University of Oxford

Doctoral Thesis

Essays on Dynamic Choice Under Uncertainty

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A thesis submitted in fulfilment of the requirements for the degree of Doctor of Philosophy in Economics

July 2023

Declaration of Authorship

I, Hannah Zillessen, declare that this thesis titled, "Essays on Dynamic Choice Under Uncertainty" and the work presented in it are my own unless otherwise stated.

I here declare the existence of joint work in this thesis. This thesis comprises four principal essays. The first essay is solely the product of my own work, except where otherwise stated. The second, third and fourth essays incorporate joint work. The second essay is based on work with Dr Marta Golin. I conceived of the empirical research design, collected the treatment information and constructed the treatment measure. All other work was done jointly. The third essay is my own write up of a project with Dr Thiemo Fetzer, Dr Lukas Hensel and Dr Christopher Roth for which we jointly designed an experiment and carried out the data collection as well as analysis. Finally, the fourth essay is based on work with Mr Luke Heath Milsom, Mr Samuel Altmann, Dr Johannes Abeler, Dr Séverine Toussaert, Mr Raffaele Blasone, Dr Daniele Nosenzo, Dr Frauke Kreuter, Mr Frederic Gerdon and Dr Ruben Bach that was published in the Journal of Medial Internet Research mHealth and UHealth. I was responsible for study design, data collection, data analysis, and writing.

Hannah Zillessen, July 2023

Abstract

This thesis explores how uncertainty shapes dynamic choices and subsequent economic outcomes. It documents that uncertainty over states of the world can render individuals hesitant to sacrifice present for future utility, dissuading long term investments. This cautiousness is rational a priori, but can render individuals worse off once the uncertainty has been resolved – exacerbating existing inequalities. However, this thesis also documents that targeted policies can effectively reduce uncertainty across domains and improve long term outcomes. This thesis consists of four principal essays. In the first essay, I theoretically formalise the link between uncertainty over future states of the world and hesitation to save and invest. Standard economic theory predicts that in the presence of greater uncertainty, individuals should save at higher rates. Yet, I show that this prediction hinges on three implicit assumptions: (i) equal access to resources across states of the world, (ii) state independent utility, and (iii) predictable utility. If any of these assumptions fails, uncertainty over future states of the world reduces incentives to save and invest. I confirm the importance of this channel empirically. In the first essay, I show that migrants who face uncertainty over their future right to stay save 30% less than comparable natives. This residual gap is closed completely when these migrants gain access to citizenship as they become more likely to invest long term, in housing and pension plans for example. In the second essay, I show that education policies which increase the salience of and perceived returns to children's non-cognitive skills can decrease detrimental gaps between children from more and less advantaged backgrounds. In the third essay, I show that in a situation where democratic norms are ambiguous individuals become more willing to campaign for what they believe is right if they learn that other people are more uncertain about their stance. Finally, in the fourth essay, I document that individuals become more willing to use a novel public health care measure if adoption is voluntary and trust in the source is high.

Acknowledgements

This thesis has benefited from many thoughtful comments, random conversations and emergency pick me up sessions throughout my graduate studies. I am indebted to too many kind people to fit them onto two pages. Yet, I want to thank here the ones who have shared this journey with me the longest.

This thesis would not have been possible without my supervisor since the masters, Johannes Abeler. He has seen these projects evolve from bullet points on an ideas list, pushed me to keep moving forward, cheered me up and cheered me on when I thought things were doomed, helped me countless times with his precise insights, and always tells me to write shorter sentences. It also would not have been the same without Hamish Low who arrived in Oxford at the moment I started my PhD, encouraged me to keep on digging deeper and aim higher, and who has made both this thesis as well as the whole experience so much better. I am further indebted to Abi Adams-Prassl who I learn from every time I see her, and to Steve Bond whose incomparably sharp eye ensured the formation of this thesis' empirical core during my masters. I am extremely grateful to all four of them for so generously sharing their time and wisdom with me over the years, for shaping how I do research and what I believe possible, and for taking me seriously from the beginning.

I have learned a lot from the entire Applied Micro faculty who have accompanied me over the years and whose in depth comments have shaped this thesis. I would like to particularly thank Séverine Toussaert, Ian Crawford and Romuald Méango who have helped and encouraged me on numerous occasions. Further, I have been lucky enough to do my PhD alongside wonderful peers and brilliant economists, Shihang Hou, Binta Zahra Diop, Sanghamitra Mukherjee, Giulio Schinaia, Tatjana Schulze, Marta Golin, Lukas Hensel, Samuel Altmann, Lisa Martin and Vanessa Schreiber to name but a few. Not only have they made made this thesis continuously better with their feedback, but filled the entire five years with fun and games as well.

There would have been no thesis, if I had not gotten so excited about research during my masters. However, I have had the incredibly good fortune to begin my time in Oxford as well as my adventures in economics with some of best people, with an infectious love for research: Lena Anayi, Katarina Kuske, Lukas Freund, Sara

Holttinen and Joel Kariel, again to name but just a few. I am especially grateful to Gunjita Gupta, my first friend & housemate in Oxford, whose boundless knowledge and kindness, as well as steel are among the main reasons this thesis was ever possible.

I am always thankful for Farina Klein, Julia Granderath and Lara Schmitt who have been sharing all my highs and lows since childhood and who have kept me grounded when the PhD threatened to swallow me up.

I could not have finished this PhD without my housemates, Vatsal Khandelwal, Verena Wiedemann and Raphael Bradenbrink. We lived through a pandemic and a job market together, discussed any topic on earth on our stairs, saw each of our papers presented at least twenty times (JSS forever) and most importantly were there for each little moment in between. I will always be grateful for the six years in Jeune Street. To Vatsal for being my sibling, my telepathic companion, and each white board session. To Raphael, for insisting on taking breaks and the existence of an outside world. And to Verena, the person I began and finished this journey with, my partner in all crimes, and surely the one person I will always think of the most when I look back at this time.

To my family, not only would there be no PhD but nothing else without you. To my grandparents, Anneliese, Josef, Karl and Josefine, who always insisted everything was possible – I finished my book! To my brother and ride or die, Patrick who is the best bellwether for whether an idea is any good, who can make me laugh even when everything feels terrible, and who reminds me that nothing is ever as serious as it may seem. To my parents, Irmgard and Reinhard, who have encouraged me to ask questions and filled my life with stories, love and endless wonder from the beginning. Who never stop believing in me, even when I have serious doubts. And who keep on insisting that even if the mood for work does not strike you all the time – dann machst du es eben ohne Lust. None of this would have been possible without you.

Finally, to Luke. Who I tell all my ideas to first, who makes me happy each morning I wake up and each night I go to sleep, and fills all the days in between with more happiness, peace, and silliness than I ever could have dreamed of. This thesis is immeasurably better because of you. But then really, so is everything else.

Contents

Declaration of Authorship **iii**

Abstract **v**

Acknowledgements **vii**

Introduction **xxv**

1 Uncertainty, Citizenship & Migrant Saving Choices **1**

1.1 Introduction . . . 2

1.2 Theoretical Framework . . . 10

1.2.1 Life Cycle Model with Uncertain Retirement Location . . . 10

Preferences . . . 11

Resource Constraint . . . 13

Optimisation Problem . . . 15

Access to Citizenship . . . 16

1.2.2 Implications . . . 16

Temporary and Permanent Migrants . . . 17

Resource Uncertainty . . . 18

Uncertainty over States of the World . . . 19

1.3 Empirical Setting . . . 23

1.3.1 Institutional Context . . . 24

1.3.2 Identification . . . 27

1.3.3 Data . . . 29

1.4 Causal Impact of Access to Citizenship on Migrants' Saving Rate . . . 31

1.4.1 Empirical Strategy . . . 31

1.4.2 Sample . . . 33

1.4.3 The Saving Rate Gap . . . 35
1.4.4 Closing the Gap . . . 37
1.4.5 Impact on Transfers Abroad . . . 41
1.5 Mechanisms . . . 42
1.5.1 Empirical Specification . . . 42
1.5.2 Long Term Investments . . . 44
1.5.3 Resource Constraint . . . 46
1.5.4 Uncertainty over Preferences or Right to Stay . . . 48
1.5.5 Direct Evidence from Interviews . . . 50
1.6 Aggregate Implications . . . 53
1.7 Conclusion . . . 55

Appendices **59**
1.A Formal Proofs . . . 59
1.B Further Identification Examples . . . 66
1.C Variable Definitions . . . 67
1.D Treatment Group Assignment in the Difference-in-Difference Analysis around the 1999 Reform . . . 69
1.E Treatment Group Assignment Using Variation in Eligibility Over Time 70
1.F Additional Result Tables . . . 71
1.G Additional Result Figures . . . 74
1.H Robustness of Main Results . . . 81

2 The Effect of Conduct Grades on Socio-Emotional Skill Accumulation **87**
2.1 Introduction . . . 88
2.2 Institutional Context . . . 94
2.3 Data . . . 95
2.3.1 Sample . . . 96
2.3.2 Treatment Variables . . . 96
2.3.3 Outcome Variables . . . 98
2.4 Empirical Strategy . . . 102
2.5 Results . . . 103

2.5.1 Conduct Grades and Personality Traits . . . 104
2.5.2 Conduct Grades and Economic Preferences . . . 106
2.5.3 Robustness . . . 108
2.5.4 Conduct Grades in High School . . . 109
2.6 Conclusion . . . 111

Appendices **113**

2.A Data Description . . . 113
2.B Additional Results . . . 115
2.C Examples of Legal Documents . . . 126
2.D Description of the High School Variation . . . 128

3 Political Participation Under Uncertain Democratic Norms **131**

3.1 Introduction . . . 132
3.2 Empirical Design . . . 137
3.2.1 Institutional Context . . . 137
3.2.2 Experimental Design . . . 140
3.2.3 Data . . . 144
3.2.4 Empirical Strategy . . . 147
3.3 Impact on Political Participation . . . 151
3.3.1 Impact on Political Campaigning . . . 152
3.3.2 Impact on Voting Intentions . . . 157
3.3.3 LATE Estimates . . . 160
3.4 Mechanisms . . . 161
3.4.1 Social Learning . . . 162
3.4.2 Signalling . . . 171
3.4.3 Strategic Considerations . . . 173
3.5 Discussion . . . 175
3.6 Conclusion . . . 177

Appendices **179**

3.A Additional Figures . . . 179
3.B Additional Reduced Form Tables . . . 180

3.C LATE Tables 192

4 Acceptability of App-Based Contact Tracing for COVID-19 201

4.1 Introduction 202

4.2 Methods 204

4.2.1 Survey Design 204

4.2.2 Data 206

4.2.3 Empirical Strategy 207

4.3 Results 207

4.4 Discussion 211

4.4.1 Implication of Principal Findings 211

4.4.2 Limitations 212

4.5 Conclusions 214

Appendices 215

4.A Survey Details 215

4.A.1 Survey Design 215

4.A.2 Technical Details 219

4.B Sample 220

4.B.1 Recruitment 220

4.B.2 Attrition and Final Sample 220

4.B.3 Sample Robustness 222

4.B.4 Selective Attrition 223

4.C Additional Results 227

4.C.1 Robustness to Modelling Assumptions 227

4.C.2 Robustness across Demographics 229

4.C.3 Take Up and Disease Severity at the Geographical Level 230

4.C.4 Reasons for/against Installation and Take Up Decisions 231

4.C.5 Determinants of the Reasons Chosen 234

4.C.6 Opt-in Versus Opt-out Preferences 235

Validity of Difference Measures 238

4.C.7 Data Usage 239

4.C.8 Compliance with the App Self-Isolation Request 240

4.C.9 Effect of Comprehension 241

Concluding Remarks **i**

Bibliography **iii**

List of Figures

1.1 Saving Rate Gap Pre-Reform 36
1.2 DID Pre/Post Residual Saving Rate 37
1.3 Progression of the Mean Residual Saving Rate Over Time 39
1.4 DID Pre/Post Residual Remittance Rate 42
1.5 Effect of Becoming Eligible on Propensity to Own Different Assets . . 45
1.6 Effect of Becoming Eligible on Intention to Stay 49
1.D.1 Assignment to Treatment and Control Groups 69
1.E.1 Changes in Eligibility Over Time 70
1.G.1 1990 DID, Residual Remittance Rate 74
1.G.2 1990 DID, Propensity to Remit 75
1.G.3 Effect of Becoming Eligible Expected or Unexpectedly On Saving and Remittances 76
1.G.4 Effect of Becoming Eligible Expected or Unexpectedly on Propensity to Own Different Assets 77
1.G.5 Effect of Eligibility and Years of Eligibility on Propensity to Own One's Home 78
1.G.6 Effect of Becoming Eligible on Saving Rates of EU Migrants 79
1.G.7 Effect of Becoming Eligible on Investment Choices of EU Migrants . . 80
1.H.1 Robustness: Effect of Citizenship on Already Eligible Migrants' Saving Rate 82
1.H.2 Robustness: Effect of Citizenship on Not Already Eligible Migrants' Saving Rate 83
1.H.3 Robustness: Effect of Citizenship on Not Already Eligible Migrants' Saving Rate 84

