IFAU
Institute for Evaluation of Labour
Market and Education Policy

Essays on Politics, Law and Economics

Linna Martén

DISSERTATION SERIES 2016:4

Presented at the Department of Economics, Uppsala University

The Institute for Evaluation of Labour Market and Education Policy (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market and educational policies, studies of the functioning of the labour market and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

IFAU also provides funding for research projects within its areas of interest. The deadline for applications is October 1 each year. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The institute has a scientific council, consisting of a chairman, the Director-General and five other members. Among other things, the scientific council proposes a decision for the allocation of research grants. A reference group including representatives for employer organizations and trade unions, as well as the ministries and authorities concerned is also connected to the institute.

Postal address: P O Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

This doctoral dissertation was defended for the degree of Doctor in Philosophy at the Department of Economics, Uppsala University, May 28, 2016.

ISSN 1651-4149

Dissertation presented at Uppsala University to be publicly examined in Lecture Hall 2, Ekonomikum, Kyrkogårdsgatan 10, Uppsala, Saturday, 28 May 2016 at 13:15 for the degree of Doctor of Philosophy. The examination will be conducted in English. Faculty examiner: Professor Erik Snowberg (California Institute of Technology).

Abstract
Martén, L. 2016. Essays on Politics, Law, and Economics. *Economic studies* 160. 150 pp. Uppsala: Department of Economics, Uppsala University. ISBN 978-91-85519-67-5.

Essay 1: Several countries practice a system where laymen, who lack legal education, participate in the judicial decision making. Yet, little is known about their potential influence on the court rulings. In Sweden lay judges (nämndemän) are affiliated with the political parties and appointed in proportion to political party representation in the last local elections. This paper investigates the influence of their partisan belonging when ruling in asylum appeals in the Migration Courts, where laymen are effectively randomly assigned to cases. The results show that the approval rate is affected by the policy position of the laymen's political parties. In particular, asylum appeals are more likely to be rejected when laymen from the anti-immigrant party the Swedish Democrats participate, and less likely to be rejected when laymen from the Left Party, the Christian Democrats or the Green Party participate. This indicates that asylum seekers do not receive an impartial trial, and raises concerns that laymen in the courts can compromise the legal security in general.
Essay 2: Although economic circumstances have been argued to be a major determining factor of attitudes to redistribution, there is little well identified evidence at the individual level. Utilizing a unique dataset, with detailed individual information, provides new and convincing evidence on the link between economic circumstances and demand for redistribution (in the form of social benefits). The Swedish National Election Studies are constructed as a rotating survey panel, which makes it possible to estimate the causal effect of economic changes. The empirical analysis shows that individuals who experience a job loss become considerably more supportive of redistribution. Yet, attitudes to redistribution return to their initial level as economic prospects improve, suggesting that the effect is only temporary. Although a job loss also changes attitudes to the political parties, the probability to vote for the left-wing is not affected.
Essay 3: A well-functioning labor market is characterized by job reallocations, but the individual costs can be vast. We examine if individual's ability to cope with such adjustments depends on their cognitive and non-cognitive skills (measured by the enlistment tests). Since selection into unemployment is a function of skills, we solve the endogeneity of a job loss by using the exogenous labor market shock provided by the military base closures in Sweden following the end of the Cold War. We find, first, that, on average, labor earnings decrease and unemployment and labor-related benefits increase for those affected. Second, there are heterogeneous treatment effects in terms of unemployment; the treated individuals with high non-cognitive and cognitive skills face lower unemployment effects than the treated individuals with low non-cognitive and cognitive skills.

Keywords: Political attitudes, Decision making, Court, Immigration, Legal system, Redistribution, Social insurance, Unemployment, Cognitive and Non-cognitive skills, Displaced workers, Plant closure, Defense draw down

Linna Martén, Department of Economics, Box 513, Uppsala University, SE-75120 Uppsala, Sweden.

ISSN 0283-7668
ISBN 978-91-85519-67-5
urn:nbn:se:uu:diva-282782 (http://urn.kb.se/resolve?urn=urn:nbn:se:uu:diva-282782)

Acknowledgments

When I started at the university I was convinced that I would eventually become a journalist. Working on diverse topics, searching for tomorrow's news, and uncovering the real facts. It all seemed so thrilling. Well, there are actually some similarities with doing research, but there is of course one major difference – everything about research happens in slow-motion.

Yet, I am convinced that the work on this thesis would have progressed even slower without the guidance of my advisors, Matz Dahlberg and Olle Folke. Thank you for many enlightening discussions, and for motivating me to think big and not take the easy way. You managed to push me to do better, but also convinced me that I had the ability to do so. I am grateful that you were always available for a quick chat, and I always left our meetings feeling slightly better about my research than before. Your encouragement, understanding, and advice in times of doubt have truly been invaluable.

There is a saying that "the more you learn, the more you realize how much you do not know." I have certainly found that to be true. The feeling is only enhanced by being surrounded by all the clever people you meet in academia. Out of all of these people, there are some in particular that have been important for my time as a Ph.D. student. To Katarina Nordblom, I would never have applied to the Ph.D. program to begin with, if you had not suggested it. Thank you for believing in my capacity to do research. To Helena Holmlund and Heléne Lundqvist, my discussants during the licentiate and final seminar, I am grateful for your thorough reading and well thought comments on my work. To Eva Mörk, Mikael Elinder, and Jon Fiva, thank you for the interest you have shown for my research and I have benefited greatly from your feedback. To Jim Snyder, thank you for your hospitality and for inviting me to spend a year at Harvard University. To Shuhei, thank you for all the discussions and dinners we shared in Cambridge. I also extend my gratitude to the administrative staff at the economics department in Uppsala for making everything run so smooth.

To fellow Ph.D. students at the department, thank you for numerous lunches, coffee breaks, and after works. Your friendliness and team spirit has definitely made my time here so much more memorable. Lovisa, Selva, Oskar, and Irina, I am so happy that we started this whole journey

together. We did not only pull through all the exams, but also mastered the art of rowing, karaoke, and liberal dance moves. I thank you for outstanding team effort. Tove, I am grateful that you showed me that everything about research becomes more fun if you also know how to tap dance. Sebastian, thank you for all the laughs that made the whole job market experience somewhat less nerve-wrecking. I am glad that it ended well for both of us. Fredrik, I am lucky to have had you on the other side of the office. During these past years we have shared the ups and downs of research as well as life in general, and I could not have asked for a better companion. I know that I complain too often about our differences, but please know that I value your opinion particularly high.

Writing a thesis is a long and bumpy journey, not least because your mood almost perfectly tracks your latest regression results. I am grateful to all my friends for providing moral support and much needed distractions. In particular I want to thank Sandra, Matilda, and Nina for many fun trips, Hanna for all the late night dinners, and Pär for our regular board game evenings.

Finally, to my parents. Thank you for being there whenever I need you and for always encouraging me to follow my interests.

Uppsala, April 2016

Linna Martén

Contents

Introduction 1
1 Economic Imperialism 1
2 The history of JEL codes 5
3 The chapters 8
References 12
Appendices 13

I Political Bias in Court? Lay Judges and Asylum Appeals 15
1 Introduction 16
2 Institutional background 20
2.1 Lay judges in the courts 22
2.2 Court committees and scheduling 23
3 Data and measurements 25
3.1 Attitudes to refugees 27
3.2 Summary statistics 29
3.3 Direction of disagreement 31
4 Empirical specification 33
5 Results 36
5.1 Peer influence and majority composition 38
5.2 Political party and case characteristics interactions 40
5.3 Judge and laymen interactions 43
5.4 Between-judge variation 44
5.5 Robustness 45
6 Conclusions 46
References 48
Appendices 51

II Demand for Redistribution: Individuals' Response to Economic Setbacks 63
1 Introduction 64
2 Economic circumstances: mechanism 66
3 Data and measurement 68
3.1 Survey data and register information 68
3.2 Demand for redistribution 69
3.3 Summary statistics 73
4 Estimation method 76

5 Results ... 77
5.1 Heterogeneous effects ... 80
5.2 Placebo test ... 83
6 Conclusion ... 85
References ... 86
Appendices ... 89

III Labor Market Effects of Job Loss: The Importance of Cognitive and Non-Cognitive Skills ... 99
1 Introduction ... 100
2 Previous literature ... 105
2.1 Plant closures and the effects on individual labor market outcomes ... 105
2.2 The importance of cognitive and non-cognitive skills for the job reallocation process ... 107
2.3 Regional effects of military base closures ... 108
3 Institutional background ... 109
4 Data description and summary statistics ... 112
4.1 Data description ... 112
4.2 Summary statistics ... 115
5 Econometric specification ... 118
6 Labor market effects of base closures ... 119
6.1 Mean effects ... 120
6.2 Heterogenous effects: cognitive and non-cognitive skills ... 122
6.3 Separating military and civil personnel ... 126
7 Conclusions ... 127
References ... 129
Appendices ... 132

Introduction

> *So economics is an imperial science: it has been aggressive in addressing central problems in a considerable number of neighboring social disciplines, and without any invitations.*
>
> — George J. Stigler (1984, p. 311)

The introduction of a thesis is expected to put the following chapters in a context, but it is quite clear that my papers lack a unifying theme, and some would even question if this is really economics. Yet, I will attempt to convince the reader that this is, in fact, in concordance with the development of the field. Economics was once an integrated part of the social sciences, and closely related to economic history and sociology. Following the neoclassical breakthrough, economics experienced a consolidation both in terms of methodology as well as substance, as focus shifted to deductive methods and classic economic issues. Yet, in recent decades the unification around a methodological core has been paired with a process of fragmentation and specialization as economists have addressed new research areas. This is the story of how *political economy* became *economics* and then moved on to *freakonomics*.

1 Economic Imperialism

In the past, economics was not distinct from other social sciences. Aristotle, Xenofon, and Plato discussed economic issues in the context of social philosophy. Although they focused more on ethics and justice, topics such as division of labor, efficiency, and interest rate were also covered. Within classical political economy Adam Smith, David Ricardo, and John Stuart Mill also analyzed economic factors in much broader context than most economists do today, and political economy was seen as a unified social science. Inductive and deductive methods were mixed with anecdotes and normative arguments. They addressed topics such as human nature and the influence of nature versus nurture, as well as the relationship between the state and the individual. In terms of economic issues, common features of the classical thinkers were the interest

for growth, the focus on the returns to the production factors land, labor and capital, and the view that the economy functioned best without government interventions (Sandelin et al., 2008). Most of them imagined that there were limits to growth, and that the economy would eventually end up in a stationary state.

Following the *Methodenstreit* (a debate over the relative merits of deduction versus induction) and the neoclassical breakthrough in the late 19th century, synthetic methodological approaches were replaced with more deductive methods (Milonakis and Fine, 2009b). The expositions became increasingly mathematical as economists started looking at the methods of natural science rather than social science. At the same time it became increasingly common to refer to the field as *economics* rather than *political economy*.[1] Stanley Jevons, Carl Menger, and Léon Walras were some of the prominent figures within the first generation of the neoclassical school. The previous focus on macroeconomics now shifted to microeconomics, where individuals and companies maximize their utility, and the concept of marginal units gained importance – the marginal revolution. Just as before, prices were central to resource allocation, but while the classics primarily viewed prices as determined by production costs and supply, the neoclassics added the demand side to the economic analysis. Unlike the classic school there was also an aim to separate analysis from recommendations. With the second generation of neoclassical thinkers (for instance Alfred Marshall, Arthur C. Pigou, and Vilfredo Pareto) neoclassical theories became the dominant school of thoughts within economics. As the scope of economic inquiry narrowed, social science became fragmented into separate disciplines, and sociology and economic history emerged as separate disciplines.

Milonakis and Fine (2009a) establish that the marginalist revolution led to two parallel and contradictory developments. On the one hand, the scope of application was reduced to the economy considered simply as market relations, on the other hand, its basic conceptual principles such as equilibrium, rationality, scarcity and choice became more and more universal in content and application. Georg J. Stigler argues that the extended application, that was to come, was due to its growing abstractness and generality (Stigler, 1984). Precisely because economics had been so heavily reduced in content and application, an outward expansion was almost inevitable as soon as the principles gained general acceptance. One widely cited definition of economics from this period stated that economics is "the science which studies human behavior as a relationship between ends and scarce means which have alternative use" (Lionel Rob-

[1] The name political economy has returned as the sub-field within economics that studies political behavior with economic methods.

bins, 1932, p. 15). Hence, economics was not identified with a particular subject, but rather as the science of choice. Lazear (2000) claims that economics focus on three factors that distinguish it from other fields: rational individuals who engage in maximization behavior, equilibrium, and efficiency. These are concepts that can clearly be applied to a wide range of topics.

Over time economists started to consider topics that are more traditionally associated with sociology, political science, law, and psychology – such as political institutions, peer effects and social mobility. Economics become a method and a set of techniques rather than a subject matter. Lazear (2000) defines economic imperialism as the extension of economics to topics that go beyond the classical scope of issues, i.e. consumer choice, theory of the firm, explicit markets, macroeconomic activity, and the fields spawned directly by these areas. Although the term "economic imperialism" was coined already in the 1930s, it only became a force to be reckoned with from the late 1950s.

Front runners such as Gary Becker branched into areas traditionally considered as topics of sociology, and analyzed a wide range of topics, such as discrimination and marriage, as a rational process where individuals maximize their utility. The idea that the law should deal with efficiency rather than justice also attracted economists to legal studies. For instance, Ronald Coase's work has been important for the analyses of government regulation and Becker investigated the deterrence effects of penalties. The renewed interest for politics is perhaps not surprising, considering the past. Issues of voting rules, legislative structures, and rent seeking have been studied by scholars such as Anthony Downs, James M. Buchanan and Gordon Tullock. Game theoretical models are typically used to model how voters, politicians and bureaucrats behave strategically. Economists have also approached health care, where problems with asymmetric information and externalities are of great relevance. In particular Victor Fuchs made early contributions by investigating the role of financial incentives in determining healthcare expenditures and trade-offs between treatments. Psychology has had a major impact on economics by questioning the assumption that individuals choose rationally (see for instance Kahneman and Tversky, 1979; Tversky and Thaler, 1990; Ainslie, 1991). Economists such as George Akerlof, Ariel Rubinstein and Robert Thaler have examined concepts such as cognitive dissonance and bounded rationality within behavioral economics. The bestselling book Freakonomics (2005) describes the research of economist Steven Levitts, and cover topics as diverse as cheating teachers, socioeconomic patterns of naming children and the relationship between legalized abortion and crime. Levitt states that economics is, at its root, the study of incentives.

With the causal revolution – or the credibility revolution – that has characterized the field in the last decades economics has taken a new step, as new methods to analyze causal connections have developed. Angrist and Pischke (2010) argue that the improved empirical work has been driven by access to more and better data, advances in econometric understanding, and, in particular, that research design has moved front and center in much of empirical microeconomics. Following the causal breakthrough some would argue that almost any paper on a social science topic employing a credible identification method has a good chance of publishing in a highly ranked economics journal.

Lazear (2000) argues that economic imperialism has been successful both in terms of influencing scholars outside economics to use economic analyses to understand social issues, and in expanding the boundaries of economics. Looking at the list of Nobel Memorial Prize laureates in Economics, several of them earned a Ph.D. in a neighboring discipline. Daniel Kahneman earned his doctorate in psychology, Elinor Ostrom in political science, and Friedrich Hayek in law and political science. One could, of course, argue that the expansion of the field has the potential to unify the social sciences again, but the pattern of exchange remains deeply asymmetrical. Looking at cross-citations Fourcade et al. (2015) find that economics has a much higher level of within-field citations compared to political science and sociology. Moreover, the American Political Science Review cite the top 25 economics journals more than five times as often as the articles in the American Economic Review cite the top 25 political science journals. The asymmetry is even starker compared to the American Sociological Review.[2] Clearly other fields, in particular psychology, have also had an impact on economics, but the overall pattern is one of insularity. It is perhaps no surprise, then, that proponents of this process call it "economic imperialism" (e.g. Lazear, 2000), while opponents stick to "economic's imperialism" (e.g. Milonakis and Fine, 2009a).

Looking at definitions of economics in contemporary economics textbooks, Backhouse and Medema (2009) conclude that economics is the study of the economy, the study of coordination processes, the study of the effects of scarcity, the science of choice, and the study of human behavior. Hence, economists are far from unanimous about the definition of their subject. Still, it is clear that these definitions are also relevant to topics primarily studied by the other social sciences.

[2]Keep in mind that the larger role of books within political science and sociology compared to economics will give the numbers a downward bias, since books are not accounted for.

2 The history of JEL codes

A way to illustrate how economists have perceived their discipline is to tell the story of JEL codes. Articles in economic journals are usually classified according to the JEL classification codes, a system originated by the Journal of Economic Literature, and published by the American Economic Association (AEA). Although one can naturally also find articles by keywords, most journals offer the possibility to search their archive by JEL code filtering. JEL codes are also used to describe job offers and submit papers to referees. Classifying economics might appear uncontroversial, but as Cherrier (2016) points out, revisions of the JEL codes have raised fundamental questions about the role of economic theory, the relation between microeconomics and macroeconomics and the relation between theory and applied work.[3] Hence, proposed changes to the codes have been seen as threatening or enhancing the status, and thereby the future prospects, of classified fields.

Table 1. Classification 1911

General Work, Theory and its History
Economic History and Geography
Agriculture, Mining, Forestry and Fisheries
Manufacturing Industries
Transportation and Communication
Trade, Commerce, and Commercial Crises
Accounting, Business Methods, Investments and the Exchanges
Capital and Capitalistic Organizations
Labor and Labor Organizations
Money, Prices, Credit and Banking

Note: See (Cherrier, 2016) for further details.

The first AEA classification (see Table 1) in 1911 used 10 categories, and clearly focused on classic economic issues, such as manufacturing, trade, and banking. The first revision in the 1940s established 17 categories belonging to 3 principal groups; Methodology, General and Special (see Table A1). The first one included theory, economic history and statistical methods, the second one combined methods and applications, and the last one covered 11 applied fields. By the 1960s this had changed to 10 main categories (see Table A2). A new, but diverse, category was *Welfare Programs, Consumer Economics, Urban and Regional Economics*, indicating an increased interest in applied microeconomics. Nowadays

[3]The major revisions took place in 1938–1944,1955–1956,1966, and 1988–1990. A new one is pending.

Table 2. Current JEL classification (from 1991)

JEL	Definition
A	General Economics and Teaching
B	History of Economic Thought, Methodology, and Heterodox Approaches
C	Mathematical and Quantitative Methods
D	Microeconomics
E	Macroeconomics and Monetary Economics
F	International Economics
G	Financial Economics
H	Public Economics
I	Health, Education, and Welfare
J	Labor and Demographic Economics
K	Law and Economics
L	Industrial Organization
M	Business Administration and Business Economics; Marketing; Accounting; Personnel Economics
N	Economic History
O	Economic Development, Innovation, Technological Change, and Growth
P	Economic Systems
Q	Agricultural and Natural Resource Economics; Environmental and Ecological Economics
R	Urban, Rural, Regional, Real Estate, and Transportation Economics
Y	Miscellaneous Categories
Z	Other Special Topics

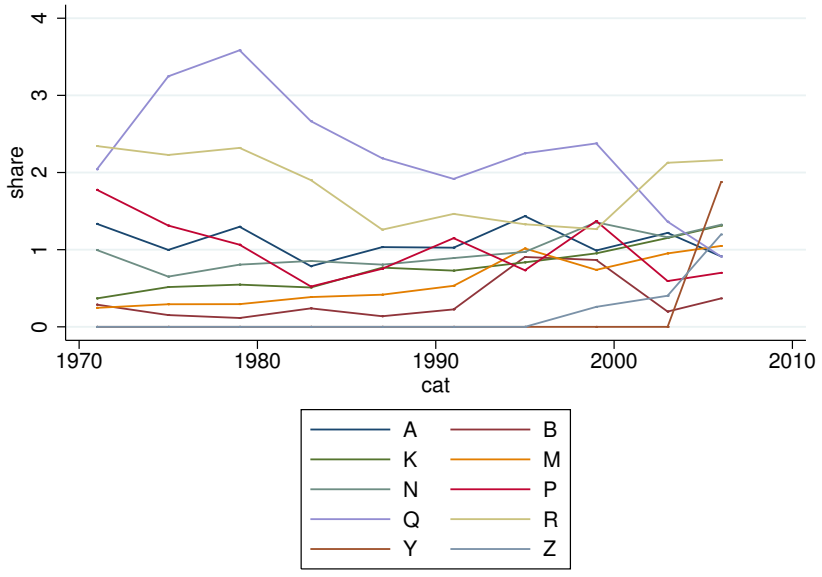
we typically view economics as having a core of microeconomics and macroeconomics, but it wasn't until the latest revision that this core emerged. At the same time the theory category, whose existence had been debated for half a century, disappeared (Cherrier, 2016). This emphasized how theoretical and applied work had become more integrated.

The current classification (see Table 2) spans 20 different groups with 861 subcategories. Note that new categories such as *Health, Education, and Welfare* (I), *Law and Economics* (K), and *Public Economics* (H) have emerged. Also, *Other Special Topics* (Z) include the subcategories cultural economics, economic sociology, and economic anthropology. Hence, with the widening of economics, there are JEL-codes even for the topics discussed by the classical thinkers.

Tracking the evolution of articles published in top economic journals also describes how the discipline has developed. Kelly and Bruestle (2011) depict the evolution of subject areas by looking at the share of articles published within different JEL codes in eight top journals 1969-2007.[4]

[4]The journals are American Economic Review, Econometrica, International Economic Review, Journal of Economic Theory, Journal of Political Economy, Quarterly Journal

Figure 1. Articles in Eight General Journals by JEL codes: Minor fields

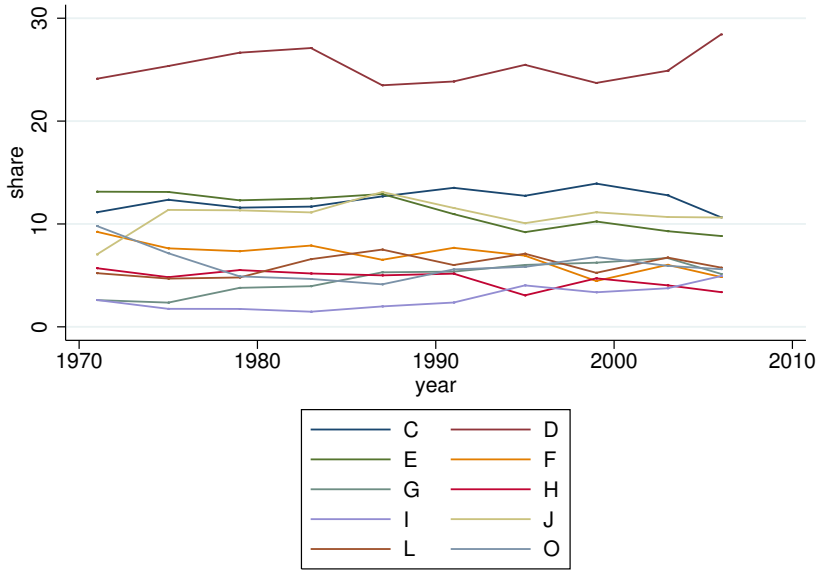


Note: Average percentage of articles by subject published in eight general journals. Raw data from Kelly and Bruestle (2011) and re-calculated to four year averages.

Figure 1 displays the development for the minor categories, while Figure 2 shows the larger ones. Among the fields that have experienced the largest increase, in relative terms, one finds *Business Administration and Business Economics* (M), *History of Economic Thought, Methodology, and Heterodox Approaches* (B), *Law and Economics* (K), *Financial Economics* (G), and *Health, Education, and Welfare* (I). Among those that have witnessed a decrease are *Agricultural and Natural Resource Economics* (Q), *Economic Systems* (P), *International Economics* (F) and *Macroeconomics and Monetary Economics* (E). Thus, the traditional topics find it increasingly difficult to publish in high ranked journals.

of Economics, Review of Economic Studies, Review of Economics and Statistics. The new JEL codes have been mapped to subcategories of the previous ones.

Figure 2. Articles in Eight General Journals by JEL codes: Major fields



Note: Average percentage of articles by subject published in eight general journals. Raw data from Kelly and Bruestle (2011) and re-calculated to four year averages.

3 The chapters

We have now learned how the history of economics can be understood through JEL codes, and I will use the same tool to describe the essays in this dissertation. *Essay 1* uses JEL codes primarily from the new category *Law and Economics* (K), but also from *Microeconomics* (D), where several of the subcategories for modern political economy are located. Several countries practice a system where laymen, who lack legal education, participate in the judicial decision making. Yet, little is known about their potential influence on the court rulings. In Sweden lay judges (nämndemän) are affiliated with the political parties and appointed in proportion to political party representation in the last local elections. In *Essay 1*, I examine examines if laymen's political attitudes bias decision making in asylum appeals in the Migration Courts, where laymen are effectively randomly assigned to cases. The results show that the approval rate is affected by the policy position of the laymen's political parties. In particular, asylum appeals are more likely to be rejected when laymen from the anti-immigrant party the Swedish Democrats participate, and less likely to be rejected when laymen from the Left Party, the Christian Democrats or the Green Party participate. This indicates that asylum

seekers do not receive an impartial trial, and raises concerns that laymen in the courts can compromise the legal security in general.

This topic is clearly related to both political science and law, but the lack of empirical studies is astonishing. The potential influence of laymen in the court has been discussed for decades in Sweden, and a number of Swedish Government Official Reports have examined the system and suggested ways to modify it (e.g., SOU, 1994; SOU, 2002; SOU, 2013). These reports have stated that laymen's party affiliation is unlikely to influence the decision making in court. Yet, there has been no empirical support for such a claim. This was clearly an issue where new insight could be brought by applying the statistical methods used in economics.

Essay 2 uses JEL codes from the new categories *Microeconomics* (D) and *Public Economics* (H), as well as the old one *Labor and Demographic Economics* (J). Economic circumstances have been argued to be a major determining factor of attitudes to redistribution, but there is in fact very little well identified evidence at the individual level. This is the starting point for *Essay 2*. Given that individuals' attitudes also influence policy outcomes, understanding the determinants of individuals' demand for redistribution is a primary issue. The topic is relevant to both economists and political scientists, but whereas economists typically assume that preferences are stable, political scientists view social processes as shaping preferences. To investigate the potential link between attitudes to redistribution and economic circumstances, I use the Swedish National Election Studies, which are constructed as a rotating survey panel. Thereby it is possible to estimate how attitudes to redistribution (in the form of social benefits) changes in response to economic setbacks. The empirical analysis shows that individuals who experience a job loss become considerably more supportive of redistribution. Yet, attitudes to redistribution return to their initial level as economic prospects improve, suggesting that the effect is only temporary. Moreover, even though a job loss also changes attitudes to the political parties, the probability to vote for the left-wing is not affected.

Essay 3 is closer to the traditional topics studied by economists, and uses JEL codes from *Labor and Demographic Economics* (J) and *Public Economics* (H). In a well-functioning economy individuals experience periods of unemployment as structural changes and competition benefit some companies but not others. While reallocations are beneficial from an economy point of view, the individual costs can be vast. To be able to implement relevant polices, it is important for policy makers to know if individuals' ability to cope with such adjustments depends on their skills. It is well known that the probability of entering into unemployment is highest for individuals in the lower part of the skill-distributions,

whereas (conditional on being unemployed) the opposite is true for exiting unemployment. This indicates that cognitive and non-cognitive skills might be important for understanding the transition to new employment after a negative labor market shock, but, also, that there is important, non-random, selection of individuals into unemployment. Hence, one faces the methodological problem of endogenous job losses.

To solve this issue, *Essay 3* use the exogenous labor market shock provided by the substantial military base closures in Sweden following the end of the Cold War. The advantage of using these closures is, first, that individuals get displaced following a political decision, i.e. it should not be related to their productivity. Second, we have access to information on the individuals' cognitive (IQ-tests) and non-cognitive (evaluations by psychologists) skills from the military draft in Sweden. We find that, on average, labor earnings decrease and unemployment and labor-related benefits increase for those affected. We also observe heterogeneous treatment effects in terms of unemployment: the treated individuals with high non-cognitive and cognitive skills face lower unemployment effects than the treated individuals with low non-cognitive and cognitive skills.

My dissertation mirrors the development of economics. *Essay 2* addresses a topic that already interested the thinkers within classical political economy. Voters' preferences are typically the basis of any model in political economy, but if these attitudes also respond to economic shifts, this generates an interaction between politics and the economy, thereby affecting the observed political equilibrium. *Essay 3* is closest to the topics addressed by the neoclassical thinkers, by analyzing how job reallocations, that may be efficient at an aggregate level, can also be detrimental to the individuals directly affected. *Essay 1* is closer to the new field of law and economics and modern political economy, where the influence of decision makers' background on their behavior is an important issue. To the extent that individuals are found to behave differently depending on their political belonging or demographic background, policy outcomes will be different depending on what groups are given power.

So, what about the future of economics? Some say that the causal revolution has taken things too far, that economics needs to go back to examining fundamental economic questions, and that some important questions are not being addressed due to the requirement of credible causal connections. Others predict a development where the fragmentation of economics continues, as the social science fields become increasingly integrated, and the major divisions are based on topics rather than field. Yet, others argue that the division could just as well be based on

methodology. One indication of this is the increased interest for computer science among economists and political scientists.

Perhaps an attempt to pin down the core of economics in a few words is doomed to fail. Maybe one should simply stick to the words of Jacob Viner: "Economics is what economists do."[5]

[5] According to Backhouse and Medema (2009) the quote cannot be found in any of Viner's publication, but supposedly arose in a conversation.

References

Ainslie, G. (1991). Derivation of 'Rational' Economic Behavior from Hyperboliv Discount Curves. *American Economic Review (Papers and Proceedings*, 81(2):334–340.

Angrist, J. D. and Pischke, J.-S. (2010). The Credibility Revolution in Empirical Economics: How Better Research Design Is Taking the Con out of Econometrics. *Journal of Economic Perspectives*, 24(2):3–30.

Backhouse, R. E. and Medema, S. G. (2009). Retrospectives: On the Definition of Economics. *Journal of Economic Perspectives*, 23(1):221–233.

Cherrier, B. (2016). Classifying Economics: a History of the JEL Codes. *Journal of Economic Literature*, forthcoming.

Fourcade, M., Ollion, E., and Algan, Y. (2015). The Superiority of Economics. *Journal of Economic Perspectives*, 29(1):89–114.

Kahneman, D. and Tversky, A. (1979). Prospect Theory: An Analysis of Decision under Risk. *Econometrica*, 47(2):263–292.

Kelly, M. A. and Bruestle, S. (2011). Trend of Subjects Published in Economics Journals 1969-2007. *Economic Inquiry*, 49(3):658–673.

Lazear, E. P. (2000). Economic Imperialism. *Quarterly Journal of Economics*, 115(1):99–146.

Levitt, S. D. and Dubner, S. J. (2005). *Freakonomics: A Roughe Economist Explores the Hidden Side of Everything*. William Morrow, New York.

Milonakis, D. and Fine, B. (2009a). *From Economic's Imperialism to Freakonomics*. Routledge, New York.

Milonakis, D. and Fine, B. (2009b). *From Political Economy to Economics*. Routledge, New York.

Robbins, L. (1932). *An Essay on the Nature and Significance of Economic Science*. Macmillan, London.

Sandelin, B., Trautwein, H.-M., and Wundrak, R. (2008). *Det ekonomiska tänkandets historia*. SNS Förlag, Stockholm.

SOU (1994). *Domaren i Sverige inför framtiden -utgångspunkter för fortsatt utredningsarbete*. Number 1994:99. Statens Offentliga Utredningar, Stockholm.

SOU (2002). *Framtidens nämndemän*. Number 2002:61. Statens Offentliga Utredningar, Stockholm.

SOU (2013). *Nämndemannauppdraget -breddad rekrytering och kvalificerad medverkan*. Number 2013:49. Statens Offentliga Utredningar, Stockholm.

Stigler, G. (1984). Economics—The imperial science? *Scandinavian Journal of Economics*, 86(3):301–313.

Tversky, A. and Thaler, R. H. (1990). Anomalies Preference Reversal. *Journal of Economic Perspectives*, 4(2):201–211.

Appendices

A.1 Tables

Table A1. Classification 1948

Methodology (Analytical, historical, quantitative)	General (Aggregative or macrocosmic approach -connective subjects -methods and application)	Special (Segments or applied fields -conventional subdivisions)
1. Economic Theory	4. Economic Systems,	7. Money and Banking etc.
2. Economic History,	Planning and Reform,	8. Business Finance etc.
National Economics	Co-operation	9. Public Finance
3. Statistical Methods	5. National Income etc.	10. International Economics
	6. Business Fluctuations	11. Business Administration
		12. Indus. Org, Pub. Reg.
		13. Public Utilities etc.
		14. Industry Studies
		15. Land Economics etc.
		16. Labor
		17. Social Welfare

Note: See Cherrier (2016) for further details.

Table A2. Classification 1967

0. General Economic Theory, History, Systems
1. Economic Growth, Development, Planning, Fluctuations
2. Quantitative Economic Methods and Data
3. Domestic Monetary and Fiscal Theory and Institutions
4. International Economics
5. Administration, Business, Finance, Marketing, Accounting
6. Industrial Organization, Technological Change, Industry Studies
7. Agriculture, Natural Resources
8. Manpower, Labor, Population
9. Welfare Programs, Consumer Economics, Urban and Regional Economics

Note: See Cherrier (2016) for further details.

I. Political Bias in Court? Lay Judges and Asylum Appeals

1 Introduction

All individuals are entitled to a "fair and public hearing by an independent and impartial tribunal, in the determination of his rights and obligations", according to the United Nations' Universal Declaration of Human Rights (1948/UDHR, § 10). Hence, understanding to what extent judicial decisions are influenced by the decision makers own values and beliefs is important in terms of equality of rights, as systematic variation would raise concerns about fair trials. Several countries practice a system where laymen participate in court, either as jury members or lay judges.[1] Laymen are thought to represent the people and their participation has historically been viewed as a way to ensure democratic control of the courts, by preventing the state to use them as a means to enforce their power. Moreover, laymen have been argued to complement the professional judges' legal knowledge and experience, by representing a common sense of justice in the judicial decision making.[2] Yet, there are also concerns that laymen, who lack legal education, will be influenced by their own personal beliefs.

This paper looks at the effect of lay judge's partisan affiliation when ruling in asylum appeals in the Migration Courts in Sweden. If an appeal is approved the asylum seeker is typically given a permanent residence permit, meaning that he or she can live and work under the same conditions as every other Swedish resident. On the other hand, asylum seekers who got their appeals rejected are expected to return to their native countries. Needless to say, these court rulings have major consequences for asylum seekers' future prospects.

The paper makes several important contributions. First, it adds to the literature on judicial decision making, where the influence of laymen's partisanship has not previously been studied with quantitative methods. In fact, very few papers have studied the influence of laymen, probably due to data limitations or the fact that the participating laymen in some judicial systems are not randomly selected. The Swedish system offers a suitable setting, since laymen (nämndemän) are affiliated with the political parties and the assignment of cases to laymen is random, given that laymen serve on a pre-determined schedule and cases are handled on a first come, first served basis. Thereby, the political

[1] Unlike juries, lay judges do not form a jury separate from the judges, but decide on both questions of guilt and sentencing together with the judge. Some countries (such as the UK, Australia, and the US) also have Justices of the Peace, judicial officers who lack legal education and mainly deal with misdemeanor cases.

[2] Russia, Japan and Spain have recently re-introduced lay participation in the courts after previously abolishing it, viewing it as an important part of a democratic system (Diesen, 2011).

composition of the court committee is unrelated to characteristics of the case. Asylum cases are also well suited for investigating the influence of partisan affiliation, given that cases are numerous, rather similar, and there is substantial variation between the political parties regarding their stand on the refugee issue. Second, since the laymen rule together with a judge, I can also analyze the interaction between laymen and different types of judges as well as the variation between judges. Third, the number of refugees has increased dramatically in Europe, in response to recent conflicts (see Figure A1). At the same time, anti-immigrant parties throughout Europe mobilize voters with campaigns that demand more restrictive policies towards immigrants and asylum seekers (e.g., Norris, 2005; Dancygier, 2010). Hence, it is important to examine the degree of discretion in asylum decisions.[3]

The literature on biases in judicial decision making has mainly focused on judges. A notable exception is Anwar et al. (2012) and Anwar et al. (2014), who find that the age and race of the jury pool affects the conviction rate in criminal cases. Looking at judges, on the other hand, several studies have examined the existence of an in-group bias with respect to gender, race and ethnicity (e.g., Boyd et al., 2010; Glynn and Sen, 2015; Shayo and Zussman, 2011; Kastellec, 2012; Abrams et al., 2012; Lim et al., 2015a). There are also some studies examining the impact of judges' nationality in international courts (e.g. Voeten, 2008) and sports competitions (Zitzewitz, 2006; Emerson et al., 2009). A few papers have also examined the influence of party affiliation. Sunstein et al. (2006) and Hall (2010) find that partisanship affects how judges vote in the US Courts of Appeal on several issue areas. On the other hand, Lim et al. (2015a) find no effect of judges' partisan affiliation in Texas State District Courts on criminal sentencing decisions.

