschancen junger Menschen einschränken könnten; steigende Bildungserträge, die Investitionen in den eigenen akademischen Erfolg rentabler machen und den Arbeitsmarktwettbewerb durch Migranten, der, ähnlich wie Mindestlöhne, Jugendliche aus dem Markt auspreisen könnte. Um dieser Frage nachzugehen, schätzen wir ein multinominales Logit-Modell unter Verwendung von CPS-Daten (*Current Population Survey*) und ergänzenden Quellen, um die Auswirkung dieser Faktoren auf den Anteil der Jugendlichen in Arbeit, in einer Bildungsmaßnahme oder einer Kombination von beiden zu bestimmen. Von den untersuchten Erklärungsfaktoren stellen sich Mindestlöhne als der stärkste Faktor heraus, der die Erwerbstätigkeit junger Menschen senkt. Wettbewerb von Immigration spielt womöglich eine minderwertige, aber signifikante Rolle während wir keine Evidenz für die Relevanz von steigenden Bildungserträgen finden.

Während Kapitel eins die zeitliche und räumliche Variation von inkrementellen Mindestlohnerhöhungen in den US-Bundesstaaten für die Identifikation kausaler Effekte verwendet, erfolgt die Identifikation im Kapitel zwei mittels eines Differenz-von-Differenzen-Ansatzes infolge einer verhältnismäßig großen Reform. Dieses Kapitel untersucht die Auswirkungen der Einführung eines flächendeckenden Mindestlohns 2015 in Deutschland auf die Reservationslöhne der nichtarbeitenden Bevölkerung. Die Ergebnisse zeigen, dass die Mindestlohneinführung zu einer Anhebung der Lohnerwartungen um 18 Prozent unter nichterwerbstätigen Arbeitssuchenden geführt hat. Diesen Erkenntnissen zufolge führen Mindestlöhne nicht zwangsläufig zu einer erhöhten Arbeitsmarktpartizipation, da Arbeitssuchende ihre Reservationslöhne entsprechend anpassen könnten.

Wie die ersten zwei Kapitel, widmet sich Kapitel drei der Erklärung von Arbeitsangebotsentscheidungen auf der individuellen Ebene, allerdings mit besonderer Betrachtung des Haushaltskontextes. Dieses Kapitel analysiert inwieweit die jeweiligen Steuer-Transfer-Systeme in 12 europäischen Ländern zu unterschiedlichen Arbeitsanreizen und

Erwerbstätigenraten beitragen. Auf der Basis von EUROMOD-Daten und dem harmonisierten Mikrosimulationsmodell berechnen wir Partizipationssteuersätze (PTRs) und untersuchen ihre Auswirkung auf die Erwerbswahrscheinlichkeit. Die Ergebnisse weisen heterogene Elastizitäten auf, die, unabhängig vom Geschlecht, für Zweitverdiener am größten und bei Erstverdienern vernachlässigbar sind. Daher weist diese Analyse auf die Wichtigkeit hin, heterogene Elastizitäten des Arbeitsangebotes auf der Basis ökonomischer Konzepte wie Opportunitätskosten anstatt Geschlecht zu messen.

Kapitel vier untersucht schließlich die Auswirkungen einer wichtigen Sachleistung, nämlich der gesetzlichen Krankenversicherung, auf das medizinische Ausgabenverhalten von Haushalten mit niedrigem Einkommen in den USA. Die Analyse setzt empirisch auf dem Medical Expenditures Panel Survey (MEPS) und einem quasi-natürlichen Experiment durch die Ausweitung von Medicaid unter dem Patient Protection and Affordable Care Act (ACA) auf. Die Anspruchskriterien für Medicaid werden verwendet, um die Programmteilnahme (Anspruchsberechtigung) auf individueller Ebene zu bestimmen und die Teilnahmeintensität auf der Haushaltebene als den Anteil der Anspruchsberechtigten im Haushalt zu definieren.

Die Ergebnisse aus den Differenz-von-Differenzen(-von-Differenzen) (DD und DDD) Ansätzen mithilfe der Variation über Regionen, Zeit und Einkommensgruppen zeigen, dass eine Standardabweichung nach oben im Anteil der berechtigten Haushaltsmitglieder die eigenen Auslagen für medizinische Versorgung und Produkte um 8,8 Prozent und die Auslagen für Krankenversicherung um 12,0 Prozent verringert. Des Weiteren reduziert die Anspruchsberechtigung auf die gesetzliche Krankenversicherung das Risiko, besonders hohen Auslagen ausgesetzt zu sein. Dennoch, obwohl Gesundheitsleistungen monetär erschwinglicher für anspruchsberechtigte Haushalte werden, finde ich weder Effekte auf den Zugang zu akut benötigter Versorgung noch auf die Inanspruchnahme von präventiven Maßnahmen, die von der gesetzlichen Versicherung abgedeckt werden. Die Ergebnisse weisen auf eine Verdrängung privater Versicherung um 4,6 Prozent, aber auch auf eine Kostenreduzierung beim ineffizienten *'Charity Care'*, oder informeller Sorgearbeit, zugunsten formeller gesetzlicher Versicherung hin. Insgesamt erhöht die Anspruchsberechtigung auf Medicaid den Anteil der Kosten für Gesundheitsleistungen, die vom Steuerzahler übernommen werden, um 10,9 Prozent.

Erklärungen

Erklärung gemäß §4 Abs. 2

Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

Erklärung gemäß §10 Abs. 3

Hiermit erkläre ich, dass ich für die Dissertation folgende Hilfsmittel und Hilfen verwendet habe:

- Statistiken und Regressionen: Stata 14.1, 15.1
- Schriftsatz und Formatierungen: LaTeX,TeXstudio,JabRef

Auf dieser Grundlage und soweit nicht anders vermerkt (Siehe 'Zusammenarbeit mit Koautoren') habe ich die Arbeit selbstständig verfasst.

(Cortnie Shupe)
Berlin, 10. April 2019