1.H.4 Robustness: Effect of Citizenship on Not Already Eligible Migrants' Saving Rate . . . 85

2.3.1 Variation in Feedback on Conduct in Primary School . . . 99
2.3.2 Variation in Feedback on Conduct in Primary School . . . 100
2.5.1 Conduct Grades in Primary School and the Big Five Personality Traits 105
2.5.2 Conduct Grades in Primary School and Preferences . . . 107
2.5.3 Conduct Grades in High School and the Big Five Personality Traits . 110
2.5.4 Conduct Grades in High School and Preferences . . . 111
2.C.1 Current Sample Transcript (1.Page) for Primary Schools in Mecklenburg-Vorpommern . . . 126
2.C.2 Excerpt of Current Legislation on Primary School Grading in Saxony-Anhalt . . . 127
2.C.3 Excerpt of 1999 Reform of Primary School Grading Legislation in Saxony 128
2.D.1 Variation in Feedback on Conduct in High School . . . 129
2.D.2 Variation in Feedback on Conduct in High School . . . 130

3.A.1 Example Treatment Screen . . . 179

4.2.1 Attrition and Final Sample . . . 206
4.3.1 Likelihood of Having the App Installed, under Opt-in and Opt-out Regimes and by Country . . . 208
4.3.2 Determinants of Stating Definitely Install or Probably Install . . . 209
4.B.1 Would You Install the App? – Robustness in the German Sample . . . 223
4.B.2 Selection Model . . . 225
4.B.3 Inverse Probability Weights of Staying in Sample . . . 226
4.C.1 Ordered Logit Specification . . . 227
4.C.2 Determinants of Stating Definitely or Probably Keep . . . 228
4.C.3 Would You Install the App? (Opt-in): Comorbidities . . . 229
4.C.4 Would You Install the App? (Opt-in): Age . . . 230
4.C.5 Would You Install the App? (Opt-in): Trust in Govt . . . 230
4.C.6 Number of Reasons Given for/against the App, by Installation Preference 232
4.C.7 Percentage of Respondents Who Choose Each Reason for Installation . 233

4.C.8 Percentage of Respondents Who Choose Each Reason against Installation 233
4.C.9 Impact of Reasons on Installation Probability 234
4.C.10 Determinants of Reasons Given Against Installation 235
4.C.11 Determinants of Reasons Given for Installation 236
4.C.12 Opt-in/Opt-out Preferences in Each Country 237
4.C.13 Determinants of Opt-in vs. Opt-out Preference 238
4.C.14 Percentage of Respondents Wanting the Data to Be Made Available . . 239
4.C.15 Determinants of Wanting the Data to Be Made Available 240
4.C.16 Compliance with Isolation Request 241

List of Tables

1.1 Pre-Reform Characteristics of the Sample 34
1.2 Effect of the Citizenship Act on Saving & Remittances 40
1.F.1 Effect of the Citizenship Act on Saving & Remittances: Migrants Only 71
1.F.2 Effect of Eligibility for Citizenship on Migrants' Resources 72
1.F.3 Effect of Eligibility for Citizenship on Identity & Worries 73

2.3.1 Sample Characteristics 96
2.A.1 Outcome Variables 113
2.A.2 Summary Statistics of Outcome Variables by gender and SES 114
2.B.1 Conduct Grades and the Big Five Personality Traits 115
2.B.2 Conduct Grades and Preferences 115
2.B.3 Conduct Grades and Preferences – Heterogeneity 116
2.B.4 Conduct Grades and the Big Five Personality Traits – Heterogeneity . 117
2.B.5 Numeric Conduct Grades and the Big Five Personality Traits 118
2.B.6 Numeric Conduct Grades and Preferences 119
2.B.7 Grade-like Feedback and the Big Five Personality Traits 120
2.B.8 Grade-like Feedback and Preferences 121
2.B.9 Standardised Feedback and the Big Five Personality Traits 122
2.B.10 Standardised Feedback and Preferences 123
2.B.11 Conduct Grades in High School and the Big Five Personality Traits . . 124
2.B.12 Conduct Grades in High School and Preferences 125

3.2.1 Treatment Overview 143
3.2.2 Summary Statistics 146
3.2.3 Balance Between Treatment Arms 147
3.3.1 Campaigning, Pooled 153

3.3.2 Campaigning, Own/Other . . . 154
3.3.3 Campaigning, Leavers . . . 155
3.3.4 Campaigning, Remainers . . . 156
3.3.5 Plans to Vote . . . 158
3.3.6 Plans to Vote, Own/Other . . . 159
3.4.1 Manipulation Checks: Appropriateness of Repeat Referendum . . . 163
3.4.2 Manipulation Checks: Private Assessment of Appropriateness . . . 164
3.4.3 Manipulation Checks: Support for Repeat Referendum . . . 165
3.4.4 Beliefs about Brexit . . . 168
3.4.5 Plans for a Potential Repeat Referendum . . . 170
3.4.6 Beliefs about Perception of Supporters & Opponents of a Referendum . 172
3.4.7 Beliefs about Effectiveness . . . 174
3.B.1 Plans to Vote, Leavers . . . 180
3.B.2 Plans to Vote, Remainers . . . 181
3.B.3 Manipulation Checks: Support for 2nd Referendum, Leavers . . . 182
3.B.4 Manipulation Checks: Support for 2nd Referendum, Remainers . . . 183
3.B.5 Beliefs about Brexit, Leavers . . . 184
3.B.6 Beliefs about Brexit, Remainers . . . 185
3.B.7 Plans for a Potential Repeat Referendum, Leavers . . . 186
3.B.8 Plans for a Potential Repeat Referendum, Remainers . . . 187
3.B.9 Beliefs about Perception of Supporters & Opponents of a Referendum, Leavers . . . 188
3.B.10 Beliefs about Perception of Supporters & Opponents of a Referendum, Remainers . . . 189
3.B.11 Beliefs about Effectiveness, Leavers . . . 190
3.B.12 Beliefs about Effectiveness, Remainers . . . 191
3.C.1 Campaigning: Pooled . . . 192
3.C.2 Campaigning: Pooled, Own/Other . . . 193
3.C.3 Campaigning: Leavers . . . 194
3.C.4 Campaigning: Remainers . . . 195
3.C.5 Plans to Vote, Pooled . . . 196
3.C.6 Plans to Vote, Own Other . . . 197

3.C.7 Plans to Vote, Leave 198
3.C.8 Plans to Vote, Remainers 199

4.A.1 Structure of the Survey 216
4.B.1 Demographics 221

Introduction

Uncertainty is an inherent fact of life, one that underpins most decisions. It becomes an economic issue when it exacerbates existing inequalities, or puts people at a greater risk of future poverty or ill health. This thesis explores how uncertainty shapes dynamic decision making in contexts as diverse as the saving choices of migrants, childhood investments into different skill sets, political participation or public health. The thesis draws out how in each of these settings uncertainty can worsen long term outcomes for those more exposed to it. It further documents how in each or these situations, public policy in the form of citizenship legislation, education regulation and public information campaigns can reduce inequalities and improve societal well-being by reducing uncertainty or rendering potential benefits more salient.

In the first chapter, I focus on the saving and investment choices of migrants. Across Western countries, migrants own substantially less wealth than natives (e.g. Bauer et al. [2011]). The gap is particularly pronounced with regard to housing and pension wealth (e.g. Muckenhuber et al. [2022]). This puts migrants who stay in the immigration country at a greater risk of poverty once they retire (e.g. Bárcena-Martín and Pérez-Moreno [2017]). The observable wealth gap is driven crucially by differences in saving rates. Temporary migrants save at higher rates than natives, but migrants planning to stay long term save at much lower rates (e.g. Albert and Monras [2022]). Further, the differences cannot be fully explained by factors traditionally brought forward in this literature. Even after controlling for age, gender, years spent in the host country, education, family constellation, labour market outcomes, as well as permanent unobservable differences through fixed effects, more than a quarter of the saving gap remains unexplained. In the first chapter, I show that it is instead *uncertainty over their future location* that fully accounts for the residual gap. Exploiting quasi-experimental variation in access to citizenship and comprehensive panel

data from Germany, I document that migrants with access to citizenship save on par with natives once the above mentioned influencing factors are controlled for. Yet, migrants without this right persistently save at least 30% less. This unexplained gap is closed completely when migrants in the latter group gain the right to citizenship. The saving measure includes savings in Germany as well as any other country. Hence, the treatment effect represents an overall increase in saving rather than a shift from one country to another.

To understand this result, I have to move beyond standard theory on dynamic financial choice. Standard intertemporal models predict that in the presence of greater uncertainty saving rates should increase. This is because uncertainty over future resources raises the marginal utility of any unit of consumption shifted from the present into the future, generating a precautionary saving motive. Consequently, migrants, facing higher levels of uncertainty both in terms of their right to stay and their labour market outcomes (e.g. Borjas [1985, 1987]), should save at higher levels than natives. Moreover, as access to citizenship lowers both labour market uncertainty and worries over having to leave the host country (e.g. Gathmann and Keller [2018]), gaining this right should decrease their saving rate. However, as I show in the first chapter, these models rely on three key assumptions, all of which are unlikely to hold in this setting. First, standard models assume that resources are equally accessible across future states of the world. Second, they assume that the utility derived from these resources is independent of future states of the world, and finally, they assume that future preferences generating utility are perfectly predictable. For international migrants, resources accrued in one country are unlikely to be equally accessible if they move (back) to another. Also, assuming that migrants have a preference for one country over another (Dustmann [1997]), they will derive different marginal utility from consumption in either place. Finally, preferences for either country will likely only be revealed through experience.

I formalise all three channels in a life cycle model with uncertain retirement location and heterogeneous country preferences. With it, I show that uncertainty can

have an ambiguous impact on migrants' saving and investment choices. While uncertainty over future income does prompt higher precautionary savings, uncertainty over future location or preferences decreases the effective rate of return and marginal utility agents expect from any unit of saving. As a consequence, agents become less inclined to save in general but particularly in country-specific, illiquid assets. I show that, empirically, this is indeed the case as migrants not only increase their general saving rate when they become eligible for citizenship, but become particularly more likely to invest long term into assets such as housing or pension plans. These shifts are not driven by an increase in resources, but rather willingness to save. Moreover, I leverage variation in initial right to stay between migrants from within or outside the European Union to confirm that the results are driven by uncertainty over right to stay rather than updates of country preferences. Finally, using the fact that assets are differentially transferable across country borders, I show that both the accessibility of resources and the utility derived from them are quantitatively important channels. The findings imply that governments can use citizenship legislation to narrow the wealth gap between migrants and natives. This insight is particularly important as demographic change has prompted both a decline in public pensions and increased demand for immigration in many Western countries.