While most studies have focused on the variation between judges, judges could also be sensitive to other surrounding factors. Lim et al. (2015b) find that press coverage increase the sentence length by non-partisan elected judges, Shayo and Zussman (2011) show that judges' in-group bias is associated with terrorism intensity, and Danziger et al. (2011) find that parole decisions are affected by the proximity to judges' food breaks. Overall, there is at least some evidence that judicial rulings can be swayed by factors that should be completely unrelated to the merits of the case.

Beyond judicial decision making, the paper is related to a larger body of research which examines the influence of agents' preferences on collective decision making. A number of experimental studies have shown

[3]For discrimination in naturalization decisions see Hainmueller and Hangartner (2013).

that individuals tend to favor their own group (for instance defined by ethnicity, gender or political affiliation), but discriminate against others (e.g., Bernhard et al., 2006; Leider et al., 2009; Rand et al., 2009). However, there are also studies showing that deliberation can alter opinion. For instance Zitek and Hebl (2007) find that social influence can alter prejudice-related attitudes, such that hearing one person condemn discrimination can influence another one to do the same. Yet, the effect of deliberation varies with the initial preference heterogeneity between the individuals, the quality and diversity of the arguments as well as participants degree of open-mindedness (e.g., Barabas, 2004; Goeree and Yariv, 2011).[4]

In Sweden, lay judges existence has been a hotly discussed topic, partly as a result of the recent success for the anti-immigrant party the Swedish Democrats, giving them political power as well as representation in court.[5] Lay judges' are expected to be impartial, and their current participation in the courts is mainly motivated by the hope that it will increase the courts' transparency and thereby support the public's confidence in the courts (SOU, 2013). However, according to surveys 30-60 percent of the laymen stated that their work as lay judges was influenced by the ideology of their party (Rundkvist, 1995; Dahlgren, 2011), and and many judges think the system should be abolished (Cronholm, 2014).[6] Yet, descriptive studies have shown that most cases are decided unanimous, and it's extremely rare for the laymen to overrule the judge (e.g., Karnov Nyheter, 2012; Diesen, 1996). Based on these studies, the common belief, so far, appears to be that there is little disagreement within the courts and that laymen's participation has no influence on the judicial rulings (e.g., SVT, 2012; SOU, 2013).[7] This is not necessarily the case. First of all, previous studies have found a strong consensus norm

[4] A growing game-theoretic literature also models how individuals can behave strategic when making collective decisions under incomplete information, by choosing whether or not to reveal their private information (e.g., Austen-Smith and Feddersen, 2006; Iaryczower et al., 2014).

[5] For instance, Anne Ramberg, secretary in general for the Swedish Bar Association, has argued that lay judges from the Swedish Democrats do not fulfill the requirement of impartiality (Ramberg, 2012).

[6] Surveys have also shown that lay judges sometimes find it difficult to separate the question of guilt and sanction, arguing that weak evidence should result in a less severe punishment (Diesen, 1996).

[7] Several Swedish Government Official Reports (SOUs) have examined the laymen system and more or less stated that there are nu reasons for concern: "A weakness of the current system is, as illustrated by the critique, that it could cause suspicions that laymen are influenced by their partisan belonging. We emphasize that there is no such evidence." (SOU 1994, p. 303), "The fact that laymen are elected by the political parties is not incompatible with the courts independence from the political system" (SOU 2002, p. 71), "The number of cases where the judge is outvoted by

among judges (e.g. Fischman, 2011), i.e. participants that do not agree with the majority will not always state this, leading descriptive studies to underestimate the amount of disagreement. Second, experimental studies have shown that deliberation can alter opinion (e.g. Goeree and Yariv, 2011). Hence, it is possible that laymen will affect the opinion of their co-judges.

I collect a unique data set consisting of all asylum cases in the Swedish Migration Courts 2011-2013 where lay judges participated, and focus on cases with an oral hearing (around 7 000 case files). I also gather information about the laymen's party affiliation, gender, and age. Whereas studies looking at criminal cases use a variety of outcome measures, it is straightforward how to categorize and interpret the judicial decisions in asylum appeals, as the outcome falls into two categories -reject or approve.[8]

The results show that the approval rate is affected by the predicted policy position of the court committee, based on the laymen's partisan affiliation. Looking at specific parties I also find that the approval rate is affected by the laymen's party affiliation. The approval rate is around 2.5–4 percentage points higher when laymen from the Left Party, The Green Party or the Christian Democrats participate, whereas it is around 4.5 percentage points lower when laymen from the Swedish Democrats participate, compared to cases where only laymen from the Moderate Party and the Social Democrats participate. Compared to an average approval rate of 21 percent, these effects are substantial. It is difficult to disentangle if the results are driven by voting power or deliberation, although the probability of approval increases with the number of pro-immigration laymen. I also detect a large variation between the regular judges approval rate, despite the fact that cases are randomly assigned to judges. This suggests that individual judges consistently apply their own interpretation of the law. Taken together, this raises concerns about the practice of justice to asylum seekers appellations. Concurrent with this paper, and well in line with the results, Anwar et al. (2015) find that convictions for defendants with Arabic sounding names increase when a layman from the Swedish Democrats participates in the Gothenburg District Court. This suggests that laymen's influence is not confined to the Migration Courts.

---

the laymen are very few (see the study by Karnov Nyheter (2012)), and can not be considered as a problem" (SOU 2013, p. 122).

[8]For instance, Lim et al. (2015a) use different measures of harshness, Sunstein et al. (2006) code decision as being liberal or conservative, and Anwar et al. (2012) use the conviction rate.

The remainder of the paper proceeds as follows. The next section discusses the institutional background, section 3 describes the data and measurements, and section 4 formalizes the empirical strategy. Section 5 presents the empirical results and, finally, section 6 concludes.

2 Institutional background

Immigrants whose application for asylum has been rejected by the Migration Board (Migrationsverket) are able to have their decision reconsidered.[9] In 2006 appeals were transferred from an administrative process to a court process, aiming to strengthen the rule of law and increase transparency. There are 12 administrative courts in Sweden, and three of these (Stockholm, Gothenburg, and Malmö) also serve as Migration Courts since 2006. In 2013 a fourth Migration Court was established in Luleå, taking over part of Stockholm's judicial district. The Migration Courts mainly handle asylum cases, i.e. applications motivated by risk of persecution or armed conflicts in the individuals' native country, and family cases, where the motive is to join relatives or a partner in Sweden. Asylum applicants can get a residence permit based on being a refugee, i.e. risk of persecution due to race, nationality, religion, gender, sexual orientation, political views, or belonging to a particular social group, but a person who is not a refugee can also get a residence permit if they are in the need of protection.[10] Applicants who are not judged to need protection can receive a residence permit based on "exceptionally distressing circumstances" such as serious health issues, adaptation to Sweden, or the situation in the complainant's native country.

If the appeal is approved the complainant typically gets a permanent residence permit.[11] After living in Sweden for around five years, the asylum seeker can apply to become a Swedish citizen. On the other hand, if the

[9] The Migration Board's decision should be appealed within 3 weeks, since the decision is thereafter given legal force. If the decision is not appealed, the asylum seeker is expected to leave the country. Failure to do so may result in a re-entry ban to the Schengen Area for one year.

[10] Individuals can get a residence permit based on being in need of "subsidiary protection" (e.g., risk of being sentenced to death, subject to torture or risk of injury due to armed conflict) or in need of "other protection" (e.g., due to armed conflict, environmental disaster, or risk of serious violation). The possibility to get "other protection" only exists in the Swedish Aliens Act, and has no equivalent in EU legislation or international conventions.

[11] Approval could also mean that an individual is given a temporary residence permit (never less than a year), that the case is returned to the Migration Board (this should only be coded as approval if this was one of the applicants' claims), or that the country/countries the asylum seeker will be evicted to has been changed. For cases

court rejects the appeal, the asylum seeker can apply for a leave to appeal at the Supreme Migration Court. Less than one percent of these cases are given leave to appeal, since the Supreme Court only takes on cases that are of interest regarding precedent or where extraordinary mistakes have been conducted. Thus, in most cases the Migration Courts' rulings constitute the final decision.

It is often difficult for asylum seekers to make their identity probable and present a comprehensive line of argument, particularly since documents and ID records are not always available. For some countries, e.g. Afghanistan and Somalia, the evidence value of a passport is also very low, since they do not fulfill the security requirements. Sometimes a language analysis is performed to assess the asylum seekers legal domicile. A medical examination can also be performed to evaluate the asylum seekers age, but it only delivers an approximate age interval.[12] The European Qualification Directive states that when aspects of the applicant's statements are not supported by documentary or other evidence, those aspects shall not need confirmation, if (among other things) "the general credibility of the applicant has been established" (2004/83/EC, § 4).[13] Thus, assessing the asylum seekers trustworthiness is often an essential part of the investigation process, and lack of credibility is a common reason for the Migration Board to reject an application.[14] When evaluating an asylum applicant's credibility, particular focus would be given to whether the applicant's story is coherent, detailed, corroborated by information regarding the situation in the asylum seeker's native country, and has remained constant during all stages of the asylum evaluation (Diesen et al., 2012).

---

where someone dissented in 2012 I have coded the actual decision. Out of 166 cases that were partly/fully approved only 13 were not given a permanent residence permit.

[12] Whether an asylum seeker is a child or an adult affects the evaluation of whether the requirements for asylum are fulfilled.

[13] Asylum applicants who had their asylum request rejected could also appeal to the European Court of Human Rights. For instance, Sweden was convicted in the case R.C. v. Sweden, when the court stated that there were substantial grounds for believing that the applicant, if deported to Iran, would be subject to torture or ill-treatment. Sweden has also been convicted a number of times by the UN Committee against Torture due to deficiencies in assessing asylum seekers credibility. The committee has emphasized the fact that the existence of implausible claims is not a reason in itself to dismiss an application, since the credibility assessment should focus on the essential parts of the applicant's narrative (UNHCR, 2011). For instance, incorrect details about the applicant's itinerary or family should be of no importance when assessing whether the applicant requires protection. Incorrect information could be due to lack of confidence for authorities, that the asylum applicant is trying not to reveal sensitive information about others, post-traumatic stress disorder etc.

[14] UNHCR (2011) examined 200 cases were individuals from Iraq, Iran, Somalia and Russia had applied for asylum to the Swedish Migration Board. In 38 percent of the cases that were dismissed, lack of credibility was part of the motivation.

2.1 Lay judges in the courts

Lay judges are elected by the county council, and the elections are proportional, meaning that the number of lay judges nominated by a political party will reflect the party's share of seats in the county council.[15] In the beginning of an election period the county council elects the laymen for the following four years (i.e. the laymen in this study were elected in the fall 2010). It is up to the parties to decide who is at suitable candidate for their party, and most parties require their candidates to be party members (SOU, 2013).[16] Laymen participate both in the administrative and general courts. The fact that different courts deal with various areas of the law, could affect the type of individuals that are elected for different courts within a given party.[17] To be eligible a person also has to be a Swedish citizen, registered in the county, and at least 18 years old. The law states that the regional councils should aim for a versatile composition of lay judges, regarding gender, ethnicity, age and occupation. Whereas recent statistics show that the share of lay judges that are women or have a foreign background is similar to the general public, older lay judges are still over represented (SOU, 2013). 40 percent of the laymen in 2011 were older than 65 years, indicating that many laymen are retired. The fact that the laymen are not representative of the population has been a growing public concern.[18]

Cases in the Migration Court are decided either by a professional judge, or by a professional judge and a court committee.[19] Laymen typically participate in asylum hearings, whereas most family cases are decided by a judge. According to the web page of the Swedish National Court Administration (Domstolsverket) neither the judge nor the laymen can

[15] The law states that the elections have to be proportional if the number of county councilors in favor of proportional elections is larger than the total number of councilors divided by the number of councilors in favor plus one. In practice, the elections are always proportional.

[16] According to a survey of how parties recruit lay judges, only the Green Party stated explicitly that they wanted suggestions of candidates that were not members (SOU, 2013).

[17] For instance, party members with a particular interest in the refugee issue might be more likely to sign up as laymen in the Migration Courts rather than the District Courts. If this is the case laymen in the Migration Courts may not represent the average policy opinion of their party.

[18] In 2010 the government asked the Swedish National Courts Administration to conduct an information campaign specifically targeted at increasing the share of young laymen (DV, 2011).

[19] The court decides whether laymen should participate or not, and the general rule is that cases that can be considered as "simple cases" ("av enkel beskaffenhet") can be decided without lay judges. If laymen participate, the legally qualified judge is responsible for leading the discussion with the lay judges and explaining the legal requirements to attain a residence permit.

choose what cases to participate in, since the court randomly assigns cases. In practice, cases are randomized across departments at the administrative courts (using the computer system VERA), and within each department cases are distributed between judges according to turn-taking.[20] Each department consists of up to five judges and a number of rapporteurs and assistants. Regular judges rule in all types of migration cases, but junior judges that work at the courts during their training (6-month periods) are given fewer and possibly "easier" cases. Moreover, the randomization between departments in Stockholm accounts for the fact that some departments are specialized at certain countries.Cases are handled on a first come first serve basis, and asylum cases are given priority over other cases, such as family cases.[21]

In the court, cases are presented either by a rapporteur or during an oral hearing, where both the asylum seeker and a representative from the Migration Board participate.[22] The laymen and the judge have the possibility to ask questions to the asylum seeker or the rapporteur to gather more information. Afterwards they deliberate and decide whether to approve or reject the appeal. If there is no oral hearing, the rapporteur (and the judge) have usually prepared a draft proposal before the presentation in court. Each individual (including the judge) has one vote, and if the voting is inconclusive the judge has the decisive vote. All dissenting opinions are recorded if the decision is not unanimous. Cronholm (2014) conducted a survey of laymen and judges, and found that they had vastly different opinions regarding the workings of the system. 35 percent (3 percent) of the laymen (judges) stated that the laymen ask questions often or always, 96 percent (43 percent) responded that they were active or very active during the hearing and 56 percent (12 percent) stated that the influence of their opinion was large or very large.

2.2 Court committees and scheduling

In Stockholm, departments that deal with migration cases only focus on these cases, whereas departments in Malmö and Gothenburg are typically specialized at two different types, such as migration and social in-

[20] The only exceptions is if a judge is ill, on vacation, or has a too high workload. Then the judge is typically not assigned new cases for a temporary period.

[21] According to the governments yearly objective for the Administrative Courts, i.e. 90 % of the migration cases should be decided within 4 months.

[22] An oral hearing is typically held if the evaluation of the asylum seekers trustworthiness is seen as essential information. The court can suggest that an oral hearing should be conducted, but in most cases it is the asylum applicants' claim. The rapporteur preparing the case then decides (together with the judge) if it is necessary.

surance.[23] In the beginning of a term the court divides the newly elected laymen into court committees (groups of three persons). In Gothenburg laymen are asked about preferences for what weekday they would like to serve, and laymen in Stockholm are asked if they prefer to participate in a specific type of cases (e.g. migration cases). To the extent possible, these preferences would be taken into account when creating the court committees. The committees in Gothenburg always participate the same weekday (e.g. every fourth Wednesday), whereas the committees in Malmö and Stockholm are not connected to a particular day of the week. Moreover, the court in Malmö stated that they try to create groups that are balanced on gender and party affiliation, the court in Gothenburg aim to create groups that are balanced on gender, and the Stockholm court stated that they focus on gender and age.[24] Laymen are supposed to sit in the same group for the following four years, although given the fact that some laymen resign before their term ends, some groups change as new laymen are elected to fill vacancies.

Once a year lay judges are given a schedule for the upcoming year, and typically serve one day each month.[25] A few days before the laymen are scheduled to attend they will receive documents from the courts containing information about the cases they will participate in (such as the parties' claims). Laymen are paid 800 SEK ($\approx$ \$ 95) for a full day, but can also get reimbursement for travel expenditures and lost earnings.

In Malmö and Stockholm the departments are connected to different courtrooms, i.e. when cases are scheduled they would use the courtrooms reserved for their department. In Gothenburg rooms are not connected to different departments, but they are scheduled in order, i.e. on a given day cases are first scheduled to courtroom one and when it is fully booked cases are scheduled to room two and so on. When the court committees are scheduled they are also assigned to specific courtrooms. All groups are given a specific number, and they are scheduled in that order, i.e.

[23] Lay judges in Gothenburg and Malmö are not connected to a specific department at the court and serve on all type of cases, whereas lay judges in Stockholm are connected to a particular department and mainly participate in the type of cases handled by that department.

[24] The courts in Gothenburg and Malmö also try to create groups consisting of individuals living close to the different hospitals in the region, since court committees sometimes go there with the judge to decide on cases concerning custodial care. In the jurisdiction covered by the court in Gothenburg/Malmö, hospitals are located in three/four different areas.

[25] If a layman is unable to attend the day they are scheduled they are responsible for notifying the court well in advance. In Malmö laymen are also responsible for finding another layman to replace them, whereas this is done by the court in Stockholm and Gothenburg. The court would then contact laymen, who stated that they can take on extra shifts, in alphabetical order.

group one is assigned to the first available day and courtroom in the calendar, then group two and so on. When all court committees have been assigned a day, the procedure starts over until the whole year is booked. Hence, this is done well before it is known what cases will be scheduled on a given day. Most importantly, laymen are not able to choose what cases to participate in.

3 Data and measurements

The Swedish National Court Administration provided me with a list of the case ID:s of all cases where laymen participated in the Migration Courts in Stockholm, Gothenburg, and Malmö 2011-2013.[26] I then restrict the sample to cases that were coded as asylum cases with an oral hearing by the courts, since there are no drafted proposals for these cases.[27] Almost all court rulings come from the data base JP Rättfallsnet Migration, to which the courts send case files, i.e. court rulings and possible attachments, each week. All cases not found there (mainly cases with classified attachments from the court in Stockholm) were collected directly from the courts.[28] The final data set constitutes around 7 000 pdf-files

When laymen participate in court the name of the three lay judges is always written in the case files. By writing a program in Python I have automatically extracted the case ID and names of the participating laymen from the case files, as well as other information mentioned such as countries, religions, and specific words related to conversion, sexual orientation, and gender related topics. See the Appendix (section A.3) for an exact description of each category. Unfortunately I do not know in what context the information is mentioned. For instance, the fact that "homosexual" is mentioned would probably mean that the asylum seeker has applied for a refugee status and residence permit based on persecution due to sexual orientation, but I do not know this for sure. If the judicial ruling was not unanimous, the case files would also contain

[26] The migration court in Luleå is not included since they only handle a very small share of asylum appeals, less than 30 cases (code 60/01 and 60/03) where laymen participated in 2013.

[27] Code 60/01 is labeled "residence permit: adults and family members" and code 60/03 "residence permit: child without legal guardian".

[28] If a case contains sensitive information the court can decide to classify such information. It will then be excluded from the case file, and put in a confidential attachment. As a general rule, the court in Stockholm has decided not to send cases with classified information to JP Infonet. Gothenburg and Malmö send cases that are confidential, but of course the attachments with the confidential information are not included.

information about who dissented. Although it is possible to extract if anyone disagreed automatically, I have to code who dissented manually (since it is not written in a uniform manner in the case files).

I have also collected all information that the courts register for each case, such as case ID, the name of the legally qualified judge, the court department in charge of the case, and whether the case was appealed to the Supreme Court.[29] The court also registers if the case was fully approved, partly approved or not approved. In the following analysis I code cases as approved if they were either partly or fully approved.[30] Information about the lay judges' names and political belonging has been collected from the county councils. These lists also contain information about age and gender for most laymen, and I code gender based on name for all laymen without this information. I also code whether laymen have a Scandinavian or non-Scandinavian first and surname. The administrative court in Gothenburg has close to 300 lay judges, Malmö has around 350, and Stockholm a bit more than 700 lay judges.

Based on the case ID and names of the lay judges I merge the information from the case files with information from the case registers as well as the lists of laymen.[31] Around 5 percent of the observations are excluded, since I have not managed to find a unique match between the names and the party lists for all three participating laymen.[32]

[29] For around 100 cases I lack information about which judge the case was assigned to. These cases are still included, and the judge is coded as "unknown judge court X".

[30] Cases that are partly approved typically have multiple claims. For instance, many appeal to get both a residence permit and a refugee status. If such a case is partly approved that would generally mean that the asylum seeker is given a residence permit, but not a refugee status. However, during the manual coding of dissents I noted that some of these cases were instead coded as fully approved. Hence, I only use the distinction approved or not approved in the analysis.

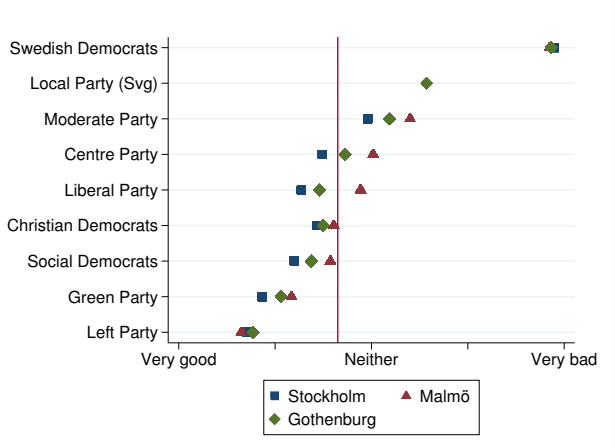
[31] Sometimes a case file contains multiple case ID. This is often due to the fact that family members might have appealed their cases to the court at different points in times, and each appeal gets its own case ID. If their asylum motives are similar, the court will handle it as one case and there will only be one case file. Hence, the unit of observation will be case files, rather than case ID.

[32] 50 % of these cases are from Stockholm, and 25 % from Gothenburg and Malmö respectively. In most cases the lack of a match is due to the fact that the name is not extracted properly. The case files are scanned pdf-files, and the text has been digitized by OCR. Given that the files are scanned, the OCR is not perfect and will sometimes produce minor typos. I correct for minor variations of a name (due to typo and spelling errors), but for larger errors it is not always possible to find a unique match with the names on the county councils' lists of lay judges. In some rare cases I have the full name, but cannot find the individuals party affiliation. These individuals are most likely replacement laymen, i.e. someone elected in the middle of the term to replace a layman who did not sit a full term.

3.1 Attitudes to refugees

The extent to which laymen's party affiliation is expected to affect their behavior in court, naturally depends on whether there are any differences regarding the political parties' stand on the refugee issue. The figures below display the result from two different surveys sent out to politicians and the public, see the Appendix for exact wording of the used questions. Besides the seven political parties that are represented in all three Migration Courts, there is a local party (the Health Care Party in Västra Götaland, Svg) in Gothenburg, and an anti-immigrant party (the Swedish Democrats, Sd) represented in Gothenburg and Malmö. Figure 1 is based on a survey sent out to all local and regional politicians, asking them about working environment and policy issues.[33] The figure shows the parties' average attitudes to receiving more refugees to their municipality, and the red line indicates the national average position. The sample is restricted to politicians in the jurisdiction of the courts.[34] The parties' attitudes largely track the traditional left-right scale, although some parties (particularly the Christian Democrats) are closer to the left-wing parties (V, S and Mp) than usual. The Left Party (V), the Green Party (Mp) and the Social Democrats (S) are most positive to receiving refugees. The Christian Democrats (Kd), the Liberal Party (Fp) and the Centre Party (C) are close to the national average, whereas the most negative parties are the Moderate Party (M), the local health care party (Svg), and, in particular, the Swedish Democrats (Sd).

Figure 1. Receive more refugees to municipality



[33]The KOLFU survey was sent out in 2008 and 2012. See Gilljam et al. (2010) for a description of survey results.

[34]The results for the parties national average position is displayed in Figure A2.

Figure 2. Motive for asylum (weight given to different circumstances)

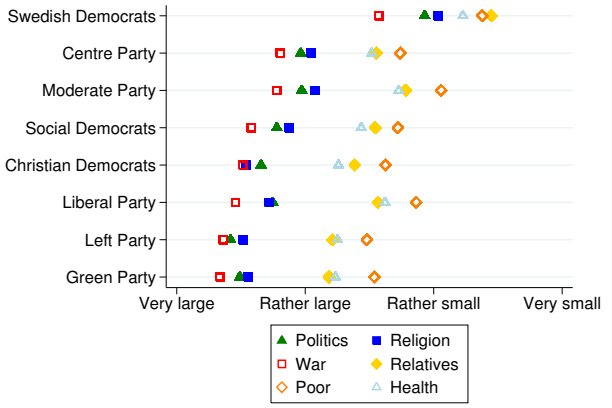


Figure 2 is based on another survey sent out yearly to a representative sample (age 16-85) of individuals living in Sweden, asking them about their opinion on a number of different policy issues.[35] Individuals are asked about what weight different circumstances (war, health issues, religious/political persecution, poverty, relatives) should be given for refugees to receive a residence permit in Sweden. Figure 2 displays the average values, based on the individuals declared party preference. War and political and religious persecution are viewed as the most important circumstances. The order of the parties is fairly similar to Figure 2, although supporters of the Christian Democrats and the Liberal Party put more weight on these circumstances than the Social Democrats.

Figure A3 also show the results from these two surveys, but based on the respondents' background characteristics (e.g. gender and age). The differences are very small, indicating that party affiliation, rather than demographic characteristics determine attitudes to immigration.

[35]This specific question was included in the SOM survey 2012 and 2013, and is included in a project directed by Marie Demker and financed by FORTE (see Sandberg and Demker (2014)). The survey was conducted by the SOM-institute at Gothenburg University. The principal investigator was Henrik Oscarsson. The survey data has been made available by the Swedish National Data Service (SND). Neither SND nor the principal investigators bear responsibility for the analytical findings in this paper.

3.2 Summary statistics

The data set is summarized in Table 1.[36] The average approval rate is 21 percent, and varies somewhat between the courts (18-25 percent). The approval rate is highest in Gothenburg, but this could be due to the fact that the asylum applicants' native countries also differ between the courts (see Table A2). There are around 170 judges in total, and the regular judges (rådmän) have handled around 55 asylum cases each during 2011-2013, whereas the junior judges (fiskaler) have managed very few cases. Most court rulings are unanimous, but in 17 percent of the cases at least one person dissented. Most cases that are not approved are appealed to the Supreme Migration Court.

I only include cases that the courts have coded as asylum cases, and these are divided into two categories - children (5 percent of the cases) or adults/families. Moreover, 9 percent mention conversion, 5 percent of the cases state sexual orientation, 15 percent mention something potentially related to persecution due to gender (i.e. rape, genital mutilation, compulsory marriage, honor violence), and around 22 percent of the cases mention something that presumably concerns politics (i.e. political opinion/activity). These circumstances could be related to persecution in the asylum seekers native country, and thereby motivate the approval of the appeal. However, it is possible that more cases actually mention conversion, sexual orientation, or gender related issues, as this information could be classified, given that it is very sensitive for asylum seekers from particular countries.[37] Around 12 percent of the cases mention nationless, although around 55 percent of these cases also have a native country identified. One of the major determinants for whether an asylum seeker gets a residence permit is the current conditions in their native country. Table A2 in the Appendix display the average approval rate by country, for countries most likely to be the asylum seekers native country (see description in section A.3).[38] As expected, countries with recent and ongoing conflicts, such as Afghanistan and Syria, have a higher approval rate.

[36] Note that a case could include several asylum seekers, since a family would typically be handled as one case.

[37] When classifying information, the judge and the laymen can choose to either just classify the name of the asylum seeker, or to classify part of the sensitive information. Even when a case has classified information, the asylum seekers' claims would most often still be written in the case file, but specific details about certain events or assaults would be classified. Hence, under reporting of these sensitive variables is expected to be minor. Unfortunately there is no statistics on how common the different approaches are, but Stockholm and Malmö stated that they typically only classify the name.

[38] As an alternative, Table A3 show the average approval rate by country, for all cases mentioning a given country.

Table 1. Summary statistics

	mean	sd	min	max
Approval	0.207	0.405	0.000	1.000
Disagreement	0.170	0.376	0.000	1.000
Appeal (Supreme Court)	0.747	0.435	0.000	1.000
Cases/Judge	55.902	37.529	1.000	167.000
Cases/Junior Judge	6.439	5.654	1.000	21.000
Closed hearing	0.406	0.491	0.000	1.000
Confidential	0.262	0.440	0.000	1.000
Case cov.				
Judgement of age	0.037	0.188	0.000	1.000
Language analysis	0.116	0.320	0.000	1.000
Child/ren (no caregiver)	0.047	0.211	0.000	1.000
Christian	0.109	0.312	0.000	1.000
Muslim	0.229	0.420	0.000	1.000
Convert	0.086	0.280	0.000	1.000
Sexual orientation	0.046	0.209	0.000	1.000
Gender related	0.152	0.359	0.000	1.000
Politics	0.221	0.415	0.000	1.000
Health	0.177	0.382	0.000	1.000
Nationless	0.119	0.324	0.000	1.000
Court committee cov.				
Average age	60.030	7.750	22.667	78.333
Women (at least two)	0.504	0.500	0.000	1.000
Non-Scandinavian (at least one)	0.218	0.413	0.000	1.000
Policy position	2.763	0.325	1.914	4.280
Policy position (local)	2.732	0.396	1.755	4.367
Left Party	0.122	0.327	0.000	1.000
Green Party	0.209	0.407	0.000	1.000
Social Democrats	0.648	0.478	0.000	1.000
Christian Democrats	0.165	0.371	0.000	1.000
Liberal Party	0.295	0.456	0.000	1.000
Centre Party	0.126	0.332	0.000	1.000
Moderate Party	0.769	0.422	0.000	1.000
Swedish Democrats	0.105	0.306	0.000	1.000
Local party (Svg)	0.043	0.204	0.000	1.000
Observations	6759			

Note: See section A.3 for a description of case characteristics.

Table 1 also displays the characteristics of the court committee. In half of the cases the court committee consists of a majority of women, and every fifth case has a court committee with at least one person with a non-Scandinavian name. The average age is relatively high, at 60 years (in 2011). Looking at the policy position (described below), there is not much difference between using the average national or local policy position. Most court committees consist of laymen from the Moderate Party (almost 80 percent of the cases), the Social Democrats (around 65 percent), and some other party. In 22 percent of the cases the court committee consists of only laymen from the Social Democrats and the Moderate Party. The Swedish Democrats are not represented in Stockholm, but laymen from their party participate in about one fourth of the cases in Malmö, and one tenth of the cases in Gothenburg. Laymen's background information is displayed in Table A1. Not surprisingly, the Green Party and the Left Party have the highest share of women and laymen with non-Scandinavian names, whereas the Swedish Democrats has the lowest share.[39]

3.3 Direction of disagreement

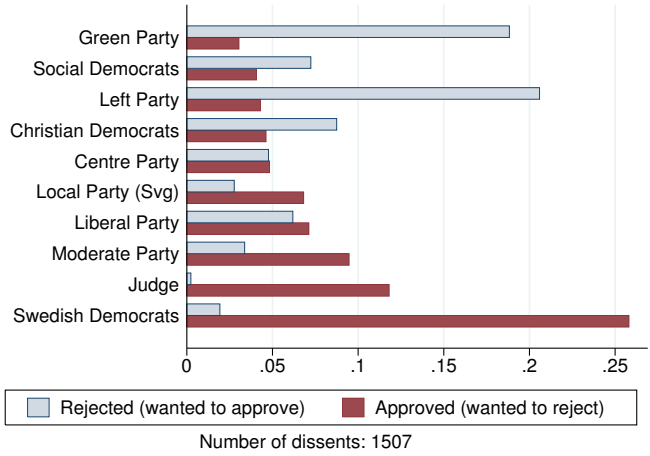
In cases that are decided unanimous, it is not possible to know how laymen from different parties have behaved during the deliberation, but the dissenting opinions can indicate if laymen from specific parties systematically express different opinions. If the judge or at least one of the laymen dissented this is always written in the case file, along with a short motivation. The motivations typically focus on having a different opinion regarding the asylum seekers trustworthiness and, thereby, need of protection. Other than that, laymen or the judge sometimes dissent arguing that that the age determination is incorrect, that the judgment of the current conditions in the asylum seeker's home country is inaccurate, or that the case should be returned to the Migration Board due to an inadequate investigation. Also, sometimes the judge and the laymen agree about the fact that the asylum seeker is not in need of protection, but disagree about whether the requirements for "exceptionally distressing circumstances" are fulfilled. Figure 3 displays the share of dissents for laymen from each party as well as the judge. For cases that were either fully approved or rejected, the dissents are always in the opposite

[39]Note that the average age of all laymen is lower than the average age of the participating laymen. This is due both to the fact that younger laymen are more likely to resign in advance, and the fact that a younger layman who is unable to attend when s/he is scheduled is more likely to be replaced with someone older, since older laymen tend to be more available with a short notice.

direction, whereas it is possible to dissent in both directions for cases that were partly approved.[40]

The direction of disagreement largely tracks the party differences in Figure 1 and 2. Looking at cases where laymen from the Swedish Democrats participated, one finds that they dissented in around 25 percent of the cases where they participated and the majority opinion was to fully approve the case. Similarly, the Moderate Party dissented in close to 10 percent of these cases. On the other hand, looking at cases where the majority wanted to reject the appeal, the Left Party and the Green Party disagreed in around 20 percent of these cases. The Social Democrats and the Christian Democrats were also more likely to dissent in rejected than approved cases, whereas the Centre Party and Liberal Party were quite balanced, with almost the same probability of dissent in both approved and rejected cases. Moreover, the judge was outvoted 185 times, corresponding to almost 3 percent of all asylum cases. In almost all of these appeals the judge wanted to reject the case, whereas all the laymen wanted to approve it.

Figure 3. Share of dissents in fully approved/rejected cases



[40]Cases where someone agrees about the court ruling, but disagrees about the reasoning (e.g. what protection category the individual belongs to) are not coded as dissents. Given that it is rare for cases to be partly approved, there are also very few dissents in those cases, but Figure A5 shows the dissents for them.

4 Empirical specification

Identification requires that the political composition of the court committee is exogenous to unobservable characteristics of the case, which is achieved by the random assignment of laymen to cases. The dependent variable y_{ijct} is a binary variable for whether the appeal was approved (fully/partly) or rejected for case i, with judge j, in court department c, and in year t.

$$y_{ijct} = \beta_0 + \beta_1 D_{ijct} + \beta_2 X_{ijct} + \beta_3 Z_{ijct} + \theta_j + \theta_c + C * \theta_t + \varepsilon_{ijct} \qquad (1)$$

The variable of interest (D_{ijct}) refers to the court committees' party affiliation, and it is measured in two ways. First, the committees' predicted average policy position is based on the parties' response to the survey question about receiving more refugees to their municipality (using the KOLFU survey described in section 3.1). The average position of a court committee (cc) is measured as $\sum_{p \in cc} \frac{s_p}{3}(\omega_p)$, where s is the number of laymen from party p, and ω is the policy position. As an alternative measure, I use dummy variables to indicate cases where at least one layman from a given party participated.

Case characteristics (X_{ijct}) represents dummies for the countries (around 90) most likely to be the applicants' native countries. I also include dummies for the other case characteristics described in Table 1.[41] Court committee characteristics (Z_{ijct}) include dummies for whether the committee has a majority of women, whether there is at least one layman with a non-Scandinavian name, and average age. The specification also includes fixed effects for the judges (θ_j) and court departments θ_c. This should capture variation from the other decision makers involved in preparing and deciding a case, and also account for the fact that departments in Stockholm are specialized at certain countries. $C*\theta_t$ is a court*year fixed effect to control for yearly changes in migration flows to each court. However, given that the political composition is constant over time (only one election period), and laymen from all parties participate regularly during the whole year, laymen's party affiliation should not be correlated with any time-variation in case characteristics. Standard errors are clustered at the judge, although I also show the results clustered at other levels in Table 9.

[41] I do not control for whether the case contains classified information or not, since it is up to the judge and laymen to decide if information should be classified. Hence, controlling for it could be problematic, due to post-treatment bias. Although there are some minor changes of the point estimates of the party dummies when it is included, the significance level is hardly affected.

A potential concern would be that the court departments schedule cases selectively to get a court committee with a certain political composition.[42] To confirm that the political composition of the court committee is unrelated to case characteristics, I run regressions on case covariates and party dummies or policy position (including controls for department and court*year fixed effects). I then test the joint significance of the party variables using an F-test and the result is presented in Table 2. I can not reject the null hypothesis for any case at the 5 percent significance level. At the 10 percent level I can reject it for 5 percent (for party dummies) and 10 percent (for policy position) of the cases, close to what one would expect at random.

I also conduct a Pearson χ^2-test for each court and court department after tabulating the laymen's party affiliation against characteristics of the cases and judges. Table A4 summarizes p-values from the test, where the observed distribution is random under the null hypothesis. For Gothenburg and Malmo the null hypothesis can not be rejected for any characteristics at the court level. For Stockholm it can be rejected for country, nationless, and gender related issues at the court level. This is not surprising given that laymen in Stockholm are assigned to specific departments and the departments deal with different countries. Hence, it is important to include department fixed effects, given that this is the level of the random assignment. Overall, the null hypothesis is rejected for almost 5 percent of the tabulations at the department level, as expected. This indicates that there is no evidence of any systematic sorting, and consistent with this, I also show that the results are not sensitive to controlling for these covariates.

[42]This is unlikely since cases should be handled according to turn-taking. Cases are prepared by rapporteurs, and when all material has been collected the secretary at the department schedules it for a hearing as soon as possible (taking into account when the judge is free, when an interpreter, if needed, is available, and when the asylum seekers' public counsel is available).