In the second chapter, I study how a lack of salience of kids' socio-emotional skills as well as returns to these skills can disincentivise investments into them. Socio-emotional skills are important predictors of life outcomes such as labour market success or interpersonal relationships (e.g. Heckman et al. [2006], Deming [2017]). Hence, it is worrying that from an early age we see gaps open up between children from more or less advantages backgrounds that never appear to fully close (e.g. Heckman [2008]). One reason for the emergence of these gaps could be that parents, as well as teachers and kids, who are more constrained both in terms of time and resources have to strategically choose between investments into their kids' cognitive and socio-emotional skills. A growing literature examines how beliefs affect how parents spend shared time and how this in turn affects their children (e.g. Attanasio et al. [2015]). However, the exact factors determining skill investments are still to be established. In the second chapter, together with my co-author Marta Golin, I propose that gaps in

socio-emotional skills form in part because socio-emotional skills are less salient and are perceived to have lower returns than cognitive skills. As soon as children enter school, they receive regular standardised feedback on their academic performance. This plausibly raises the salience of kids' cognitive skill development. Furthermore, the mere fact that children are graded on maths and chemistry rather than conflict management may implicitly shape expectations over the relative importance of the skill sets required for either. As a result, parents along with teachers and kids themselves may prioritise investments into algebra over de-escalation strategies.

With this is mind, in the second chapter, we test whether grading students on non-academic attainments such as work ethic or social conduct can affect the formation of their socio-emotional skills long term. Specifically, we exploit quasi-experimental variation in German states introducing and removing conduct grades ("Kopfnoten"). We code up current and archival legal texts to construct a measure of whether a child received numeric grades, grade-like or standardised feedback on their conduct in school. In the main analysis, we focus on grades in primary school as socio-emotional traits have been shown to be particularly malleable at an early age (e.g. Cunha et al. [2010]). Looking at the Big Five Personality Traits, we find that children who receive conduct grades in primary school grow up to be generally more "open to experience" at age 17. More importantly, receiving formal feedback on conduct significantly reduces the detrimental gap in neuroticism we generally observe between kids from different socioeconomic backgrounds. Furthermore, looking at economic preferences and self control, we find that receiving conduct grades reduces another detrimental gap, in present bias, while rendering everyone on average more willing to accept risks. The results are robust to using different grading schemes, underlining that it is the content of the grades rather than their format that shifts skill formation. Most interventions that have been shown to decrease gaps in socio-emotional skills (e.g. Heckman et al. [2008], Kosse et al. [2018]) are time and resource intensive and thus hard to scale up. In contrast, in this chapter, we identify a light touch intervention that reduces detrimental gaps by increasing the salience of both children's current skills as well as future rates of return of investments into them.

The third chapter explores how uncertainty over democratic norms and peers' opinion on them shapes political participation. Functioning democracies require both debate about political issues and consensus on the rules governing said debate. However, sometimes the rules of the game are ambiguous and multiple equilibria can arise, requiring coordination. This process is hindered by the fact that polarisation on political issues is increasingly accompanied by polarisation on norms as well. As a result, what should be institutional debates over questions such as voting systems or the scope of legitimate protest become partisan issues. A possible reason for this could be that individuals underestimate how uncertain others are in their beliefs about legitimate behaviour. This could lead them to overestimate the social sanctions they would face if they voiced support for a norm that is unpopular in their reference group, while at the same time underestimating the effectiveness of political activity in changing people's minds. Informing them about the true uncertainty and shifting their beliefs over their peers might therefore prompt individuals to become more politically active and potentially decrease polarisation.

I test this proposition together with my co-authors Thiemo Fetzer, Lukas Hensel and Christopher Roth in the context of Brexit and the debate over the appropriateness of a repeat referendum on the UK's relationship to the European Union. In the weeks leading up to the 2019 general election which was decided on precisely those two questions, we ran an online experiment with more than 4000 respondents where we exogenously varied participants' beliefs over the fraction of the population that agreed with the appropriateness of a repeat referendum, as well as was certain about their views. We find that respondents become significantly more likely to donate to and tweet about political campaigns that lobby for or against a repeat referendum if they learn that a larger share of Leave and Remain supporters are uncertain about the legitimacy. This is driven by Remain (Leave) supporters becoming more likely to campaign against (for) another referendum if they learn that other Remain (Leave) supporters are uncertain about their view. The results are not driven by social learning as respondents' private beliefs about the appropriateness of another referendum as well as the likelihood and consequences of Brexit are unchanged. Instead, the findings reflects a shift in respondents' beliefs about peers' reactions. After learning

that others are more uncertain, they think that political action is more effective in convincing others. They also think that there is less to be lost by going against the norm preferred by people who share their views on political outcomes. The results imply that information campaigns that report confidence bands alongside level support for specific norms could stimulate political dialogue about democratic norms and decrease polarisation on them.

Finally, the fourth chapter explores how dissemination strategies of public health tools affect uptake in a situation when uncertainty over potential benefits and costs is high. In the first months of the Covid-19 pandemic, with a team of researchers, I explored how willingness to use a contact tracing app varied with installation regime, trust in the administering body, data protection concerns and phone use. We surveyed a total of 5995 participants in France, Germany, Italy, the United Kingdom and the United States. We find that willingness to use a contact tracing app is high irrespective of installation regime. 74.8% of all respondents say they would download a contact tracing app if they were asked to download it (opt-in regime), and 67.7% of respondents say they would keep such an app if it were installed on their phone automatically (opt-out regime). Support is high across all five countries, though particularly pronounced in Italy, France and the UK. Individuals who have greater trust in their government, those who are less concerned about privacy and those who use their phone regularly are most inclined to declare their intent to use a contact tracing app. The findings shed light onto the broad support for app-based contact tracing in the population while highlighting how governments and other administering bodies could maximise take-up – a crucial factor in determining such apps' potential to limit the diffusion of infections. The results were published in the Journal of Medical Internet Research mHealth and UHealth and informed contact tracing app initiatives such as the one by the NHS in the United Kingdom.

Summing up, this thesis explores how uncertainty over states of the world as well as a lack of salience or information affects dynamic choices in a variety of settings. It provides evidence that state uncertainty can cause those who are more exposed to it to have worse outcomes long term and exacerbate existing inequalities, for example

with regard to migrant native wealth inequality. It documents further that a lack of salience and information can keep individuals from adopting potentially advantageous behaviours such as investments into socio-emotional skills or campaigning across political divides, to the detriment of society at large. Yet, this thesis also shows that policies which reduce uncertainties, raise awareness over productive investments, or provide information on public opinion or health initiatives can have large effects. This suggests that there is scope for targeted policy interventions to both reduce existing inequalities and improve aggregate outcomes.

Chapter 1

Uncertainty, Citizenship & Migrant Saving Choices

In most Western countries, migrants hold significantly less wealth than natives. Migrants also face significantly more uncertainty about their future. This paper examines the central role of uncertainty over citizenship prospects and future location in explaining their saving choices. Exploiting quasi-experimental variation and panel data from Germany, I show that migrants with a right to citizenship save as much as comparable natives, while migrants without this right save 30% less. This unexplained gap is closed completely when migrants in the latter group gain access to citizenship. The effect is not driven by changes in resources, but rather willingness to save. While standard theory predicts that saving increases in uncertainty, I show that the effect can reverse if utility is state-dependent, malleable, or resources are not equally accessible across states. I build a life-cycle saving model with uncertain retirement location and heterogeneous country preferences. The model captures the ambiguous effect of uncertainty by documenting that willingness to save decreases in uncertainty over future location. I confirm the importance of this novel channel empirically, showing that migrants become significantly more likely to invest in illiquid assets if they gain certainty about their right to stay.

1.1 Introduction

In most Western countries, migrants hold significantly less wealth and save at lower rates than natives. The migrant wealth gap is large, more than $100,000 at the median, and is particularly pronounced with regard to housing and pension wealth (e.g. Muckenhuber et al. [2022], Porpiglia et al. [2011], Cobb-Clark and Hildebrand [2006], Bertocchi et al. [2022]). While temporary migrants save at higher rates than natives (Albert and Monras [2022], Amuedo-Dorantes and Pozo [2002]), migrants planning to stay long term save at lower rates[1]. Low wealth puts migrants who stay in the immigration country at a greater risk of poverty in old age (e.g. Bárcena-Martín and Pérez-Moreno [2017], Muñoz de Bustillo and Antón [2011]). It also reduces the economic prospects of the next generation, lowering health and education outcomes for migrant children (e.g. Aronowitz [1984], Tienda and Haskins [2011], Perreira and Ornelas [2011]).

The existing literature has been unable to fully account for this gap in savings. Migrants, on average, face more challenges in the labour market (Borjas [1985, 1987], Brell et al. [2020]) and take care financially of a larger group of dependants (Amuedo-Dorantes and Pozo [2006], De Arcangelis and Joxhe [2015]). Both factors may lower the fraction of income they can set aside to save. Yet, even after controlling for age, years spent in Germany, labour market outcomes, income, remittances, household constellation, education and even time-invariant unobserved heterogeneity via individual fixed effects, over a quarter of the migrant wealth gap remains unexplained.

In this paper, I analyse the role of uncertainty about citizenship prospects and the right to permanently reside in a country as a key determinant affecting migrants' saving rates. While uncertainty is one of the largest and most obvious differences between many migrants and natives, this dimension has been previously overlooked in the literature. Many migrants face substantial uncertainty over their right to stay, especially long-term. In the United States, 51% of non-naturalised migrants do not have a permanent right to stay, and in Germany, 61.2% of non-EU migrants do not have such a right. Furthermore, unlike natives, international migrants often have only limited information about life in the immigration country when they first arrive

[1] I find that in Germany migrants who plan to stay for a minimum of 10 years save at 50% the rate of natives.

(McKenzie et al. [2013]), and are uncertain about how much they will enjoy living there (Gibson and McKenzie [2011]).

Understanding migrants' saving behaviour requires moving beyond standard theory. In the presence of greater uncertainty, standard intertemporal models predict an increase in saving rates due to a precautionary saving motive. These models rely on three key assumptions: first, that resources carried into the next period are equally accessible whatever state of the world is realised; second, that utility is not state-dependent; and third that utility does not evolve over time in an unpredictable manner. In the context of understanding migrants' saving choices, all assumptions are unlikely to hold. Resources accrued in one country may not be easily accessible in another. Further, the utility of a given amount of consumption likely depends on where it is consumed. Finally, the utility derived from consumption in the immigration country likely evolves with time spent there. I term the first channel the "accessibility channel". It arises because assets, such as illiquid saving in pension plans or housing, can be country specific. Some of this illiquid saving may only be accessible after paying a transaction cost when changing countries. This lowers the return to saving when future location is uncertain. The second channel, I term the "state channel". It arises because the utility migrants derive from consumption can vary with their preference over their location. Whenever future location is uncertain and they may not be able to live in the state they prefer, this will lower the utility migrants expect to derive from any unit of future consumption. The third channel, I term the "preference channel". It arises due to uncertainty over state preferences that will only be revealed through experience and can lower expected utility even when one's future state is secure. I characterise the three channels as generating an ambiguous impact of uncertainty on saving. While uncertainty over future resources increases incentives to save (especially in liquid assets), uncertainty over future preferences or location reduces incentives to save (especially in illiquid assets). Access to citizenship lowers income uncertainty (Gathmann and Keller [2018]), uncertainty over preferences (Avitabile et al. [2014]) and uncertainty over future right to stay, affecting all three channels.

I formalise the intuitive link between location uncertainty and wealth accumulation in a three period life-cycle model with uncertain retirement location and heterogeneous country preferences. In the first two periods, everyone works in the immigration country and chooses how much to save and to consume. In the third period, agents retire in either the immigration or their home country, depending on their location choice and right to stay. Where an agent lives determines both their level and enjoyment of consumption. Agents can save in liquid and illiquid assets. The latter yield higher returns, but incur liquidation costs when sold, varying the saving stock and level of consumption migrants expect to access in either country. Agents also differ in their country preferences. While some derive higher marginal utility from consuming in the immigration rather than their home country, for others the reverse is true. Agents are risk averse and have to consider different sources of uncertainty in their choices. They face idiosyncratic shocks to their income and country preference plus a risk to their right to stay. With probability θ they will have to leave the immigration country in retirement regardless of their choice. Access to citizenship reduces θ to zero, decreases the variance of the income and the country preference process and may increase income as well as attachment to the immigration country.

The model yields several key predictions. First, it captures the ambiguous impact of uncertainty on the saving choices of risk-averse agents. Uncertainty over future income and consumption levels increases the expected value of any additional unit of future consumption, prompting higher precautionary savings. In contrast, uncertainty over future location or preferences decreases the effective rate of return and marginal utility agents expect to derive from any unit of future consumption. This prompts them to hold lower "preparatory" savings. How the overall saving rate responds to changes in uncertainty hinges on the relative importance of both saving motives. Furthermore, the two saving motives determine which saving technologies agents use. While uncertainty over future income prompts saving in liquid assets, uncertainty over future location reduces saving in illiquid assets. The model yields these and further testable implications, plus generalises to other contexts where agents have to make forward looking choices while uncertain over future paths.

I test the predictions of the model and the relevance of location uncertainty for the emergence of wealth gaps by exploiting two German reforms under which some

migrants unexpectedly gained access to citizenship. Historically, Germany had a particularly restrictive citizenship regime. Until 1990, migrants had no right to citizenship. In 1990, an unexpected reform established a formal pathway to citizenship for adults and adolescents who had legally resided in the country for 15 and 8 years respectively. A second reform in 1999 reduced the residency requirements to 8 years for all migrants.[2] These reforms thus radically altered, in an unexpected way, the uncertainty migrants faced over citizenship (Gathmann and Keller [2018]). This allows me to estimate the impact of access to citizenship on saving without bias from changing composition of the migrant pool in response to changing citizenship regimes.

I identify the causal impact of access to citizenship on migrants' level and type of saving by using a difference-in-difference analysis with multiple control groups around the 1999 reform. Using data from the German Socioeconomic Panel (GSOEP) allows me to follow individuals over time, and limit my sample to migrants who lived in Germany before the reforms. Pre-reform, migrants without access to citizenship save less than 3% of their net income — substantially less than migrants with that right or natives who save 7% and 11.5% respectively.[3] Controlling for differences in age, income, labour market outcomes, education, household constellation and time plus individual level fixed effects narrows, but does not close the gap. While migrants with the right to citizenship become indistinguishable from natives, those without still save 30% less. That gap is entirely driven by differences in access to citizenship: I show that gaining access to citizenship causally increases the saving rate of migrants by 2.5pp — completely and permanently[4] closing the unexplained gap. The saving measure includes saving in Germany as well as abroad. The effect thus represents an overall increase rather than a shift from one country to another.[5]

Next, I analyse the impact of access to citizenship on migrants' long term investments. These investments might be differentially affected depending on whether

[2]The first reform followed a supreme court ruling on electoral discrimination while the second came on the heels of an upset electoral victory. Both were unexpected and not on the parliamentary calendar a year before.

[3]I focus on a sample of households headed by either natives or migrants from outside the EU, where non of the partners has German citizenship (and thus access to an indefinite right to stay).

[4]I follow migrants up to 15 years in the GSOEP and see no drop off in their savings rates.

[5]Indeed, I find that the increase in saving is not financed out of a decrease in transfers sent abroad, which are unchanged by the reform. I find that eligibility for citizenship reduces remittances send for the purpose of saving by 1pp. That shift appears driven by the extensive margin and implies that migrants' saving rate in Germany actually increased by about than 3.5pp.

migrants became eligible expectedly, within a given citizenship regime, or unexpectedly, through a reform. I leverage the time varying, exogenous shifts in expected and unexpected eligibility and compare the behaviour of earlier and later cohorts of migrants. I find that migrants become 6pp more likely to own their home, 7pp more likely to own a mortgage saving plan and 12pp more likely to own a private pension plan. They also become more likely to invest in life insurance and firm capital, as well as hold a bank account. This change appears not driven by an increase in the resources migrants have at their disposal. I find no changes in migrants' employment and income in response to becoming eligible for citizenship.[6] I also find no impact on credit uptake[7]. Instead, I find evidence that migrants, in the lead up to these investments, build up liquidity through their own savings.

The results show that gaining access to citizenship does not change migrants' resources but rather their willingness to save, providing strong evidence that uncertainty over future states, unlike uncertainty over future resources, lowers incentives to save. The fact that investments in country specific assets such as pension plans increase the most shows that accessibility of future resources is an important factor for migrants' saving choices. Yet, since investments in country-invariant illiquid assets such as firm shares and liquid savings increase as well, it is not the only factor. Instead, it appears that access to citizenship increases migrants' general valuation of future consumption by raising the marginal utility they expect to derive from any level of future consumption.

Access to citizenship can raise the marginal utility migrants expect to derive from future consumption in two ways . First, it eliminates the risk of being asked to leave the immigration country in retirement (the state channel). Second, it can increase the attachment migrants feel to the immigration country and make them more certain over the quality of life they can expect there (the preference channel). If the preference channel is the driving force, it should affect temporary and permanent migrants alike and intentions to stay should shift upwards along the entire distribution. However, I find that migrants who want to stay short term are unaffected. Migrants planning

[6]This is in line with findings by Gathmann and Keller [2018] who find that only female migrants benefited from access to citizenship in the labour market, and only to a limited degree.

[7]The data does not allow me to look at the accessibility of credit options separately. Yet, the null effect on credit uptake I find is in line with findings from Hainmueller et al. [2023] of a null effect on credit access.

to stay long term become more intent on doing so.[8] Furthermore, I find that only migrants from outside the EU increase their saving and investments. EU migrants, who have a secure right to stay irrespective of German citizenship, are unaffected even though they could still see their preferences changed. This suggests that uncertainty over location induced by uncertainty over right to stay, rather than uncertainty over one's own future location choice, is the main driver behind the observable changes.

My findings have immediate policy implications. Most factors driving the wealth inequality between migrants and natives are systemic and hard for governments to shift (for example, labour market discrimination). In contrast, governments have direct control over citizenship legislation — giving them an instrument to increase migrants' saving choices.[9] This insight is particularly important amidst demographic change. As Western countries grow older, they increasingly reduce state pensions and require constituents to prepare for retirement privately.[10] This makes private wealth an increasingly strong predictor of economic security in retirement, and a lack of wealth a predictor of precarity (Poterba [2014]). At the same time, many governments try to counteract the effects of their ageing workforce by attracting skilled foreign professionals (Bonin et al. [2000]). Yet, the extent to which immigration can stabilise pension systems hinges on the ability of migrants to accumulate wealth and support themselves in old age (Gustafsson et al. [2019]).

This paper contributes to several strands of literature. First, it contributes to the migration literature, specifically the strand on migrant/native wealth inequality. There is robust evidence that migrants own significantly less wealth than natives across Western countries (e.g. Borjas [1987], Cobb-Clark and Hildebrand [2006], Gibson et al. [2007], Sinning [2007], Doiron and Guttmann [2009], Bauer et al. [2011], Porpiglia et al. [2011], Halbmeier [2019], Ferrari [2020]). To explain this gap, the

[8] Migrants who report wanting to stay in Germany "for several years" become 8pp less likely to do so. This drop is matched almost exactly by an increase in migrants saying they want to stay "indefinitely".

[9] In theory, an unconditional, indefinite right to stay should be enough to eliminate the detrimental impact of uncertainty. However, in practice even indefinite rights to stay are tied to conditions such as not leaving the immigration country for more than a set number of years. This reintroduces uncertainty over still having this right to stay many years into the future. An exception (at least in this period, before the UK left the European Union) is the right to stay conferred though inner EU migration. In line with this, EU migrants seem indeed unaffected by access to citizenship.

[10] Most countries in continental Europe now have a three pillar pension system where pay-as-you-go state pensions have to be supplemented by fully funded occupational and private pensions. In most English speaking countries such as the UK, the US or Australia, the role of private pensions is even more pronounced as the basic pensions are low and have to be accompanied by private plans.

literature has explored systemic differences in income and remittances. Migrants tend to have lower income levels than natives due to differential labour market outcomes (e.g. Chiswick [1978], Borjas [1985, 1994], Duleep and Regets [1996, 2002], Abramitzky et al. [2012], Brell et al. [2020]). They are also more likely to remit money to their country of origin, although this is driven by temporary migrants (e.g. Galor and Stark [1990], Dustmann [1997], Amuedo-Dorantes and Pozo [2006], Rapoport and Docquier [2006], De Arcangelis and Joxhe [2015]). Yet, even when accounting for these and other factors, about a third of the gap in wealth accumulation rates remains persistently unexplained. I contribute by identifying a novel factor that explains the residual saving rate gap, and a policy measure that can close it. Furthermore, I add to the growing literature that explores how expected duration of stay shapes migrants' choices (e.g. Kirdar [2012], Nakajima [2015], Dustmann and Görlach [2016], Kovak and Lessem [2020], Adda et al. [2022], Albert and Monras [2022]).

The paper also contributes to the literature on the impact of citizenship. Most of this literature finds positive effects on migrants' social integration (e.g. Avitabile et al. [2013, 2014], Hainmueller et al. [2016, 2017a], Felfe et al. [2020], Gathmann and Garbers [2023]). However, it finds more muted and heterogeneous effects on labour market outcomes (e.g. Adamopoulou and Kaya [2020], Steinhardt [2012], Gathmann and Keller [2018], Hainmueller et al. [2019, 2023]. I show that access to citizenship can yield large economic gains through saving effects. I further add to the literature by examining the drivers of the impact of access to citizenship. Hainmueller et al. [2017b], Dahl et al. [2022], Hainmueller et al. [2023] show that access to citizenship can impact identity concerns and mental health of migrants. I show that uncertainty over future utilities determines its dynamic effects.

Third, the paper contributes to the household finance literature. The life-cycle saving literature has robustly established that in the presence of income uncertainty, individuals save at higher rates due to a precautionary motive (e.g. Zeldes [1989], Deaton [1992], Hubbard et al. [1995], Gourinchas and Parker [2002, 2001], Parker and Julliard [2005], Low [2005], Blundell et al. [2008], Low et al. [2010], Carroll et al. [2021]). I show that the unambiguously positive impact of uncertainty on saving hinges on the assumption that marginal utility derived from consumption is state

independent and resources are equally accessible across states. Whenever either of the two assumptions fails, uncertainty can have a negative impact on saving by reducing incentives to shift consumption into said future. The firm and business cycle literature has shown that high levels of uninsurable "disaster risk" can discourage firms from investing (e.g. Bloom et al. [2007], Bloom [2009], Gourio [2012], Koijen et al. [2016], Gourio [2013], Bachmann and Bayer [2013]). However, these dynamics are driven by the irreversibility of investments only and rely on aggregate uncertainty measures (e.g. Guiso et al. [1996], Guiso and Paiella [2008], Baker et al. [2016]). By tracing the impact of preference and location uncertainty I contribute to the growing literature exploring personal sources of uncertainty (e.g. Low and Pistaferri [2015], Caldwell et al. [2022]).

The paper also contributes to the literature on retirement preparedness which has found pronounced heterogeneity in pension wealth (e.g. Cutler et al. [1990], Dynan et al. [2004], Scholz et al. [2006], De Nardi and Fella [2017], Crawford and O'Dea [2020]) and shown that this stems in part from differential rates of return to wealth (e.g. Benhabib et al. [2015], Fagereng et al. [2016], Cioffi [2021]). I contribute to this literature by identifying a new driver of differential returns to wealth. I further show that the pension incentive scheme of the German government exacerbates existing inequalities due to the country specificity of pension benefits. This adds to the nascent literature on the negative distributional consequences of retirement policies (e.g. Choukhmane et al. [2022]).

The rest of the paper is organised as follows: Section 1.2 develops the theoretical framework and derives testable predictions. Section 1.3 describes the empirical setting. Section 1.4 presents evidence on the migrant/native saving rate gap and how it is shaped by access to citizenship. Section 1.5 explores the different channels through which the effect could operate. Section 1.6 discusses the aggregate implication of the results and Section 1.7 concludes.

1.2 Theoretical Framework

Migrants have to make saving choices while accounting for different, intersecting sources of uncertainty. In the following, I develop a stylised life-cycle model with uncertain retirement location, heterogeneous country preferences and variation in access to citizenship. The model captures documented saving patterns in the migration and household finance literature. Crucially, it further allows me to derive novel, testable predictions about the forces driving these patterns. First, I detail the model set up. Then I lay out the theoretical predictions and their empirical implications. In particular, I describe how the different types of uncertainty generate both a precautionary and a "preparatory" saving motive, and how the two affect saving choices in opposite directions. All formal proofs can be found in Section 1.A in the Appendix.