Table 2. F-test of joint significance for political party variables

	Gender	Muslim	Christian	Convert	Sexual	Politics	Age	Language	Health	Child
F-test (pd)	1.305	1.188	0.309	0.392	0.735	0.923	0.991	0.750	0.628	1.316
p-value (pd)	0.244	0.306	0.950	0.908	0.642	0.487	0.436	0.630	0.733	0.238
F-test (pp)	3.311	0.007	0.176	0.015	0.014	0.104	2.683	0.967	1.600	0.016
p-value (pp)	0.069	0.935	0.675	0.902	0.905	0.747	0.101	0.326	0.206	0.901

	Nationless	Africa	CAsia	SEAsia	SAmeri	MidE	EEuro	J_woman	J_age	J_tenure
F-test (pd)	1.735	0.443	0.228	0.190	0.983	0.594	0.659	1.645	0.478	0.285
p-value (pd)	0.096	0.875	0.979	0.988	0.442	0.761	0.707	0.118	0.851	0.960
F-test (pp)	0.073	0.104	0.051	0.348	0.517	0.010	1.266	2.737	0.031	1.491
p-value (pp)	0.788	0.747	0.821	0.555	0.472	0.922	0.261	0.098	0.861	0.222

Note: Party dummies (pd) or Policy position (pp). Age refers to age determination, Language to language analysis, CAsia to Central Asia, SEAsia to South East Asia, SAmeri to South America, MidE to Middle East, EEuro to East Europe, J_woman to the judge's gender, J_age to the judge's age, and J_tenure to the judges' tenure.

5 Results

This section starts by looking at the effect of the court committees predicted policy position and the effect of laymen from specific parties.

Table 3. Approval rate, policy position

	(1)	(2)	(3)	(4)	(5)	(6)
Policy position	-0.083***	-0.080***	-0.080***			
	(0.015)	(0.016)	(0.016)			
Policy position (local)				-0.083***	-0.079***	-0.079***
				(0.015)	(0.016)	(0.016)
Court dep. FE	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	No	Yes	Yes	No	Yes	Yes
Laymen cov.	No	No	Yes	No	No	Yes
Case cov.	No	No	Yes	No	No	Yes
Adjusted R^2	0.027	0.056	0.082	0.027	0.056	0.082
Observations	6759	6759	6627	6759	6759	6627

Note: All regressions include year*court FE. Standard errors are clustered on judge. Case characteristics also include country FE. ** p < 0.1, ** p < 0.05, *** p < 0.01.*

Table 3 shows the results from estimating equation (1). I use the measure of average policy composition based on the laymen's party affiliation (as described above). Increasing the court committees' predicted average position by one point, i.e. becoming more negative to receiving refugees to one's municipality, decreases the approval rate by around 8 percentage points. Considering an average approval rate of 21 percent, this is quite substantial. Including case and court committee covariates has little impact on the results, as expected. The sample in column (1)-(3) uses the policy position based on the political parties' average national position. To account for the fact that there is also regional variation with respect to the parties view on refugees (see Figure 1), I use the local policy position of the parties in column (4)-(6). The results are almost identical.

In Table 4 instead of using the laymen's predicted position, I look directly at the effect of having laymen from different parties, compared to court committees consisting of only laymen from the Social Democrats or the Moderate Party (S + M) or just the Moderate Party (M). The probability of approval is around 2.5–4 percentage points higher when laymen from the Christian Democrats (Kd), the Green Party (Mp), or the Left Party (V) participate, compared to committees consisting of the Social Democrats and the Moderate Party. To the contrary, the approval rate is around 4.5 percentage points lower when laymen from the Swedish Democrats (Sd) participate. When the Moderate Party is the reference group in column (4)–(6), one notes that the probability of approval is also a bit higher when laymen from the Social Democrats

participate. The p-values from an F-test are also added, showing that the party dummies are jointly significant. Overall, these results are well in line with the parties' stand on the refugee issue as well as in what direction they tend to dissent.

Table 4. Approval rate, party effects

	M+S reference			M reference		
	(1)	(2)	(3)	(4)	(5)	(6)
Left Party	0.040**	0.034**	0.039**	0.046***	0.038**	0.044**
	(0.016)	(0.016)	(0.017)	(0.016)	(0.016)	(0.017)
Green Party	0.026*	0.025*	0.027**	0.032**	0.030**	0.032**
	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.013)
Christian Democrats	0.037**	0.044***	0.041***	0.042**	0.048***	0.046***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Liberal Party	-0.013	-0.007	-0.005	-0.007	-0.002	-0.001
	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)	(0.013)
Centre Party	0.004	0.005	0.007	0.010	0.010	0.012
	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)	(0.014)
Swedish Democrats	-0.047***	-0.050***	-0.046***	-0.041***	-0.045***	-0.042***
	(0.014)	(0.016)	(0.016)	(0.014)	(0.015)	(0.016)
Local party (Svg)	-0.054	-0.049	-0.050	-0.048	-0.044	-0.046
	(0.034)	(0.034)	(0.033)	(0.034)	(0.035)	(0.034)
Social Democrats				0.026**	0.022**	0.021**
				(0.011)	(0.011)	(0.010)
Court dep. FE	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	No	Yes	Yes	No	Yes	Yes
Laymen cov.	No	No	Yes	No	No	Yes
Case cov.	No	No	Yes	No	No	Yes
Adjusted R^2	0.027	0.056	0.083	0.028	0.056	0.083
F-test (p-value)	0.000	0.000	0.000	0.000	0.000	0.000
Observations	6759	6759	6627	6759	6759	6627

Note: All regressions include year*court FE. Standard errors are clustered on judge. Case characteristics also include country FE. ** p < 0.1, ** p < 0.05, *** p < 0.01.*

Regarding the covariates, several of the country fixed effects and case characteristics have a significant effect on the probability of approval (see Table A5). In particular, the probability of approval increases by 7-10 percent for cases mentioning conversion, sexual orientation, or nationless. This indicates that the words extracted from the case files also capture something meaningful. On the other hand, there is no significant effect of the laymen's gender, age, or having a layman with a non-Scandinavian name in the court committee.[43] This indicates that it is their political affiliation, and not other background characteristics that matter.

[43]Including or excluding dummies for laymen's party affiliation does not affect this.

5.1 Peer influence and majority composition

Laymen can potentially affect the decision making in two ways, through their votes (i.e. individual preferences are simply aggregated) or during the deliberation with the judge and the other laymen (i.e. they can affect the opinion of the co-judges). Experimental studies (e.g. Goeree and Yariv, 2011) have shown that such deliberation can decrease the impact of voting rules by changing the views of the decision makers. An essential part of evaluating an asylum case is to decide whether the asylum applicant is credible or not. This could create room for discretion. It is possible that a layman who points to inconsistencies in the asylum seekers story or emphasizes mitigating circumstances to such inconsistencies will affect the co-judges' opinion.

To examine if laymen actually vote in line with their co-judges from the Left Party, the Christian Democrats, the Green Party and the Swedish Democrats I turn the dataset into individual level observation, i.e. each case level observation is turned into three laymen observations.

$$y_{ljct} = \beta_0 + \beta_1 D_{ljct} + \beta_2 D_{ljct}^{other} + \beta_3 X_{ljct} + \beta_4 Z_{ljct} + \theta_j + \theta_c + C * \theta_t + \varepsilon_{ljct} \quad (2)$$

In this specification y_{ljct} is the vote by laymen l, sitting with judge j, in court department c, and in year t. (D_{ljct}) refers to the layman's own party affiliation (using the Moderate party as a reference), and D_{ljct}^{other} capture the party affiliation of the other laymen on the committee. Just as before, I also control for the laymen's demographic characteristics, and the specification also includes fixed effects for the judges, court departments, and court*year.

Table 5 display the point estimates of the co-judges' party affiliation on laymen's probability to vote for approval. It is clear that the effects at the case level also carry over to the individual level. Hence, the previous results are not simply driven by cases where the judge or the laymen are being outvoted. This could suggests that the results are at least partly driven by the fact that laymen convince the co-judges of their perspective. However, a complication when evaluating how decisions are made, is the fact that participants that do not agree with the majority will not always state this due to dissent aversion (e.g. Fischman, 2011). Assuming that there is a cost associated with dissenting, some individuals will avoid stating a minority opinion.[44]

[44] Although laymen do not have to write the dissenting opinion themselves (this is done by an assistant to the judge) it could be costly to the working climate, given that they are supposed to continue serving in the same court committee for the rest of the term.

Table 5. Approval rate, peer influence

	(1)	(2)	(3)
Left Party	0.051***	0.046***	0.045***
	(0.013)	(0.013)	(0.013)
Green Party	0.036***	0.034***	0.029***
	(0.011)	(0.010)	(0.010)
Christian Democrats	0.039***	0.044***	0.040***
	(0.011)	(0.011)	(0.011)
Liberal Party	0.000	0.002	0.005
	(0.009)	(0.009)	(0.009)
Centre Party	0.013	0.013	0.014
	(0.011)	(0.011)	(0.011)
Swedish Democrats	-0.032**	-0.033***	-0.033***
	(0.013)	(0.013)	(0.013)
Local Party (Svg)	-0.037*	-0.032	-0.034*
	(0.020)	(0.020)	(0.019)
Social Democrats	0.022***	0.020***	0.019***
	(0.007)	(0.007)	(0.007)
Court dep. FE	Yes	Yes	Yes
Judge FE	No	Yes	Yes
Laymen cov.	No	No	Yes
Case cov.	No	No	Yes
Adjusted R^2	0.035	0.067	0.099
F-test (p-value)	0.000	0.000	0.000
Observations	20277	20277	20127

Note: All regressions include year*court FE. Standard errors are clustered on judge. Case characteristics also include country FE. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

To examine the impact of voting power I examine if the probability of approval depends on the number of laymen that are positive to immigration. Based on the results from the surveys and the dissents I define laymen from the Left Party, the Green Party, the Social Democrats and the Christian Democrats as being "pro-immigration". The results in Table 6 show that the number of like-minded laymen matter for the results. Having two or three pro-immigration laymen increases the probability of approval by 5-8 percentage points, and the effect is significantly different from having just one pro-immigration layman. This shows that voting power matters. Yet, these effects are surprisingly linear, while voting power is not (since it requires three votes). This suggests that deliberation is also influencing the results.

Table 6. Approval rate, majority effects

	(1)	(2)	(3)
One pro-immigration	0.026**	0.018	0.016
	(0.012)	(0.012)	(0.013)
Two pro-immigration	0.058***	0.053***	0.053***
	(0.013)	(0.013)	(0.012)
Three pro-immigration	0.103***	0.087***	0.084***
	(0.024)	(0.024)	(0.024)
Court dep. FE	Yes	Yes	Yes
Judge FE	No	Yes	Yes
Laymen cov.	No	No	Yes
Case cov.	No	No	Yes
Observations	6759	6759	6627
R^2	0.032	0.083	0.123

Note: All regressions include year*court FE. Standard errors are clustered on judge. Case characteristics also include country FE. ** p < 0.1, ** p < 0.05, *** p < 0.01.*

5.2 Political party and case characteristics interactions

Laymen from different political parties may differ both in their view on asylum seekers in general, but there could also be differences depending on the asylum seekers' asylum motives or characteristics, such as gender related issues or whether the asylum seeker is a child or not. I look at whether laymen's individual votes varies over the case characteristics that appear to be important for the approval rate according to the results in Table A5, but also add religion as this could be particularly relevant for the Christian Democrats.

Table 7 shows the results when interacting case characteristics with the laymen's party affiliation.[45] Overall, most interaction effects are not significant, suggests that it is the laymen's general attitudes towards asylum seekers, rather than a specific subgroup of cases, that drives the results in the main analysis. The Christian Democrats are, however, an exception, since their probability to vote for approval increases when "conversion", "Christian", or something gender related is mentioned in the case file. This suggests that laymen who do not represent a party with a strong stance on the refugee issue, are more likely to adapt their behavior in court depending on the case characteristics.

[45] Due to lack of space I only display the interaction effect for the parties that influence decision making, according to previous results.

Table 7. Interaction effects, individual voting

	Child	Gender	Sexual	Convert	Christian	Muslim	Nationless
Left Party	0.177***	0.177***	0.174***	0.173***	0.171***	0.189***	0.172***
	(0.034)	(0.032)	(0.033)	(0.032)	(0.033)	(0.033)	(0.033)
Green Party	0.147***	0.151***	0.146***	0.151***	0.157***	0.154***	0.157***
	(0.027)	(0.028)	(0.026)	(0.027)	(0.027)	(0.028)	(0.027)
Christian	0.0874***	0.0770***	0.0924***	0.0802***	0.0772***	0.0929***	0.0879***
	(0.021)	(0.022)	(0.020)	(0.018)	(0.017)	(0.021)	(0.020)
Swedish	-0.0484***	-0.0407**	-0.0459***	-0.0485***	-0.0512***	-0.0442***	-0.0406**
	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)	(0.017)	(0.018)
Left Party*X	0.0469	0.0153	0.107	0.0648	0.0707	-0.0458	0.0550
	(0.101)	(0.053)	(0.088)	(0.076)	(0.059)	(0.045)	(0.065)
Green Party*X	0.0853	0.00628	0.107	0.00823	-0.0424	-0.00822	-0.0488
	(0.069)	(0.034)	(0.072)	(0.049)	(0.044)	(0.028)	(0.045)
Christian*X	0.0490	0.0946**	-0.0580	0.106**	0.115**	-0.0123	0.0179
	(0.068)	(0.041)	(0.046)	(0.051)	(0.053)	(0.030)	(0.033)
Swedish*X	-0.0537	-0.0546	-0.0867*	-0.0176	0.0252	-0.0188	-0.0464
	(0.105)	(0.034)	(0.048)	(0.066)	(0.052)	(0.032)	(0.036)
X	0.0314	0.0533***	0.0612***	0.0785***	-0.00154	0.00867	0.0528***
	(0.025)	(0.015)	(0.024)	(0.020)	(0.017)	(0.012)	(0.017)
Observations	20127	20127	20127	20127	20127	20127	20127
R^2	0.113	0.113	0.113	0.113	0.114	0.113	0.113
Court dep. FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Laymen cov.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Case cov.	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: X refers to the interaction variable specified in column title. All regressions include year*court FE. Standard errors are clustered on the individual level. Case characteristics also include country FE. Each observation refers to an individual layman observation, i.e. one case generates three laymen observations.
** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

5.3 Judge and laymen interactions

To what extent laymen influence decision making is also likely to depend on the judge they are sitting with, as some judges may be more or less dominant during the deliberation, thereby limiting the laymen's influence. As seen in Table A1 around 60 percent of the judges are women, and the average age in 2011 was 47 years. I split the sample according to judges' gender, age, and tenure in Table 8. The results reveal that the influence of laymen is larger for cases with female judges, both when using the committees' policy position and the party dummies. It is also interesting to note that the effects are larger in both directions, i.e. the probability of approval is higher when a pro-immigration party participates, but lower when laymen from the Swedish Democrats participate.

One potential explanation is that women also have less experience since they are two years younger on average, but looking at the result depending on judges' age and tenure reveals that there are no systematic patterns with respect to these characteristics. Another alternative could be that laymen behave differently when sitting with a male or a female judge, but laymen do not appear to dissent more often when sitting with a female judge compared to a male in general. Also, there is almost no difference between the share of cases where male and female judges were outvoted by the laymen. However, it is possible that there are gender differences regarding judges willingness to state a minority opinion, i.e. female judges may simply have stronger dissent aversion. Another possibility is, of course, that female judges are simply more responsive to the laymen's opinions.

Table 8. Interaction effects, laymen and judges

	Young	Old	Tenure, low	Tenure, high	Woman	Man
Policy position	-0.061***	-0.087***	-0.071***	-0.075**	-0.100***	-0.056**
	(0.020)	(0.026)	(0.021)	(0.030)	(0.024)	(0.022)
Left Party	0.052**	0.031	0.050*	0.020	0.046	0.034
	(0.025)	(0.027)	(0.027)	(0.024)	(0.029)	(0.022)
Green Party	0.036*	0.033*	0.035*	0.042*	0.052**	0.018
	(0.019)	(0.019)	(0.020)	(0.021)	(0.021)	(0.015)
Christian Democrats	0.011	0.073***	0.034	0.068**	0.054**	0.033
	(0.024)	(0.024)	(0.021)	(0.027)	(0.023)	(0.024)
Liberal Party	0.002	-0.003	-0.002	0.005	0.007	-0.004
	(0.020)	(0.017)	(0.017)	(0.021)	(0.015)	(0.022)
Centre Party	0.011	0.005	0.028	-0.023	0.017	-0.006
	(0.020)	(0.022)	(0.017)	(0.025)	(0.022)	(0.018)
Swedish Democrats	-0.020	-0.060**	-0.041**	-0.020	-0.061***	-0.011
	(0.021)	(0.025)	(0.019)	(0.034)	(0.021)	(0.023)
Local party (Svg)	-0.046	-0.036	-0.041	-0.033	-0.039	-0.056
	(0.055)	(0.045)	(0.041)	(0.066)	(0.037)	(0.063)
Social Democrats	0.005	0.029**	0.019	0.016	0.030**	0.014
	(0.017)	(0.014)	(0.015)	(0.017)	(0.014)	(0.017)
Court dep. FE	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes	Yes	Yes
Laymen cov.	Yes	Yes	Yes	Yes	Yes	Yes
Case cov.	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2899	3273	3540	2586	3314	2970
R^2	0.162	0.116	0.148	0.139	0.168	0.102

Note: The sample in column (1) and (2) is restricted to judges below or above the median age, and column (3) and (4) is restricted to judges below or above the median tenure. All regressions include year*court FE. Standard errors are clustered at the judge. Case characteristics also include country FE. *$* p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

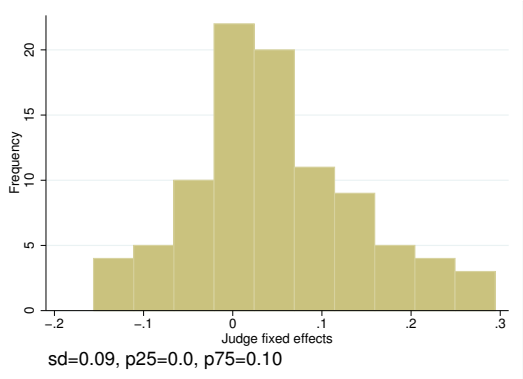
5.4 Between-judge variation

So far, the focus has been on explaining the within-judge variation, based on laymen's political affiliation. However, looking at the observed differences between judges also indicates to what extent there is discretion in these decisions. Figure 4 shows a histogram of the judge fixed effects from equation (1), estimated for judges who have handled at least 50 cases. The standard deviation is 0.09, confirming that there is large variation regarding judges' leniency toward asylum seekers. The magnitude is comparable to the difference in predicted probability of approval for an asylum seeker from Belarus or Morocco compared to Afghanistan, i.e. countries with completely different conditions. The interquartile range is 0.1, a considerable difference, but in line with other studies documenting substantial judge heterogeneity.[46]

[46] Abrams et al. (2012) find an 11 percentage point difference in the racial incarceration gap between the judge at the 25th percentile and the one at the 75th percentile. Lim et al. (2015a) find an interquartile range of 0.14 percentage points when measuring

One can also compare the explanatory power of the judge fixed effects and the case characteristics (including the country fixed effects). Adding judge fixed effects to a regression with only court*year fixed effects and department fixed effects increases the adjusted R^2 by 2.4 percentage points, whereas instead adding the covariates increases it by 3.5 percentage points. This also suggests that judges have a substantial impact on the probability of approval. Using an F-test one can reject the hypothesis that the judge fixed effects do not affect the probability of getting an asylum appeal approved. It is possible that the fixed effects are correlated with observable characteristics of the judges, but I find no significant correlation regarding judges' gender, age, or tenure. Given that almost all judges have Scandinavian names, it is not possible to examine the impact of a "foreign" background. Other explanations could be differences in judges' previous employment, social background, or their individual preferences.

Figure 4. Judge fixed effects



5.5 Robustness

Standard errors have been clustered at the judge in the baseline analysis, to account for possible correlation between cases by the same judge. In Table 9 I display the result when standard errors are not clustered, as well as when they are clustered on judge, court committee, judge*court committee, and department*court committee. The standard errors do not change all that much between the different levels of clustering, and having laymen from the Christian Democrats, the Left Party, the Green Party, or the Swedish Democrats still has a significant effect on the approval rate.

judges sentencing harshness in criminal cases, and argue that there is substantial cross-judge heterogeneity.

Table 9. Cluster level

	(1) Not clustered	(2) Judge	(3) CC	(4) Judge*CC	(5) Dep*CC
Left Party	0.044***	0.044**	0.044***	0.044***	0.044***
	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)
Green Party	0.032**	0.032**	0.032**	0.032**	0.032**
	(0.013)	(0.013)	(0.013)	(0.014)	(0.014)
Christian Democrats	0.046***	0.046***	0.046***	0.046***	0.046***
	(0.014)	(0.016)	(0.015)	(0.014)	(0.014)
Liberal Party	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.012)	(0.013)	(0.011)	(0.012)	(0.012)
Centre Party	0.012	0.012	0.012	0.012	0.012
	(0.015)	(0.014)	(0.015)	(0.015)	(0.015)
Swedish Democrats	-0.042**	-0.042***	-0.042**	-0.042**	-0.042**
	(0.018)	(0.016)	(0.017)	(0.017)	(0.017)
Local party (Svg)	-0.047*	-0.047	-0.047*	-0.047*	-0.047*
	(0.026)	(0.034)	(0.026)	(0.026)	(0.026)
Social Democrats	0.021*	0.021**	0.021*	0.021*	0.021*
	(0.011)	(0.010)	(0.011)	(0.012)	(0.011)
Court dep. FE	Yes	Yes	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes	Yes	Yes
Laymen cov.	Yes	Yes	Yes	Yes	Yes
Case cov.	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.083	0.083	0.083	0.083	0.083
F-test (p-value)	0.000	0.000	0.000	0.000	0.000
Observations	6627	6627	6627	6627	6627

Note: All regressions include year*court FE. CC=court committee and Dep=court department. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

6 Conclusions

Examining how decisions vary by judges' background characteristics and over different areas of law should be of primary interest. Both in terms of justice, but also because it could pinpoint where interventions are necessary to achieve greater coherence. This paper uses the random assignment of cases to laymen, to estimate the influence of partisanship on judicial decision making. I find that the approval rate in asylum cases is higher when laymen are affiliated with political parties that are positive to immigration, and lower when laymen from parties that are negative to immigration participate in court. An asylum applicant who is unlucky and gets a layman from the Swedish Democrats on the committee will

have a nine percentage point lower probability of approval, compared to someone who is lucky and gets a layman from the Left Party during the hearing. This indicates that lay judges' political beliefs affect their evaluation of asylum seekers motive to apply for a residence permit, as well as the judicial rulings. It is difficult to disentangle if the results are driven by voting power or deliberation. Looking at individual voting, there is an effect of the partisanship of laymen's co-judges, suggesting that laymen are influenced by their co-judges. On the other hand, having several pro-immigration laymen on the committee increases the probability of approval, suggesting that voting power also matter.

There are also large variations between the regular judges approval rate, indicating that they apply their own interpretation of the law. This result is in line with several other studies that find substantial judge heterogeneity (e.g., Abrams et al., 2012 ; Lim et al., 2015a).

Taken together, the results raise concerns about the practice of justice to asylum seekers appellations. Although the procedure and requirements to grant asylum differ between countries, the observed degree of discretion in these decisions is problematic for asylum applicants in general. In particular, as anti-immigrant parties throughout Europe gain support. Whether or not laymen should participate in court has been debated in Sweden for years. These results suggest that even if the system with laymen were to be abolished, the application of the law might still not be consistent, given the degree of judge heterogeneity. Future research is clearly necessary to determine how these differences vary over other areas of the law.

References

Abrams, D. S., Bertrand, M., and Mullainathan, S. (2012). Do Judges Vary in Their Treatment of Race? . *The Journal of Legal Studies*, 41(2):347–383.

Anwar, S., Bayer, P., and Hjalmarsson, R. (2012). The Impact of Jury Race in Criminal Trials . *The Quarterly Journal of Economics*, 127:1017–1055.

Anwar, S., Bayer, P., and Hjalmarsson, R. (2014). The Role of Age in Jury Selection and Trial Outcomes. *Journal of Law and Economics*, 57(4):1001–1030.

Anwar, S., Bayer, P., and Hjalmarsson, R. (2015). Politics in the Courtroom: Political Ideology and Jury Decision Making . mimeo, Gothenburg University.

Austen-Smith, D. and Feddersen, T. J. (2006). Deliberation, Preference Uncertainty, and Voting Rules. *American Political Science Review*, 100(2):209–217.

Barabas, J. (2004). How Deliberation Affects Policy Opinions. *American Political Science Review*, 98(4):687–701.

Bernhard, H., Fehr, E., and Fischbacher, U. (2006). Group Affiliation and Altruistic Norm Enforcement . *American Economic Review*, 96(2):217–221.

Boyd, C. L., Epstein, L., and Martin, A. D. (2010). Untangling the Causal Effects of Sex on Judging . *American Journal of Political Science*, 54(2):389–411.

Cronholm, E. (2014). Nämndemannasystemet -en studie om nämndemän och lagfarna domares uppfattning om det svenska nämndemannasystemet. Master's thesis, Uppsala University.

Dahlgren, M. (2011). Nämndemannaposten -det(o)politiska ämbetet? Master's thesis, Stockholm University.

Dancygier, R. M. (2010). *Immigration and Conflict in Europe*. Cambridge University Press, Cambridge, England.

Danziger, S., Levav, J., and Avnaim-Pesso, L. (2011). Extraneous factors in judicial decisions. *Proceedings of the National Academy of Science of the United States of America*, 108(17):6889–6892.

Diesen, C. (1996). *Lekmän som domare*. Juristförlaget, Stockholm.

Diesen, C. (2011). För och emot nämndemän . *Juridisk Tidskrift*, 23(3):531–546.

Diesen, C., Roca, A. L. V., Billing, K. L., Seidlitz, M., and Wahren, A. W. (2012). *Prövning av migrationsärenden*. Nordtedts Juridik, Stockholm.

DV (2011). *Genomföra särskilda informationsinsatser i syfte att få till stånd en bredare rekrytering av nämndemän*. Number 952-2010. Domstolsverket (the Swedish National Courts Administration).

Emerson, J. W., Seltzer, M., and Lin, D. (2009). Assessing Judging Bias: An Example From the 2000 Olympic Games. *American Statistician*, 63(2):124–131.

Fischman, J. B. (2011). Estimating Preferences of Circuit Judges: A Model of Consensus Voting . *Journal of Law and Economics*, 54(4):781–809.

Gilljam, M., Karlsson, D., and Sundell, A. (2010). *Politik på hemmaplan. Tiotusen fullmäktigeledamöter tycker om politik och demokrati*. SKL

Kommentus, Stockholm.
Glynn, A. N. and Sen, M. (2015). Identifying Judicial Empathy: Does Having daughters Cause Judges to Rule for Women's Issues. *American Journal of Political Science*, 59(1):37–54.
Goeree, J. K. and Yariv, L. (2011). An Experimental Study of Collective Deliberation. *Econometrica*, 79(3):893–921.
Hainmueller, J. and Hangartner, D. (2013). Who Gets a Swiss Passport? A Natural Experiment in Immigrant Discrimination. *American Political Science Review*, 107(1):159–187.
Hall, M. (2010). Randomness Reconsidered: Modeling Random Judicial Assignment in the U.S. Courts of Appeals . *Journal of Empirical Legal Studies*, 7(3):574–589.
Iaryczower, M., Shi, X., and Shum, M. (2014). Can Words Get in the Way? The Effect of Deliberation in Collective Decision-Making. mimeo, Princeton University.
JP Infonet (2014). *JP Rättfallsnet Migration*. JP Infonet Förlag AB, Stockholm.
Karnov Nyheter (2012). *Karnov nyheters kartläggning om nämndemannasystemet*. Karnov Group Sweden AB, Stockholm.
Kastellec, J. P. (2012). Racial Diversity and Judicial Influence on Appellate Courts. *American Journal of Political Science*, 57(1):167–183.
Leider, S., Möbius, M. M., Rosenblat, T., and Do, Q. (2009). Directed Altruism and Enforced Reciprocity in Social Networks . *Quarterly Journal of Economics*, 124(4):1815–1851.
Lim, C. S. H., Silveira, B., and Snyder, J. M. (2015a). Do Judges' Characteristics matter? Ethnicity, Gender, and Partisanship in Texas State Trial Courts. mimeo, Cornell University.
Lim, C. S. H., Snyder, J. M., and Strömberg, D. (2015b). The Judge, the Politician, and the Press: Newspaper Coverage and Criminal Sentencing Across Electoral Systems. *forthcoming in the American Economic Journal: Applied Economics*.
Norris, P. (2005). *Radical Right: Voters and Parties in the Electoral Market*. Cambridge University Press, New York.
Ramberg, A. (2012). Partipolitiskt bagage kan vara olämpligt. Svenska Dagbladet.
Rand, D. G., Pfeiffer, T., Draber, A., Sheketoff, R. W., Wernerfelt, N. C., and Benkler, Y. (2009). Dynamic remodeling of in-group bias during the 2008 presidential election. *Proceedings of the National Academy of Science of the United States of America*, 106(15):6187–6191.
Rundkvist, C. (1995). Nämndemän - Politiker som dömer. Examination Paper, Stockholm University.
Sandberg, L. and Demker, M. (2014). *Mittfåra och marginal*, chapter Starkare oro för främlingsfientlighet än för invandring, pages 71–82. Göteborgs Universitet: SOM-institutet.
Shayo, M. and Zussman, A. (2011). Judicial Ingroup Bias in the Shadow of Terrorism . *Quarterly Journal of Economics*, 126(3):1447–1484.

SOU (1994). *Domaren i Sverige inför framtiden -utgångspunkter för fortsatt utredningsarbete*. Number 1994:99. Statens Offentliga Utredningar, Stockholm.
SOU (2002). *Framtidens nämndemän*. Number 2002:61. Statens Offentliga Utredningar, Stockholm.
SOU (2013). *Nämndemannauppdraget -breddad rekrytering och kvalificerad medverkan*. Number 2013:49. Statens Offentliga Utredningar, Stockholm.
Sunstein, C. R., Schakade, D., Ellman, L. M., and Sawicki, A. (2006). *Are Judges Political? An Empirical Analysis of the Federal Judiciary*. Brookings, Washington DC.
SVT (2012). Litet inflytande för nämndemän. Sveriges Television.
UNHCR (2011). *Förhöjd kvalitet i svensk asylprövning*. United Nations High Commissioner for Refugees (UNHCR), Stockholm.
Voeten, E. (2008). The Impartiality of International Judges: Evidence from the European Court of Human Rights . *American Political Science Review*, 102(4):417–433.
Zitek, E. M. and Hebl, M. R. (2007). The role of social norm clarity in the influenced expression of prejudice over time. *Journal of Experimental Social Psychology*, 43(6):867–876.
Zitzewitz, E. (2006). Nationalism in Winter Sports Judging and Its Lessons for Organizational Decision Making. *Journal of Economic and Management Strategy*, 15(1):67–99.

Appendices

A.1 Kommun- och Landstingsfullmäktigeundersökningen (KOLFU) 2008, 2012

Attitudes to refugees
Here are a number of proposals that have occurred in the political debate. What is your opinion about the following?
-Receive fewer refugees to Sweden
Regarding the municipality you live in: What is your opinion about the following proposals?
-Receive more refugees to the municipality

1=Very good proposal
2=Rather bad proposal
3=Neither god or bad
4=Rather god proposal
5=Very bad proposal

[Answers for the first question are coded to correspond to the scale of the second question]

A.2 Samhälle, Opinion och Medier-undersökningen (SOM) 2012, 2013

Motive to get a residence permit
What weight should the following circumstances be given, for refugees to receive a residence permit in Sweden?

-Poverty
-Disease
-Relatives already living in Sweden
-War in native country
-Persecution due to religion
-Persecution due to political opinion

1=Very large weight
2=Rather large weight
3=Rather small weight
4=Very small weight

A.3 Definition of case characteristics

Country FE: I search for all countries from which an individual had their application for asylum rejected by the Migration Board 2010-2013 (150 countries). Several files mention multiple countries. However, almost all files from Malmö and Stockholm have the decision from the Migration Board attached to the case files. For those cases I use the country mentioned in the attached file (it is always written at a specific place). Otherwise, if there are multiple countries, but only one mentioned as "citizen of country X", I define that country as the native country. Case files where a single country cannot be identified as the native country are coded as having "multiple" countries. I end up with around 90 different countries. Cases where no country is found, either due to typo or being classified (40 percent of these cases have classified attachments), are coded as having "no country".

Nationless: nationless [statslös]
Christian: Christian, Catholic, Protestant [kristen, katolik, protestant]
Muslim: Muslim, Islam [muslim, islam]
Gender related: rape, genital mutilation, compulsory marriage, extramarital, honor violence/culture/murder [våldtagen/våldtäkt, könsstympa/könsstympning, tvångsäktenskap, tvångsgifte, utomäktenskaplig, hedersvåld, hederskultur, hedersmord]
Sexual orientation: homosexual, bisexual [homosexuell, bisexuell]
Conversion: convert [konvertit/konvertera/konversion]
Politics: political opinion/activity, opposition, demonstration [politisk/a/t åsikt/aktivitet/arbete/engagemang/ verksamhet, opposition, demonstration]
Health: medical care, doctor's certificate, psychological evaluation, HIV [sjukvård, läkarvård, läkarintyg, psykologisk bedömning, HIV]
Confidential: secrecy [sekretess]
Language analysis: Language analysis [språkanalys, språkanalytiker]
Age determination: Age determination [åldersbedömning, bedömning av ålder]

A.4 Figures

Figure A1. Asylum applicants EU (28) and Sweden

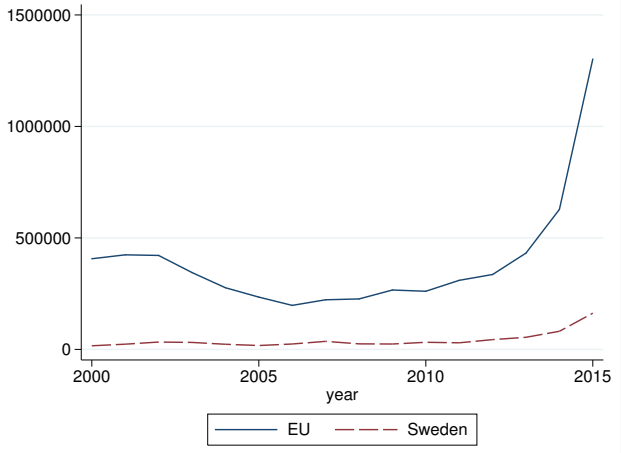


Figure A2. Receive more refugees to municipality/Sweden (national average)

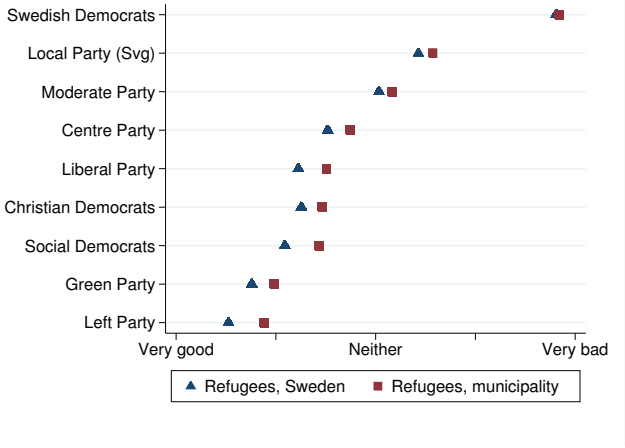
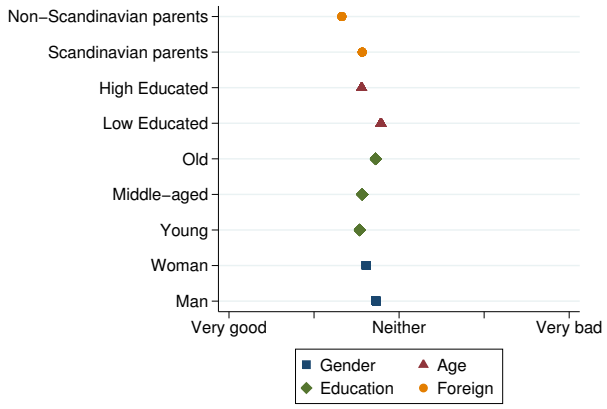
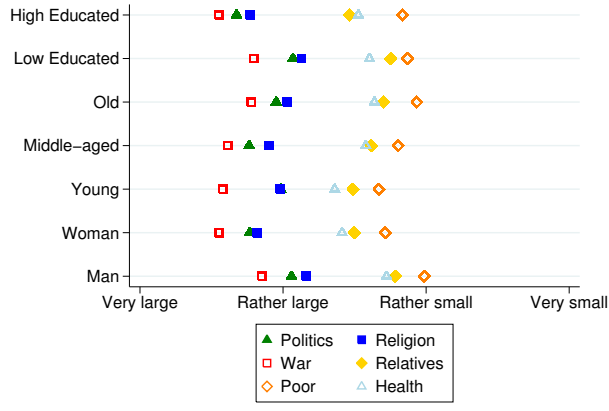


Figure A3. Receive more refugees to municipality



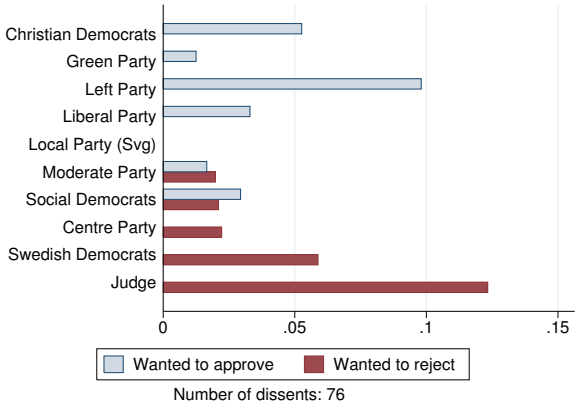
Note: High Educated (university education). Young/Old (<35/>49 years).