1.2.1 Life Cycle Model with Uncertain Retirement Location

I specify a three period life-cycle saving model, to capture migrants' decision problem. In the first two periods, the working age, all agents live and work in the immigration country.[11] In the third period, agents retire and live off their savings, either in the immigration or their home country. Where agents spend their retirement depends on both their choice as well as their legal right to stay.

Agents maximise the expected discounted sum of utility from the remaining part of their life by choosing how much to consume and save for the future, in which saving technology to invest, and where to retire. I let $s = 1$ denote when migrants decide to stay in the immigration country in retirement, and $s = 0$ if they decide to return to their home country[12]. I allow for two different types of saving technology, liquid and illiquid assets. I let b_t for $t = \{1, 2, 3\}$ denote the amount migrants save in the liquid asset which can be thought of as bank deposits. I let a_t^J for $J = \{I, H\}$ denote the amounts migrants save in illiquid assets in the immigration and their home country

[11] I do not model migrants' decision to migrate. For an analysis of how this decision varies with different, expected right to stay, see Adda et al. [2022].

[12] For simplicity, I only allow for two retirement locations, the immigration and migrants' home country in this version. However, the mechanisms laid out here easily generalise to a setting with more than two country options.

respectively. Those can be thought of as government sponsored pension plans[13] for example. Finally, I let c_t^J denote the consumption levels of migrants in either country.

Agents have to take into consideration a variety of exogenous state variables when making their choices. I denote agents' attachment to their home country by η_t and their income during their working age by y_t. r_t and q_t denote the rates of return on liquid and illiquid assets, and k_t denotes liquidation costs which can be incurred through the sale of an illiquid asset. Finally, I let θ denote the (perceived) risk to migrants' right to stay.

Preferences

Agents derive utility solely from consumption. How much utility exactly they derive from consumption in either country $J = \{I, H\}$ in a given period $t = \{1, 2, 3\}$ is given by:

$$u_t^I(c_t^I) = \frac{(1-\eta_t)^{1-\alpha}}{(1-\alpha)} * \frac{(c_t^I)^{1-\rho}-1}{(1-\rho)}; \qquad u_t^H(c_t^H) = \frac{\eta_t^{1-\alpha}}{(1-\alpha)} * \frac{(c_t^H)^{1-\rho}-1}{(1-\rho)}. \quad (1.1)$$

The second component in both expressions is a standard constant relative risk aversion (CRRA) value of consumption where $\rho \geq 0$ denotes the CRRA coefficient. The first component measures how much migrants enjoy consumption in the immigration or their home country and is determined by their attachment to the latter.

I assume that migrants differ in their preference for their home vis-à-vis the immigration country. For example, they can have a different appreciation for weather conditions, friends and family networks or a country's cuisine. At the beginning of their life, migrants' attachment to their home country is determined through a random draw $\eta_1 \in U[0, 1]$. Whenever migrants' attachment to their home country $\eta_t > 0.5$, they will derive more utility from consumption in their home country than they would from consumption in the immigration country. If $\eta_t < 0.5$ the reverse is true, and if $\eta = 0.5$ they are indifferent.

Since I assume that agents are risk averse with regard to consumption, I also allow them to be risk averse with regard to their country preference and thus the

[13] For example, in Germany the government subsidises saving in so-called "Riester" pension plans – yet asks for these subsidies back if people move away from Germany in retirement.

effective utility they will derive from consumption. I assume that $\alpha \geq 0$. This nests the specific case where agents are risk neutral with regard to preferences. Further, I allow risk aversion over preferences to differ from risk aversion over consumption: $\alpha \lessgtr \rho$.[14] This nests the specific case where $\alpha = \rho$ and relative changes in preferences or consumption have the same impact on experienced utility.

This attachment is not static, but subject to idiosyncratic shocks. Having moved to a new environment, migrants may not be sure how much they will enjoy living there. Hence, unforeseeable events such as forming a friendship or having a xenophobic interaction may impact how their preference for the immigration (as well as their home country) evolve:

$$\eta_t = \eta_{t-1} - \chi + \epsilon_t \tag{1.2}$$

where $\epsilon_t \sim U[-\iota\lambda, \iota\lambda]$ with $\lambda = \min\{\eta_{t-1}, (1-\eta_{t-1})\}$ and $\iota \in [0,1]$. λ denotes the maximum absolute value of the shock. ι is a scaling parameter. Generally, I assume $\iota = 1$. However, when access gain access to citizenship, I allow $\iota < 1$, reducing the variance of the shocks. In a given period t, migrants are equally likely to feel more or less attached to their home country than they did in the preceding one. As η_t is bound to the unit interval, λ and thus the size of the shocks decreases in the greater the strength of an individual's country preference.[15] While this process is in line with current evidence on identify formation (Bénabou and Tirole [2011], Bhugra and Becker [2005], Hogg [2003]), all theorems and proofs hold equally in the population if I allow for asymmetric preference shocks that do not decrease in strength of attachment. Adda et al. [2022] also model heterogeneous and time variant country preferences. Unlike them, I assume that preferences stabilise the more pronounced they are. Like in Adda et al. [2022], agents' preferences determine their return intentions. Hence, uncertainty over future preferences also implies uncertainty over future location.

χ denotes a level shift in attachment. In general, I assume that $\chi = 0$ and thus that preferences do not systematically move into a specific direction. However, if agents gain access to citizenship I allow $\chi < 0$. That is, I allow access to citizenship

[14] The only condition is that whenever $\rho \geq 1$, $\alpha \geq 1$ as well, and whenever $\rho < 1$, $\alpha < 1$. This is to ensure that agents who are more attached to their home than the immigration country, prefer consumption there over the immigration country (and vice versa).

[15] The variance collapses to zero if an individual is fully certain over their attachment.

to decrease the attachment to their home country and increase their enjoyment of consumption in the immigration country.

Resource Constraint

Migrants' choices must satisfy their intertemporal budget constraint:

$$\begin{aligned} c_1^I + a_1^I + a_1^H + b_1 &= y_1^I \\ c_2^I + a_2^I + a_2^H + b_2 &= y_2^I + (1+r)b_1 \\ c_3^I &= (1+r)b_2 + (1+q^I)(a_1^I + a_2^I) + [(1+q^H) - k^H](a_1^H + a_2^H) \\ c_3^H &= (1+r)b_2 + [(1+q^I) - k^I](a_1^I + a_2^I) + (1+q^H)(a_1^H + a_2^H) \end{aligned} \tag{1.3}$$

where c_t^I and c_3^H denote levels of consumption in period t in the immigration or the home country respectively. I denote by b_t, a_t^I, a_t^H the amounts held in the liquid as well as illiquid assets in the immigration and home country respectively. Finally, r and q^J for $J = \{I, H\}$ denote the rates of return to the liquid as well as the illiquid assets in either country, while k^J denotes the liquidation cost that is incurred when agents sell an illiquid asset in either country. For tractability, I do not allow agents to go into debt.[16]

Agents are able to access funds held in the liquid asset at any point during their working age or retirement. The rate of return for liquid savings is $r \geq 0$. In contrast, agents can only access the sum of their savings in the illiquid assets in retirement.[17] I assume that the rate of return is greater for illiquid savings in either country than the one on the liquid savings: $0 < r < q^J$ for $J = \{I, H\}$.

However, I also assume that illiquid assets are country specific and that they incur a transaction cost k^J when they are liquidated. The bulk of private wealth in retirement is made up of pension plans and housing wealth. Both of these asset categories require a long term commitment to be profitable. In many countries,

[16] Empirically, credit uptake does not appear to be a driver of migrants' and natives' differential saving trajectories. It is also not affected by migrants gaining access to citizenship. Hence, I capture variation in the resources migrants have at their disposal through variation in income only.

[17] This is akin to how (government sponsored) pension plans are set up in many countries where individuals are either not able to withdraw money before maturation at all (e.g. NEST pension scheme in the UK) or only under heavy monetary penalties (e.g. Riester pension scheme in Germany).

pension plans are subsidised by the government, making them particularly profitable saving technologies. However, they also incur high liquidation penalties if individuals withdraw money prior to their retirement or draw the pension outside the subsidising country. Similarly, housing has high returns for a majority of investors (Fagereng et al. [2020]), but if the property is held for a sufficiently long time horizon. In the model, agents will only receive the full rate of return q^J on an illiquid asset if they retire in the same country where that asset is held. Whenever, upon retirement, they have illiquid savings outside of their country of residence, they have to liquidate these savings to access them. This incurs the liquidation cost k^J, meaning that their effective rate of return on these savings is not $(1+\mathrm{q}^J)$ but $((1+\mathrm{q}^J)\text{-}\mathrm{k}^J)$[18].

In the main specification of the model, I assume that $\mathrm{q}^I = \mathrm{q}^H$ and $\mathrm{k}^I = \mathrm{k}^H$. This is to capture the general case where migrants come from anywhere in the world, from sending countries which greatly vary in their financial systems and interest rates. In the general case, the differences will average out. Furthermore, since liquidation costs already introduce a wedge in effective rates of return by country, the main specification captures the primary mechanism of country specific rates of returns (and liquidation costs). The framework allows for $\mathrm{q}^I \lesseqgtr \mathrm{q}^H$ and $\mathrm{k}^I \lesseqgtr \mathrm{k}^H$ and all theorems and proofs follow. Only exact cut off values will vary.[19]

Finally, y_t^I denotes agents' income in period t. Migrants generally face higher levels of income uncertainty than natives do (Duleep and Regets [2002, 1996], Borjas [1994, 1985], Chiswick [1978]). They are more likely to be on short term contracts, earn lower wages and often have no access to secure public sector jobs. Hence, income is determined exogenously, but subject to shocks.

$$\mathrm{y}_2^I = \mathrm{y}_1^I + \nu_t \tag{1.4}$$

where $\nu_t \sim \mathrm{N}(\mu, \sigma_y)$. In general, I assume that $\mu = 0$, and thus that there are no systematic trends in migrants income. Only if migrants gain access to citizenship,

[18] Again, this is similar to the stipulations of many pension schemes. In Germany, individuals have to pay the government the sum of all tax breaks and subsidies that have received on their Riester pension savings if they move abroad in retirement and wish to access the funds.

[19] Migrants may also face greater uncertainty over the rates of return to their investment. However, if macroeconomic shocks are uncorrelated, they may also be able to more effectively hedge against rate of return fluctuations than natives by investing in both the immigration and their home country (Dustmann [1997]).

I allow income to be positively affected $\mu > 0$. This is in line with evidence from Gathmann and Keller [2018].

Optimisation Problem

Migrants maximise their expected lifetime utility given by the sum of discounted expected utilities in their working age and retirement:

$$\begin{aligned}\max_{\mathrm{s},\mathrm{b}_1,\mathrm{b}_2,\mathrm{a}_1^H,\mathrm{a}_1^I,\mathrm{a}_2^H,\mathrm{a}_2^I} \mathrm{V}(\mathrm{c}_1,\mathrm{c}_2,\mathrm{c}_3) \;=\; & \underbrace{\mathrm{u}_1^I(\mathrm{c}_1^I) \;+\; \beta\, \mathrm{E}[\mathrm{u}_2^I(\mathrm{c}_2^I)]}_{\text{Working Age Utility}} \\ & +\; \beta^2\, (\underbrace{(1-\theta) * \max\,\{\mathrm{E}[\mathrm{u}_3^I(\mathrm{c}_3^I;\mathrm{s}=1)], \mathrm{E}[\mathrm{u}_3^H(\mathrm{c}_3^H;\mathrm{s}=0)]\}}_{\text{Retirement Utility in Country of Choice}} \\ & +\; \underbrace{\theta\, \mathrm{E}[\mathrm{u}_3^H(\mathrm{c}_3^H;\mathrm{s}=0/1)])}_{\text{Retirement Utility When Having to Leave}}\end{aligned} \tag{1.5}$$

subject to the intertemporal budget constraint given in 1.3. In the model, $0 \leq \beta \leq 1$ denotes agents' discount factor while s denotes their choice over whether or not they want to stay in the immigration country. If they choose to (try to) stay in the immigration country $\mathrm{s} = 1$. If instead they choose to return to their home country, $\mathrm{s} = 0$. Neither choice incurs costs beyond the liquidiation costs captured in 1.3.[20] $0 \leq \theta \leq 1$ denotes the (perceived) risk to migrants' right to stay in the immigration country in retirement. With probability $1-\theta$ migrants are free to retire in the country of their choosing. With probability θ they will have to leave the immigration country irrespective of their location choice. Migrants often have only a temporary right to stay. Even if they have a permanent one, it is frequently tied to conditions such as not leaving the country for prolonged periods of time.[21] I assume that agents have rational expectations over their future income and attachment to their home country and that they observe the values of all state variables in a given period before making their choices. Migrants who anticipate that in retirement they will want to stay in the

[20] While applying for citizenship does incur monetary costs, the empirical focus of this chapter is the *right* to citizenship which is costless.

[21] The uncertainty is even more pronounced for undocumented migrants. In Germany, official statistics assume that in 2016 between 180,000 and 520,000 lived in the country without documents (Vogel [2016]). That is 0.9% - 2.8% of all direct migrants living in Germany in 2016. In the US, more than 11 million persons, almost a quarter off all direct migrants, live in the country without documents (Borjas [2017]), making this a more salient margin.

immigration country due to $\mathrm{E}[\mathrm{u}_3^I(\mathrm{c}_3^I; \mathrm{s}=1)] > \mathrm{E}[\mathrm{u}_3^H(\mathrm{c}_3^H; \mathrm{s}=0)]$, have to factor into their working age saving choices in that with probability θ they will have to leave. Note, that θ can be interpreted as a perceived risk to right to stay and need not be an accurate belief.[22]

Access to Citizenship

I assume that agents can gain access to citizenship at the beginning of period 1. This effects their decision problem in three ways. First, if agents gain access to citizenship, the risk they perceive to their right to stay θ reduces to 0. Second, I assume that gaining access to citizenship reduces variance in future income σ_y and can raise expectations over future income by shifting the expected value of the income shock from $\mu = 0$ to $\mu > 0$. This is in line with empirical findings that gaining access to citizenship can improve earnings prospects (Gathmann and Keller [2018], Hainmueller et al. [2019]). Third, I assume that access to citizenship reduces the variance of shocks to country preferences by reducing the scaling parameter ι. Finally, I allow for access to citizenship to increase expected attachment to the immigration country, which in this framework is equivalent to reducing attachment to the home country, lowering $\chi = 0$ to $\chi < 0$. This is in line with empirical evidence that gaining access to citizenship fosters interaction with natives and increases migrants' propensity to learn the language and engage with the culture of the immigration country (Avitabile et al. [2013], Hainmueller et al. [2017a], Gathmann and Monscheuer [2018]).

1.2.2 Implications

The model yields a variety of predictions about migrants' saving choices in a situation where their retirement location as well as their level and enjoyment of future consumption is uncertain. It further allows me to derive testable implications about the existence and relative importance of the different channels driving these choices. In the following, I lay out the theoretical predictions, the intuition behind them, and their empirical implications. First, I describe the model predictions with regard to

[22] May Khourshed and Méango [2021] document that refugees living in Germany overestimate the risk of having to leave Germany by 20pp.

the differential saving behaviour of temporary and permanent migrants, and the impact of resource uncertainty. These confirm findings in the migration and household finance literature respectively. Then, I describe which consequences the model predicts for uncertainty over future states of the world, as well as how to test them. All formal proofs can be found in Section 1.A in the Appendix.

Temporary and Permanent Migrants

Implication 1: *Migrants who want to stay in the immigration country only for a short period of time save at greater rates than migrants wanting to stay long term.*

In the model, there are no restrictions on migrants' ability to return to their home country.[23] Hence, every agent who derives greater utility from retiring in their home rather than the immigration country will be able to do so. All else equal, these will be agents who are relatively attached to their home country, $\eta_t > 0.5$. Absent any uncertainty, these agents will save in only the illiquid home country asset a_t^H, as this yields the highest returns for them. If there is income uncertainty, they will also save in the liquid asset in period 1 b_1 to smooth consumption in period 2. In retirement, they will always choose to follow through on their initial intention to leave as $u_3^H(c_3^H) > u_3^I(c_3^I)$ for them. For agents who are relatively more attached to the immigration country, $\eta_t < 0.5$ the same pattern holds, but they will invest in the illiquid asset in the immigration country a_t^I. For them, $u_3^H(c_3^H) < u_3^I(c_3^I)$, so they will try to stay.

Migrants with $\eta_t > 0.5$ always save more than migrants with $\eta_t < 0.5$. This is because for the former group $u_t'^H(\bullet) > u_t'^I(\bullet)$ which means that for them consumption in retirement will yield greater marginal utility than consumption during their working age — prompting them to move it into the future. The effect will be larger, the stronger migrants' attachment to their home over the immigration country is, i.e the larger $\frac{\eta_t}{(1-\eta)}$. This prediction is in line with and provides an explanation for empirical findings that temporary migrants save at higher rates than permanent ones (De Arcangelis and Joxhe [2015], Amuedo-Dorantes and Pozo [2006]).

[23] Thus, the model does not capture the experience of displaced persons who would like to return to their home country but are unable to do so due to political persecution, conflict, or other reasons.

Empirically, the model prediction holds if migrants who want to stay in the immigration country maximally for a few years save at higher rates than comparable migrants who want to stay for an indeterminate amount of time.

Resource Uncertainty

Implication 2 (Precautionary Saving Motive): *Uncertainty over future resources increases agents' propensity to save. The effect is particularly strong for liquid assets.*

Agents allocate resources to where they yield the greatest marginal utility. Under CRRA utility, marginal utility decreases in consumption levels. Consequently, uncertainty over future consumption levels increases the expected marginal utility of said consumption, prompting agents to increase saving and shift consumption into the future. Intuitively, they insure against future consumption fluctuations through saving. This is the precautionary saving motive (Carroll et al. [2021], Gourinchas and Parker [2002], Hubbard et al. [1995], Zeldes [1989]).

In this framework, uncertainty over future resources derives from uncertainty over future income y_2^I. Greater variance in income σ_y increases the propensity of agents to invest in both liquid and illiquid assets, although the positive effect is greater for liquid savings which agents can draw from in both the second and the third period. The effect is greater, the more risk averse and patient agents are, i.e. the greater γ and β. It is also greater if they intend to leave the immigration country in retirement and rises in η for this group. This is because migrants intending to leave derive higher levels of marginal utility from retirement relative to working age consumption.

Empirically, if the precautionary saving motive is the dominant one, migrants should save at higher rates than comparable natives[24]. They should hold a larger proportion of their savings in liquid assets than natives do. Controlling for differences in current and past employment should lower the (positive) gap. Finally, giving migrants access to citizenship should decrease their saving rate, with a particularly strong effect on their propensity to hold liquid assets.

[24]That is, natives who differ neither in their resources (such as labour market outcomes or number of dependants), nor in their non-uncertainty related willingness to save (such as education or preferences captured through individual fixed effects).

Uncertainty over States of the World

The precautionary saving motive captures individuals' impetus to insure against undesirable fluctuations in future resources. However, individuals are commonly also motivated to save, because they want to prepare for a specific future they hope comes to be. Standard intertemporal models assume that agents' utility function is independent of state and remains the same over time. They further assume that resources saved in the present are equally accessible across future states. In such models, even if agents are uncertain over future states (e.g. over their future family status, health status, or location), this does not impact their saving choices. Yet, in reality we would expect that agents' enjoyment of consumption or their ability to draw on resources does vary with the state they find themselves in. And that they will only want to save for the future and give up present consumption, if they are reasonably certain they will be in a position to access and enjoy these savings. This is why uncertainty over future states of the world, unlike uncertainty over future resources, can decrease individuals' propensity to save.

Empirically, migrants should save at *lower* levels than comparable natives if the impact of uncertainty over future states of the world outweighs the impact of resource uncertainty. The greater the negative residual gap, the stronger the impact of the uncertainty over future states of the world. What is more, access to citizenship will *increase* migrants' saving rates whenever it reduces uncertainty over future utility and accessibility of resources by more than it reduces uncertainty over resources. If access to citizenship strongly reduces or even closes a previously detrimental gap, this would be evidence of uncertainty over future states of the world being particularly important in this context.

Uncertainty over future states of the world can affect saving choices through three distinct channels:

Implication 3 (State Channel): *When utility is country specific, uncertainty over future location lowers the marginal utility agents expect to derive from any level of consumption. This lowers their propensity to save across assets.*

As with the precautionary saving motive, this channel arises because agents want to shift consumption to the period and location where it yields the highest marginal utility. If agents are indifferent between countries, uncertainty over location has no bearing on the marginal utility they expect to derive from consumption in the future. However, whenever agents prefer one country over another, uncertainty over future location also means uncertainty over expected utility. Assuming that in the case without uncertainty agents will choose to retire in the location they prefer, uncertainty over location always lowers the marginal utility they expect to derive from future consumption — and thus their incentive to save. This is true even when agents are risk neutral.

In the model, the state channel derives from uncertainty over future right to stay θ. Greater uncertainty over future right to stay induced by a positive $\theta > 0$ reduces the propensity to save for agents intending to stay and does so even if $\alpha, \gamma = 0$. However, the effect is stronger, the greater α and the greater the country preference differential $\frac{(1-\eta_t)}{\eta_t}$. This is because the potential loss of future marginal utility when having to leave is more pronounced.

Empirically, if the state channel is a driving force, gaining access to citizenship should increase migrants' propensity to save across saving technologies as their overall valuation of future consumption increases. Hence, they should become more likely to save both in liquid assets such as bank deposits as well as illiquid assets such as life insurance. Furthermore, since the empirical focus of this paper is Germany, if uncertainty over future right to stay is an important factor in migrants' saving choices, then gaining access to citizenship should increase the saving rates of migrants from non-EU countries by more than it increases those of migrants from within the EU. This is because migrants in the latter group have an indefinite right to stay in Germany regardless of German citizenship.

Implication 4 (Preference Channel): *Uncertainty over the evolution of preferences, lowers the marginal utility agents expect to derive from any level of consumption. This lowers their propensity to save across assets.*

The state channel shows that whenever preferences are state dependent, undesirable uncertainty over future state reduces agents' propensity to save. However, even if future location is known to agents and shocks to their country preferences can also be positive, uncertainty over said preferences will decrease their incentives to save. When preferences evolve over time in an unpredictable manner and are only revealed with experience, the prospect of a negative preference looms larger for risk averse agents than the potential of a positive shock. Consequently, uncertainty over future preferences lowers the marginal utility agents expect to derive from any level of level of consumption. This prompts them to shift consumption into the present.[25]

In this framework, the preference channel derives from uncertainty over future country preference η_t. Greater variance in η_t induced by a greater ι reduces the propensity to save for all agents whenever they are risk averse, $\alpha, \gamma > 0$. The effect is stronger, the greater α.

Empirically, just as for the state channel, if the preference channel is a crucial factor, gaining access to citizenship should increase migrants' propensity to save across saving technologies as their overall valuation of future consumption increases. However, unlike for the state channel, migrants from EU countries should increase their saving rates upon gaining access to citizenship if they previously saved less due to uncertainty over the evolution of their preferences. While migrants from EU countries should be unaffected in their right to stay, gaining access to citizenship should still increase their interactions with natives and experiences in the immigration country. Furthermore, if access to citizenship is seen as a sign of goodwill that increases migrants' attachment to the immigration country, this too should affect EU and non-EU migrants alike. Such a preference shock should also affect both temporary and permanent migrants, prompting an increase in the desired length of stay in the immigration country across the initial distribution of intended durations of stay.

Implication 5 (Accessibility Channel): *When assets are country specific, uncertainty over future location lowers the rates of return agents expect from investments.*

[25] In that, it is also different from uncertainty over future rates of returns which can decrease saving if the substitution effect is dominant — but for a risk averse agent can also increase saving if the income outweighs the substitution effect.