Figure A4. Motive for asylum (weight given to different circumstances)



Note: High Educated (university education). Young/Old (<35/>49 years).

Figure A5. Share of dissents in partly approved cases



A.5 Tables

Table A1. Laymen's and judges' characteristics

	Women		Non-Scandinavian		Age	
	mean	number	mean	number	mean	number
All judges	0.63	172	0.02	172	46.62	133
Centre Party	0.46	67	0.03	67	50.75	64
Christian Democrats	0.53	64	0.08	64	55.19	59
Green Party	0.56	103	0.19	103	47.69	101
Left Party	0.55	69	0.13	69	50.98	62
Liberal Party	0.50	111	0.06	111	55.55	104
Local party (Svg)	0.43	14	0.07	14	59.29	14
Moderate Party	0.53	386	0.04	386	58.30	368
Social Democrats	0.52	348	0.14	348	57.24	327
Swedish Democrats	0.25	44	0.00	44	51.95	44

Note: Includes original/replacement laymen and judges who participated in asylum cases. Information about judges' age is not available for junior judges. Age measured in year 2011.

Table A2. Approval rate by country, most likely native country

	All		Malmő		Gothenburg		Stockholm	
	mean	cases	mean	cases	mean	cases	mean	cases
Afghanistan	0.23	923	0.19	389	0.28	105	0.25	429
Albania	0.21	47	0.19	16	0.22	9	0.23	22
Algeria	0.09	33	0.05	20	0.00	6	0.29	7
Armenia	0.19	42	0.14	21	0.23	13	0.25	8
Azerbaijan	0.16	61	0.23	22	0.17	18	0.10	21
Bangladesh	0.17	95	0.00	2	0.00	1	0.17	92
Belarus	0.04	57	0.00	24	0.00	10	0.09	23
Egypt	0.09	87	0.04	25	0.17	12	0.10	50
Ethiopia	0.18	127	0.31	16	0.19	16	0.16	95
Iran	0.31	603	0.33	84	0.42	186	0.24	333
Iraq	0.23	451	0.17	212	0.15	81	0.34	158
Kazakhstan	0.18	22	0.00	5	0.50	2	0.20	15
Kenya	0.16	63	0.50	4	0.29	7	0.12	52
Kosovo	0.18	39	0.04	23	0.60	5	0.27	11
Kyrgyzstan	0.18	51	0.27	11	0.00	7	0.18	33
Lebanon	0.10	81	0.10	59	0.06	17	0.20	5
Libya	0.06	68	0.07	15	0.00	3	0.06	50
Mongolia	0.15	55	0.10	10	0.00	12	0.21	33
Morocco	0.15	40	0.25	16	0.00	6	0.11	18
Multiple counties	0.22	1510	0.20	354	0.24	537	0.21	619
Nigeria	0.05	159	0.03	63	0.09	32	0.05	64
No country	0.28	197	0.25	67	0.27	103	0.41	27
Pakistan	0.22	97	0.27	37	0.24	17	0.16	43
Russia	0.16	176	0.17	71	0.23	30	0.12	75
Serbia	0.14	29	0.08	26	0.00	1	1.00	2
Somalia	0.19	398	0.23	97	0.18	158	0.19	143
Syria	0.33	103	0.32	38	0.56	32	0.12	33
Turkey	0.20	75	0.31	29	0.29	17	0.03	29
Uzbekistan	0.14	150	0.25	4	0.20	5	0.13	141

Note: Top countries. These cases could also mention "nationless". "No country" are cases where no country is found either due to typo or being classified (40 % of these cases have classified attachments). "Multiple countries" are cases mentioning several countries, where no specific native country has been identified.

Table A3. Top countries. Approval rate by country, all countries mentioned

	All		Malmő		Gothenburg		Stockholm	
	mean	cases	mean	cases	mean	cases	mean	cases
Afghanistan	0.22	1923	0.20	781	0.27	279	0.23	863
Albania	0.22	82	0.19	31	0.18	11	0.25	40
Algeria	0.13	89	0.10	51	0.00	13	0.28	25
Armenia	0.20	131	0.18	77	0.22	36	0.22	18
Azerbaijan	0.19	150	0.22	64	0.24	34	0.13	52
Belarus	0.07	123	0.02	42	0.05	19	0.10	62
Egypt	0.17	260	0.17	98	0.32	31	0.14	131
Eritrea	0.17	235	0.33	18	0.30	30	0.14	187
Ethiopia	0.18	440	0.29	80	0.22	78	0.15	282
Georgia	0.21	72	0.21	43	0.21	14	0.20	15
Iran	0.26	1566	0.22	414	0.36	370	0.23	782
Iraq	0.24	1026	0.18	477	0.23	173	0.33	376
Israel	0.20	210	0.17	120	0.30	33	0.18	57
Jordan	0.25	193	0.15	124	0.27	26	0.51	43
Kazakhstan	0.15	137	0.13	32	0.50	10	0.12	95
Kenya	0.17	326	0.28	57	0.23	69	0.12	200
Kosovo	0.20	95	0.11	56	0.56	9	0.27	30
Kyrgyzstan	0.18	125	0.22	27	0.19	16	0.16	82
Lebanon	0.17	289	0.13	176	0.28	65	0.17	48
Libya	0.10	194	0.13	63	0.14	7	0.08	124
Macedonia	0.13	48	0.05	38	0.75	4	0.17	6
Mali	0.20	239	0.23	74	0.19	47	0.18	118
Mongolia	0.17	83	0.07	14	0.00	13	0.23	56
Morocco	0.14	118	0.17	46	0.05	19	0.15	53
Nigeria	0.04	293	0.04	117	0.08	50	0.03	126
Pakistan	0.23	509	0.24	199	0.26	87	0.21	223
Russia	0.17	580	0.16	211	0.24	79	0.15	290
Serbia	0.16	68	0.04	46	0.33	3	0.42	19
Somalia	0.19	817	0.23	202	0.19	296	0.18	319
Sudan	0.19	244	0.09	33	0.32	34	0.18	177
Syria	0.29	607	0.22	298	0.43	124	0.32	185
Turkey	0.22	632	0.20	227	0.27	98	0.21	307
Uganda	0.13	173	0.10	50	0.25	16	0.13	107
Uzbekistan	0.13	336	0.24	25	0.15	13	0.11	298

Note: One case file can mention several different countries.

Table A4. Test for independence between party affiliation and case characteristics

	Dep	J_woman	J_age	J_tenure	Language	Age	Health	Convert	Christian	Muslim	Gender	Sexual	Politics	Child	Country	Nationless	N
Sthlm	.	0.328	0.397	0.202	0.176	0.981	0.111	0.357	0.570	0.697	0.021	0.123	0.581	0.165	0.000	0.047	9498
Sthlm	21	0.061	0.400	0.823	0.265	0.329	0.873	0.234	0.345	0.904	0.416	0.354	0.998	0.718	0.784	0.354	913
Sthlm	22	0.419	0.276	0.770	0.143	0.617	0.857	0.251	0.398	0.612	0.629	0.638	0.209	0.070	0.939	0.035	1163
Sthlm	23	0.018	0.170	0.230	0.418	0.795	0.728	0.676	0.258	0.183	0.098	0.139	0.621	0.800	0.901	0.395	575
Sthlm	25	0.944	0.284	0.556	0.044	.	0.085	0.791	0.945	0.209	0.260	0.285	0.918	0.484	0.186	0.997	611
Sthlm	26	0.231	0.957	0.673	0.444	0.369	0.697	0.267	0.403	0.957	0.556	0.437	0.790	0.767	0.013	0.480	1796
Sthlm	27	0.216	0.322	0.260	0.831	0.053	0.392	0.458	0.651	0.800	0.071	0.167	0.592	0.158	0.862	0.405	752
Sthlm	28	0.910	0.768	0.300	0.095	0.798	0.553	0.528	0.613	0.234	0.345	0.462	0.073	0.820	0.078	0.005	1381
Sthlm	29	0.408	0.066	0.441	0.245	0.304	0.848	0.615	0.853	0.502	0.020	0.888	0.189	0.231	0.712	0.647	1446
Gbg	.	0.362	0.281	0.783	0.801	0.908	0.858	0.965	0.740	0.429	0.261	0.413	0.706	0.097	0.786	0.638	4984
Gbg	13	0.980	0.592	0.924	0.998	0.663	0.311	0.519	0.582	0.679	0.264	0.732	0.577	0.516	0.794	0.858	655
Gbg	14	0.673	0.826	0.886	0.994	0.781	0.117	0.609	0.430	0.295	0.273	0.689	0.455	0.700	0.986	0.957	811
Gbg	15	0.339	0.406	0.637	0.508	0.399	0.806	0.292	0.342	0.041	0.770	.	0.644	0.495	0.849	0.846	383
Gbg	16	0.949	0.342	0.322	0.858	0.549	0.726	0.119	0.291	0.998	0.373	0.315	0.477	0.002	0.725	0.337	448
Gbg	23	0.765	0.404	0.610	0.239	0.883	0.044	0.616	0.797	0.514	0.888	0.844	0.265	0.031	0.726	0.381	883
Gbg	24	0.363	0.753	0.477	0.946	0.853	0.918	0.308	0.264	0.661	0.659	0.118	0.568	0.104	0.803	0.065	706
Gbg	25	0.500	0.401	0.713	0.107	0.968	0.827	0.999	0.984	0.896	0.853	.	0.089	0.323	0.902	0.059	448
Gbg	26	0.004	0.012	0.100	0.393	0.480	0.716	0.345	0.593	0.677	0.927	0.714	0.785	0.218	0.885	0.998	416
Malmo	.	0.079	0.489	0.424	0.961	0.789	0.441	0.718	0.791	0.503	0.648	0.145	0.356	0.684	0.992	0.482	6647
Malmo	3	0.398	0.762	0.619	0.220	0.852	0.293	0.190	0.881	0.207	0.557	0.942	0.601	0.699	0.811	0.985	914
Malmo	4	0.203	0.101	0.464	0.913	0.229	0.176	0.787	0.342	0.225	0.985	0.436	0.153	0.873	0.989	0.233	1831
Malmo	5	0.674	0.974	0.238	0.400	0.030	0.881	0.606	0.979	0.102	0.716	0.394	0.675	0.814	0.989	0.252	2286
Malmo	6	0.355	0.000	0.118	0.936	0.354	0.689	0.645	0.816	0.538	0.724	0.381	0.218	0.905	0.995	0.666	1616

Note: Reported p-value from Pearson χ^2-test. For country, sample restricted to cases with at least 10 country observation (restriction in Stata). J_woman is a dummy for judges who are women, and J_age is a dummy for judges above the median age, and J_tenure is a dummy for judges with tenure above the median. Sthlm refers to Stockholm and Gbg to Gothenburg.

Table A5. Approval rate, covariates

	(1)	(2)	(3)
Non-Scandinavian (at least one)	-0.008	-0.009	-0.006
	(0.014)	(0.013)	(0.013)
Women (at least two)	0.007	0.009	0.005
	(0.010)	(0.010)	(0.010)
Average age	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)
Language analysis		0.011	-0.002
		(0.019)	(0.020)
Judgement of age		0.001	-0.018
		(0.024)	(0.025)
Health		-0.021	-0.013
		(0.013)	(0.013)
Christian		-0.028	-0.012
		(0.021)	(0.022)
Muslim		-0.005	0.003
		(0.013)	(0.013)
Child/ren (no caregiver)		0.052**	0.052**
		(0.025)	(0.026)
Nationless		0.056***	0.063***
		(0.018)	(0.019)
Politics		0.025*	0.015
		(0.014)	(0.013)
Gender related		0.051***	0.049***
		(0.015)	(0.015)
Sexual orientation		0.027	0.069**
		(0.034)	(0.034)
Convert		0.130***	0.101***
		(0.027)	(0.026)
Court dep. FE	Yes	Yes	Yes
Judge FE	Yes	Yes	Yes
Country FE	No	No	Yes
Adjusted R^2	0.057	0.066	0.082
Observations	6627	6627	6627

Note: All regressions include year*court FE. Standard errors are clustered on judge. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table A6. Approval rate, all mentioned countries

	M+S reference			M reference		
	(1)	(2)	(3)	(4)	(5)	(6)
Left Party	0.040**	0.034**	0.037**	0.046***	0.038**	0.042**
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Green Party	0.026*	0.025*	0.024*	0.032**	0.030**	0.030**
	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)
Christian Democrats	0.037**	0.044***	0.045***	0.042**	0.048***	0.049***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Liberal Party	-0.013	-0.007	-0.011	-0.007	-0.002	-0.005
	(0.012)	(0.012)	(0.013)	(0.013)	(0.012)	(0.013)
Centre Party	0.004	0.005	0.003	0.010	0.010	0.009
	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)	(0.015)
Swedish Democrats	-0.047***	-0.050***	-0.046***	-0.041***	-0.045***	-0.041**
	(0.014)	(0.016)	(0.016)	(0.014)	(0.015)	(0.016)
Local party (Svg)	-0.054	-0.049	-0.052	-0.048	-0.044	-0.047
	(0.034)	(0.034)	(0.034)	(0.034)	(0.035)	(0.035)
Social Democrats				0.026**	0.022**	0.022**
				(0.011)	(0.011)	(0.011)
Court dep. FE	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	No	Yes	Yes	No	Yes	Yes
Laymen cov.	No	No	Yes	No	No	Yes
Case cov.	No	No	Yes	No	No	Yes
Adjusted R^2	0.027	0.056	0.078	0.028	0.056	0.079
F-test (p-value)	0.000	0.000	0.000	0.000	0.000	0.000
Observations	6759	6759	6627	6759	6759	6627

Note: All regressions include year*court FE. Standard errors are clustered on judge. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7. Approval rate, stepwise

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Left Party	0.048***	0.046***	0.038**	0.044***	0.042**	0.044**	0.044**
	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)
Green Party	0.031**	0.032**	0.030**	0.036**	0.033**	0.032**	0.031**
	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.013)
Christian Democrats	0.046***	0.042**	0.048***	0.050***	0.048***	0.046***	0.045***
	(0.017)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Liberal Party	-0.001	-0.007	-0.002	-0.005	-0.005	-0.000	-0.001
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)
Centre Party	0.010	0.010	0.010	0.009	0.009	0.012	0.012
	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)	(0.014)	(0.014)
Swedish Democrats	-0.046***	-0.041***	-0.045***	-0.043***	-0.042***	-0.042***	-0.042***
	(0.015)	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)
Local party (Svg)	-0.058*	-0.048	-0.044	-0.044	-0.044	-0.047	-0.046
	(0.035)	(0.034)	(0.035)	(0.035)	(0.034)	(0.034)	(0.034)
Social Democrats	0.024**	0.026**	0.022**	0.022**	0.021*	0.021**	0.020*
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)
Court dep. FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Judge FE	No	No	Yes	Yes	Yes	Yes	Yes
Laymen cov.	No	No	No	Yes	Yes	Yes	Yes
Case cov.	No	No	No	No	Yes	Yes	Yes
Country FE	No	No	No	No	No	Yes	Yes
Weekday*Court FE	No	No	No	No	No	No	Yes
Adjusted R^2	0.009	0.028	0.056	0.058	0.067	0.083	0.084
F-test (p-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Observations	6759	6759	6759	6627	6627	6627	6626

Note: All regressions include year*court FE. Standard errors are clustered on judge. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

II. Demand for Redistribution: Individuals' Response to Economic Setbacks

1 Introduction

While attitudes to redistribution differ between countries, there are also substantial differences within countries (see Alesina and Giuliano, 2011 for an overview). Given that individuals' attitudes also influence policy outcomes, understanding the determinants of individuals' demand for redistribution is a primary issue. Yet, there is still a lot of uncertainty as to how social policy attitudes are formed and to what extent they are resistant to change. Although economic circumstances have been argued to be a major determining factor of attitudes to redistribution (e.g. Alesina and La Ferrara, 2005), there is little well-identified evidence at the individual level. This is problematic given that theoretical models often assume such a relationship (e.g. Meltzer and Richard, 1981). Furthermore, political attitudes have been found to be rather stable over the life span (e.g. Sears and Funk, 1999), but the empirical evidence is scant and largely based on small non-representative samples. Hence, it is unclear if changes of individuals' economic conditions really have an impact on their attitudes to redistribution. Using data from Sweden, this paper examines whether short-term variation in individuals' economic circumstances causes them to *change* their demand for redistribution (in the form of social benefits/allowances).

The paper is mainly related the literature regarding attitudes to redistribution that focus on economic circumstances as determinants, rather than social preferences and beliefs.[1] While both may be important in explaining attitudes to redistribution, one would expect social preferences to be rather stable over time. Thus, short run variation in attitudes, if it exists, is more likely to be driven by economic circumstances. Several studies based on cross-sectional survey data find that peoples' labor market position, current and expected income level, and the risk of layoff are associated with attitudes to redistribution (Ravallion and Lokshin, 2000; Alesina and La Ferrara, 2005; Rainer and Siedler, 2008; Rehm, 2009). However, other studies using a similar methodology find no association between attitudes to redistribution and economic insecurity or expected gain from welfare programs (e.g., Mughan, 2007; Lynch and Myrskyla, 2009). Margalit (2013) uses survey panel data from the US and finds

[1] There are a number of studies that focus on social preferences, and emphasizes the importance of altruism, inequality aversion, and beliefs about the determinants of poverty (e.g., Fong, 2001; Galasso, 2003). Systematic differences in the support for redistribution have been attributed to culture, social capital, political institutions and historical experiences (e.g., Alesina et al., 2001; Corneo and Grüner, 2002; Alesina and Angeletos, 2006; Alesina and Fuchs-Schündeln, 2007; Luttmer and Singhal, 2011; Eugster et al., 2011; Yamamura, 2012; Algan et al., 2014). Group identity has also been found to predict attitudes to redistribution (e.g., Luttmer, 2001; Klor and Shayo, 2010; Fong and Luttmer, 2011; Dahlberg et al., 2012).

that individuals who experienced a major economic set back, such as a job loss or an increase of subjective job insecurity, became more supportive of welfare spending. To the contrary, there was no effect of finding employment or experiencing a sizable drop of self-reported income on attitudes to redistribution. Looking at relative changes instead, Cruces et al. (2013) set up a survey experiment and find that individuals who overestimated their relative income ranking, demand higher levels of redistribution if they are informed about their true position. The article also relates to the literature on the formation and stability of attitudes. Many studies find a high degree of persistence (e.g., Alwin and Krosnick, 1991; Sears and Funk, 1999), although the degree of persistence is likely to vary depending on attitude domain (Sears, 1983) and country (Niemi and Westholm, 1984). Giuliano and Spilimbergo (2014) argue that individuals' attitudes are particularly responsive to experiences during early adulthood. They find that individuals who grew up during a recession are more supportive of redistribution and believe that success in life depends more on luck than effort.

The paper makes several contributions. First it addresses two main problems that previous studies examining the impact of economic circumstances have faced. Since almost all studies rely on cross-sectional survey data, a causal link between attitudes to redistribution and personal economic circumstances remains unclear. Although it is possible that individuals' economic circumstances shape their demand for redistribution, unobservable characteristics (e.g. the social background of an individual's parents) could account for both an individual's economic situation and attitudes to redistribution. Also, due to lack of detailed information regarding the respondents' home districts, most studies face the problem of separating individual and aggregate effects. For instance, the local unemployment rate will be correlated with the individual probability of a job loss. Do unemployed individuals react to their own unemployment or to the aggregate unemployment? Second, besides attitudes to redistribution I also examine if respondents' evaluation of the political parties as well as what party bloc they vote for is affected. Hence, is a substantial change of economic circumstances, such as a job loss, enough to affect party preferences? Third, most previous studies are based on data from the US, where inequality is considerably higher and the coverage and replacement rate of social insurance is lower than in Sweden (Ferrarini and Nelson, 2003; Scruggs, 2006). This may affect how individuals react to changes of their economic circumstances. Furthermore, the different political systems may also have an impact on attitudes (e.g. Granberg and Holmberg, 1988). For instance, attitudes of the Swedish electorate have been found to be consistently more stable than its American counterpart (Niemi and Westholm, 1984). Hence, there is much

need for research to investigate whether the American results extends to other institutional and political contexts.

I use survey data from the Swedish National Election Studies (SNES) to estimate the effect of economic conditions on demand for redistribution (in the form of social benefits/allowances). The SNES are constructed as a rotating panel, i.e. the same individuals are interviewed in two subsequent elections. The data set covers the years 1991-2010, and includes both periods of economic recession and boom. Compared to previous studies the data set has two main advantages. The panel structure makes it possible to exploit within-subject variation, which strengthens the causal interpretation of the results.[2] The surveys also include detailed information about the location where the respondents live, making it possible to differentiate between the effects of aggregate and individual economic conditions.

The paper examines the impact of income changes, as well as variation of employment status (experiencing a job loss or finding employment). The empirical analysis shows that individuals who lose their job become considerably more supportive of redistribution, whereas individuals who regain employment appear to react in the opposite direction. Respondents who experience a job loss also become more positive to the largest left wing party (the Social Democrats) and more negative to the largest right-wing party (the Moderate party), but the probability to vote for the left-wing is not affected. Clearly economic circumstances is only one among many other issue areas that can affect the vote choice, and the results indicates that a job loss is not enough to alter which political bloc the respondents vote for. Overall, the result suggests that individuals respond due to self-interest, and that attitudes to redistribution return to their initial level as economic prospects eventually improve.

The remainder of the paper proceeds as follows. The next section discusses potential mechanisms. Section 3 describes the data and measurements, and section 4 formalizes the empirical strategy. Section 5 presents the empirical results and, finally, section 6 concludes.

2 Economic circumstances: mechanism

Economic circumstances could affect attitudes to redistribution through several different mechanisms. Whether the effect of a setback, such as a

[2]While Margalit (2013) has panel data and estimates a lagged dependent variable model, I use a first difference specification, to account for unobserved heterogeneity.

job loss, is transitory, i.e. disappear as economic conditions improve, or persistent, has implications for what mechanism is at hand.

First, a change of individuals' economic situation could affect not only their current financial gain from redistribution, but also the probability of being a net contributor/recipient of redistribution in the future. Assuming that peoples' main objective is to maximize their after tax income, income changes should affect individual's demand for redistribution (Meltzer and Richard, 1981; Bénabou and Ok, 2001). Also, if redistribution is seen as insurance against uncertain future income streams, then one would expect individuals with higher risk aversion to favor redistribution (Alesina and La Ferrara, 2005). An economic setback could increases the individual's expected variation of future earnings, and thereby affect the demand for social insurance.[3] These two self-interest mechanisms can affect demand for redistribution, while preferences are constant. Hence, one would only observe a short-term effect on attitudes following a job loss, since demand for redistribution returns to its initial level as economic prospects improve.[4]

On the other hand, if peoples preferences are dependent on uncertain beliefs, new information may change individuals' policy preferences (Page and Shapiro, 1992). Individuals who experience economic setbacks may perceive those that are unemployed or poor as less responsible for their economic conditions, and more deserving of welfare assistance, than they did before. Thus, they might put more weight on the relative importance of luck, as compared to hard work, in determining intra-generational social mobility. In this case experiencing economic changes could involve a learning experience, thereby altering preferences for redistribution.[5] The learning mechanism should affect preferences (and demand), and thereby induce a long-lasting effect on attitudes, even for a temporary setback.

[3]To the extent that private insurance is available, one could of course argue that this would increase the demand for private insurances, rather than social insurance.

[4]Of course, individuals who regain employment could experience continued economic difficulties. For instance, involuntary job losses have been found to result in decreased income and increased earnings variability, even in the long run (e.g. Sullivan and von Wachter, 2009; Couch and Placzek, 2010).

[5]This is quite similar to Piketty (1995), who model how rational agents learn the relative importance of effort and predetermined factors in generating income inequality from their history of inter-generational income mobility.

3 Data and measurement

3.1 Survey data and register information

The survey data on individuals' attitudes to redistribution comes from the Swedish National Election Studies (SNES).[6] The surveys have been carried out at every election (to the Swedish Parliament) since 1956, and the respondents consist of a national representative sample of the population. Since the election 1973 the survey is constructed as a rotating panel, where each individual is interviewed in two successive elections.[7]

The surveys contain information about political attitudes and voting habits, as well as information about the respondents' background characteristics. Most importantly, register information regarding pre-tax income, age, gender, civil status and home district (municipality) has been added by Statistics Sweden.[8] However, the respondents' pre-tax income is only available from 1991. Hence, I use information from the election studies in 1991, 1994, 1998, 2002, 2006 and 2010. The number of individuals included in each panel ranges from 1 200 to 1 800 individuals, and on average 1 500 individuals are included in each panel.[9] Based on the panel feature of the survey it is possible to construct five panels, and each panel consists of individuals that were interviewed in both of the respective years. Information regarding the respondents' home districts make it possible to match the individual survey information with data regarding local economic conditions.[10]

[6]The surveys are conducted as a collaboration between the Department of Political Science at Gothenburg University and Statistics Sweden. The principal investigators were Sören Holmberg and Mikael Gilliam (1991, 1994), Sören Holmberg (1998) and Sören Holmberg and Henrik Oscarsson (2002, 2006, 2010). See http://www.valforskning.pol.gu.se for more information. The survey data has been made available by the Swedish National Data Service (SND). Neither SND nor the principal investigators bear responsibility for the analytical findings in this paper.

[7]Half of the respondents are new each election year. For instance, in 2006 the sample consisted of both the"old group", i.e. individuals that had already been interviewed in 2002, and the "new group", i.e. individuals that would also be interviewed in 2010. The majority of the respondents are interviewed in their homes, whereas those that were "busy or difficult to get in touch with" were interviewed over the phone.

[8]Municipalities (local governments) are responsible for providing a significant proportion of all public services, and have a considerable degree of political autonomy as well as independent powers of taxation.

[9]Calculations are based on the respondents with added register information in both surveys. Register data will not be available for respondents who, for instance, move abroad or decease.

[10]Information regarding the municipal unemployment rate is obtained from the Public Employment Service, income support expenditures are acquired from the National Board of Health and Welfare and data regarding expenditures of sickness allowance and early retirement pension comes from the Institute for Housing and Urban Re-

3.2 Demand for redistribution

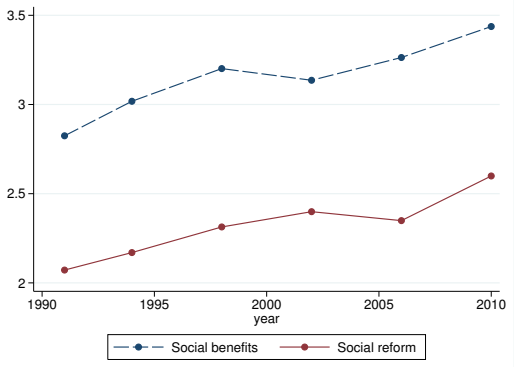
While some aspects of the welfare state are mainly redistributive, such as the progressivity of the income tax, others, such as unemployment benefits, primarily provide social insurance. Although redistribution and social insurance are not the same, social insurance also has redistributive consequences (e.g. Ferrarini and Nelson, 2003; Mahler and Jesuit, 2006). Survey questions often face the problem of distinguishing whether the respondent favors redistribution or social insurance, but most studies assume that they are highly correlated and treat them as equivalent for analytical purposes (e.g. Alesina and Giuliano, 2011; Margalit, 2013).

In the subsequent analysis I focus on two questions that are clearly closer related to social insurance than redistribution. The key dependent variable is the respondents' answers to the question: "What is your opinion about the proposal to reduce social benefits?". Responses were located on a five-point scale: (1) Very good; (2) Fairly good; (3) Neither good nor bad; (4) Fairly bad; (5) Very bad. As an alternative measurement of attitudes to redistribution, I look at individuals' response to the statement "Social reforms have gone too far in this country and the government should decrease rather than increase allowances and support to the citizens in the future." Answers are located on a four-point scale ranging from (1) "Fully agree" to (4) "Fully disagree". This is a somewhat stronger statement that the previous question about social benefits, but one should expect individuals to respond fairly similar.[11]

search and the Swedish Social Insurance Agency. Finally, data concerning the size of the local tax base is provided by Statistics Sweden.

[11] The respondents are not reminded of the potential trade-off between increased spending on social benefits and lower taxes. Research show that respondents tend to express high support for both more social spending and lower taxes, when not reminded about this trade-off (e.g. Page and Shapiro, 1992). Although this could cause respondents to overstate their support for social benefits/allowances, it should not be a problem when it is *changes* of support that is the primary variable.

Figure 1. Response over time

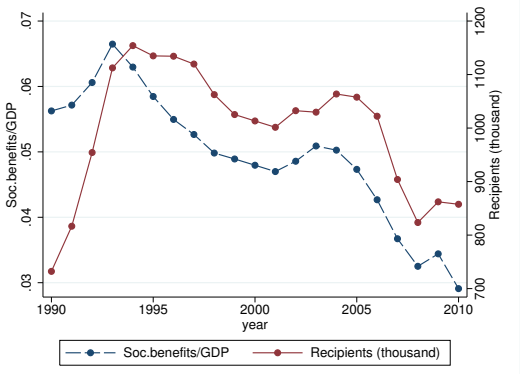


Note: Average responses (by survey year) to the proposal to reduce social benefits or the statement that social reforms have gone too far.

Figure 1 displays the development of the respondents' proposal ratings for the two question using the six surveys included in the analysis. Over time, more people have become negative to the proposal to reduce social benefits, in line with findings by Svallfors (2011). However, if spending on social benefits has also changed over the years, the trend could simply be due to the fact that individuals' reference point has changed. Figure 2 displays how the number of recipients of social benefits as well as spending on social benefits have evolved over time, whereas Figure 3 displays the GDP growth rate and the unemployment rate.[12] It is evident that spending on social benefits has decreased over time. Hence, when examining attitudes to redistribution it is important to account for both these nation-wide trends, as well as changes of spending on social benefits in the respondents' home district.

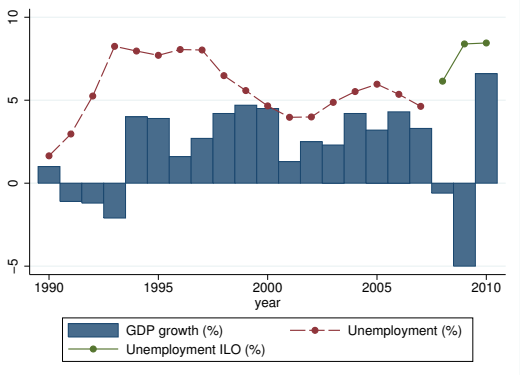
[12]The rules and levels of unemployment benefits, early retirement relief and sickness allowance are decided at the national level. To the contrary, there are cross-municipality differences regarding income support, although there are national guidelines.

Figure 2. Spending on social benefits and number of recipients 1990-2010



Note: Social benefits include unemployment benefits (excluding vocational training), sickness allowance, early retirement pension, and income support. The number of recipients includes individuals that receive unemployment benefits (and those in vocational training), income support, early retirement and sickness allowance. Source: Statistics Sweden, the National Board of Health and Welfare, the Swedish Social Insurance Agency, the Swedish Unemployment Insurance Board

Figure 3. GDP growth rate and unemployment rate 1990-2010



Note: GDP is measured in fixed prices. The definition of unemployment was changed to ILOs definition in 2007. Source: Statistics Sweden

By taking the difference between the respondent's answer in both surveys I construct a variable measuring if the respondents' attitudes to the proposals regarding redistribution have changed. Individuals who become more negative to the proposal to reduce social benefits/allowances (i.e. possibly more positive towards redistribution) are given a positive number, and vice versa. Respondent who state that they "do not know" are excluded. Figures 4 and 5 show the distribution of the constructed variable using the questions about social benefits and social reform. The distribution is fairly symmetric around zero. Around 40-50 percent of the individuals in the sample do not change their rating between the surveys, whereas almost 40 percent change their answer ranking one step along preference rating. Very few individuals shift from stating that the proposal is "very good/fully agree" to stating that it is "very bad/fully disagree", or vice versa.

Figure 4. △ Social benefits

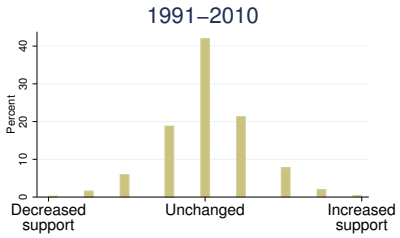
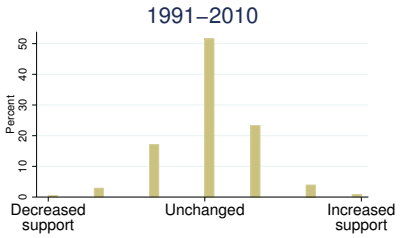


Figure 5. △ Social reform



Note: Change of proposal rating between survey year t and t_{-1}.

Besides the questions about redistribution, I also look at survey questions regarding the attitudes to the major left- and right-wing party. The Moderate Party was in charge of the coalition government 1991-1994 and 2006-2010, whereas the Social Democrats held office 1994-2006. The Social Democrats have consistently favored higher taxes and more generous social benefits than the Moderate Party. Respondents are asked about their attitudes to the different parties on an 11-point scale ranging from "Strongly approve" to "Strongly disapprove". I also look at if respondents state that they will vote/have voted (depending on if they are interviewed just before or after the election) for a party from the left- or right-wing in the national election. The left-wing is defined as the Social Democrats, the Left Party, the Environmental Party and Feminist Initiative.[13]

[13] Some minor parties have not been represented in all elections. Excluding respondents who reported that they voted for a party the first time they were interviewed, that was not available the second time they were interviewed, does not affect the results.

3.3 Summary statistics

Table 1 provides summary statistics regarding the respondents during the two surveys that they were interviewed. The average age at the first survey wave (t_{-1}) was 44 years, 46 percent of the respondents were women, and almost 70 percent of the respondents were employed. The mean change of the proposal rating to reduce social benefits was 0.074, whereas the mean change regarding the statement about social reform was 0.09, indicating that respondents had become more negative to the proposal to reduce social benefits/allowances. On average, respondents also became more negative to the Social Democrats (S) and more positive to the Moderate Party (M). Although the probability to vote for the left-wing has changed between surveys, there is no strong trend over time among the respondents.

Looking at economic circumstances, the average income change is extremely large. When respondents with the top and bottom income at t_{-1} are excluded, the average income increase drops to 32 percent. This is still substantial. The large increase is driven by individuals with very low income in the first survey, and the median income change is considerably smaller, at 4 percent. I use binary variables to indicate changes of employment status, and 3 percent of the respondents experienced a job loss whereas 3 percent found employment. A description of all variables can be found in section A.1 in the Appendix.

This paper examines the effect of income and employment changes on attitudes to redistribution. Most previous studies are based on surveys where respondents have been asked about their income the preceding year, which may be problematic since self-reported income is likely to suffer from problems with measurement error. Statistics Sweden, on the other hand, adds register information regarding the respondents' pre-tax income to the SNES.[14] However, the data refers to the latest assessment (year t_{-1} or t_{-2}), a fact that could also cause some measurement error.[15] Information regarding the individual's labor market position comes from the surveys and is thereby self-reported. Based on this information I define individuals as being either employed, unemployed (also includes individuals in relief work or labor market programs), retired (also includes early retirement pensioners) or students.[16]

[14] Income includes wage, sickness allowance, unemployment benefits, parental allowance, income from business operations, pension, and capital income.

[15] Income data in the 2010 and 2002 year surveys refers to the preceding years' income (t_{-1}), whereas income in the survey 2006 refers to income 2004 (t_{-2}). Statistics Sweden lacks information about whether year t_{-1} or t_{-2} was used in the other surveys.

[16] See the Appendix for details on the exact question wording used for all self-reported variables. Homeworkers are not included in any of the groups.

Whether a change of economic circumstances is unexpected, is obviously hard to know. Some individuals certainly have a higher (objective) probability of losing their job (e.g. workers in certain sectors). If individuals expect their economic situation to change, they are likely to adjust their attitudes in advance. Consequently, to the extent that economic shifts are expected, I would only capture the effect of a change in economic conditions and not the effect of a change in expectations.

When using survey panel data, two potential problems emerge. First, the respondents may differ from the non-respondents regarding observable and unobservable characteristics.[17] Second, one faces the problem of sample attrition when respondents don't participate in all survey waves.[18] This may cause problems since individuals experiencing an economic setback may be less willing to participate the second time. Also, 138 individuals answer that they "do not know/do not want to answer" the question about social benefits in at least one of their interviews. These respondents are excluded from the analysis, resulting in a sample of 3334 respondents, i.e. 6668 observations.[19]

The background characteristics of the respondents and non-respondents (in the panel sample) are compared in Table A1. Only information from administrative registers can be used, since self-reported information is not available for non-respondents. Women are somewhat underrepresented, and the average respondent has a higher income and is more likely to be married than the non-respondent. Hence, one would expect the average respondent to be more negative to redistribution (cf. Table A3).[20] The respondents also come from municipalities with lower tax base, higher unemployment, and lower social benefits expenditures. The fact that the tax base is lower and unemployment higher, would, on the

[17] Although the response rate (based on the first interviews) is quite high, at 88 percent, respondents that were busy or unwilling to participate (19 percent) were given a shorter questionnaire that did not include the question about demand for redistribution. Thus, the response rate to the question of interest (social benefits) is only 69 percent.

[18] Attrition is evident since only 65 percent of those who answered the question about social benefits in the first survey also answer it the second time. The decrease is mainly driven by the fact that 20 percent of them choose to answer the short survey the second time they are interviewed.

[19] 12 of them miss information about their labor market status in at least one survey.

[20] If respondents struggle with cognitive dissonance (e.g., Akerlof and Dickens, 1982; Bénabou and Tirole, 2006) it is possible that they will respond more to positive economic changes than negative, given their initial predisposition.

Table 1. Summary statistics

	mean	sd	min	max
△ Social benefits	0.074	1.171	-4.000	4.000
△ Social reform	0.090	0.884	-3.000	3.000
△ Attitude, S	-0.113	2.105	-10.000	9.000
△ Attitude, M	0.122	2.404	-10.000	10.000
△ Vote left	-0.001	0.348	-1.000	1.000
△ Income (%)	7.800	240.084	-1.000	13137.611
△ Income* (%)	0.324	1.516	-1.000	23.985
Found job	0.029	0.168	0.000	1.000
Lost job	0.030	0.171	0.000	1.000
Got married	0.070	0.255	0.000	1.000
Got divorced	0.054	0.225	0.000	1.000
Student found job	0.046	0.210	0.000	1.000
Student got unemployed	0.007	0.083	0.000	1.000
Got retired	0.062	0.241	0.000	1.000
Increased to full time	0.073	0.260	0.000	1.000
Decreased to part time	0.067	0.250	0.000	1.000
New high school	0.019	0.136	0.000	1.000
New university	0.056	0.230	0.000	1.000
Background characteristics				
Women	0.461	0.499	0.000	1.000
Immigrant	0.025	0.155	0.000	1.000
$\text{Age}_{\text{t-1}}$	44.953	16.151	18.000	80.000
$\text{Education}_{\text{t-1}}$	1.928	0.925	0.000	3.000
$\text{Income}_{\text{t-1}}$	232.519	238.992	0.000	9365.132
$\text{Employed}_{\text{t-1}}$	0.678	0.467	0.000	1.000
$\text{Unemployed}_{\text{t-1}}$	0.059	0.235	0.000	1.000
$\text{Student}_{\text{t-1}}$	0.077	0.267	0.000	1.000
$\text{Retired}_{\text{t-1}}$	0.176	0.381	0.000	1.000
$\text{Tax base}_{\text{m,t-1}}$	133.554	25.403	84.119	284.626
$\text{Unemployment}_{\text{m,t-1}}$	7.182	3.338	1.200	20.100
$\text{Social benefits}_{\text{m,t-1}}$	9.332	2.279	3.937	18.825
Observations	3334			

Note: Income, tax base, and social benefits are given in 1000 SEK (≈ $ 120) and 2010 year value. Income* excludes respondents with top 5 % and bottom 5 % income at t-1. Attitude S/M measure respondents' attitudes to the Social Democrats and the Moderate party. All variables are described in section A.1.

other hand, lead us to expect the respondents to be more positive to redistribution (cf. Table A3). All these differences should be kept in mind when drawing conclusions about the impact of economic conditions on attitudes to redistribution, but it is not evident how they will affect the results.

4 Estimation method

As a benchmark, I start off with a specification similar to previous cross-sectional studies, although municipality characteristics are now also included. The model looks as follows:

$$y_{imt} = \alpha_0 + \alpha_1 E_{imt} + \alpha_2 X_{imt} + \alpha_3 M_{mt} + \theta_t + \upsilon_{imt} \quad (1)$$

The dependent variable (y_{imt}) measures attitudes to social benefits, social reform, the political parties, or voting, as described above. E_{imt} includes the logarithm of income and/or dummy variables that refer to the individuals' labor market position. Most studies (see Alesina and Giuliano (2011) for an overview) also assume that background characteristics such as age, gender, race, and education can affect attitudes to redistribution.[21] Hence, X_{imt} includes age, age^2, gender, immigrant, education, part-time work, and marital status, whereas M_{mt} includes the municipal unemployment rate, tax base (per capita), and social benefits expenditures (per capita).[22] Finally, θ_t is a year fixed effect. The error term (υ_{imt}) is allowed to be arbitrarily correlated within individuals (i.e. the residual is clustered at the individual level).

Now, any observed relationship between the individuals economic circumstances and demand for redistribution could be driven by an omitted variable (θ_i), in which case the estimator is biased. For instance, an individual's previous experiences and social background are likely to affect both the individual's attitudes and economic situation. In this case the key identifying assumption in equation (1), i.e. $\mathrm{E}(\upsilon_{imt}|E_{imt}) = 0$, is unlikely to hold since $\upsilon_{imt} = \theta_i + \varepsilon_{imt}$.

The panel dimension makes it possible to control for unobserved heterogeneity. By differencing equation (1) I eliminate θ_i, and this results in the following model:

$$\triangle y_{ims} = \beta_0 + \beta_1 \triangle E_{ims} + \beta_2 \triangle X_{ims} + \beta_3 \triangle M_{ms} + \theta_s + \triangle \varepsilon_{ims} \quad (2)$$

[21] Including income may introduce a post-treatment bias, so income is exclude. Note that although the size of the point estimates is affected by this, the significance level is not.

[22] Social benefits expenditures do not include spending on unemployment benefits, since the information is not available at municipality level for all years.

The dependent variable ($\triangle y_{ims}$) measures the change of attitudes. The subscript s indicate that all differences are taken between individuals in the same survey panel (i.e. differences are taken between survey year t and t_{-1}). $\triangle E_{ims}$ includes the economic variables, which are the percentage change of income, and/or the binary variables that indicate changes of labor market status.[23] Since only the time-variant variables are included, $\triangle X_{ims}$ include changes of education, marital status, part- and full-time work, as well as the linear age term (age*) that is left after differencing equation (1). A potential concern is that some variables might generate post-treatment bias (Angrist and Pischke, 2008). Hence, it is important to compare the results both with and without the individual covariates. Finally, $\triangle M_{ms}$ includes changes of the municipal unemployment rate, tax base and social benefits expenditures. By conditioning on the municipal variables I can separate the individual and aggregate economic conditions, to make sure that a change of demand for redistribution does not simply reflect a change of local circumstances.[24] Instead of the year fixed effects, survey panel fixed effects (θ_s) are constructed to capture nation-wide trends in the demand for social benefits between the survey panels, due to e.g. the business cycle. The survey panel fixed effects will also capture any variation of attitudes that is simply due to a change of reference point.

5 Results

In order to be able to compare the results with previous studies Table A3 presents the result of estimating equation (1) by pooled OLS.[25] Respondents with a higher income are found to be more negative towards redistribution, whereas unemployed are more positive (compared to employed individuals).[26] Overall, the results are quite similar to results obtained by using survey data for the US (cf. Alesina and Giuliano, 2011). Attitudes are by their very nature context-dependent. Hence, asking about redistribution in Sweden and the US is to pose entirely different questions, since the countries are typically thought to belong

[23] The actual percentage change of income is used rather than the log difference approximation. The percentage change is not calculated for individuals with zero income in the first period.

[24] See Elinder (2010) for the association between local economic conditions and voting in Sweden.

[25] Using an ordered probit model provides similar results.

[26] High income earners are also more positive to the largest right-wing party the Moderate Party (M) and less likely to vote for a party from the left-wing. Unemployed individuals, on the other hand, are more positive to the largest left-wing party the Social Democrats (S), and more likely to vote for a party from the left-wing.

to different 'welfare state regimes' (Esping-Andersen, 1989). While the level of support for redistribution differs between the countries, the result shows that the attitudinal social cleavages are similar.

Table 2 presents the results of estimating equation (2) by OLS.[27] The sample is restricted to individuals who answer the question about social benefits or social reform both times they are interviewed. The point estimate for newly employed has the expected negative sign, but is only significant regarding the question about social reform and not when looking at the question about social benefits. Respondents who lose their job, on the other hand, become significantly more positive towards redistribution both regarding social reform and social benefits. Their support increases by 0.17 and 0.34 points on the preference ordering, respectively. This accounts for around 1/4 of one standard deviation of the outcome variable, a quite substantial change.[28]

Newly unemployed respondents also become more positive to the Social Democrats and more negative to the Moderate Party, although the probability to vote for the left-wing is not affected.[29] One explanation could be that respondents state that several other issue areas (such as health care, elderly care and education) are also important when deciding what party to vote for. The point estimate for newly unemployed respondents does not change much when controlling for changes at the municipality level, indicating that unemployed individuals do in fact respond to their own job loss (cf. Table A4 and A5).

[27] Using an ordered probit model provides similar results. The results are very similar to Table 2.

[28] Looking at income changes in table X, the point estimate of the income change has the expected sign, it is very small and not significant when looking at attitudes to social benefits. In fact, it is only significant regarding attitudes to the Social Democrats, although it does not have the expected sign, i.e. respondents with income increases have become more positive to the party. However, when outliers are excluded (panel B), none of the point estimates are significant.

[29] The lack of response is not driven by the fact that all individuals who got unemployed already voted for the left-wing, although a majority (60 percent) of them reported that they voted for the left-wing the first time they were interviewed. Also, the result is not driven by the fact that the sample size in column(5) is somewhat smaller since respondents who state that they voted for an "other" party or did not know what party to vote for are excluded, i.e. restricting column (1)-(4) to respondents who also answered the question about voting does not affect the results.

Table 2. First difference specification

	(1) S. benefits	(2) S. reform	(3) Attitude, S	(4) Attitude, M	(5) Vote, left
Panel A					
△ Income (%)	-0.000011	0.000033	0.00027***	-0.000055	-0.000017
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	3234	2971	3263	3248	2790
R^2	0.017	0.023	0.087	0.093	0.042
Panel B					
△ Income* (%)	0.0055	0.0023	-0.027	-0.0062	-0.00036
	(0.013)	(0.010)	(0.022)	(0.025)	(0.005)
Observations	3072	2824	3096	3083	2661
R^2	0.017	0.025	0.090	0.093	0.042
Panel C					
Found job	-0.11	-0.21**	-0.015	0.019	-0.061
	(0.146)	(0.104)	(0.220)	(0.256)	(0.052)
Lost job	0.34***	0.17*	0.47**	-0.79***	0.023
	(0.131)	(0.095)	(0.207)	(0.260)	(0.046)
Observations	3279	3010	3309	3294	2822
R^2	0.017	0.023	0.086	0.095	0.041
Municipality cov.	Yes	Yes	Yes	Yes	Yes
Individual cov.	Yes	Yes	Yes	Yes	Yes

Note: Panel A uses all respondents, while Panel B excludes respondents with top 5 % and bottom 5 % income at t-1. Panel C only looks at change of employment and does not control for income changes. All regressions include survey fixed effects. Robust standard errors in parenthesis. Controls: age*, change of university, high school, married, divorced, retired, part-time, full-time, unemployment rate, tax base, social benefits. Panel A and B also control for job loss/found job. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Overall, the results are similar to results for the US (Margalit, 2013), although the model specification differs.[30] Unlike Margalit I find an effect on attitudes to redistribution not just of losing a job, but also of regaining employment when looking at the question about social reform. Even though there is no significant effect of regaining employment on attitudes towards social benefits, I cannot reject the hypothesis that the absolute values of finding a job and a job loss are equal.[31] Overall,

[30] Margalit (2013) uses a lagged dependent variable model, and a potential concern is the fact that the empirical design does not fully account for the problems caused by unobserved heterogeneity. The data set is constructed as a four wave panel and the following specification is estimated: $Welfare_{i,t} = \alpha + \beta_1 Welfare_{i,t-1} + \beta_2 Shock_i + \gamma Demographics_{i,t} + \phi SurveyWave + \epsilon_i$, where $\hat{\beta}_1 \approx 0.6$. Instead of using the panel dimension to account for unobserved heterogeneity (θ_i) directly, $Welfare_{i,t-1}$ is used as a proxy for θ_i. Using a specification with a lagged-dependent variable on the right hand side when using the Swedish data, increases the point estimate of a job loss by almost 50 %, and increases precision (significant at the 1 percent level).

[31] Table A2 also compare individuals who lost or found a job, and show that their attitudes to redistribution are similar at the point in time when they are both employed.

this suggests that respondents only change attitudes temporary. Hence, attitudes to redistribution return to their initial level when economic prospects eventually improve, in line with the self-interest mechanisms.

5.1 Heterogeneous effects

How individuals respond to employment changes will depend on a number of factors. First, the economic setback of unemployment is expected to be higher for high income earners, since unemployment benefits cover a lower share of their lost earnings.[32] Second, the impact of losing a job is likely to depend both on the prospects of regaining employment as well as to what extent the event was expected. In municipalities with a high unemployment rate, the probability of finding a new job is probably lower, resulting in longer unemployment spells, but respondents in those municipalities may also be more aware about the risk of unemployment, i.e. the coefficient may not capture the change of expectations. Similarly, finding a job when the unemployment rate is high could also be more unexpected compared to when the unemployment rate is low, and also give the respondent a clearer signal of their own capacity, i.e. they get by even when times are difficult.

In Table 3 I look at the difference depending on the respondents income in period t_{-1}, the municipal unemployment rate at period t, and whether the unemployment rate increased or decreased during the surveys. It is clear that the effect of unemployment is mainly driven by high income respondents, whereas there is not all that much of a difference between respondents depending on the local unemployment rate.[33] The fact that high income respondents, who are expected to experience a larger economic setback, react more is consistent with the view that respondents react due to self-interest, rather than learning about determinants of social mobility.

Looking at the point estimates, they are, on average, larger for individuals who found a job in a municipality with a high unemployment rate or where the unemployment rate increased, while the reverse is true for those who lost their job. Although these differences are not significant,

[32] During all years included in the sample unemployed individuals satisfying specific conditions (such as work requirements and membership in an unemployment insurance fund) were entitled to income compensation up to 80 % (90 % in 1991) of their previous income. However, individuals with income above a defined income ceiling got less than 80 %.

[33] Around 60 % of the respondents who lost their job had income below the median and lived in a municipality with an unemployment rate above the median. 70 % of them also lived in a municipality where the unemployment rate increased.

it could indicate that individuals react stronger to employment changes when their economic situation deviates from the development of the rest of the economy. This could be due both to the fact that the employment change is less expected, and that it provides a clearer signal of their own capacity, as discussed above.

Table 3. Effects depending on income and local unemployment rate

	Low inc.		High inc.		Unemp high.		Unemp low.		Unemp inc.		Unemp dec.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	S. ben.	S. ref.	S. ben.	S. ref.	S. ben.	S. ref.	S. ben.	S. ref.	S. ben.	S. ref	S. ben.	S. ref.
Found job	-0.121	-0.182	-0.106	-0.309	-0.260	-0.197	0.073	-0.177	-0.318	-0.370**	0.037	-0.072
	(0.180)	(0.122)	(0.264)	(0.189)	(0.186)	(0.124)	(0.237)	(0.187)	(0.234)	(0.147)	(0.189)	(0.144)
Lost job	0.264	0.149	0.420**	0.206	0.330*	0.039	0.296*	0.317**	0.363**	0.129	0.290	0.277*
	(0.181)	(0.132)	(0.193)	(0.132)	(0.191)	(0.136)	(0.166)	(0.124)	(0.154)	(0.120)	(0.242)	(0.147)
Municipality cov.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual cov.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1652	1470	1627	1540	1607	1449	1672	1561	1739	1588	1540	1422
R^2	0.030	0.036	0.016	0.028	0.029	0.023	0.022	0.030	0.024	0.027	0.022	0.030

Note: All regressions include survey fixed effects. Low/High income is based on the respondents' median income at time t_{-1}, and Unemp. high/low. is based on the municipalities' median unemployment rate at time t. Robust standard errors in parenthesis. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

5.2 Placebo test

The use of rotating panel data, rather than reliance on cross-sectional data, decreases the concern that omitted variables are the real determinants of demand for redistribution. Although I can control for time constant unobservables, I can not exclude the possibility that there are time invariant unobservables (for instance a health shock) causing both unemployment and a change of attitudes. Also, a substantial change of economic circumstances, such as a job loss, could be associated with a general feeling of disorientation, causing individuals to change attitudes on a number of issues. To strengthen the causal interpretation, individuals experiencing such a change should not respond differently than individuals not undergoing it, regarding policy areas unrelated to redistribution. The SNES includes a set of questions on other policy domains, which I use to conduct a placebo test. I use questions regarding a bunch of different proposals, such as forbidding all forms of pornography, privatizing health care and reducing foreign aid (all questions are available in the Appendix). Although some of them could have an indirect effect, through the government budget, on the resources left over for redistribution, one would at least expect the point estimates to be considerably smaller. Responses are located on the same five point scale as the question about social benefits.

As expected, the results in Table 4 show no effect of employment changes on attitudes to other issue areas. This indicates that a change of economic circumstances is associated with a change of attitudes to a policy area directly related to social insurance, but not with a corresponding attitudinal shift in other policy domains. The result strengthens the belief that the estimated effect on demand for redistribution is driven by a change of economic conditions.

Table 4. Placebo test

	(1) S. benefits	(2) S. reform.	(3) Privatize	(4) Defence	(5) Porn	(6) Health	(7) Refugee	(8) Aid	(9) Immigrant
Found job	-0.115	-0.211**	-0.050	-0.084	0.144	0.117	-0.077	0.092	0.047
	(0.146)	(0.104)	(0.128)	(0.123)	(0.127)	(0.136)	(0.117)	(0.117)	(0.121)
Lost job	0.338**	0.172*	0.020	0.121	-0.126	-0.003	0.042	0.023	-0.124
	(0.131)	(0.095)	(0.143)	(0.125)	(0.118)	(0.132)	(0.116)	(0.100)	(0.108)
Municipality cov.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual cov.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3279	3010	3130	3241	3258	3247	3232	3231	3217
R^2	0.017	0.023	0.057	0.028	0.061	0.125	0.007	0.044	0.040

Note: The sample is restricted to individuals who respond to the question about social benefits or social reform in both surveys. All regressions include survey fixed effects. Robust standard errors in parenthesis. The proposals for the placebo test are (3) sell public companies to private buyers (4) reduce defense spending (5) forbid all forms of pornography (6) run more health care under private direction (7) receive fewer refugees to Sweden (8) reduce foreign aid (9) increase the economic support to immigrants so they can preserve their own culture.
** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

6 Conclusion

The existence of a causal link between demand for redistribution and personal economic circumstances has remained unclear due to the reliance on cross-sectional survey data. By using survey panel data, I estimate the effect of economic circumstances on demand for redistribution in the form of social benefits/allowances. The empirical analysis shows no effect of income changes. To the contrary, individuals who experience a job loss become considerably more supportive of redistribution. Yet, suggestive evidence indicates that attitudes to redistribution return to their initial level as economic prospects improve. Hence, individuals appear to react temporary due to self-interest by demanding insurance, rather than permanently changing their attitudes and demanding more redistribution per see.

The results are similar to findings in the US by Margalit (2013). This is surprising since the economic consequences of unemployment are expected to be smaller in Sweden, given a more generous welfare system. On the other hand, there is a stronger egalitarian norm in Sweden which could induce individuals to respond even to small changes of their economic situation. While newly unemployed respondents also become more positive to the largest left-wing party, and more negative to the largest right-wing party, the probability to vote for the left-wing is unaffected, suggesting that party preferences are more stable.

References

Akerlof, G. A. and Dickens, W. T. (1982). The Economic Consequences of Cognitive Dissonance. *American Economic Review*, 72(3):307–319.

Alesina, A. and Angeletos, G. (2006). Fairness and Redistribution. *American Economic Review*, 95(4):960–80.

Alesina, A. and Fuchs-Schündeln, N. (2007). Goosbye Lenin (or Not?): The Effect of Communism on People. *American Economic Review*, 97(4):1507–1528.

Alesina, A. and Giuliano, P. (2011). Preferences for redistribution. In Benhabib, J., Jackson, M., and Bisin, A., editors, *Handbook of Social Economics, Vol. 1A*, pages 93–131. North Holland, Amsterdam.

Alesina, A., Glaeser, E., and Sacerdote, B. (2001). Why Doesn't the US Have a European-Style Welfare State? *Brookings Papers on Economic Activity*, (2).

Alesina, A. and La Ferrara, E. (2005). Preferences for redistribution in the land of opportunities. *Journal of Public Economics*, 89(5-6):897–931.

Algan, Y., Cahuc, P., and Sangnier, M. (2014). Efficient and Inefficient Welfare States. *forthcoming Economic Journal*.

Alwin, D. and Krosnick, J. (1991). Aging, cohorts, and the stability of sociopolitical orientations over the life span. *Handbook of Social Economics, Vol. 1A*, 97(1):169–95.

Angrist, J. and Pischke, J.-S. (2008). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press, Princeton, NJ.

Bénabou, R. and Ok, E. (2001). Social Mobility and the Demand for Redistribution: the POUM Hypothesis. *The Quarterly Journal of Economics*, 116:447–487.

Bénabou, R. and Tirole, J. (2006). Belief in a Just World and Redistributive Politics. *Quarterly Journal of Economics*, 121(2):699–746.

Corneo, G. and Grüner, H. (2002). Individual preferences for political redistribution. *Journal of Public Economics*, 83:83–107.

Couch, K. and Placzek, D. (2010). Earnings Losses of Displaced Workers Revisited. *American Economic Review*, 100(1):572–589.

Cruces, G., Perez-Truglia, R., and Tetaz, M. (2013). Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment. *Journal of Public Economics*, 98:100–112.

Dahlberg, M., Edmark, K., and Lundqvist, H. (2012). Ethnic Diversity and Preferences for Redistribution. *Journal of Political Economy*, 120(1):41–76.

Elinder, M. (2010). Local economies and general elections: The influence of municipal and regional economic conditions on voting in sweden 1985-2002. *European Journal of Political Economy*, 26(2):279–292.

Esping-Andersen, G. (1989). *Three Worlds of Welfare Capitalism*. Polity Press, Cambridge, UK.

Eugster, B., Lalive, R., Steinhauer, A., and Zweimüller, J. (2011). The Demand for Social Insurance: Does Culture Matter? *Economic Journal*, 121(556):F413–F448.

Ferrarini, T. and Nelson, K. (2003). Taxation of social insurance and redistribution: a comparative analysis of ten welfare states. *Journal of European Social Policy*, 13(1):21–33.

Fong, C. (2001). Social preferences, self-interest, and the demand for redistribution. *Journal of Public Economics*, 82:225–246.

Fong, C. and Luttmer, E. (2011). Do fairness and race matter in generosity? evidence from a nationally representative charity experiment. *Journal of Public Economics*, 95:372–394.

Galasso, V. (2003). Redistribution and fairness: a note. *European Journal of Political Economy*, 19(4):885–892.

Giuliano, P. and Spilimbergo, A. (2014). Growing Up in a Recession. *Review of Economic Studies*, 81(2):787–817.

Granberg, D. and Holmberg, S. (1988). *The Political System Matters: Social Psychology and Voting Behavior in Sweden and the United States*. Cambridge University Press, Cambridge, UK.

Klor, E. and Shayo, M. (2010). Social identity and preferences over redistribution . *Journal of Public Economics*, 94(3-4):269–278.

Luttmer, E. (2001). Group Loyalty and the Taste for Redistribution. *Journal of Political Economy*, 109(3):500–528.

Luttmer, E. and Singhal, M. (2011). Culture, Context, and the Taste for Redistribution. *American Economic Journal: Economic Policy*, 3(1):157–179.

Lynch, J. and Myrskyla, M. (2009). Always the Third Rail? Pension Income and Policy Preferences in European Democracies. *Comparative Political Studies*, 42(8):1068–1097.

Mahler, V. and Jesuit, D. (2006). Fiscal redistribution in the developed countries: new insights from the Luxembourg Income Study. *Socio-Economic Review*, 4:483–511.

Margalit, Y. (2013). Explaining Social Policy Preferences: Evidence from the Great Recession. *American Political Science Review*, 107(1):80–103.

Meltzer, A. and Richard, S. (1981). A Rational Theory of the Size of Government. *Journal of Political Economy*, 89:914–927.

Mughan, A. (2007). Economic Insecurity and Welfare Policy Preferences: A Micro Level Analysis. *Comparative Politics*, 39(3):293–310.

Niemi, R. and Westholm, A. (1984). Issues, parties and attitudinal stability: A comparative study of Sweden and the United States. *Electoral Studies*, 3(1):65–83.

Page, B. and Shapiro, R. (1992). *The Rational Public: Fifty Years of Trends in Americans' Policy Preferences*. University of Chicago Press, Chicago.

Piketty, T. (1995). Social Mobility and Redistributive Politics. *Quarterly Journal of Economics*, 110:551–584.

Rainer, H. and Siedler, T. (2008). Subjective income and employment expectations and preferences for redistribution. *Economic letters*, 99(3):449–453.

Ravallion, M. and Lokshin, M. (2000). Who wants to redistribute? The tunnel effect in 1990s Russia. *Journal of Public Economics*, 76(1):87–104.

Rehm, P. (2009). Risk and Redistribution an Individual-Level Analysis. *Comparative Political Studies*, 42:855–881.

Scruggs, L. (2006). The Generosity of Social Insurance 1971-2002. *Oxford Review of Economic Policy*, 22(3):349–364.

Sears, D. (1983). The persistence of Early Political Predispositions: The Role of Attitude Object and Life Stage. In Wheeler, L. and Shaver, P., editors, *Review of of Personality and and Social Psychology*, volume 4, pages 79–116. Sage, Berverly Hills.

Sears, D. and Funk, C. (1999). Evidence of the Long-Term Persistence of Adults' Political Predispositions. *Journal of Politics*, 61(1):1–28.

Sullivan, D. and von Wachter, T. (2009). Job Displacement and Mortality: an Analysis using Administrative Data . *Quarterly Journal of Economics*, 124(3):1265–1306.

Svallfors, S. (2011). A Bedrock of Support? Trends in Welfare State Attitudes in Sweden, 1981-2010. *Social Policy and Administration*, 45(7):806–825.

Yamamura, E. (2012). Social capital, household income, and preferences for income redistribution. *European Journal of Political Economy*, 28(4):498–511.

Appendices

A.1 Definition of variables

Found job/Lost job are binary variables that refer to individuals that reported that they were unemployed (employed) the first time they were interviewed but employed (unemployed) the second time they were interviewed.

Student found job/got unemployed refer to respondents that reported that they were students in the first survey but employed (unemployed) in the second survey.

Got retired refers to respondents that change labor market group from being employed to retired.

Increased to full time and *Decreased to part time* are binary variables that indicate if the individual worked/had worked part-time in the first survey and full-time in the second survey or vice versa.

Got married and *Got divorced* are binary variables that indicate if the individual was single in the first survey and married/cohabitant in the second survey or vice versa.

New University and *New High school* are binary variable that refers to individuals that were studying (or had graduated) at the university/high school the second time they were interviewed, but not the first time.

Immigrant refers to a respondent that grew up in a non-Nordic country.

A.2 Survey questions

If the number given to a particular answer varies over the surveys, the original number is given in parenthesis.

Social benefits: I will now read to you a list of things which some people think ought to be implemented in Sweden. For each of them could you say if it is:

1. a very good proposal
2. a fairly good proposal
3. neither a good nor a bad proposal
4. a fairly bad proposal
5. a very bad proposal
8. Do not know/refuse

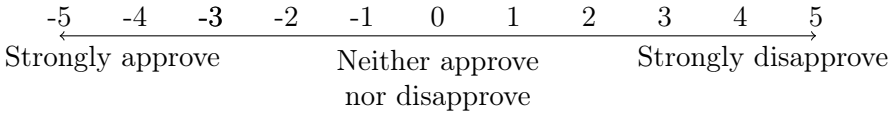
Proposal: reduce social benefits/ forbid all forms of pornography/ reduce foreign aid, reduce defense spending/ sell public companies to private buyers/ run more health care under private direction/ receive fewer refugees to Sweden/ increase the economic support to immigrants so they can preserve their own culture

Social reform: Do you agree with the following statement or do you think it is wrong?

Social reforms have gone too far in this country and the government should decrease rather than increase allowances and support to the citizens in the future

1. Agree completely
2. Largely agree
3. Largely disagree
4. Disagree completely
8. Do not know/refuse

How much do you like or dislike the different parties?



[recoded to a scale from 0 to 10]

Political parties: Social Democrats, Left Party, Environmental Party, Center Party, Liberal Party, Christian Democrats, Moderate Party, New Democracy (1991, 1994), Swedish Democrats (2006-2010), June List

(2006), Pirate Party (2010), Feminist Initiative (2006-2010)

What party will you/ did you vote for in the national election?
1. Left Party
2. Social Democrats
3. Center Party
4. Liberal Party
5. Moderate Party
6. Christian Democrats
7. Environmental Party
8. New Democracy (1991,1994)
8. Swedish Democrats (2006;20,2010;8)
9. Other (1991-1998,2006;9,2002;8,2010;12)
10. Pirate Party (2010)
21. Feminist Initiative (2006;21,2010;9)
22. June List (2006)
85. Did not vote
86. No party
88. Do not know/refuse

Labor market group: Which of the groups on this card do you belong to?

1. Gainfully employed
2. In relief work (1991:2)
   In relief work/youth training/unemployment program (1994-2006:2)
   In unemployment program or labor market training course (2010:2)
3. In labor market training courses (1991-2006:3)
4. In education supported by the so called 'competence boost' (1998-2002:4)
5. Unemployed (1991-1994:4; 1998-2010:5)
6. Old age pensioner (1991-1994:5; 1998-2010:6)
7. Pre-retirement age pensioner (1991-1994:6; 1998-2010:7)
8. Housewife/Domestic worker (1991-1994:7; 1998-2010:8)
9. Student (1991-1994:8; 1998-2010:9)

Part- or Full-time work: On average how much do/did you work? [Question only given to respondents who answer that they have/have had paid work.]

1. Full-time
2. Part-time (1991-1994)
   Part-time, at least 15 h/week (1998-2010)

3. Part-time, less than 15 h/week (1998-2010)

Immigrant: Where did you (for the most part) live as a child?

1. Countryside in Sweden
2. Built up area in Sweden
3. Town or city (except three cities below) in Sweden
4. Stockholm, Göteborg or Malmö
5. Other Nordic country
6. Other European country outside the Nordic region (1998-2006:6)
   Country outside the Nordic region (1994:6)
7. Country outside Europe (7:1998-2006)

Marital status: Concerning your marital status, which alternative on this card is the best description of your situation?

1. Married/unmarried but living permanently with partner
2. Single: widowed
3. Single: divorced
4. Single: never married (1991-1994:3; 1998-2010:4)
5. Other answer (2002:5; 1998, 2006-2010:7)

Education: What kind of education do you have/What kind of education are you studying for? The respondents' educational level is self-reported and the number of different educational levels has changed over the year, due to school reforms and refinement of the categories.

I categorize education in three levels; (1) Primary and secondary school (low education); (2) High school (neither low nor high education); (3) University (high education).

1. Low education [primary and secondary school]

· Primary school: 6 or 7 year primary/secondary school (old system) (1991-2006:1)
· Not completed primary school/comprehensive school (2010:1)
· Comprehensive school: 8 or 9 year of comprehensive school (current system (1991-2006:2)
· Completed primary school/comprehensive school (2010:2)

2. Neither low nor high education [high school]

· Vocational school: various forms of vocational and apprentice education received in publicly organized schools, 1 year trade college (1991-2010:3)
· Secondary school: 2 year secondary school (old system), total 9 years

education (1991-2010:4)
· Secondary/high school: 2 year secondary school (current system), total 11 years education (1991-2010:5)
· Upper secondary/high school: 3 year secondary school (current system), total 12 years education (1991-1994:6)
· Upper secondary/high school: 3 or 4 year secondary school (current system), total: 12 years education. No degree, study at this level (1998-2010:6)
· Upper secondary/high school: 3 or 4 year secondary school (current system), total: 12 years education. Completed degree (1998-2010:7)
· Post-upper secondary/high school education, not university. No degree, study at this level (2006-2010:8)
· Post-upper secondary/high school education, not university. Completed degree (2006-2010:9)

3. High education [university]

· University (1991-1994:7)
· University: No degree, study at this level (1998-2002:8; 2006-2010:10)
· University: Completed degree/continued education (1998-2002:9; 2006-2010:11)
· University: Ph.D., study at this level or completed degree (2010:12)

A.3 Tables

Table A1. Respondents and Non-respondents

	Non-respondents	N	Respondents	N	Difference
Women	0.517	4346	0.461	3334	0.056***
$\text{Age}_{\text{t-1}}$	45.362	4344	44.953	3334	0.410
$\text{Married}_{\text{t-1}}$ (SCB)	1.426	4346	1.499	3334	-0.072***
Got married (SCB)	0.046	4342	0.050	3334	-0.004
Got divorced (SCB)	0.033	4342	0.031	3334	0.002
$\text{Income}_{\text{t-1}}$	205.869	4346	232.519	3334	-26.651***
△ Income (%)	5.830	4235	7.800	3287	-1.970
△ Income* (%)	0.413	4025	0.324	3121	0.089**
$\text{Tax base}_{\text{m,t-1}}$	143.524	4328	133.554	3329	9.970***
△ $\text{Tax base}_{\text{m}}$	10.931	4312	10.540	3325	0.391
$\text{Unemployment}_{\text{m,t-1}}$	6.571	4328	7.182	3329	-0.611***
△ $\text{Unemployment}_{\text{m}}$	0.513	4312	0.519	3325	-0.006
$\text{Social benefits}_{\text{m,t-1}}$	9.614	4328	9.332	3329	0.282***
△ $\text{Social benefits}_{\text{m}}$	-0.986	4312	-0.620	3325	-0.366***

Note: Income is given in 2010 year value. Tax base and social benefit expenditures are given in 1000 SEK per capita (2010 year value). Marital status is defined using register information from Statistics Sweden (SCB). Non-respondents include all individuals who do not answer both times, either because they choose not to participate, answer the short survey or have non-response to the question of interest at least once.

Table A2. Individuals experiencing change of employment status

	Lost job	N	Found job	N	Difference
Social benefits	3.190	100	3.330	97	-0.140
Social reform	2.385	96	2.357	84	0.028
Women	0.440	100	0.412	97	0.028
Immigrant	0.020	100	0.010	97	0.010
Age	39.270	100	38.124	97	1.146
Married	1.590	100	1.598	97	-0.008
High school	0.490	100	0.667	96	-0.177**
University	0.150	100	0.208	96	-0.058
Part time	0.242	99	0.302	96	-0.060
Income	207.810	100	190.691	97	17.119
Tax base$_{m}$	129.383	100	141.964	97	-12.580***
Social benefits$_{m}$	9.722	100	9.043	97	0.679**
Unemployment$_{m}$	7.023	100	7.653	97	-0.630

Note: Sample restricted to individuals who either lose their job or find a job during the survey period, and their characteristics are compared for the time period when they are employed.