This lowers their propensity to save in these assets.

If agents can access their entire savings equally regardless of the country they live in or the saving technology they use, location uncertainty has no bearing on expected rates of return or investment choices. Similarly, liquidation costs have no impact on agents' saving level if agents are certain about their future location. However, as soon as agents are uncertain over their eventual location, liquidation costs make country specific investments partially irreversible. In expectation, this lowers their rates of return, making them a less attractive ex-ante investment.

In the model, it is optimal for agents to hold at least part of their savings in the illiquid asset of the country they intend to retire in, as this saving technology yields the highest rate of return $0 < \mathrm{r} < \mathrm{q}^J$ for $\mathrm{J} = \{\mathrm{I}, \mathrm{H}\}$. However, when future location is uncertain, the liquidation cost a move would incur lowers the rate of return agents expect on the illiquid asset. For small probabilities of switching country P(switch), agents still invest in the illiquid assest of their country of choice, albeit at lower levels. For $\mathrm{k} > \mathrm{q} - \mathrm{r}$ and medium level of P(switch), agents shift saving the liquid asset which has path independent rate of returns. For high P(switch), they shift to saving in the illiquid asset of the country they did not initially intend to retire in.[26] For $\mathrm{k} < \mathrm{q} - \mathrm{r}$, agents shift from illiquid saving in one country to that in the other as soon as they pass the probability cutoff. In general, the higher the liquidation cost k, the greater is the probability range for which agents choose to save only in the liquid asset. The exact values of P(switch) depend on agents' attachment to either country captured in η_t, their risk aversion captured in α and γ, and the ratio of the rates of return on the illiquid and the liquid asset $\frac{\mathrm{q}}{\mathrm{r}}$. In all cases, agents cumulative saving rate falls in uncertainty over future location, as attainable rates of return are reduced.

Empirically, if the accessibility channel is important, migrants should hold lower levels of their saving in illiquid assets such as pension plans or housing than natives do. Gaining access to citizenship should increase saving in these assets by more than it increases saving in either liquid (e.g. bank deposits) or transferable illiquid assets (e.g. firm shares). If uncertainty over future location impacts saving *only* through

[26] Since shocks to country attachment η_t can be either positive or negative, the resulting P(switch) will only ever be small. In contrast, the perceived risk to right to stay θ can take any value and thus induce even a high P(switch).

the accessibility channel, migrants will hold a much higher fraction of their savings in liquid rather than illiquid assets with the overall saving amount only slightly below that of natives. In this case, access to citizenship will increase saving in illiquid assets, while decreasing savings held in liquid assets.

Welfare Implications. In general, uncertainty over future income, preferences or right to stay can reduce the welfare of an individual agent by inducing an ex-ante consumption/saving allocation that will turn out to be suboptimal ex-post. If investments are partly country specific, uncertainty over right to stay can reduce individuals' welfare even more substantially. In the model, if an agent wants to retire in the immigration country, but believes that with a high probability P(switch) they will have to leave, they should invest in illiquid assets in their home country. Since these investments are partially irreversible, they lock agents into their country choice. At the beginning of their retirement in period 3, agents decide where they would like to retire by comparing the consumption utility they can attain in either country. At similar wealth levels, an agent with a strong attachment to their home country will want to return there, while an agent who prefers the immigration country will prefer to stay in the there. However, with liquidation costs there exists a cutoff wealth differential for illiquid assets held in either country $\Delta = (\mathrm{a}_1^H + \mathrm{a}_2^H) - (\mathrm{a}_1^I + \mathrm{a}_2^I)$ where if Δ is positive and large enough, even agents with a preference for the immigration country will choose to leave. In this case, the experienced utility that agents lose compared to a scenario where they had location certainty from the beginning substantially increases the ex-post welfare loss resulting from their ex-ante decisions. The exact Δ depends on agents' country preference at the time of the location decision η_3, the curvature parameters of their utility function α, γ, as well as their overall saving stock. Δ will be smaller, the larger the liquidation cost k is. The welfare loss of retiring in a less preferred country will be larger, the greater the preference differential $\frac{(1-\eta_3)}{\eta_3}$.

1.3 Empirical Setting

The empirical part of this paper focuses on Germany. The country provides an ideal setting to study migrants' saving choices and how they are shaped by uncertainty

about their future and the availability of a pathway to citizenship due to two intersecting natural experiments. I leverage the resulting exogenous variation to circumvent selection and composition challenges and identify the causal impact of access to citizenship on migrants' saving choices.

1.3.1 Institutional Context

Migration to Germany. More than 20 million people, a quarter of the German population, are first or second generation migrants. That is, they either have a direct migration experience (68.2%) or at least one parent who does (31.8%).[27] There is large heterogeneity in their legal status: 47.7% of first and second generation migrants are foreign citizens. And about 33% of foreign citizens do not have the right to stay in the country indefinitely. These differences in legal status seem to correlate with economic outcomes: On average, compared to German born individuals, first generation migrants with foreign citizenship are less likely to have a professional or academic degree (43.2%, compared to 64.4%), are equally likely to be employed (though at lower income levels), less likely to draw a pension (9.8%, compared to 21.8%)[28] and more likely to depend on benefits (9.4%, compared to 3.0%)[29].

Germany's citizenship legislation is tied closely to the country's immigration experience. Since the second World War, the number of migrants living in Germany has continuously risen, albeit in waves. During the 1950s, millions of "Aussiedler" ("ethnic Germans" whose families were forced to flee the country during the 1930s and 1940s) returned from Eastern Europe. During the 1960s and 1970s, the German government recruited workers from Turkey, former Yugoslavia, Greece, Spain and Italy to fill low skilled labour vacancies via the so called "guest worker programme". During the 1990s, millions of individuals relocated to Germany from Eastern Europe following the disintegration of the Soviet Union and the Yugoslav war (Zimmermann [1995]). The EU enlargement in 2004[30], increased migration from new member states.

[27] Table with the numbers provided by the German statistical office can be found here.

[28] This is likely in part a reflection of migrants being younger on average. However, given that migrants are on average only 5-10 years younger than natives, the observable gaps are sizeable.

[29] All numbers are based on the 2019 census information provided by the German Federal Statistical Office.

[30] On 1. January 2004, Poland, Hungary, Estonia, Latvia, Lithuania, Slovenia, Slovakia, the Czech Republic, Malta and Cyprus joined the EU, giving their citizens the right to move freely and work

Finally, since 2015, more than two million people from Syria, Iraq, Afghanistan, and Ukraine have resettled in Germany.

This illustrates two key points about the German migration experience. First, migrants differ greatly between their cohorts. For example, most "guest workers" moved from Southern Europe, towards a specific job. Most migrants who arrived during the 1990s fled a war and had no immediate job prospects. It is crucial to carefully account for this heterogeneity in any empirical analysis. Second, while Germany has had a large migrant community for several decades, it has historically viewed the majority of these migrants as temporary ones — rather than individuals who want to permanently resettle. The guest worker programme was intended as a short term scheme wherein individuals would work in Germany for a set number of years and then return to their home country. Similarly, refugees were expected to return to their country of origin after the original reason for asylum had subsided. Only migrants with a family history in the country were thought of as "returning" to stay. This understanding of migration as a temporary phenomenon shaped immigration policies and informed the legislative environment.

Legal Context. Even though Germany has had a large migrant community for the better part of a century, for the longest time it did not regard itself as a "country of immigration that strives to increase the number of German citizens by way of naturalisation" — as laid out clearly in the Federal Naturalisation Guidelines from 1977 (see, for example, Hailbronner and Renner [1992]). Instead, German citizenship law (following 1913 legislation) was based on the "ius sanguini" principle whereby individuals acquired German citizenship through German parentage. Although the 1977 guidelines stipulated that exceptions could be made in cases where there was a significant "public interest" in an individual becoming naturalised, by and large German citizenship law regarded citizenship as a heritable trait during most of the 20^{th} century and therefore did not include provisions for an individual seeking German citizenship out of their own wishes.

This changed in April 1990 when the German parliament passed the "Alien Act"

in any EU member state. In 2007, Bulgaria and Romania joined, and in 2013, Croatia became the 28th member state.

("Ausländergesetz"), which came into effect on 1^{st} January 1991. The law followed a Federal Constitutional Court ruling on immigrant voting rights[31] and established a formal naturalisation process for migrants seeking German citizenship. This meant that, for the first time, naturalisation decisions were no longer at the discretion of the public servant assigned a specific case. Instead, migrants who fulfilled the criteria stipulated by the law now were *entitled* to German citizenship. The specific requirements were as follows: Adult migrants needed to demonstrate economic self-sufficiency, i.e. the ability to support both themselves and dependent family members without state assistance. Adolescent migrants needed to have spend a minimum of 6 years in a German school. All migrants applying for citizenship had to demonstrate a clean criminal record, declare their loyalty to the democratic constitution of Germany and renounce their previous citizenship[32]. Finally, and crucially, the new law established age dependent residency requirements. While adult migrants aged 23 and older had to have resided in Germany regularly for at least 15 years, adolescent migrants aged 16-22 became eligible to naturalise after only 8 years of residence.

These regulations remained in place for nearly a decade, until May 1999, when the newly elected German parliament passed the Citizenship Act ("Staatsangehörigkeitsgesetz") which came into effect on 1^{st} January 2000. The law represented another drastic overhaul of the existing naturalisation framework: First, it reduced the residency requirements for adult migrants from 15 to 8 years. Second, it moved away from the "ius sanguinis" principle which tied citizenship to German ancestry. Through the Citizenship Act, children of foreign parents got German citizenship upon birth, given that at least one parent had legally resided in Germany for at least 8 years and been in possession of a permanent residence permit for at least 3 years. Moreover, the children were allowed to hold both the German and their parents' citizenship until

[31] The Court, in 1989, ruled that under given law, immigrants did not have the right to vote in local elections. However, in the accompanying statement, the Court urged policymakers to reform the naturalisation process.

[32] Usually, individuals have to give up their original citizenship upon naturalisation in Germany. However, there are many exceptions to this rule. Migrants can retain several passports if their home country does not or only under exceptionally difficult circumstances allow citizens to give up their citizenship. Migrants from countries that have bilateral agreements about passport recognition such as Brazil can retain both citizenships. And migrants from EU countries can always retain their initial citizenship as long as their home country allows for multiple citizenships.

reaching adulthood when they had to choose one[33]. Finally, the law granted "ethnic Germans" ("Aussiedlern") automatic citizenship upon them settling in Germany. These comparatively restrictive regulations[34] have been in place until today.

The legal changes affected several dimensions of the immigration and integration process: First, they brought about a rise in naturalisations. Prior to 1991, only about 34,000 individuals naturalised in a given year, usually following marriage to a German citizen. After the reforms, that number rose to an annual 230,000 naturalisations (Gathmann and Keller [2018]). Second, the reforms impacted migrants' labour market outcomes, albeit modestly. Gathmann and Keller [2018] find that becoming eligible for citizenship after 8 rather than 15 years improved women's employment and earning prospects, rendering them 5.6pp more likely to be employed and earn a 0.112 log point higher income. In contrast, men's employment and earnings were unaffected. Third, the reforms affected family formation, with women delaying both marriage and the birth of their first child (Gathmann and Monscheuer [2018]). Finally, the change to birthright citizenship impacted the integration outcomes of both the affected children and their parents: When children have access to birthright citizenship, their parents engage more with their neighbours and use the German language more frequently (Avitabile et al. [2013]). They also have fewer children overall and those children have better health and socio-emotional outcomes (Avitabile et al. [2014]).

1.3.2 Identification

In general, it is difficult to identify the causal impact of citizenship or even access to citizenship due to issues of composition and strategic selection. When a country changes it's citizenship regime, this changes the attractiveness of the country for different groups of migrants. Consequently, such legal changes affect both in- and out-migration and thus the composition of migrants living in a given country (Adda et al. [2022], Sajons and Clots-Figueras [2014]). Furthermore, naturalisation is not something that happens to an individual. Instead, individuals choose whether or not to seek out citizenship, meaning that those who naturalise can be systematically

[33] Parents of children born in the 1990s who would have qualified for birthright citizenship under the new law, were given the right to apply for retroactive "birthright" citizenship for their children.