Table A3. Cross-sectional specification

	(1) S. benefits	(2) S. reform	(3) Attitude, S	(4) Attitude, M	(5) Vote, left
Panel A					
ln(Income)	-0.113***	-0.0671***	-0.0327	0.245***	-0.0349***
	(0.016)	(0.012)	(0.034)	(0.041)	(0.008)
Observations	9028	8543	9061	9020	8052
R^2	0.0397	0.0470	0.0649	0.0638	0.0769
Panel B					
Unemployed	0.484***	0.300***	0.230*	-0.837***	0.110***
	(0.054)	(0.042)	(0.118)	(0.149)	(0.024)
Women	0.141***	0.118***	0.143**	-0.430***	0.0398***
	(0.029)	(0.022)	(0.068)	(0.081)	(0.014)
Immigrant	0.0388	0.0457	1.008***	0.222	0.116***
	(0.083)	(0.065)	(0.170)	(0.210)	(0.037)
Age	-0.00577	0.00000563	0.0222*	-0.0868***	0.0140***
	(0.006)	(0.004)	(0.013)	(0.015)	(0.003)
Age squared	0.0000295	-0.0000743	-0.000309**	0.00116***	-0.000189***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Married	-0.163***	-0.101***	0.0866	0.334***	-0.0406***
	(0.031)	(0.024)	(0.072)	(0.084)	(0.015)
Retired	0.119**	0.120***	0.147	-0.570***	0.0711***
	(0.057)	(0.045)	(0.129)	(0.150)	(0.025)
High school	-0.0443	-0.00531	-0.697***	0.813***	-0.156***
	(0.036)	(0.027)	(0.083)	(0.095)	(0.016)
University	-0.0951**	0.0703**	-1.207***	0.915***	-0.246***
	(0.041)	(0.030)	(0.091)	(0.110)	(0.018)
Part time	0.133***	0.0528*	0.198**	-0.281**	0.0284
	(0.042)	(0.032)	(0.094)	(0.113)	(0.019)
Tax base$_m$	0.00292***	0.00300***	-0.00417***	0.00504***	0.000224
	(0.001)	(0.000)	(0.001)	(0.002)	(0.000)
Social benefits$_m$	0.0477***	0.0352***	0.0901***	-0.154***	0.0224***
	(0.006)	(0.005)	(0.013)	(0.015)	(0.003)
Unemployment$_m$	0.0270***	0.0172***	0.0939***	-0.0823***	0.0211***
	(0.005)	(0.004)	(0.011)	(0.013)	(0.002)
Observations	9192	8688	9221	9177	8171
R^2	0.0343	0.0441	0.0650	0.0592	0.0744
Municipality cov.	Yes	Yes	Yes	Yes	Yes
Individual cov.	Yes	Yes	Yes	Yes	Yes

Note: The sample is restricted to individuals who answered the question about social benefits/social reform at least once. The dependent variables are coded with the original scale. Standard errors clustered at the individual level in parenthesis. Controls: age, age^2, gender, immigrant, high school, university, married, retired, part-time, unemployment rate, tax base, social benefits expenditures. Panel A also controls for unemployment. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Table A4. First difference specification, full specification

	S. benefits		S. reform	
	(1)	(2)	(3)	(4)
Found job	-0.151	-0.115	-0.237**	-0.211**
	(0.141)	(0.146)	(0.099)	(0.104)
Lost job	0.341***	0.338**	0.162*	0.172*
	(0.129)	(0.131)	(0.093)	(0.095)
Got married		-0.000		0.014
		(0.086)		(0.069)
Got divorced		0.193**		0.072
		(0.096)		(0.081)
Age*		0.000**		0.000**
		(0.000)		(0.000)
New university		-0.166*		-0.050
		(0.091)		(0.075)
New high school		-0.055		-0.116
		(0.169)		(0.111)
Student found job		-0.204*		-0.139*
		(0.109)		(0.077)
Student got unemployed		0.138		0.096
		(0.309)		(0.235)
Got retired		-0.119		0.083
		(0.086)		(0.067)
Decreased to part time		-0.064		0.046
		(0.075)		(0.071)
Increased to full time		-0.110		-0.007
		(0.083)		(0.065)
$\triangle$ Tax base$_{\text{m}}$		0.006**		0.003
		(0.002)		(0.002)
$\triangle$ Social benefits$_{\text{m}}$		0.065***		0.030
		(0.022)		(0.019)
$\triangle$ Unemployment$_{\text{m}}$		-0.008		0.007
		(0.016)		(0.013)
Observations	3322	3279	3045	3010
R^2	0.006	0.017	0.015	0.023

Note: All regressions include survey fixed effects. Robust standard errors in parenthesis. ** p < 0.1, ** p < 0.05, *** p < 0.01.*

Table A5. First difference specification, full specification

	Attitude, S		Attitude, M		Vote, left	
	(1)	(2)	(3)	(4)	(5)	(6)
Found job	0.074	-0.015	-0.010	0.019	-0.044	-0.061
	(0.222)	(0.220)	(0.252)	(0.256)	(0.051)	(0.052)
Lost job	0.464**	0.472**	-0.832***	-0.786***	0.027	0.023
	(0.202)	(0.207)	(0.261)	(0.260)	(0.046)	(0.046)
Got married		0.011		-0.246		0.001
		(0.147)		(0.175)		(0.030)
Got divorced		-0.070		-0.032		0.017
		(0.155)		(0.175)		(0.034)
Age*		0.000		-0.000		0.000
		(0.000)		(0.000)		(0.000)
New university		0.241		0.016		0.016
		(0.161)		(0.164)		(0.031)
New high school		0.098		-0.050		0.005
		(0.257)		(0.339)		(0.058)
Student found job		0.058		-0.107		-0.004
		(0.176)		(0.203)		(0.038)
Student got unemployed		0.011		0.213		-0.021
		(0.545)		(0.536)		(0.108)
Got retired		-0.051		0.439***		-0.061**
		(0.146)		(0.161)		(0.025)
Decreased to part time		0.119		0.258*		0.004
		(0.140)		(0.151)		(0.025)
Increased to full time		-0.079		0.103		0.003
		(0.145)		(0.176)		(0.028)
$\triangle$ Tax base$_{\text{m}}$		-0.001		-0.005		-0.000
		(0.005)		(0.005)		(0.001)
$\triangle$ Social benefits$_{\text{m}}$		-0.057		0.021		0.008
		(0.040)		(0.043)		(0.007)
$\triangle$ Unemployment$_{\text{m}}$		0.047		-0.007		0.001
		(0.029)		(0.030)		(0.004)
Observations	3349	3309	3334	3294	2856	2822
R^2	0.083	0.086	0.089	0.095	0.037	0.041

Note: All regressions include survey fixed effects. Robust standard errors in parenthesis.
** p < 0.1, ** p < 0.05, *** p < 0.01.*

III. Labor Market Effects of Job Loss: The Importance of Cognitive and Non-Cognitive Skills

Co-authored with Matz Dahlberg and Björn Öckert

1 Introduction

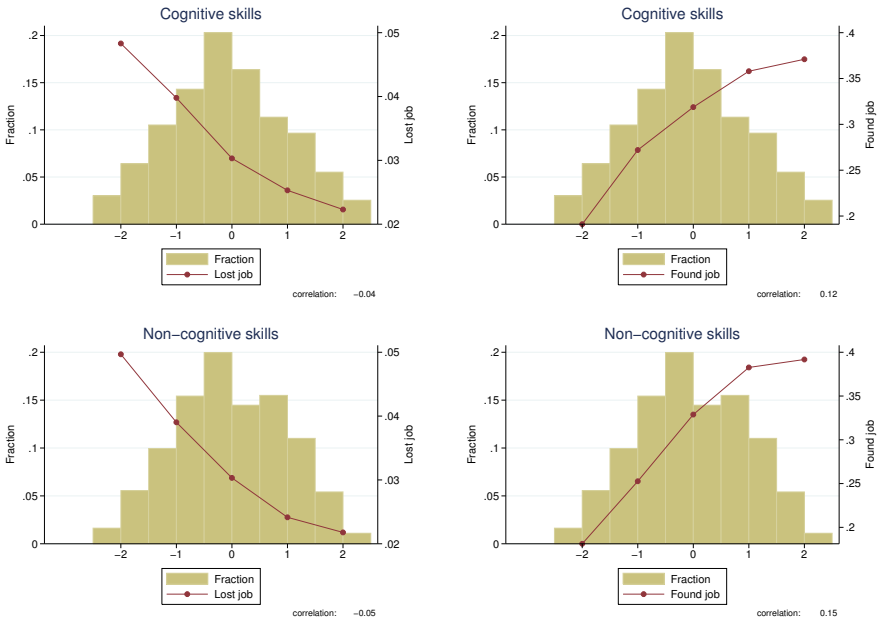
Job reallocation is a natural consequence of economic development and a well-functioning labor market, where more productive companies and plants grow at the expense of the less productive. Yet, as noted by several scholars (e.g., Huttunen et al., 2011; Black et al., 2015) there are two sides of the story. While reallocations are beneficial from an economy point of view, they can be detrimental for those individuals directly affected by large downsizing and closures. For example, several studies have found that displaced workers experience costly spells of unemployment and earnings declines (see e.g., Stern, 1972; Jacobsen et al., 1993; Eliason and Storrie, 2006; Couch and Placzek, 2010) and a deteriorated health (see e.g., Sullivan and von Wachter, 2009; Brand, 2015).

It is well-established that the risk of losing a job is unevenly distributed in the population (see Hines et al., 2001, for a survey), and that individuals with lower skills face a higher risk of unemployment (see e.g., Lindqvist and Vestman, 2011; Öckert, 2011; Seim, 2013). To the extent that low-skilled individuals also experience longer unemployment spells, the societal costs of job reallocation may be substantial. To minimize the costs of job reallocation, it is therefore important to know if the effects of job losses are heterogeneous. In particular, is it the case that those who face the worst (best) labor market outcomes following a negative labor market shock are those with the lowest (highest) skills?

To provide suggestive evidence of the importance of general skills in understanding who enters and exit unemployment, respectively, we have calculated how cognitive and non-cognitive skills are distributed among (i) those that entered into unemployment in 2013 (see the two figures on the left in Figure 1) and (ii) those that exited unemployment in 2013 (see the two figures on the right in Figure 1) based on our population-wide data.[1] A clear pattern emerges showing that it is in the lower part of the skill-distributions that the probability of entering into unemployment is highest; while around 10-20 percent of those in the bottom of the cognitive and non-cognitive distributions entered into unemployment in 2013, only a very small share of those in the top did so. The opposite is true for those who exit unemployment (with a higher probability for individuals in the upper part of the skill-distributions to exit unemployment).

[1] Cognitive and non-cognitive skills are measured via enlistment tests. See the data section for a full description of our data. The main difference is that for the figures in the introduction we use labor force participants with measures of cognitive and non-cognitive skills for *all* sectors, while we only use those employed in the military sector in the rest of the paper.

Figure 1. Exit and entry to unemployment over the skill distribution



Note: Individuals who lost a job were registered at the Employment office during 2013, but not in January. Individuals who found a job were registered at the Employment office during 2013, but not in December (and also had labor income). Points show average share for two bars, conditional on having labor income (lost job) or being registered at the employment office (found job) in 2013. Histograms are based on the full sample, i.e. not restricted to employment status. Correlation coefficient between employment status and skill is displayed in the right corner. The sample excludes outliers (more than 2.5 sd), and is restricted to men age 25-50 in 2013. Correlations for cognitive and non-cognitive measures and the underlying tests are displayed in Table A17.

The results in Figure 1 indicate, first, that general cognitive and non-cognitive, skills might be important for understanding the transition to new employment after a negative labor market shock. Second, there is an important, non-random, selection of individuals into unemployment based on cognitive and non-cognitive skills.[2] Mechanisms that might make high-skilled individuals manage the job reallocation process better than low-skilled individuals can be the availability of better quality networks, a different job search behavior, a higher flexibility in adopting new skills (yielding a higher flexibility in taking on jobs in different sectors and/or in different regions), or that they are favored in the application

[2]Using displacement announcements, Seim (2013) has also shown that there is a selection into displacement based on age and cognitive and non-cognitive skills.

process by new potential employers (see e.g. Neal, 1998 for a related discussion on some of these points). The results in Figure 1 are also in accord with the literature in labor economics showing that cognitive and non-cognitive abilities can be important determinants for different labor market outcomes in general (see e.g., Bowles et al., 2001; Heckman et al., 2006; Lindqvist and Vestman, 2011).

The purpose of this paper is to examine whether there are heterogeneous treatment effects of a negative labor market shock in terms of general (cognitive and non-cognitive) skills.[3] In doing this, we will, first, examine the individual labor market effects for those affected by a job loss without conditioning on skills. Second, we will evaluate to what extent the average, unconditional, effects that we find can be understood as a function of the individual's cognitive and non-cognitive skills. Since individuals are selected into unemployment based on skills, the methodological problem to deal with is the endogeneity of job loss. To solve this problem, we will use the exogenous labor market shock provided by the substantial military base closures in Sweden following the end of the Cold War.[4].

The paper makes three contributions. First, compared to the existing plant closure literature, we argue that we have a strong case for exogenous treatment. To deal with the selection of individuals who experience displacements, most earlier studies focus on events such as mass layoffs or plant closures, where the separation is thought to be independent of a worker's characteristics. These events are typically identified through administrative registers, which can be problematic given the limited information on how and why the mass layoffs occurred and to what extent they were expected. Furthermore, there could still be problems with selection of the displaced workers or selection between firms, since most studies lack a natural comparison group.

These problems are mitigated in our case. The end of the Cold War denoted the beginning of a new geopolitical landscape, and the government announced defense propositions in 1996, 1999, and 2004 that resulted in closures and significant downsizing of several military bases. We use these political decisions as the triggering factors behind the military base ("plant") closure. Although closing and downsizing of military bases was expected at this point in time, it was not known which bases

[3]An earlier version of the paper, Dahlberg et al. (2013), examined labor market outcomes but used data with less detailed information about industry and occupation codes and without enlistment information.

[4]The Swedish military sector experienced substantial cut-backs during this period. In 1995 the Swedish Armed Forces employed around 26 000 individuals, whereas only 16,000 were employed by 2009 (Hedin, 2011)

would be affected until the defense propositions were announced. Moreover, the decision of where to close down was based on factors such as cost efficiency and security policy, implying that it was unlikely to be endogenous to the workers' productivity. Hence, we argue that the displacements that occurred following the acceptance of the propositions were exogenous and unexpected, at least from the perspective of the employees. As will be clear from the data section, the descriptive statistics strongly supports this claim.

Second, to our knowledge we are the first to study if individual's economic setbacks following a plant closure are heterogeneous with respect to direct measures of skills.[5] Seim (2013) uses information on displacement announcements, but finds no differential effects on labor market outcomes between high- and low-skilled individuals. Displacement announcements are however quite likely to suffer from selection problems and do not necessarily lead to plant closures (see further discussion in section 2.2). We have access to information on the individuals' cognitive (IQ-tests) and non-cognitive (evaluations by psychologists) skills from the military draft (enlistment) in Sweden. The enlistment data does not cover the full population, especially not women, but since we examine the effects of military base closures, all affected military personnel, and most of the affected civilian personnel, have done the military enlistment tests. This puts us in a unique position for evaluating the role of cognitive and non-cognitive abilities following a job loss.

Third, following the end of the Cold War, reductions of military personnel and closures of military bases were carried out in many of the salient military powers, such as the UK, France and Germany. The closures were often expected to have tremendous negative consequences for the affected region, but most studies (see e.g., Hooker and Knetter, 2001; Andersson et al., 2007; Paloyo et al., 2010) find small or insignificant effects on economic growth, local employment and migration. Hence, the general conclusion has been that the negative expectations have not been realized. Although the closures may only have had negligible effects on the local economy, it is unclear if these results also apply to employees that were directly affected. To the best of our knowledge, this is the first paper studying the effect of military base closures using individual level panel data covering all military employees.

We have access to full population register data with rich background information of the individuals and covering a fairly long period (1990–

[5]That individual characteristics such as education, sex, and age correlate with post-displacement outcomes has been shown by Farber (2003). Carrington (1993) also find that those who switch industries following displacement have systematically larger earnings losses.

2013). We focus on the effects on labor earnings, employment related income (consisting of unemployment benefits, sickness allowance, early retirement pensions etc.), and disposable income. We also look at the number of days an individual is registered as a job applicant at the Employment office. The employment register also allows us to exactly identify not only those who are employed by the military and at what bases they are employed (giving us a well-defined treatment and control groups), but also in what capacity they are employed. Military employees namely consist of two distinct groups, military personnel and civil personnel. This distinction is important since the two groups often had different types of employment contracts, but also because the two groups are likely to face different labor market opportunities.

To estimate a causal effect, we employ a difference-in-differences approach where we compare military employees at bases that were affected by the propositions in 1996, 1999, and 2004 to employees at unaffected bases. Using linked employer-employee data, we construct a panel of yearly register data for all individuals that were employed in the military sector during 1994, 1997 and 2002, two year before the propositions were announced, and one year before a working group was given the task to draft a proposal of what bases to close down. This data allows us to follow individuals over several years, making it possible to separate the treatment effect over time, in order to fully capture its dynamics. By using a relevant control group, consisting of military employees at units that were not affected by the reform, we isolate the causal effect of the closures on unemployment and labor income.

We find that, on average, labor earnings decrease and unemployment and labor-related benefits increase for those affected. In terms of heterogeneous treatment effects, we find that the treated individuals with high cognitive and (in particular) high non-cognitive skills face shorter unemployment spells than the treated individuals with low cognitive and non-cognitive skills. However, we do not, in general, find any heterogeneous treatment effects on the other outcomes. Given that low-skilled individuals fare the worst in the job reallocation process, it can be motivated to identify and direct different labor market policies towards those individuals in the job reallocation process with lower abilities. In a longer run perspective, policy makers might also want to consider policies aiming at generally improving the cognitive and non-cognitive skills of individuals in the lower end of the skill-distribution. This could, for instance, be done by improving these skills through early childhood interventions (as argued by Heckman and co-authors in several papers; see the summary in Heckman, 2008).

The remainder of the paper proceeds as follows. The next section discusses the previous literature, section 3 describes the institutional background, section 4 describes the data and measurements, and section 5 formalizes the empirical strategy. Section 6 presents the empirical results and, finally, section 7 concludes.

2 Previous literature

Our paper is mainly related to three strands of the literature; the effects of plant closures on individual labor market outcomes, the importance of cognitive and non-cognitive skills for labor market outcomes, and the regional effects of military base closures.

2.1 Plant closures and the effects on individual labor market outcomes

There is a substantial literature focusing on displaced workers and the economic difficulties that they face. In the short run, the cost of displacement is ascribed to forgone earnings during unemployment as well as the loss of firm- and industry-specific human capital (e.g. Hamermesh, 1987). Besides such mechanisms, the long-term effects, often referred to as unemployment scarring, are explained by factors such as loss of general human capital and the tendency for an employer to view an individual's labor market history as a signal of productivity (see e.g., Böheim and Taylor, 2002; Arulampalam et al., 2001).

There are strong reasons to think that involuntary job losses are highly associated with unobserved individual characteristics, such as human capital and productivity. Hence, most studies in the displacement literature focus on events such as mass layoffs or plant closures, where the separation is thought to be independent of a worker's quality, to deal with the identification of non-voluntary job separations. However, expectations of a forthcoming firm closure might cause a selection of the labor turnover prior to the shutdown, as workers with better labor market opportunities may choose to quit beforehand.[6] To deal with this problem most studies define a time window before the closure and define all separations during this period as displacements. This method has been criticized for being arbitrary and using ad-hoc definitions, thereby failing to fully capture the selection process (Schwerdt, 2011).

[6]Seim (2013) also show that plant separations increases several months before a displacement announcement is made.

Even when focusing on events such as mass layoffs, displaced workers are unlikely to constitute a random sample. Many studies have found that the displaced workers suffer earning losses before the separation occurs (e.g., Jacobsen et al., 1993; Eliason, 2011a). This could be due to the fact that distressed firms cut wages or work hours before a mass layoff, or, perhaps more plausible, because of selection, i.e. the least productive workers are laid off in advance.[7] Seim (2013) uses an alternative approach by relying on information on displacement announcements.[8] This approach is also quite likely to suffer from selection problems since it is not clear that neither those that get the displacement announcement nor those that in the end actually lose their job is a random sample.[9] Using closures rather than mass layoffs is likely to mitigate these selection problems. Yet, even in the absence of selection within firms, there could be sorting between firms. Abowd et al. (2009) find that firm closures occur substantially more often in firms that hire a disproportionately high share of worker with low human capital.

Most studies examine the effect of displacements on earnings, and several studies from both the US and Europe have found that displaced workers experience periods with earnings decline. Although the initial drop in income decreases over time, many studies find long run effects. The long term earning decline relative to pre-displacement earnings varies between 13-25 percent in the US (Jacobsen et al., 1993; Couch and Placzek, 2010), 12 percent for Germany (Schmieder et al., 2010) and 7 percent for Sweden (Eliason, 2011b).[10] However, the magnitude of these estimates depends heavily on factors such as industry, macroeconomic conditions, the institutional setting, and the definition of the control group. Hence, any

[7] Eliason (2011a) finds that pre-displacement income from social insurance was higher for the displaced workers compared to their non-displaced co-workers, suggesting that the displaced workers were selected.

[8] In Sweden, when an employer is to lay off five or more workers at the same time or 20 or more workers within a 90 day period, the employer must report this to the Public Employment Service (Seim, 2013, p. 74). However, even a large displacement announcement, e.g. 80 percent of the workforce, which is the cut-off used by Seim, does not imply that the plant actually downsize with that amount in the end, let alone close down.

[9] This concern is confirmed by looking at the estimated pre-treatment trends in Seim, 2013 (see for instance the pre-trends for the low-cognitive individuals in Figures 3.3(a) and 3.3(d) and the young individuals in Figure 3.3(g).

[10] These papers all use administrative register data, and the long term effect is defined as being at least six years after displacement. They estimate some version of the model: $y_{it} = \beta X_{it} + \sum_{k \geq m} \delta_k D_{it}^k + \alpha_i + \gamma_t + \varepsilon_{it}$, were y_{it} represents a measure of annual earnings, X_{it} is a vector of time-variant characteristics, D_{it}^k are dummies indicating the k-th period, (before, during, or after the displacement), m denotes the baseline period, γ_t are year fixed effects, α_i is an individual fixed effect, and ε_{it} is the error term.

comparison is problematic since the environment in which the displacements occur will generally differ. An overview of the previous literature can be found in von Wachter (2010).

The decline in earnings could be due to unemployment, that individuals are leaving the labor force, or to the fact that there is a decline in the displaced workers re-employment wages. Some studies find that the long term effect is mainly driven by lower wages (Schmieder et al., 2010), while others find that the losses are mainly due to periods of non-employment (Hijzen et al., 2010).

2.2 The importance of cognitive and non-cognitive skills for the job reallocation process

Ever since the publication of *The Bell Curve* and its arguments that cognitive skills are the most important determinant for labor market (and other socio-economic) outcomes (Herrnstein and Murray, 1994), there has been a literature examining the role of cognitive and non-cognitive skills for different labor market outcomes. The general understanding today is that both cognitive and non-cognitive skills matter for labor market outcomes (where the main outcome examined has been wages; see e.g., Bowles et al., 2001; Heckman et al., 2006; Lindqvist and Vestman, 2011).

There is scarce evidence on the role played by cognitive and non-cognitive skills in the job reallocation process. The closest study to ours is Seim (2013). Using information on displacement announcements, he finds no differential effects on labor market outcomes between high- and low-skilled individuals (using the same cognitive and non-cognitive skill information from the Swedish enlistment data that we use in this paper). However, as noted earlier, displacement announcements, and actual layoffs following these announcements, are quite likely to suffer from selection problems.

Lindqvist and Vestman (2011) use enlistment information to examine the relationship between cognitive and non-cognitive skills and wages, unemployment, and labor market earnings. They find that men who fare poorly in the labor market (in terms of unemployment and earnings) lack non-cognitive rather than cognitive ability.[11] Conditional on being unemployed, they also find that individuals with high non-cognitive ability

[11] Their baseline model, estimated by OLS, is given by $y_i = \alpha c_i + \beta n_i + \gamma X_i + \varepsilon_i$, were y_i is one of the labor market outcomes, c_i is cognitive ability, n_i is non-cognitive ability, X_i is a vector of control variables, and ε_i is the error term.

experience shorter spells[12] However, as is clear from Figure 1, individuals who are unemployed in a certain period is clearly not a randomly selected skill-group.

2.3 Regional effects of military base closures

Military base closures have often generated concerns and objections from both the public and politicians in the local area (e.g. Warf, 1997). A military base is typically thought to be important to the local labor market, by securing employment opportunities for both military and civil servants, with wages that are primarily financed by the state rather than the region. The closures are expected to cause out-migration as well as higher unemployment rates, thereby affecting the local tax base and the local governments' ability to provide local public goods and services. Hence, base closures are expected to have substantial negative effects on the affected regions.

There are a handful of studies that have examined the effect of military base closures on the local community. Most papers find only small and economically insignificant effects on local growth, net migration, and unemployment (see Andersson et al., 2007 for Sweden, Hooker and Knetter, 2001 for the US, and Paloyo et al., 2010 for Germany).[13] Both Hooker and Knetter (2001) and Paloyo et al. (2010) argue that one explanation to the absence of negative effects is the fact that the opportunity cost of the military bases, for example land and buildings, has been overlooked. Many bases have been reused for civilian purposes, which can have positive implications for the community and local businesses and thereby contribute to economic growth.

Despite the attention surrounding a base closure, we know relatively little about how the displaced workers are affected by the closures. Loughran and Klerman (2012) look at reservists who, following a temporary period of active duty, reenter the civilian labor market. They find that reserve activation causes a temporary earnings decline, which then turns to earnings gains in subsequent years. Our paper differs in the sense that we look at individuals continuously on active duty who involuntary have to transfer to the civil labor market or a new military base. A

[12] A one standard deviation increase in non-cognitive skills decreases the unemployment duration by approximately 10 days. Cognitive ability has however no statistically significant association with the duration of unemployment.

[13] Andersson et al. (2007) study the effect of military base closures in Sweden 1983-1998 on the municipal growth rate and net migration flows, and find no effects. They argue that one potential explanation is that those previously employed at the military bases have found new employment within the region.

few descriptive studies have also examined military employees who were affected by the closures in Sweden. Jakobsson (2010) find that military employees in municipalities affected by the closures in 1999 on average perform better than non-military employees in the same municipality 5 years after the closures regarding factors such as unemployment and labor income.[14] Eriksson and Hallsten (2003) follow civil employees affected by closures in 1996 both before and after the closures. They find a lasting depreciation in several health indicators, primarily among males and regarding indicators related to anxiety.[15]

3 Institutional background

The objective of the Swedish security policy changed drastically after the end of the Cold War. A foreign invasion aiming to occupy Sweden was no longer seen as possible, although attacks at more narrow objects in Sweden could not be dismissed. The primary focus of the armed forces shifted from the ability to halt a military incursion to participation in international peace-keeping interventions.[16] These changes implied cut-backs in spending on the national defense as well as closure of a number of military bases. The government announced the military bases that were to be closed in a number of government bills.[17] In the first step, following the defense bill in 1996, the Swedish defense, which had previously focused on the threat of invasion, was said to transfer from an interventionistic defense to an adaptable defense. In 1999 and 2004 the next step was taken, as the Swedish defense was declared to move towards an interventionistic defense. The 1999 bill amounted to one of the largest reorganizations of the Swedish Armed Forces (SAF) in modern times.

Before deciding which bases to close down, the SAF was given the task to draft a proposal of how to reduce the work force and cut expenditures. The government declared what consideration should be taken into account. For instance, the proposition in 1999 stated that military,

[14] The data is cross-sectional, and given the initial difference between the groups, the difference cannot be interpreted as causal.

[15] Given the lack of a control group the results cannot be given a causal interpretation.

[16] The required number of people doing military service, which was previously mandatory for all young males, also decreased during this period, making the compulsory element less effective.

[17] The key government bills during this period were: Prop. (1991), Prop. (1995), Prop. (1999), Prop. (2004), and Prop. (2008). See the map in Figure A1 in the Appendix for an overview of which bases were closed following the decisions in 1996, 1999, and 2004. The proposition in 2008 did not result in any closures.

economic, regional and environmental pros and cons should be evaluated. Due to strategic reasons, there was a clear aim to have military bases geographically scattered over the country. At the same time, the fact that these considerations should be weighed against cost efficiency (e.g. necessary investments and synergy effects), environmental factors (e.g. availability of permits and training sites), and regional political considerations, made it difficult to predict which units would be closed down. Even places with recent investments (e.g. Ängelholm) or strategic positions (e.g. Gotland) were closed.[18] The proposals were then processed in the parliament, which caused increased uncertainty, not at least due to the fact that the Social Democratic government in office lacked a political majority. The initial proposals were modified on several occasions before the parliament adopted them.[19]

Individuals working in the military sector are employed with a military or civil contract. Military employees consist of individuals working as career officer, soldiers or mariners, whereas civil employees include, among others, mechanics, administrators, and health care staff. Those employed with a military contract prior to 1992 had contracts that guaranteed stronger employment protection (*fullmaktsanställning*), meaning that they could not be dismissed due to redundancy. Although other staff could be dismissed, the ministry of defense was unwilling to do so (in particular for military personnel) since it would require discharging primarily younger individuals and probably induce a hiring freeze. Given that the average age of the workforce was already thought to be too high, this would only enhance the problem. Rather, it was argued that it was

[18] By the end of the 1990s the new battle airplane JAS 39 Gripen had just entered services within the Swedish Air Force, which required the Wings to adjust. Major investments had recently been made to accommodate the new airplanes at the F10 Wing in Ängelholm. Yet, the government decided to close down the F10 Wing in the 1999 proposition, whereas they kept the nearby F17 Wing, which had not yet been prepared to accommodate the new airplanes. Also, even though the government had previously stated that military presence at Gotland (an island between Sweden and Russia) was necessary for strategic reasons, P18 was closed following the 2004 proposition. Due to recent developments there are currently discussions to militarize Gotland once again.

[19] The decision in 1996 and 2000 was a compromise between the Social Democrats and the Centre Party. The government made several adjustments to the 1996 proposal, allegedly influenced by the intense lobbying from politicians in the municipalities that would initially be affected by the closures. The decision in 2004 was preceded by political turbulence as the Social Democrats lacked a majority for their proposal and the Left Party threatened to vote for the opposition's proposal if the military base in Arvidsjaur was closed. Arvidsjaur was finally degraded to a detachment belonging to Boden, but without any reduction of the size of the work force. The closures suggested in the SAF proposal preceding the last government bill, Prop. (2008), were never implemented, as the government decided to cut the grant for work material instead of reducing the work force.

necessary to encourage older staff to resign voluntarily. A number of rather generous initiatives to promote individuals to leave voluntarily were launched already in the early 1990s, such as early retirement and career alternation programs.[20] Furthermore, military employees with *fullmaktsanställning* whose military bases was about to close down were offered severance pay of 6 months if they resigned.[21] All military personnel at the closing bases were also offered a position at another military base, whereas civil employees who did not go for early retirement or career alternation programs were dismissed due to redundancy.[22]

The closures generated objections and protests from both the public and politicians in the affected municipalities, since they were expected to have vast negative consequences. Local politicians attempted to overrule the decision, and at some of the affected places the inhabitants demonstrated against the decision to close their military base.[23] The government started adjustment programs for some municipalities after the closures, in particular following the 1999 and 2004 proposition. Some municipalities that had experienced downsizing of their military units or military industry, but no closures, were also included in the programs.[24]

[20] Employees could get early pension income from age 55/58 with a military/civil contract. Employees older than 35 years (with a military contract) or with at least 15 years tenure (with a civil contract) who resigned from the SAF could continue receiving part of their wage (up to 2 years), conditional on starting to study, starting their own company, or doing an internship that was expected to lead to a stable employment. Between 1999 and 2010 somewhat more than 19,700 employees resigned from the SAF, of which only 1550 transferred to old age pension. The vast majority did so with some form of early age pension or due to their own request (Hedin, 2011). Programs that promoted career alternations were not very successful, and Blomsterberg and Kadefors (2009) argue that this can partly be explained by the fact that military servants have a strong professional identity and are thus unwilling to change occupation.

[21] Employees at the Wing in Ängelholm were offered severance pay for 3 years.

[22] Just like other employees in the public sector, all SAF employees are covered by greater protection than workers in the private sector. They are offered both a longer period of notice, and more generous unemployment benefits than required by the law. It was only possible to dismiss military employees due to shortage of work if they had declined an offer to transfer to another military base.

[23] These protests received much attention in the media, for example SvD (Nov 11, 1999), TT (Sept 30, 1999), TT (June 27, 1999), TT (Sept 23, 1999), and DN (Nov 1, 1999).

[24] Following the 1996 proposition only Söderhamn was given support. The municipalities affected by the programs following the 1999 proposition were Boden, Falun, Gotland, Härnösand, Hässleholm, Kiruna, Sollefteå, Karlsborg and Karlskoga. The two last ones, Karlsborg and Karlskoga, were not affected by the proposition, but Karlsborg had been affected by military base closures in the beginning of the 1990s and the employment level in Karlskoga had decreases due to reductions within industries related to the armed forces. The affected municipalities following the 2004 proposition were Arvidsjaur, Östersund, Kristinehamn and Gotland.

As far as we know there is only one evaluation of the programs connected to the 1999 proposition, where Falkenhall (2004) concluded that they appeared not to have had any major impact.[25] It is obviously difficult to determine what long term effect the programs had on employment in the affected municipalities, but it is important to note that they were not directed towards the newly displaced workers.[26]

4 Data description and summary statistics

4.1 Data description

The defense proposition in 1996, 1999, and 2004 were adopted by the parliament in 1996, 2000, and 2004. The bills resulted in the closure of a number of battalions, forces and regiments as well as headquarters compounds, and the closures occurred within 1-2 years after the announcement. In some municipalities all military units were closed down, while at other places only a few units were affected. Although the propositions were preceded by much debate and speculation, we argue that there was substantial uncertainty around which units would be affected, and that individuals employed by the military two years before the announcements could not foresee the upcoming proposition at that time.

Our primary data is a linked employer-employee dataset (LOUISE) covering the full population 1990-2013, compiled for research purposes by Statistics Sweden, and held by the Institute for Evaluation of Labor Market and Education Policy (IFAU). The dataset is collected on a yearly basis and contains information on individual characteristics such as income, employment, and education. The data links all individuals to their employers, providing information on which sector the individual is

[25] The programs were supposed to relocate 1280 government jobs to the affected municipalities and, by grants to private companies, create 1000 private job opportunities. Evaluating the effects of these programs Falkenhall (2004) found that only 60 % of the government jobs had been relocated, whereas few private jobs had yet been realized. The grants directed at private companies were only paid out if new hiring occurred. By the end of 2002, Falkenhall (2004) found that the number of new hires only reached 62. The affected municipalities were also given general regional policy aid, and most of it was used to finance different projects, such as pilot studies. Falkenhall concluded that it was unclear if these would bring about permanent employment when the project ended or ran out of funds.

[26] Furthermore, to the extent that the programs were successful for our treated municipalities, our estimations should underestimate the full effect of the closures.

employed in and where the establishment is located.[27] This information makes it possible to identify all individuals employed by the SAF (separately coded for army, marine, air force, management, common operation, and home guard). We define individuals as military employees if they are employed by SAF and these include both employees with a military and civil contract. Information about municipalities is mainly compiled by aggregating individual level information from LOUISE, although the information about political majority comes from surveys made by the Swedish Association of Local Authorities and Regions.

We match our data set with assessments of both cognitive and non-cognitive skills from the military enlistment, which are available for the period 1969-2003 (full coverage from 1970). Most men enlist the year they turn 18 or 19 years. The procedure takes two days and includes tests of mental and physical fitness. All men take a cognitive test battery that consists of four different sub-tests; inductive ability, verbal comprehension, spatial ability, and technical understanding (including questions about chemistry and physics). The non-cognitive skills are assessed through an interview with a certified psychologist. The interviews lasts for about 20 minutes and focus on how the interviewee behaves rather than thinks. The psychological evaluation measure four different dimensions of non-cognitive skills; social maturity (extroversion, friendships, responsibility taking, independence), intensity (self-motivated, intensity and frequency of free-time activities), psychological energy (perseverance, ability to fulfill plans and remain focused), and emotional stability (anxiety inclination, ability to control/handle nervousness, stress).[28]

We use the sum of the four cognitive tests (measured on a 1-9 Stanine scale) as our main measure of cognitive skills. The psychological evaluation is graded on a five-point scale for each dimension, but the psychologist also makes an overall judgment of the "psychological fitness for military service" (on a Stanine scale). The overall judgment is not a direct function of the four dimensions, but in order to account for this information, we calculate the average value of the overall judgment for each combination of points from the four dimensions. We use this as our measure of non-cognitive skills. To ease the interpretation, we standardize each variable by enlistment year, with mean 0 and standard deviation 1 (for the full population). As pointed out by Lindqvist and Vestman (2011), the non-cognitive skills that are regarded important for fulfilling the requirements of the military service (such as independence, willingness to assume responsibility, initiative-taking ability, persistence, good social skills, ability to cope with stress, ability to work in groups,

[27] The information on employment sector builds on five digit industry codes (SNI) that are constructed in accordance with EU standards.

[28] For further details see Mood et al. (2012).

and emotional stability) are likely to be highly valued also in the labor market. Thus, these abilities are likely to help individuals in the job reallocation process.

We define treated individuals as those working within the army or the air force in a municipality where the SAF closed down all activity in the wake of the propositions in 1996, 1999, and 2004, and sample them two years before the propositions.[29] The control group are those working within the army or the air force at a military base that does not close down during the period covered by the dataset. However, note that military bases in the control group are partly affected by the downsizing. Given that military employees could not be dismissed due to redundancy, and were offered to stay within the SAF, we believe that these employees in most cases were able to stay at the military base they were working at.[30] To the extent this was not the case, we expect to underestimate the full effect of the military base closures.

Using the closures that took place in the wake of the propositions, and given the length of our panel data, we can follow the individuals 6 years before and 9 years after the proposition is announced (i.e. year 1). We sample the individuals in year -1, the year before the working group tasked with drafting a proposal is created, in order to avoid selection due to early leavers. We do not restrict the control group to be continuously employed during the post-period. Since test scores only have full coverage from 1970 the sample is restricted to individuals who are younger than 45 years the year of the propositions, and we also drop individuals younger than 25 to increase the probability that they have finished their studies. In line with previous studies on displacement (e.g. Couch and

[29] We are not using the marine since only one municipality (Härnösand) had a complete closure of all military activity and we lack similar control municipalities –Härnösand employed 200 individuals, whereas Karlskrona and Haninge (the marine units that remained during the full period) employed around 2000 each. We also exclude Linköping since their military base transferred from belonging to the army to belonging to the air force during the period. We also exclude Kristianstad since they have not, unlike other municipalities, registered SAF employees belonging to the army and the management as separate groups (all are coded as belonging to the management). According to the proposition in 1999 the Wing in Uppsala would not be closed, but the year after the decision was reversed, and we thereby add Uppsala to the closures following the 1999 proposition.

[30] Figure A2 also show that the separation rate is similar before and after the closures in the control group. A bit more than 70 percent of the control group remained employed by the SAF in the last year, compared to around 35 percent of the treated individuals.

Placzek, 2010) we also restrict them to have tenure, although we only restrict it to two years to avoid dropping too many observations.[31]

4.2 Summary statistics

Table 1 shows summary statistics for the military employees included in our analysis, divided by treatment status, the year we sample them. We also run bivariate regression for several demographic characteristic to test for the difference between the groups. The individuals are very similar with respect to outcome variables and socio-economic and demographic characteristics. If individuals differ regarding observable characteristics there could also be differences concerning unobservable features. The fact that the two groups are very similar mitigates such concerns.

Even though the individuals are very similar, a potential concern would be that they face very different labor market opportunities, given that they live in different municipalities. Hence, in Table 2 we also show the differences between the treated and untreated municipalities. It is evident that the municipalities are very similar, with similar employment and income levels. This tells us that on an aggregate level, municipalities that will eventually become treated are very similar to other municipalities that also host military units, but that will not be treated according to the defense propositions.

[31] Given that disposable income also includes capital income we observe some quite extreme variations, and to exclude the influence of outliers, we drop the top per mille in terms of disposable income.

Table 1. Summary statistics: Military employees

	Control	Sd	Treated	Sd	p-value
Civil	0.29	0.46	0.27	0.44	0.57
Air Force	0.31	0.46	0.41	0.49	0.59
Women	0.0042	0.06	0.0058	0.08	0.64
Age	32.1	5.27	32.1	5.24	0.95
Immigrants	0.021	0.14	0.023	0.15	0.45
Education	4.75	1.67	4.66	1.78	0.65
Married	0.37	0.48	0.36	0.48	0.84
Children	0.48	0.50	0.46	0.50	0.49
Labor income (wage)	3001.2	1073.31	2966.8	1085.72	0.79
Days unemployed	6.29	40.70	7.24	44.12	0.64
Unemployed (%)	0.034	0.18	0.037	0.19	0.75
Disposable income	2010.6	747.76	2048.1	760.55	0.77
Support	86.9	231.97	93.0	247.71	0.52
Non-cognitive skills	0.86	0.82	0.85	0.84	0.84
Cognitive skills	0.61	0.78	0.64	0.79	0.44
Observations	7643	.	2909	.	.

Note: All variables are measured at year -1. The table presents mean values (and their standard deviations) as well as p-values from bivariate regressions with standard errors clustered at municipality. Civil is a dummy for individuals expected to have a civil contract.[a] Immigrant is a dummy for individuals with a registered immigration year, Education level is measured on a 7-point scale (increasing in years of education), Children refers to individuals with children younger than 18, Unemployed (%) is a dummy for individuals registered at the Employment Office and Days unemployed measure the number of days registered. Employed refers to individuals working in November. All income variables are given in 100 SEK ($\approx$ \$ 12) with 2013 year value.[b]

[a]While we do not know exactly what contract individuals had, we can use information about their occupation code (SSYK) as a proxy. The classification of occupation is a four digit code, and we code individuals with SSYK 0110 (which includes officer, military, soldier etc) as military personnel, and everyone else as civil. SSYK is only available from 1995, for individuals without information about SSYK we code those with any form of military education as having a military contract.

[b]Labor income is pre-tax wage, and includes sickness allowance (if paid by employer), allowance for expenses, and severance pay. Disposable income is calculated by Statistics Sweden and individualized from household income. It constitute the net from all types of earnings and taxes. Support refers to employment related income (consisting of unemployment benefits, sickness allowance, early retirement pensions etc.).

Table 2. Summary statistics: Municipality

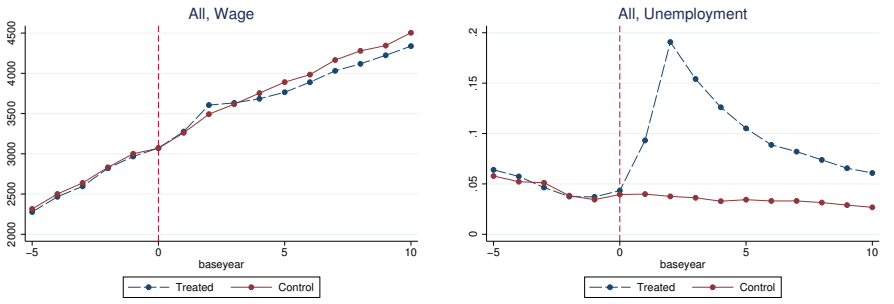
	Control	Sd	Treated	Sd	p-value
Women	0.49	0.01	0.49	0.01	0.21
Age	41.0	2.44	40.9	2.26	0.85
Immigrants	0.10	0.06	0.11	0.06	0.75
Education	3.10	0.43	2.96	0.38	0.26
Married	0.46	0.04	0.45	0.04	0.21
Children	0.39	0.04	0.40	0.03	0.73
Labor income (wage)	1494.6	146.15	1460.8	153.30	0.44
Disposable income	1495.2	202.75	1460.6	191.69	0.56
Support	321.4	51.40	330.8	49.48	0.53
Employed	0.66	0.04	0.66	0.02	0.90
Population	27521.0	20476.62	36531.1	28431.11	0.19
SAF employees	917.0	501.90	700.1	387.43	0.12
Left majority	0.71	0.46	0.64	0.50	0.68
Observations	36	.	17	.	.

Note: All variables are measured at year -1. Left majority is a dummy variable for municipalities were the governing party/coalition only consists of the Left Party, the Social Democrats or the Green Party.

5 Econometric specification

We use a difference-in-differences approach in which we compare the difference in labor market outcomes over time (before and after the defense propositions) between treated and untreated individuals. The samples from the propositions in 1996, 1999 and 2004 are pooled, but defined as separate base-samples (with year -5 to 10). Since we use a difference-in-differences setting, we need to confirm that the two groups develop similarly prior to the treatment. Figure 2 displays how the two variables yearly labor income (left figure) and share unemployed (right figure) develop over time for the control and treatment group separately. It is clear that for both variables, the groups track each other closely before year 0 (when the working group is created). After year 0, we see a sharp increase in the share of unemployed in the treatment group, amounting to an increase by almost 10 percentage points one year after the propositions were announced. The share of unemployed remains higher in the treated group compared to the control group during the whole decade following the propositions. For labor income, we see a brief early increase (probably capturing severance pay) followed by a drop of up to 20 000 SEK in yearly labor income for the remaining years.

Figure 2. Pretrends: full samples



Note: Labor income (2013 year value) given in 100 SEK ≈ $ 12. Unemployment refers to individuals registered at the Employment Office in a given year.

The baseline model (i.e. the model estimated for answering question (i)) is given by:

$$y_{imbt} = \alpha_0 + \alpha_1 X_{im} + \sum_{k \geq -5}^{10} \beta_k D_{imt} + \lambda_{imb} + \kappa_{bt} + \varepsilon_{imbt} \qquad (1)$$

The outcomes of interest, y_{imbt}, represents yearly labor market income, days registered as unemployed, disposable income or labor related allowance (support). We construct a dummy variable that indicates treatment, D_{imt}, for individual i, municipality m, base-sample b (1996, 1999, 2004), and (base)year t (-5 to 10). We interact the treatment variable with all year dummies, and estimate separate treatment effects, β_k, for all years before, during and after treatment. Thus, we can show that the parallel trend assumption is fulfilled as well as examine all dynamics of the effects of the defense propositions. We also include dummies for being in the Air Force and having a civil contract in X_{im}. In order to account for the fact that there might be unobserved differences between treated and untreated employees, we include base-sample specific municipality fixed effects, λ_{imb}.[32] Finally, κ_{bt} are base-sample specific year fixed effects that captures aggregate shocks that affect all municipalities and individuals in the same way. Standard errors (ε_{imbt}) are clustered at the municipality m, to allow for correlation of the error term across different time periods and base-samples.

Equation (1) follows the typical econometric specification in the plant closure literature. To examine whether there are any heterogeneous effects in terms of cognitive and non-cognitive abilities, i.e. to answer question (ii), we will re-estimate equation (1) and run a fully interacted model where all terms are interacted with one of our three different measures of cognitive and non-cognitive skills. We use either a dummy for being above the median in the skill distribution, the standardized measure of cognitive and non-cognitive skill, or the standardized measure transformed to percentiles (to avoid the influence of outliers).

6 Labor market effects of base closures

In this section we present our results showing the labor market effects for the individuals affected by the base closures. We present effects both for the years before the defense propositions were announced (year 1), and for nine subsequent years. We focus on the year-specific estimates of the parameters of main interest, β_k, which will be presented both graphically and in tables. Showing the point estimates for the years preceding the proposition can be seen as a placebo analysis; it checks our model assumption by examining whether the econometric specification picks up any pre-treatment differences between the treated and the control group.

[32] These are based on where the individuals worked the year we sample them, not where they work or live in subsequent years

In section 6.1 we present the mean effects, in section 6.2 we present heterogeneous effects based on cognitive and non-cognitive skills, and in section 6.3 we provide separate analyses for military and civilian personnel employed at the military bases.

6.1 Mean effects

To get the mean effects, we estimate equation (1) on our four main outcomes. The estimates are presented in Figure 3; the effects on taxable labor income in the upper left figure, the effects on days unemployed in the upper right figure, the effects on disposable income in the lower left figure, and the effects on income support in the lower right figure.[33]

First, it can be noted that the pre-reform trends look very reassuring; for all outcomes, the treatment- and control groups have very similar (parallel) pre-reform trends and for none of the outcomes can we reject the null hypothesis that the difference is equal to zero. Hence, there are no indications of selection before the propositions.

There seems to be no immediate negative effect from the military base closures on labor income; in the first year following the defense propositions, i.e. year 2, and there is even a positive and significant effect (at the ten percent significance level). There is however a negative trend for most of the period, with the estimates being significant at (at least) the ten percent significance level for the last four years of the ten-year follow-up period. The point estimates for the last years show that the individuals in the treated group earned 10,000–15,000 SEK less in yearly labor income compared to those in the control group. These effects are economically significant given that the average labor income in the treated group was approximately 297,000 SEK in the year we sampled them. The absence of an immediate negative effect are quite likely explained by the fact that the closures were implemented in a stepwise manner, and the fact that the military personnel who chose to leave their posi-

[33]The point estimates are shown in Table A1 in the Appendix. While we in the figure only present the results for days unemployed, we also present the results for the probability to be unemployed in the table. The results are qualitatively very similar between the two unemployment measures. The standard errors are clustered at the municipality level. Given that we only have 29 municipalities a concern might be that we get biased estimates of the standard errors due to few clusters. Therefore we have also estimated the model using wild bootstrap. As is clear from the estimates in Table A2 in the Appendix the results are very similar. Also, the the point estimates do not change much when municipality fixed effects are excluded (see table A3), but we lose some precision in the last years. Finally, including additional control variables (e.g. age) has almost no impact on the results.

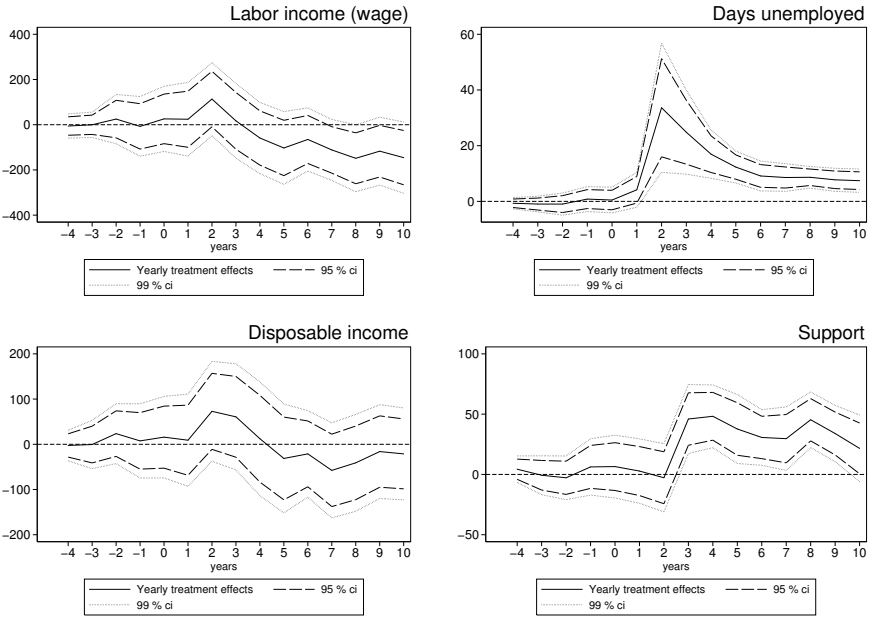
tions voluntarily could get severance payments during six months, which is counted as labor income in the income registers.

Turning to the unemployment variable, there is a pronounced and significant effect from the year following the propositions (year 2) and onward; in year 2, the base closures caused the treated individuals to be unemployed for 33.6 days more during the year than the individuals in the control group, and in the last five years of the ten-year follow-up period the corresponding figure was 7–9 days per year. The results in Table A1 show that the base closures caused the unemployment risk to be 15.7 percentage points higher among the treated individuals than among the individuals in the control group in year 2, and in the last five years 3.5–5.5 percentage points higher. This must be considered as very large effects given that the pre-reform unemployment rate among the treated individuals was very low (approximately 0.04 percent). The results for unemployment hence mirror the results for labor income, at least in the latter part of the period, indicating that at least part of the drop in earnings is due to increased unemployment.[34]

Given the results for labor earnings and unemployment, we would expect a significant drop in disposable income, but the point estimates are small and never statistically different from zero. This result is probably explained by the extended social safety net in Sweden, which dampens the negative effects on labor income and unemployment in terms of disposable income. Support for this story is also provided by the results for labor-related income support (which, among other things, include unemployment and sickness benefits, and pensions from early retirement). The point estimates are significant from year 3 and onward and fairly stable over time, showing that the base closures caused the treated individuals to get around 3,000–4,000 SEK more per year in labor-related benefits. One possible reason for why the effect on income support is delayed is that individuals could only receive unemployment benefits after the six months of severance payments.

[34]To get an impression on where the military personnel ended up, Table A16 presents the top 15 sectors of employment in year 10 for the individuals in the treatment and control group, respectively.

Figure 3. Full samples



Note: Labor income (2013 year value) is given in 100 SEK ≈ $ 12. Unemployment refers to number of days registered at the Employment Office in a given year.

6.2 Heterogenous effects: cognitive and non-cognitive skills

From the mean effects in section 6.1 it is clear that there were some detrimental effects for the personnel affected by the base closures. The question is whether those with higher cognitive and non-cognitive skills fare better in the labor market following a negative labor market shock than those with lower skills. In other words, are there heterogeneous treatment effects in terms of abilities? To examine this, we estimate separate effects for high- and low-skilled individuals.

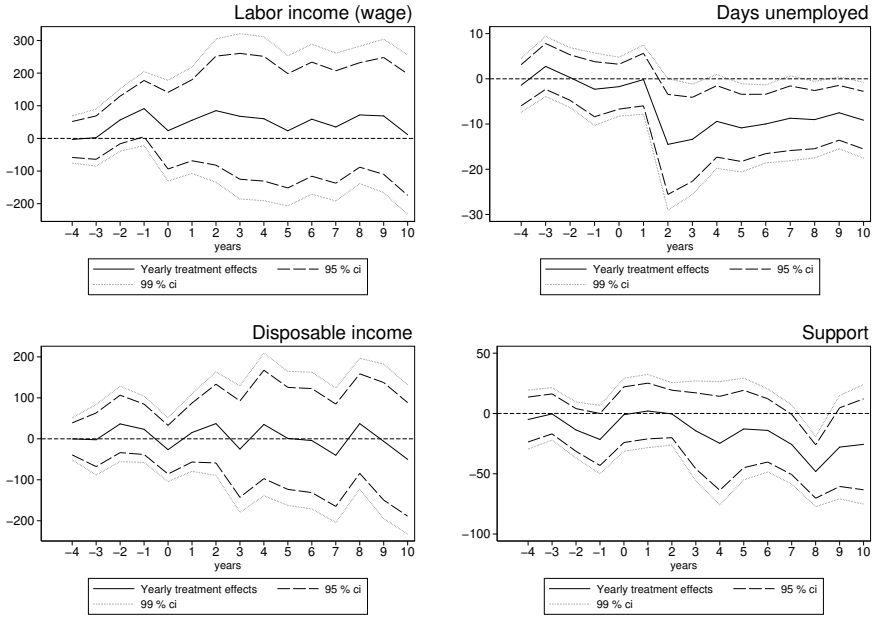
In Figure 4 (5) we present the treatment effect for individuals above the median for non-cognitive (cognitive) skills, compared to individuals below the median.[35] The pattern is very similar for these two skills; there are mainly no significant differences on earnings, disposable income or labor-related benefits, but the high-skilled individuals have fewer days of unemployment in a given year than the low-skilled individuals.

[35]The results are also presented in Table A4 (A7) in the Appendix. We also show the results when using a linear interaction term (Table A5 and A8) and a percentile ranked interaction term (Table A6 and A9).

The effect is most pronounced for those with high non-cognitive skills. In the first year after the propositions were announced (year 2), the treated individuals with high non-cognitive skills were unemployed 15 days less than the treated individuals with low non-cognitive skills. In the last year of the follow-up period, the corresponding figure is still a substantial 9.4 days. This indicates that the average long-run effects of base closures are mainly driven by individuals with low non-cognitive skills. The results for those with high cognitive skills are similar to the effects for those with high non-cognitive skills in the short run, but are not equally important in the long run.

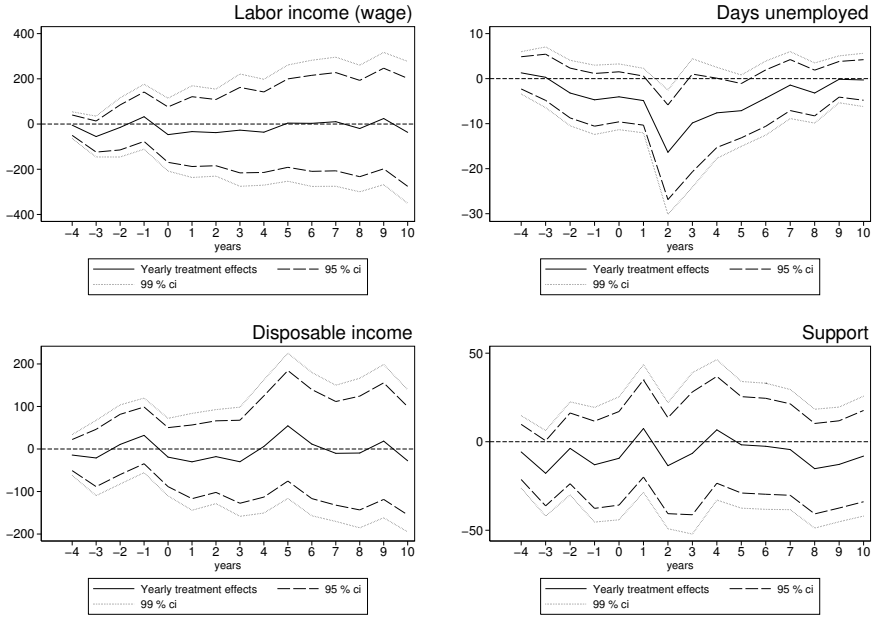
It is hard to identify the exact mechanism behind these results. One plausible explanation is that non-cognitive skills that yield a high measure in the psychological evaluation (such as willingness to assume responsibility, initiative-taking ability, good social skills, ability to work in groups) also affect how active individuals are applying for new jobs in general, and in applying for jobs that require them to adapt and learn new skills. Hence, it can influence their search behavior. Another possibility is that there is a higher demand for individuals with such skills in the labor market. Although we observe smaller differences regarding cognitive skills, the latter explanation is likely to apply to them, since individual's test scores could affect what positions they were offered when they enlisted.

Figure 4. Non-cognitive skills



Note: Labor income (2013 year value) is given in 100 SEK ≈ $ 12. Unemployment refers to number of days registered at the Employment Office in a given year.

Figure 5. Cognitive skills



Note: Labor income (2013 year value) is given in 100 SEK ≈ $ 12. Unemployment refers to number of days registered at the Employment Office in a given year.

6.3 Separating military and civil personnel

Since employees with a military or civil contract were given different options when the military bases closed down, it is of interest to examine if the effects are different for these two groups. Starting by looking at mean effects, estimating equation (1) separately for military and civil employees (and comparing with the results in section 6.1), the results in Tables A10 and A11 show that the results are fairly similar for military and civil personnel. Two of the more striking differences are that, for civilians, there are no significant effects on earnings (while there are significant and negative effects for military personnel), while they at the same time have more days of unemployment, than the military personnel. These results suggests that the civil personnel that find new employment get better paid jobs than earlier, while the military personnel gets new jobs with lower pay than earlier.

Turning to heterogeneous treatment effects, it is clear from Tables A12–A15, that the results are fairly similar (but less precise) for both groups compared the heterogeneous treatment effects found in section 6.2. The significant effects found for both groups of personnel are for unemployment (mainly days of unemployment) and these are most pronounced for those with high non-cognitive skills.

In answering the question of individual labor market effects of base closures, it is also of interest to investigate who stayed in the military sector over the years. To examine this question, we estimate the probability of being employed by SAF in year 10, 9 years after the propositions. The results, given in Table 3, show that the treated military and civilian personnel have left the military to a larger extent (42–46 percent) than the untreated personnel.[36]

Among the control group, individuals with higher non-cognitive skills and civilians with higher cognitive skills were more likely to leave SAF. Among the treated individuals, on the other hand, those with higher skills have to a larger extent stayed within the military (see the coefficients for the interaction between the treatment-dummy and the skill measures in Table 3). The estimates in columns (3) and (6) show that these results hold also when we control for all skill-variables at the same time.[37] Whether those with high cognitive and non-cognitive skills decided to stay in the military to a larger extent or had better options to find similar employment at other bases (or a combination of the two) is

[36] Overall, 71 percent in the control group and 35 percent in the treatment group were employed by the Swedish Armed Forces in 2010 (see Table A16).

[37] Note that cognitive and non-cognitive skills are clearly closely related (see Graph A3).

hard to say, but this result can explain some of the positive effects found on unemployment for those with high skills.[38]

Table 3. Probability of staying within SAF

	Military		Civil			
	(1)	(2)	(3)	(4)	(5)	(6)
Air Force	0.093***	0.092***	0.091***	0.136***	0.137***	0.135***
	(0.030)	(0.029)	(0.029)	(0.039)	(0.039)	(0.039)
Treated	-0.418***	-0.426***	-0.462***	-0.435***	-0.447***	-0.461***
	(0.038)	(0.040)	(0.048)	(0.047)	(0.044)	(0.046)
Cognitive skills	0.001		0.003	-0.076***		-0.053***
	(0.011)		(0.010)	(0.013)		(0.013)
Cognitive*Treated	0.063**		0.058**	0.120***		0.084***
	(0.027)		(0.026)	(0.026)		(0.026)
Non-cognitive skills		-0.029***	-0.029***		-0.078***	-0.055***
		(0.010)	(0.010)		(0.014)	(0.016)
Noncognitive*Treated		0.055**	0.048**		0.121***	0.085***
		(0.021)	(0.018)		(0.022)	(0.022)
Observations	7421	7402	7402	2976	2955	2955

Note: Probability of having SAF as the main employer in year 10.

7 Conclusions

The main purpose of this paper was to examine whether the effects from a negative labor market shock affected individuals with different cognitive and non-cognitive skills differently. To investigate this, we, first, examined the individual labor market effects for those affected by a job loss without conditioning on skills. Then we evaluated to what extent the average, unconditional, effects can be understood as a function of the individual's cognitive and non-cognitive skills. Since selection into unemployment is a function of skills, we need to deal with the endogeneity of job loss. We solve the selection problem by using the exogenous labor market shock provided by the substantial military base closures in Sweden following the end of the Cold War. Cognitive and non-cognitive skills are measured by information from the enlistment tests.

For average effects, we find that labor earnings decrease and unemployment increase. We do not, however, find any significant effects on disposable income. This latter result is probably explained by the extended

[38]Individuals employed at closing bases were favored if they application process if they applied to vacancies at other bases. Also, individuals with a military contract were offered a new job within the SAF if they could not find one on their own.

social safety net in Sweden (including e.g., unemployment benefits, sickness benefits and pensions from early retirement), which dampens the negative effects on labor income and unemployment in terms of disposable income. Our significant and positive results for labor-related income benefits support such a story.

For heterogeneous treatment effects, we find that there are mainly no significant effects on earnings, disposable income or labor-related benefits, but the high-skilled individuals have fewer days of unemployment in a given year than the low-skilled individuals. This effect is most pronounced for those with high non-cognitive skills. In fact, our results suggest that the persistent effect of job displacement is almost entirely driven by individuals with low non-cognitive skills.

How shall one understand the heterogeneous results? Even though specific mechanisms have not been examined in detail in this paper, a plausible explanation is the mechanism pointed out by Lindqvist and Vestman (2011); the non-cognitive skills that yield a high measure in the psychological evaluation when conducting the enlistment tests (willingness to assume responsibility, initiative-taking ability, good social skills, ability to cope with stress, and ability to work in groups) are likely to ease the job reallocation process. Both in terms of affecting individuals search behavior, but also given the fact that these skills are likely to be favored by new employers.

The earlier literature on base closures, which have focused on local aggregate effects, have found small or insignificant effects on outcomes such as local growth, local unemployment rate, or local migration (see e.g., Hooker and Knetter, 2001; Andersson et al., 2007; Paloyo et al., 2010). Our results show that the individual labor market consequences for some of the affected personnel have been more detrimental than indicated by the aggregate estimates.

To increase our understanding of different mechanisms behind some of the results we have obtained, it might be important to examine the effects on other outcomes, such as the household's migration decisions, the spouse's labor earnings, and marital dissolution. These are the next steps on our research agenda.

References

Abowd, J. M., McKinney, K. L., and Vilhuber, L. (2009). The link between human capital, mass layoffs, and firm deaths. In *Producer Dynamics: New Evidence from Micro data*, pages 447–472. University of Chicago Press.

Andersson, L., Lundberg, J., and Sjostrom, M. (2007). Regional effects of military base closures: the case of sweden. *Defence and Peace Economics*, 18(1):97–97.

Arulampalam, W., Gregg, P., and Gregory, M. (2001). Unemployment scarring. *Economic Journal*, 111(475):577–84.

Black, S. E., Devereux, P. J., and Salvanes, K. G. (2015). Losing heart? the effect of job displacement on health. *ILR Review*, 68(4):833–861.

Blomsterberg, M. and Kadefors, R. (2009). Utvärdering av karriärväxlingsprojektet slutrapport. *Department of Work Science, Gothenburg University*.

Böheim, R. and Taylor, M. (2002). The search for success: Do the unemployed find stable employment? *Labour Economics*, 19(6):717–35.

Bowles, S., Gintis, H., and Osborne, M. (2001). The determinants of earnings: A behavioral approach. *Journal of Economic Literature*, 39(4):1137–1176.

Brand, J. E. (2015). The far-reaching impact of job loss and unemployment. *Annual Review of Sociology*, 41:359–375.

Carrington, W. (1993). Wage losses for displaced workers: Is it really the firm that matters? *Journal of Human Resources*, 28(3):435–62.

Couch, K. and Placzek, D. (2010). Earnings losses of displaced workers revisied. *American Economic Review*, 100(1):572–589.

Dahlberg, M., Martén, L., and Persson, A. (2013). After the cold war: The effect of military base closures on individual labor market outcomes. Published in Persson, Anna, *Activation Programs, Benefit Take-up, and Labor Market Attachment*, Economic Studies 138, PhD Thesis, Uppsala University.

DN (Nov 1, 1999) (1999). Nedläggningsbeslut står fast. Dagens Nyheter.

Eliason, M. (2011a). Assistant and auxiliary nurses in crisis times. *Institute for Labour Market Policy Evaluation*, Working Paper 2011:1.

Eliason, M. (2011b). Income after job loss: The role of the family and the welfare state. *Applied Economics*, 43:603–618.

Eliason, M. and Storrie, D. (2006). Lasting or latent scars? swedish evidence on the long-term effects of job displacement. *Journal of Labor Economics*, 24(4):831–856.

Eriksson, M. and Hallsten, L. (2003). Tre år efter avvecklingen: arbete, hälsa och hälsoutveckling för uppsagd civilanställd personal inom försvaret. *Arbete och Hälsa, Arbetslivsinstitutet*, 2003(09).

Falkenhall, B. (2004). *Omställningsarbete efter försvarsnedläggningar*. Institutet för tillväxtpolitiska studier.

Farber, H. (2003). Job loss in the united states, 1981-2001. Working Paper 9707, National Bureau of Economic Research.

Hamermesh, D. (1987). The cost of worker displacement. *Quarterly Journal of Economics*, 102(1):51–75.

Heckman, J. J. (2008). The case for investing in disadvantaged young children. Published in *Big ideas for children: Investing in our nation's future* (pp. 49-58). Washington, DC: First Focus.

Heckman, J. J., Stixrud, J., and Urzua, S. (2006). The Effects of Cognitive and Noncognitive Abilities on Labor Market Outcomes and Social Behavior. *Journal of Labor Economics*, 24(3):411–482.

Hedin, O. (2011). Försvarets förutsättningar - en ESO-rapport om erfarenheter från 20 år av försvarsreformer. *Expertgruppen för Studier i Offentlig Ekonomi*, 2011(2).

Herrnstein, R. J. and Murray, C. A. (1994). *The bell curve: Intelligence and class structure in American life*. Free Press, New York.

Hijzen, A., Upward, R., and Wright, P. (2010). The income losses of displaced workers. *Journal of Human Resources*, 45(1):243–269.

Hines, J., Hoynes, H., and Krueger, A. (2001). Another look at whether a rising tide lifts all boats. In *The Roaring Nineties: Can Full Employment Be Sustained?*, page . Russell Sage Foundation, New York.

Hooker, M. and Knetter, M. (2001). Measuring the economic effects of military base closures. *Economic Inquiry*, 39(4):583–598.

Huttunen, K., Møen, J., and Salvanes, K. G. (2011). How destructive is creative destruction? effects of job loss on job mobility, withdrawal and income. *Journal of the European Economic Association*, 9(5):840–870.

Jacobsen, L., LaLonde, R., and Sullivan, D. (1993). Earning losses of displaced workers. *American Economic Review*, 83(4):685–709.

Jakobsson, M. (2010). Militär avveckling. problem eller möjlighet? *licentiate thesis, Department of Social and Economic Geography, Uppsala University*.

Lindqvist, E. and Vestman, R. (2011). The labor market returns to cognitive and noncognitive ability: Evidence from the swedish enlistment. *American Economic Journal: Applied Economics*, 3(1):101–28.

Loughran, D. S. and Klerman, J. A. (2012). The effect of activation on the post-activation civilian earnings of reservists. *Labor Economics*, 19(1):18–26.

Mood, C., Jonsson, J. O., and Bihagen, E. (2012). Socioeconomic persistence across generations: The role of cognitive and non-cognitive processes. In Ermisch, J., Jäntti, M., and Smeeding, T. M., editors, *From Parents to Children: The Intergenerational Transmission of Advantage*, pages 53–83. New York: Russell Sage Foundation.

Neal, D. (1998). The link between ability and specialization: An explanation for observed correlations between wages and mobility rates. *The Journal of Human Resources*, 33(1):173–200.

Öckert, B. (2011). Comment on christopher pissarides: Regular education as a tool of counter-cyclical employment policy. *Nordic Economic Policy Review*, pages 233–239.

Paloyo, A., Vance, C., and Vorell, M. (2010). The regional economic effects of military base realignments and closures in germany. *Defence and Peace Economics*, 21(5-6):567–579.

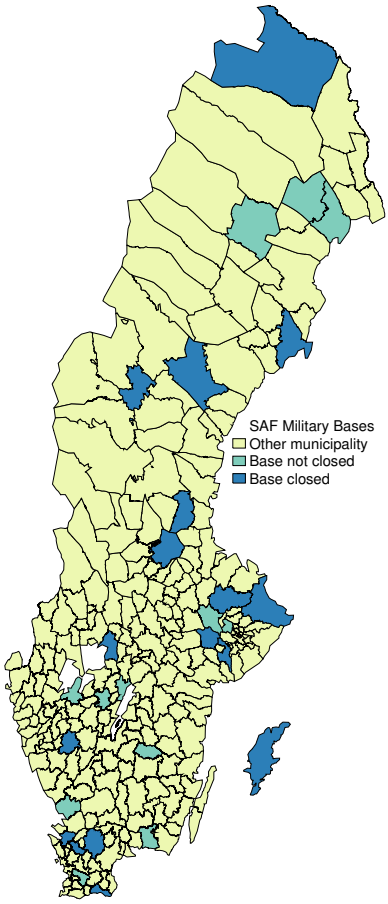
Prop. (1991). *Totalförsvarets utveckling till och med budgetåret 1996/97 samt anslag för budgetåret 1992/93*. Number 1991/92:102.

Försvarsdepartementet, Stockholm.
Prop. (1995). *Totalförsvar i förnyelse*. Number 1995/96:12. Försvarsdepartementet, Stockholm.
Prop. (1999). *Det nya försvaret*. Number 1999/00:30. Försvarsdepartementet, Stockholm.
Prop. (2004). *Vårt framtida försvar*. Number 2004/05:5. Försvarsdepartementet, Stockholm.
Prop. (2008). *Ett användbart försvar*. Number 2008/09:140. Försvarsdepartementet, Stockholm.
Schmieder, J., von Wachter, T., and Bender, S. (2010). The long-term impact of job displacement in Germany during the 1982 recession on earnings, income and employment. *IAB Discussion Paper 2010:1*.
Schwerdt, G. (2011). Labor turnover before plant closure: Leaving the sinking ship vs. captain throwing ballast overboard. *Labour Economics*, 18:93–101.
Seim, D. (2013). Job displacement and labor market outcomes by skill level. Published in Seim, David, *Essays on Public, Political and Labor Economics*, PhD Thesis, Stockholm University.
Stern, J. (1972). Consequences of plant closure. *Journal of Human Resources*, 7(1):3–25.
Sullivan, D. and von Wachter, T. (2009). Job displacement and mortality: An analysis using administrative data. *The Quarterly Journal of Economics*, 124(3):1265–1306.
SvD (Nov 11, 1999) (1999). Var tredje militär ska bort. Svenska Dagbladet.
TT (June 27, 1999) (1999). Dalkarlar på marsch mot Stockholm. Tidningarnas Telegrambyrå.
TT (Sept 23, 1999) (1999). Försvarsnedläggningarna spär på utflyttningen. Tidningarnas Telegrambyrå.
TT (Sept 30, 1999) (1999). 5000 demonstrerade mot nedläggning av regemente i Kiruna. Tidningarnas Telegrambyrå.
von Wachter, T. (2010). Summary of the literature on job displacement in the US and EU: What we know and what we would like to know. In *Wage Structures, Employment Adjustments and Globalization: Evidence from Linked and Firm-level Panel Data*, page . Applied Econometrics Association Series, Palgrave Macmillan.
Warf, B. (1997). The geopolitics/geoeconomics of military base closures in the USA. *Political Geography*, 16(7):541–563.

Appendices

A.1 Figures

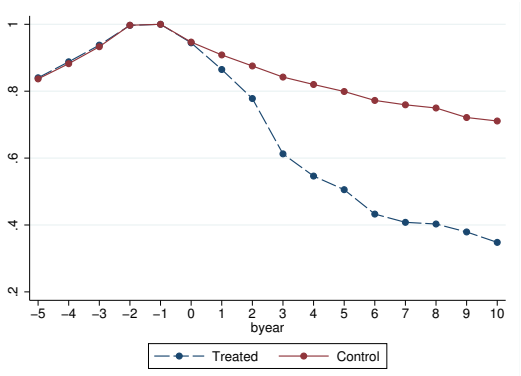
Figure A1. Municipalities in treatment and control groups



Treated:	**Control:**
Ystad	Eksjö
Södertälje	Upplands-Bro
Umeå	Boden
Borås	Halmstad
Klippan	Skövde
Söderhamn	Lund
Uppsala*	Enköping
Ängelholm*	Karlsborg
Hässleholm*	Arvidsjaur
Falun*	Luleå
Sollefteå*	Ronneby
Norrtälje*	Lidköping
Kiruna*	
Östersund**	
Kristinehamn**	
Gotland**	
Strängnäs**	

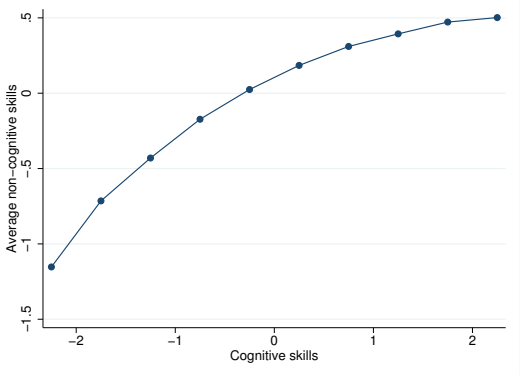
Note: Only displays bases with army and air force. */** Units at the military base in the municipality were closed due to the defense proposition in 1999/2004

Figure A2. Employed by the SAF



Note: Yearly share employed by the Swedish Armed Forces (SAF).

Figure A3. Cognitive and Non-cognitive skills, population



Note: Average non-cognitive skill by cognitive skill. All variables are standardized by enlistment year, with mean 0 and sd 1.

A.2 Tables

Table A1. All

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4	-5.815	0.002	-0.674	4.345	-2.565
	(20.797)	(0.004)	(0.780)	(4.279)	(13.139)
-3	-0.243	-0.006	-0.959	-0.673	-0.503
	(21.782)	(0.006)	(1.097)	(6.261)	(20.729)
-2	25.056	-0.002	-0.973	-2.786	23.607
	(42.307)	(0.008)	(1.542)	(7.025)	(25.702)
-1	-7.333	0.002	0.822	6.248	7.545
	(51.233)	(0.008)	(1.734)	(9.080)	(31.859)
0	25.872	0.003	0.483	6.516	15.842
	(55.938)	(0.010)	(1.787)	(10.107)	(34.994)
1	24.287	0.054*	4.156*	2.811	9.053
	(63.338)	(0.027)	(2.436)	(10.362)	(39.584)
2	113.500*	0.157***	33.628***	-2.732	72.909
	(62.638)	(0.035)	(8.997)	(10.989)	(42.874)
3	17.125	0.122***	24.829***	45.972***	60.721
	(64.066)	(0.022)	(5.849)	(11.101)	(45.563)
4	-58.690	0.095***	16.951***	48.233***	12.346
	(61.196)	(0.015)	(3.357)	(10.100)	(48.931)
5	-102.841	0.071***	12.317***	37.693***	-31.312
	(62.472)	(0.011)	(2.226)	(11.112)	(46.702)
6	-65.183	0.055***	9.125***	30.671***	-21.157
	(54.167)	(0.009)	(2.080)	(8.960)	(37.135)
7	-111.554**	0.048***	8.576***	29.662***	-57.685
	(52.397)	(0.009)	(1.928)	(10.221)	(40.832)
8	-148.505**	0.042***	8.651***	45.349***	-40.983
	(57.459)	(0.009)	(1.508)	(8.938)	(41.559)
9	-116.973*	0.037***	7.747***	33.957***	-16.062
	(58.277)	(0.008)	(1.607)	(9.064)	(40.267)
10	-145.622**	0.035***	7.430***	21.598*	-21.198
	(61.289)	(0.007)	(1.621)	(10.737)	(39.444)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract.

Table A2. All (wild bootstrap)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4	-5.815	0.002	-0.674	4.345	-2.565
	(0.804)	(0.720)	(0.396)	(0.354)	(0.826)
-3	-0.243	-0.006	-0.959	-0.673	-0.503
	(1.000)	(0.374)	(0.378)	(0.898)	(1.000)
-2	25.056	-0.002	-0.973	-2.786	23.607
	(0.538)	(0.842)	(0.580)	(0.730)	(0.398)
-1	-7.333	0.002	0.822	6.248	7.545
	(0.910)	(0.818)	(0.620)	(0.538)	(0.830)
0	25.872	0.003	0.483	6.516	15.842
	(0.700)	(0.750)	(0.794)	(0.556)	(0.696)
1	24.287	0.054*	4.156	2.811	9.053
	(0.734)	(0.084)	(0.140)	(0.798)	(0.842)
2	113.500	0.157***	33.628***	-2.732	72.909
	(0.134)	(0.000)	(0.000)	(0.792)	(0.152)
3	17.125	0.122***	24.829***	45.972***	60.721
	(0.774)	(0.000)	(0.000)	(0.002)	(0.280)
4	-58.690	0.095***	16.951***	48.233***	12.346
	(0.348)	(0.000)	(0.002)	(0.000)	(0.822)
5	-102.841	0.071***	12.317***	37.693***	-31.312
	(0.122)	(0.000)	(0.000)	(0.002)	(0.524)
6	-65.183	0.055***	9.125***	30.671***	-21.157
	(0.246)	(0.000)	(0.004)	(0.000)	(0.604)
7	-111.554*	0.048***	8.576***	29.662***	-57.685
	(0.050)	(0.000)	(0.000)	(0.008)	(0.198)
8	-148.505**	0.042***	8.651***	45.349***	-40.983
	(0.032)	(0.000)	(0.000)	(0.000)	(0.370)
9	-116.973*	0.037***	7.747***	33.957***	-16.062
	(0.070)	(0.000)	(0.000)	(0.000)	(0.706)
10	-145.622**	0.035***	7.430***	21.598*	-21.198
	(0.030)	(0.000)	(0.000)	(0.076)	(0.650)
Observations	168001	168001	168001	168001	168001

Note: Bootstrapped p-value in parenthesis (1000 repetitions). Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract

Table A3. All (no municipality FE)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4	9.902	-0.005	-1.989	4.647	5.709
	(32.873)	(0.013)	(2.344)	(6.492)	(44.379)
-3	15.381	-0.013	-2.273	-0.366	7.740
	(30.511)	(0.015)	(2.638)	(8.755)	(42.760)
-2	40.701	-0.009	-2.289	-2.490	31.891
	(47.940)	(0.017)	(3.012)	(9.860)	(33.466)
-1	8.309	-0.005	-0.494	6.542	15.827
	(55.946)	(0.015)	(2.868)	(11.909)	(36.568)
0	41.526	-0.004	-0.834	6.807	24.122
	(59.944)	(0.017)	(2.994)	(13.526)	(37.125)
1	39.929	0.047	2.840	3.105	17.334
	(66.645)	(0.035)	(3.957)	(13.547)	(41.395)
2	129.198*	0.150***	32.314***	-2.452	81.177*
	(64.013)	(0.041)	(9.923)	(14.614)	(42.487)
3	32.570	0.115***	23.522***	46.257***	69.055
	(65.247)	(0.026)	(6.336)	(13.802)	(45.476)
4	-43.549	0.088***	15.646***	48.518***	20.517
	(69.606)	(0.021)	(4.377)	(11.466)	(65.457)
5	-87.911	0.064***	11.011***	37.978***	-23.215
	(70.020)	(0.019)	(3.432)	(11.487)	(60.535)
6	-49.996	0.048***	7.822**	30.961***	-13.067
	(60.938)	(0.017)	(2.959)	(9.818)	(46.177)
7	-96.218	0.041**	7.257**	29.928**	-49.474
	(57.174)	(0.016)	(2.932)	(11.905)	(55.431)
8	-132.995**	0.035*	7.325**	45.586***	-32.666
	(60.339)	(0.018)	(2.924)	(10.860)	(56.497)
9	-101.436	0.030*	6.418*	34.238***	-7.881
	(63.422)	(0.017)	(3.158)	(11.765)	(59.873)
10	-130.090*	0.028	6.105*	21.894	-13.063
	(68.011)	(0.018)	(3.436)	(13.775)	(50.368)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract.

Table A4. All: non-cognitive (median)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Non-cog	-3.238	0.012	-1.417	-4.997	-0.192
	(28.089)	(0.009)	(2.323)	(9.495)	(19.943)
-3*Non-cog	2.411	0.021**	2.751	-0.364	-2.293
	(33.890)	(0.010)	(2.584)	(8.452)	(33.381)
-2*Non-cog	56.480	0.008	0.282	-13.670	36.430
	(37.192)	(0.010)	(2.569)	(9.036)	(35.766)
-1*Non-cog	91.307**	0.006	-2.297	-21.571*	23.277
	(43.960)	(0.013)	(3.107)	(11.000)	(31.372)
0*Non-cog	23.656	0.003	-1.736	-0.989	-26.487
	(59.889)	(0.010)	(2.531)	(11.774)	(30.292)
1*Non-cog	55.663	0.005	-0.167	2.049	15.345
	(63.292)	(0.015)	(2.964)	(11.788)	(36.729)
2*Non-cog	85.049	-0.033	-14.500**	-0.375	37.179
	(85.195)	(0.021)	(5.642)	(10.042)	(49.032)
3*Non-cog	67.680	-0.038	-13.379***	-14.430	-25.129
	(98.349)	(0.024)	(4.741)	(16.084)	(59.970)
4*Non-cog	60.272	-0.042**	-9.410**	-24.729	35.115
	(97.469)	(0.020)	(4.035)	(19.870)	(67.503)
5*Non-cog	23.360	-0.028	-10.851***	-12.838	1.042
	(89.286)	(0.018)	(3.789)	(16.370)	(63.502)
6*Non-cog	58.964	-0.027*	-9.978***	-14.032	-4.249
	(89.161)	(0.015)	(3.348)	(13.387)	(64.769)
7*Non-cog	35.028	-0.022	-8.715**	-25.728*	-40.080
	(87.848)	(0.013)	(3.647)	(12.726)	(63.731)
8*Non-cog	71.943	-0.024*	-9.020**	-48.130***	37.128
	(81.687)	(0.013)	(3.282)	(11.297)	(61.952)
9*Non-cog	68.865	-0.025*	-7.511**	-27.905	-6.146
	(91.308)	(0.013)	(3.089)	(16.644)	(73.198)
10*Non-cog	11.488	-0.035**	-9.149***	-25.584	-50.268
	(94.910)	(0.014)	(3.252)	(19.237)	(70.707)
Observations	167366	167366	167366	167366	167366

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract.

Table A5. All: non-cognitive (linear)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Non-cog	-11.605	-0.002	-2.977*	4.148	-1.022
	(17.301)	(0.006)	(1.682)	(7.874)	(14.251)
-3*Non-cog	-2.655	0.008	-0.251	5.207	-7.224
	(18.753)	(0.008)	(1.951)	(6.560)	(18.247)
-2*Non-cog	1.147	0.000	-1.868	-0.882	15.731
	(24.486)	(0.009)	(2.378)	(7.468)	(21.505)
-1*Non-cog	18.845	-0.001	-3.273	-10.361	12.697
	(30.005)	(0.010)	(2.615)	(8.478)	(20.981)
0*Non-cog	3.756	-0.004	-2.724	-0.873	-7.962
	(36.291)	(0.008)	(2.100)	(8.754)	(18.019)
1*Non-cog	6.655	-0.008	-2.000	6.649	5.439
	(40.474)	(0.011)	(2.148)	(9.105)	(21.462)
2*Non-cog	7.152	-0.038**	-13.302***	4.826	17.954
	(51.179)	(0.017)	(4.570)	(8.974)	(27.773)
3*Non-cog	4.209	-0.030*	-10.245***	-8.218	-17.680
	(56.768)	(0.016)	(3.627)	(12.150)	(29.776)
4*Non-cog	-18.572	-0.030*	-6.993**	-14.478	-10.974
	(52.021)	(0.015)	(2.879)	(13.693)	(34.406)
5*Non-cog	-45.230	-0.021	-6.826**	0.118	-8.470
	(48.410)	(0.015)	(2.981)	(11.068)	(39.062)
6*Non-cog	-23.236	-0.019	-6.204**	-5.706	-14.918
	(47.563)	(0.013)	(3.027)	(12.508)	(31.358)
7*Non-cog	-28.711	-0.013	-6.129**	-17.686*	-29.186
	(52.427)	(0.011)	(2.583)	(8.802)	(33.617)
8*Non-cog	-6.864	-0.013	-5.567**	-23.874***	21.612
	(45.448)	(0.010)	(2.341)	(8.468)	(40.875)
9*Non-cog	7.462	-0.015	-5.401*	-18.669	12.681
	(50.030)	(0.009)	(2.716)	(11.164)	(42.274)
10*Non-cog	-36.999	-0.024**	-6.461***	-9.737	-42.219
	(59.661)	(0.009)	(2.283)	(12.682)	(44.093)
Observations	167366	167366	167366	167366	167366

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract.

Table A6. All: non-cognitive (percentile)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Non-cog	-0.476	0.000	-0.057	0.025	-0.148
	(0.540)	(0.000)	(0.041)	(0.204)	(0.411)
-3*Non-cog	-0.324	0.000	0.024	0.150	-0.399
	(0.629)	(0.000)	(0.043)	(0.185)	(0.591)
-2*Non-cog	0.089	0.000	-0.025	-0.104	0.382
	(0.759)	(0.000)	(0.054)	(0.186)	(0.761)
-1*Non-cog	0.755	0.000	-0.076	-0.377*	0.158
	(0.890)	(0.000)	(0.064)	(0.221)	(0.679)
0*Non-cog	0.111	-0.000	-0.061	-0.035	-0.529
	(1.119)	(0.000)	(0.049)	(0.237)	(0.551)
1*Non-cog	0.413	-0.000	-0.039	0.167	-0.020
	(1.262)	(0.000)	(0.049)	(0.232)	(0.667)
2*Non-cog	0.332	-0.001**	-0.358***	0.105	0.338
	(1.645)	(0.000)	(0.129)	(0.215)	(0.882)
3*Non-cog	0.378	-0.001*	-0.277***	-0.247	-0.730
	(1.847)	(0.000)	(0.091)	(0.308)	(0.961)
4*Non-cog	-0.091	-0.001**	-0.192**	-0.496	-0.342
	(1.718)	(0.000)	(0.075)	(0.340)	(1.059)
5*Non-cog	-0.772	-0.001	-0.194**	-0.110	-0.295
	(1.562)	(0.000)	(0.074)	(0.284)	(1.169)
6*Non-cog	-0.075	-0.001*	-0.183**	-0.205	-0.509
	(1.582)	(0.000)	(0.069)	(0.293)	(1.013)
7*Non-cog	-0.382	-0.000	-0.171***	-0.504**	-1.011
	(1.725)	(0.000)	(0.061)	(0.224)	(1.069)
8*Non-cog	0.244	-0.000*	-0.166***	-0.715***	0.332
	(1.504)	(0.000)	(0.054)	(0.223)	(1.241)
9*Non-cog	0.629	-0.000*	-0.140**	-0.509	0.324
	(1.695)	(0.000)	(0.061)	(0.315)	(1.401)
10*Non-cog	-0.619	-0.001***	-0.168***	-0.293	-1.249
	(1.810)	(0.000)	(0.056)	(0.353)	(1.281)
Observations	167366	167366	167366	167366	167366

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract.

Table A7. All: cognitive (median)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Cog	-4.998	0.016	1.288	-5.762	-14.244
	(22.970)	(0.009)	(1.826)	(7.947)	(18.672)
-3*Cog	-55.291	0.011	0.298	-17.842*	-21.073
	(35.085)	(0.012)	(2.617)	(9.380)	(34.385)
-2*Cog	-14.986	-0.004	-3.194	-3.757	10.703
	(50.794)	(0.012)	(2.822)	(10.194)	(36.137)
-1*Cog	32.381	-0.011	-4.703	-13.012	32.065
	(55.806)	(0.013)	(2.987)	(12.567)	(34.031)
0*Cog	-46.817	-0.006	-4.034	-9.361	-19.068
	(62.526)	(0.013)	(2.835)	(13.480)	(35.396)
1*Cog	-33.434	-0.030*	-4.876*	7.466	-30.139
	(78.766)	(0.017)	(2.782)	(14.000)	(44.215)
2*Cog	-37.908	-0.044*	-16.353***	-13.530	-17.901
	(74.719)	(0.022)	(5.364)	(13.823)	(42.924)
3*Cog	-26.981	-0.040	-9.845*	-6.565	-29.944
	(96.432)	(0.027)	(5.539)	(17.694)	(49.807)
4*Cog	-36.075	-0.025	-7.595*	6.715	6.009
	(90.856)	(0.020)	(3.925)	(15.391)	(60.754)
5*Cog	4.178	-0.020	-7.118**	-1.751	54.545
	(99.798)	(0.013)	(3.077)	(13.886)	(66.296)
6*Cog	3.171	-0.013	-4.318	-2.576	11.588
	(108.364)	(0.011)	(3.194)	(13.826)	(65.405)
7*Cog	10.374	-0.003	-1.431	-4.421	-10.153
	(110.848)	(0.012)	(2.888)	(13.179)	(62.168)
8*Cog	-19.784	0.003	-3.190	-15.225	-9.469
	(108.673)	(0.012)	(2.592)	(13.029)	(68.263)
9*Cog	24.473	0.004	-0.143	-12.812	18.551
	(113.517)	(0.012)	(2.021)	(12.572)	(69.960)
10*Cog	-36.771	0.012	-0.292	-8.118	-27.732
	(121.835)	(0.011)	(2.300)	(13.150)	(64.895)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract.

Table A8. All: cognitive (linear)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Cog	4.346	0.010	-0.694	-3.066	-6.209
	(12.474)	(0.008)	(1.730)	(8.581)	(8.833)
-3*Cog	-2.443	0.007	-0.860	-8.854	-14.835
	(20.421)	(0.009)	(2.237)	(7.973)	(15.581)
-2*Cog	20.768	-0.005	-3.444	-1.950	5.976
	(33.938)	(0.011)	(2.784)	(8.569)	(19.003)
-1*Cog	60.316	-0.010	-4.498	-11.752	34.677
	(37.088)	(0.012)	(3.013)	(10.017)	(22.842)
0*Cog	24.824	-0.005	-3.065	-2.549	5.219
	(40.838)	(0.011)	(2.540)	(11.944)	(24.396)
1*Cog	28.492	-0.020*	-4.244	7.428	1.659
	(49.449)	(0.012)	(2.698)	(12.194)	(27.972)
2*Cog	30.357	-0.040**	-13.455***	-4.636	12.908
	(44.405)	(0.015)	(3.803)	(12.910)	(27.613)
3*Cog	53.607	-0.048***	-13.025***	-21.768	0.299
	(56.848)	(0.017)	(4.199)	(15.939)	(32.569)
4*Cog	34.729	-0.042**	-9.367***	-15.098	2.480
	(51.549)	(0.015)	(2.714)	(13.552)	(35.079)
5*Cog	62.631	-0.030**	-8.691***	-11.737	44.294
	(51.364)	(0.012)	(2.562)	(10.144)	(38.024)
6*Cog	38.511	-0.020*	-5.112*	-6.845	10.711
	(60.673)	(0.010)	(2.497)	(9.673)	(38.888)
7*Cog	49.524	-0.008	-3.029	-13.350	8.694
	(61.782)	(0.010)	(2.517)	(11.346)	(37.994)
8*Cog	71.228	-0.004	-3.396	-19.439*	46.474
	(58.354)	(0.011)	(2.679)	(10.196)	(43.294)
9*Cog	71.177	-0.001	-1.494	-13.424	27.228
	(67.786)	(0.011)	(2.026)	(11.165)	(48.184)
10*Cog	34.422	0.003	-1.334	-18.735*	-7.733
	(70.330)	(0.010)	(2.129)	(10.961)	(44.574)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract.

Table A9. All: cognitive (percentile)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Cog	0.126	0.000	-0.015	-0.222	-0.162
	(0.364)	(0.000)	(0.040)	(0.171)	(0.271)
-3*Cog	-0.272	0.000	-0.014	-0.380**	-0.489
	(0.573)	(0.000)	(0.053)	(0.171)	(0.430)
-2*Cog	0.457	-0.000	-0.084	-0.169	0.275
	(0.874)	(0.000)	(0.066)	(0.174)	(0.503)
-1*Cog	1.540	-0.000	-0.117	-0.439**	0.944
	(0.946)	(0.000)	(0.072)	(0.214)	(0.589)
0*Cog	0.518	-0.000	-0.081	-0.183	0.160
	(1.070)	(0.000)	(0.063)	(0.267)	(0.635)
1*Cog	0.734	-0.001*	-0.103	0.069	-0.005
	(1.323)	(0.000)	(0.064)	(0.265)	(0.757)
2*Cog	0.620	-0.001**	-0.345***	-0.316	0.229
	(1.253)	(0.000)	(0.100)	(0.265)	(0.735)
3*Cog	1.201	-0.001**	-0.301**	-0.561	0.082
	(1.584)	(0.000)	(0.112)	(0.340)	(0.907)
4*Cog	0.858	-0.001**	-0.213***	-0.379	0.265
	(1.463)	(0.000)	(0.077)	(0.295)	(0.968)
5*Cog	1.731	-0.001**	-0.194***	-0.293	1.617
	(1.503)	(0.000)	(0.064)	(0.233)	(1.062)
6*Cog	1.091	-0.000*	-0.122**	-0.119	0.530
	(1.819)	(0.000)	(0.059)	(0.216)	(1.063)
7*Cog	1.352	-0.000	-0.061	-0.255	0.386
	(1.851)	(0.000)	(0.057)	(0.235)	(1.091)
8*Cog	1.594	-0.000	-0.070	-0.385	1.257
	(1.801)	(0.000)	(0.057)	(0.237)	(1.219)
9*Cog	1.870	-0.000	-0.031	-0.303	0.660
	(1.979)	(0.000)	(0.041)	(0.227)	(1.344)
10*Cog	0.914	0.000	-0.021	-0.475*	-0.048
	(2.068)	(0.000)	(0.047)	(0.232)	(1.204)
Observations	168001	168001	168001	168001	168001

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummies for Air Force, Civil contract.

Table A10. Civil

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4	-20.196	-0.004	-0.795	10.695	-6.384
	(26.161)	(0.010)	(1.736)	(9.933)	(25.564)
-3	-6.477	-0.024	-3.657	-6.955	27.583
	(44.425)	(0.018)	(3.815)	(14.551)	(54.780)
-2	-16.640	0.003	0.474	13.148	-5.129
	(67.369)	(0.022)	(5.131)	(15.994)	(40.548)
-1	-75.771	0.014	5.807	39.248**	-44.965
	(73.354)	(0.021)	(5.252)	(19.074)	(37.876)
0	-72.487	0.020	4.520	39.445**	-34.595
	(63.717)	(0.021)	(4.533)	(15.239)	(35.433)
1	19.877	0.087**	9.829**	19.528	9.657
	(72.793)	(0.041)	(4.565)	(13.363)	(46.060)
2	90.668	0.211***	52.195***	9.580	16.857
	(81.169)	(0.050)	(14.444)	(15.672)	(48.450)
3	64.275	0.158***	34.254***	68.314***	49.526
	(89.147)	(0.031)	(7.674)	(21.420)	(53.594)
4	-50.019	0.119***	22.336***	70.841***	20.641
	(105.789)	(0.022)	(4.393)	(20.935)	(64.141)
5	-82.170	0.086***	14.901***	53.476***	-29.442
	(117.370)	(0.020)	(3.679)	(18.731)	(77.483)
6	-28.816	0.072***	12.063***	53.087***	8.123
	(111.614)	(0.018)	(4.250)	(17.032)	(63.737)
7	-101.503	0.051***	9.870***	62.580***	10.285
	(113.723)	(0.017)	(3.571)	(19.092)	(73.820)
8	-107.275	0.047***	12.343***	72.159***	38.426
	(110.476)	(0.017)	(3.372)	(17.828)	(78.633)
9	-94.214	0.052***	11.751***	55.884**	70.575
	(130.904)	(0.015)	(3.535)	(20.968)	(79.139)
10	-58.342	0.057***	13.896***	42.418*	96.332
	(135.751)	(0.016)	(3.665)	(23.262)	(77.590)
Observations	48176	48176	48176	48176	48176

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummy for Air Force.

Table A11. Military

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4	-1.459	0.004	-0.648	1.897	-1.185
	(25.940)	(0.004)	(0.656)	(4.739)	(15.576)
-3	1.099	0.001	0.007	1.757	-13.040
	(25.656)	(0.006)	(0.897)	(6.902)	(18.363)
-2	39.154	-0.004	-1.644	-8.735	32.229
	(44.562)	(0.007)	(1.045)	(5.934)	(25.335)
-1	15.725	-0.003	-1.241	-5.944	24.859
	(48.691)	(0.007)	(1.073)	(7.513)	(31.133)
0	60.274	-0.004	-1.224	-5.684	32.226
	(59.702)	(0.010)	(1.396)	(10.288)	(39.084)
1	22.401	0.041	1.810	-3.390	5.651
	(73.220)	(0.025)	(1.986)	(11.442)	(45.818)
2	118.611	0.136***	26.351***	-7.641	90.385*
	(70.331)	(0.032)	(7.307)	(12.205)	(46.724)
3	-2.843	0.107***	20.998***	36.926***	61.769
	(72.013)	(0.019)	(5.366)	(10.418)	(53.323)
4	-63.975	0.084***	14.665***	39.122***	5.931
	(63.287)	(0.016)	(3.342)	(9.694)	(54.447)
5	-112.359*	0.064***	11.104***	30.901**	-35.415
	(65.728)	(0.009)	(2.154)	(12.858)	(49.370)
6	-80.515	0.047***	7.787***	21.558*	-35.658
	(60.434)	(0.008)	(1.860)	(11.967)	(44.052)
7	-117.207**	0.046***	7.851***	16.585	-86.127**
	(54.597)	(0.008)	(1.822)	(10.981)	(39.117)
8	-165.400**	0.039***	6.993***	34.407***	-73.500
	(64.841)	(0.008)	(1.339)	(10.147)	(43.253)
9	-126.041**	0.030***	5.974***	24.879**	-50.570
	(60.574)	(0.008)	(1.548)	(9.666)	(45.564)
10	-179.903**	0.025***	4.750***	12.953	-67.806
	(67.368)	(0.007)	(1.390)	(10.776)	(47.295)
Observations	119825	119825	119825	119825	119825

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummy for Air Force.

Table A12. Civil: non-cognitive (percentile)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Non-cog	-0.569	0.000	-0.032	0.428	-0.066
	(0.905)	(0.000)	(0.085)	(0.366)	(0.835)
-3*Non-cog	0.132	0.001	0.093	0.204	1.419
	(1.162)	(0.000)	(0.100)	(0.478)	(1.856)
-2*Non-cog	-0.420	0.000	-0.029	0.159	0.258
	(1.374)	(0.000)	(0.110)	(0.483)	(0.837)
-1*Non-cog	0.337	0.000	-0.113	-0.173	0.533
	(1.484)	(0.001)	(0.126)	(0.498)	(0.894)
0*Non-cog	0.591	-0.000	-0.078	-0.133	0.487
	(1.594)	(0.001)	(0.112)	(0.615)	(0.895)
1*Non-cog	1.298	0.001	-0.005	0.340	1.589
	(1.936)	(0.001)	(0.130)	(0.554)	(1.190)
2*Non-cog	1.761	-0.001	-0.436**	0.128	1.748
	(2.274)	(0.001)	(0.200)	(0.582)	(1.270)
3*Non-cog	2.095	-0.001	-0.313*	-0.333	0.927
	(2.615)	(0.001)	(0.169)	(0.705)	(1.533)
4*Non-cog	-0.274	-0.001*	-0.324**	-0.368	2.034
	(2.298)	(0.001)	(0.129)	(0.760)	(1.758)
5*Non-cog	-1.968	-0.001	-0.254**	0.009	0.588
	(2.736)	(0.001)	(0.113)	(0.576)	(2.418)
6*Non-cog	-1.556	-0.001	-0.256**	0.171	0.942
	(2.719)	(0.001)	(0.120)	(0.683)	(1.922)
7*Non-cog	-1.380	-0.001	-0.210*	-0.825*	1.525
	(3.109)	(0.001)	(0.120)	(0.471)	(2.380)
8*Non-cog	-1.095	-0.000	-0.229*	-0.761	4.833*
	(2.228)	(0.001)	(0.120)	(0.467)	(2.828)
9*Non-cog	0.203	-0.000	-0.155	-0.951	5.314
	(3.078)	(0.001)	(0.155)	(0.646)	(3.127)
10*Non-cog	-2.355	-0.001	-0.154	-0.204	1.119
	(3.204)	(0.001)	(0.144)	(0.607)	(3.248)
Observations	47843	47843	47843	47843	47843

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummy for Air Force.

Table A13. Military: non-cognitive (percentile)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Non-cog	-0.666	-0.000	-0.052*	-0.189	-0.287
	(0.782)	(0.000)	(0.027)	(0.218)	(0.441)
-3*Non-cog	-0.743	-0.000	-0.001	0.002	-1.174
	(0.899)	(0.000)	(0.032)	(0.210)	(0.698)
-2*Non-cog	-0.197	0.000	0.021	-0.150	0.075
	(0.954)	(0.000)	(0.032)	(0.187)	(1.188)
-1*Non-cog	0.271	0.000	0.011	-0.268	-0.604
	(1.098)	(0.000)	(0.032)	(0.218)	(0.890)
0*Non-cog	-1.080	0.000	0.010	0.239	-1.497*
	(1.377)	(0.000)	(0.033)	(0.228)	(0.769)
1*Non-cog	-0.041	-0.000	0.027	0.193	-0.690
	(1.280)	(0.000)	(0.041)	(0.267)	(0.696)
2*Non-cog	-0.425	-0.001	-0.163*	0.161	-0.670
	(1.832)	(0.000)	(0.093)	(0.251)	(1.119)
3*Non-cog	0.106	-0.000	-0.179**	-0.100	-1.598
	(2.115)	(0.000)	(0.087)	(0.301)	(1.233)
4*Non-cog	0.149	-0.000	-0.078	-0.492*	-1.237
	(1.916)	(0.000)	(0.067)	(0.286)	(1.193)
5*Non-cog	-0.027	-0.000	-0.136*	-0.128	-0.625
	(1.606)	(0.000)	(0.071)	(0.332)	(1.294)
6*Non-cog	1.058	-0.000	-0.114*	-0.288	-0.785
	(1.706)	(0.000)	(0.067)	(0.346)	(1.233)
7*Non-cog	0.285	-0.000	-0.127**	-0.213	-1.489
	(1.840)	(0.000)	(0.056)	(0.247)	(1.155)
8*Non-cog	1.460	-0.000	-0.095*	-0.619*	-0.677
	(1.666)	(0.000)	(0.051)	(0.308)	(1.111)
9*Non-cog	1.229	-0.000	-0.081	-0.247	-0.892
	(1.881)	(0.000)	(0.060)	(0.293)	(1.239)
10*Non-cog	0.955	-0.000	-0.101	-0.255	-1.286
	(1.763)	(0.000)	(0.063)	(0.340)	(1.039)
Observations	119523	119523	119523	119523	119523

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummy for Air Force.

Table A14. Civil: cognitive (percentile)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Cog	0.666	0.000	-0.005	-0.180	-0.173
	(0.660)	(0.000)	(0.074)	(0.298)	(0.479)
-3*Cog	1.959	0.000	-0.052	-0.784*	1.888*
	(1.178)	(0.000)	(0.083)	(0.407)	(1.052)
-2*Cog	1.246	-0.000	-0.137	-0.371	1.062
	(1.376)	(0.001)	(0.131)	(0.557)	(1.117)
-1*Cog	2.016	-0.000	-0.145	-0.964	1.471
	(1.287)	(0.001)	(0.148)	(0.605)	(1.029)
0*Cog	1.487	-0.000	-0.120	-0.374	1.648
	(1.489)	(0.001)	(0.128)	(0.614)	(1.020)
1*Cog	1.130	-0.000	-0.099	0.105	2.056
	(2.074)	(0.001)	(0.142)	(0.548)	(1.540)
2*Cog	2.537	-0.001*	-0.447**	-0.529	2.058
	(2.657)	(0.001)	(0.164)	(0.645)	(1.598)
3*Cog	2.635	-0.002*	-0.449**	-1.037	1.692
	(3.147)	(0.001)	(0.171)	(0.649)	(1.768)
4*Cog	1.051	-0.001	-0.260**	-0.568	2.226
	(3.183)	(0.001)	(0.118)	(0.486)	(2.081)
5*Cog	1.794	-0.001	-0.188*	-0.499	3.075
	(3.461)	(0.001)	(0.102)	(0.455)	(2.344)
6*Cog	0.360	-0.000	-0.068	-0.222	1.907
	(3.832)	(0.001)	(0.106)	(0.531)	(2.071)
7*Cog	-0.128	0.000	-0.080	-0.777*	1.866
	(3.609)	(0.001)	(0.112)	(0.428)	(2.265)
8*Cog	1.620	0.000	-0.123	-1.449***	2.663
	(3.640)	(0.001)	(0.126)	(0.444)	(2.035)
9*Cog	2.136	0.000	0.015	-1.136**	4.051
	(4.014)	(0.001)	(0.094)	(0.506)	(2.962)
10*Cog	0.690	0.000	0.032	-1.453***	1.838
	(4.300)	(0.001)	(0.092)	(0.337)	(2.253)
Observations	48176	48176	48176	48176	48176

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummy for Air Force.

Table A15. Military:: cognitive (percentile)

	(1) Wage	(2) Unemp.(%)	(3) Days unemp.	(4) Support	(5) Disp. inc.
-4*Cog	-0.244	0.000	-0.002	-0.385**	-0.071
	(0.482)	(0.000)	(0.026)	(0.175)	(0.385)
-3*Cog	-1.473**	0.000	0.015	-0.402*	-1.451**
	(0.695)	(0.000)	(0.031)	(0.207)	(0.637)
-2*Cog	-0.255	-0.000	-0.023	-0.122	-0.210
	(1.062)	(0.000)	(0.029)	(0.195)	(0.678)
-1*Cog	0.815	-0.000	-0.048	-0.127	0.462
	(1.240)	(0.000)	(0.033)	(0.197)	(0.775)
0*Cog	-0.695	0.000	-0.012	-0.022	-0.824
	(1.326)	(0.000)	(0.036)	(0.264)	(0.820)
1*Cog	0.438	-0.000	-0.047	0.020	-0.931
	(1.672)	(0.000)	(0.048)	(0.310)	(1.056)
2*Cog	-0.479	-0.000	-0.168*	-0.308	-0.920
	(1.351)	(0.000)	(0.083)	(0.209)	(0.876)
3*Cog	0.778	-0.001	-0.156	-0.360	-0.724
	(1.717)	(0.000)	(0.093)	(0.301)	(0.885)
4*Cog	0.757	-0.001*	-0.138*	-0.308	-0.581
	(1.917)	(0.000)	(0.068)	(0.332)	(1.210)
5*Cog	1.753	-0.001*	-0.159**	-0.250	0.994
	(1.920)	(0.000)	(0.065)	(0.284)	(1.315)
6*Cog	1.624	-0.000*	-0.109*	-0.090	0.109
	(1.997)	(0.000)	(0.061)	(0.299)	(1.323)
7*Cog	2.110	-0.000	-0.022	0.029	0.138
	(1.951)	(0.000)	(0.045)	(0.274)	(1.320)
8*Cog	1.837	0.000	0.000	0.116	1.180
	(2.114)	(0.000)	(0.049)	(0.379)	(1.499)
9*Cog	1.878	-0.000	-0.004	0.069	-0.399
	(1.996)	(0.000)	(0.038)	(0.324)	(1.294)
10*Cog	1.461	0.000	0.018	-0.052	-0.252
	(1.944)	(0.000)	(0.059)	(0.327)	(1.092)
Observations	119825	119825	119825	119825	119825

Note: Standard errors clustered at the municipality (29 clusters). Support measures work related income (including unemployment benefits). Control variables: dummy for Air Force.

Table A16. Sector of employment (year 10): Top 15

	Control		Treated	
	Percent	N	Percent	N
Institutional care	0.52	39	1.05	30
Other industries	16.31	1230	34.14	978
Consultant, computer	1.37	103	2.30	66
Renting or administer real estate	0.48	36	0.94	27
Consultant, organization	0.84	63	1.36	39
Consultant, technical	1.06	80	3.14	90
Public administration	0.80	60	2.41	69
Administration, health care, education, culture etc.	0.21	16	1.15	33
SAF	71.05	5359	34.76	996
Police department	0.68	51	1.75	50
Education, elementary school	0.84	63	2.02	58
Education, high school	0.41	31	1.22	35
Education, high school, voccational training	0.40	30	1.36	39
Education, post high school/university	4.65	351	11.45	328
Education, other	0.41	31	0.94	27

Note: Based on 4-digit industry code (SNI 2002/2007).

Table A17. Correlation matrix: cognitive and non-cognitive skills (full population 2013)

	Cognitive	Spatial	Verbal	Technical	Inductive	Non-cognitive	Social	Intensity	Psychological	Emotional
Cognitive	1									
Spatial	0.801***	1								
Verbal	0.796***	0.480***	1							
Technical	0.820***	0.614***	0.553***	1						
Inductive	0.862***	0.600***	0.691***	0.611***	1					
Non-cognitive	0.370***	0.261***	0.316***	0.306***	0.357***	1				
Social	0.377***	0.273***	0.334***	0.302***	0.355***	0.802***	1			
Intensity	0.189***	0.121***	0.146***	0.170***	0.195***	0.798***	0.456***	1		
Psychological	0.324***	0.230***	0.284***	0.260***	0.314***	0.793***	0.613***	0.540***	1	
Emotional	0.334***	0.244***	0.283***	0.278***	0.313***	0.813***	0.621***	0.472***	0.555***	1

Note: All variables are standardized by enlistment year, with mean 0 and sd 1. ** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*