[34] For example, the US, UK or France, allow regular applications after 5 years of residency as well as dual citizenship irrespective of the country of origin.

different from those who do not, even after accounting for observable differences. This can be true even for access to citizenship, as prospective migrants can strategically select into easier or harder access to citizenship through their choice of immigration country.

I circumvent both of these empirical challenges by combining the panel structure of my data with the unexpected, quasi-experimental variation in access to citizenship introduced by the two reforms in German citizenship law. I use data from the German Socioeconomic Panel (details in the next section). The panel structure of this data set allows me to trace individuals and households over time and isolate within unit changes in behaviour. To shut down any compositional effects, I limit my sample to migrants who had been living in Germany prior to the legal changes. Since both reforms were unexpected — the first was brought about by a surprise court ruling and the second followed an upset electoral win — this sample restriction also means that migrants were not able to strategically select into citizenship. Migrants who moved to Germany before 2000, before their move could neither correctly predict when they would become eligible, nor how their immigration date would affect their eventual eligibility date.

To illustrate this point, think about three migrants who arrived in Germany at different points in time and at different ages: Antonio who is born in 1963 and moves to Germany in 1975, Blanca who is also born in 1963, but only moved to Germany in 1980, and Claudette who is born in 1970 and also moved to Germany in 1980. Antonio, Blanca and Claudette all moved to Germany under the assumption that they would not be able to attain German citizenship, i.e. they were not able to strategically select into citizenship. What is more, when the Alien Act passes in 1991, Antonio is 28 and has been living in Germany for 16 years, immediately becoming eligible for citizenship. Blanca is 28 years old as well, but has only been in Germany for 11 years when the law changes. Hence, she can only apply for citizenship in 1995, when her 15 years of residence are complete. Finally, while Claudette too has only lived in Germany for 11 years, she does become immediately eligible in 1991 since she is 21 when the Alien Act passes and thus falls under the adolescent scheme. Due to differences in their immigration and birth cohort, the three become eligible for citizenship at different, equally unpredictable points in time. The exact same logic applies to migrants who

migrated to Germany during the 1990s, before the second reform passed. The only difference is that for them, their eligibility came unexpectedly early rather than was introduced at all. For more sample biographies, see Section 1.B in the Appendix. It is important to note that even though the first reform introduced an eventual right to citizenship for all migrants, the law during the 1990s still required migrants to continuously stay in Germany for 15 years as well as fulfill the non-residence related conditions for citizenship which might be hard to predict several years into the future. Due to this, it is likely that migrants still feel significant uncertainty over their future right to citizenship until they actually become eligible to naturalise.

The cross variation in birth and immigration cohorts determining access to citizenship means that it is unlikely that earlier and later cohorts of migrants systematically differ — something that could be a concern in the case where eligibility is determined solely through immigration or birth cohort. However, to ensure that changes in eligibility only capture exactly this, I also control for a rich set of observables and leverage the panel structure of the data to control for even unobservable differences with individual level fixed effects. Thus, I account for both differential cohort characteristics and the economic conditions they encountered upon arrival. Finally, I test for differential pre-trends between migrants of earlier and later immigration cohorts by looking at investment choices in the lead up to their legal right changing. The results can be seen in Figure 1.3 which documents the parallel pre-trends (conditional on the control variables). Consequently, conditional on a set of variables accounting for differences in observable and unobservable household characteristics, migrants who became eligible early are not systematically different from cohorts who became eligible for citizenship later. Thus, estimates of the impact of changes in eligibility for citizenship capture the causal impact of access to citizenship.

1.3.3 Data

The data I use is from the German Socioeconomic Panel (GSOEP) - a representative panel interviewing more than 11,000 households with 20,000 members annually. The first wave of the panel was collected in 1984 and the version of the data set I use in this paper includes years up to 2016 (wave 33, SOEP [2017]). To isolate the causal impact of access to citizenship on saving choices, I need detailed information

on households' financial choices over time as well as individual migration biographies. This is because I can only estimate the causal impact of access to citizenship if I can cleanly identify (i) which migrants see their right to stay significantly changed through becoming eligible and (ii) when exactly their status changes. Migrants from EU15 countries and migrants who already naturalised prior to the eligibility scheme being established (most commonly following marriage to a native) should not see their prospects of staying in Germany swayed drastically through getting access to citizenship. Furthermore, I need information on the year an individual was born in and migrated to Germany as well as time spent away from Germany after first arriving to determine the exact year they became eligible to naturalise. The GSOEP is the only German data set that inquires about individuals' migration biography in sufficient detail to isolate the sample of interest whilst also recording information on household saving behaviour. Moreover, it collects information on both the individual and the household level[35] allowing me to control for personal as well as household level determinants of saving – such as education, employment history, number of dependants, income, partner characteristics, etc. A full description of how I construct my variables can be found in Section 1.C in the Appendix. The German Socioeconomic Panel works with self reports, consequently caveats over self-reported data apply. In particular, it may be easier for respondents to recall saving in Germany while living there, leading to a potential underreporting of saving abroad. It should be noted that the interviewers prompt respondents on the saving measure being intended to capture saving across countries. Furthermore, while the saving gap between migrants and natives might be larger due to this data limitation, the investment measures which record the types of assets households hold should suffer significantly less from potential recall bias.

[35]Every member of a GSOEP household above the age of 17 is administered the individual questionnaire. In addition to this, one adult member of the household who can account for the household's housing situation, income, insurances, wealth, etc., the household head, fills out the household questionnaire.

1.4 Causal Impact of Access to Citizenship on Migrants' Saving Rate

1.4.1 Empirical Strategy

I examine the impact of access to citizenship on migrants' saving choices by using a difference-in-difference analysis around the passage of the Citizenship Act in 1999. Looking at the impact of the second reform has the advantage that I can document differences in saving behaviour by access to citizenship in the pre-reform period, as well as quantify the causal impact of *gaining* access to citizenship through the difference-in-difference coefficient.

My treatment group are migrants who had been in Germany for less than 15 years and thus were unable to apply for citizenship before the Citizenship Act reduced the residency requirements to 8 years. I compare their behaviour before and after the reform passed parliament to the behaviour of two control groups. First, I compare them to natives who should be unaffected by the Citizenship Act and whose behaviour represents the benchmark governments target when trying to create a level playing field. However, migrants and natives might have different motivations to transfer money abroad, as well as be subject to different shocks. Hence, I also use migrants who had been in Germany for more than 15 years (or fell under the adolescent regime) and thus could apply for citizenship even before the Citizenship Act passed as a second control group. I detail the assignment to treatment and control group of the different migrants along with the identifying variation in Section 1.D in the Appendix.

I estimate the following two empirical models to quantify the impact of the reform on behaviour:

$$\begin{aligned} \mathrm{Y}_{it} = \alpha + \beta * \mathrm{TreatedMig}_i * \mathrm{PostCitAct}_t + \gamma * \mathrm{ControlMig}_i * \mathrm{PostCitAct}_t + \theta * \mathrm{X}_{it} \\ + \mathrm{Year}_t + \mathrm{State}_s + \mathrm{HH}_i + \epsilon_{it} \end{aligned} \tag{1.6}$$

$$\mathrm{Y}_{it} = \alpha + \beta * \mathrm{TreatedMig}_i * \mathrm{PostCitAct}_t + \theta * \mathrm{X}_{it} + \mathrm{Year}_t + \mathrm{State}_s + \mathrm{HH}_i + \epsilon_{it} \tag{1.7}$$

In the first specification, natives form the hidden category and migrants are sorted

into treatment (TreatedMig_i) and control group (ControlMig_i) in the way laid out above. In the second specification, I restrict my sample to migrants only, with migrants in the control group forming the hidden category. I estimate the two models separately. The post-reform variable PostCitAct_t is a binary variable which goes to 1 in 1999 when the Citizenship Act passed parliament.[36] β and γ denote the difference-in-difference coefficients on the interaction of the two categories. Specifically, β is the difference-in-difference coefficient of interest which measures the effect of access to citizenship on the treated opposite the two control groups. I control for a variety of individual and household characteristics (X_{it}). First, I control for age and years spent in Germany in quadratic terms. This is in addition to the household and year fixed effects, which control for birth and immigration cohort as well as the progression of time and thus the linear effect of age and and years in Germany. Second, I control for the employment status of the household head with two binary variables indicating being employed either full or part time as well as two variables indicating years spent in full or part time employment up to the survey date. Third, I control for the monthly net income of the household adjusted for inflation, measured at the household level. Fourth, I control for the education status of the household head, both with a categorical and a continuous variable reflecting years of education. Fifth, I control for the marital status of the household head through a dummy variable; and the household's constellation by including variables recording the number of people and underage children living in the household. Finally, I include fixed effects for the survey year, the state a household resides in, and the household itself. Thus, I can control for unobservable characteristics that are stable over time but may systematically vary between natives and migrants as well as different groups of migrants. Y_{it} denotes the outcome of interest (saving & remittance amounts and rates) and standard errors are clustered at the household level.

While the saving and remittance measures I have available after 1992 and 1996 respectively allow me to trace the impact of access to citizenship on saving across countries and transfers sent to individuals living abroad by both migrants and natives, it does not allow me to separately examine saving abroad. However, the allocation of

[36] I focus on the reform passing parliament rather than being enacted in January 2000 as the former act plausibly reduced uncertainty more significantly.

savings in Germany or abroad is important both in terms of political implication and shedding light onto the determinants of migrants' saving choices. Hence, I exploit a second measure of remittances that distinguishes remittances sent for different reasons: to support relatives or friends, to save and invest, or for other reasons. This measure is only available for the period from 1984 to 1995, and only for migrants. Hence, to examine whether access to citizenship had any bearing on the allocation of saving across countries, I use a second difference in difference analysis around the passage of the Alien Act in 1990. I compare migrants who became immediately eligible through the passage of the Act to those who only learned that they would become eligible in the future. Specifically, I estimate the following specification:

$$\mathrm{Y}_{it} = \alpha + \beta * \mathrm{TreatedMig}_i * \mathrm{PostAlAct}_t + \theta * \mathrm{X}_{it} + \mathrm{Year}_t + \mathrm{State}_s + \mathrm{HH}_i + \epsilon_{it} \quad (1.8)$$

where $\mathrm{PostAlAct}_t$ denotes a binary variable that goes to 1 in the period after the Alien Act passed parliament. The set of outcomes now denoted by Y_{it} includes the set of binary variables indicating whether a migrant sent money abroad for any of the reasons laid out above, as well as the associated amounts. All other denotations retain their interpretation.

1.4.2 Sample

The sample I focus on consists of households headed by either natives or migrants with a direct migration experience. I restrict the sample to households headed by individuals under the age of 60, i.e. who have at least five years before reaching their legal retirement age. Furthermore, in the main analysis, I drop households headed by migrants from EU-15 countries as individuals from these countries have a right to stay indefinitely in Germany regardless of German citizenship . I also drop households that are headed by migrants who are married to a German citizen as such a household would not experience a significant drop in uncertainty about the future in response to the foreign household head becoming eligible to naturalise. Finally, I drop households headed by migrants who naturalised before the Citizenship Act passed. I then assign the households headed by migrants to the treatment and control group depending on the household head's eligibility to apply for German citizenship.

TABLE 1.1: Pre-Reform Characteristics of the Sample

	Natives	Eligible Migrants	Non-Eligible Migrants
Age	40.377	43.914	34.253
	(10.909)	(11.098)	(8.339)
Years in Germany	40.377	22.661	6.259
	(10.909)	(4.847)	(2.814)
Employed Full Time	0.794	0.827	0.631
	(0.404)	(0.378)	(0.484)
Employed Part Time	0.086	0.016	0.083
	(0.280)	(0.124)	(0.277)
Years FT Employment	16.981	21.546	8.585
	(11.430)	(11.004)	(7.178)
Years PT Employment	1.082	0.546	0.795
	(3.273)	(1.906)	(2.923)
HH Net Income	2573.878	2384.651	1636.421
	(1365.792)	(1116.160)	(629.928)
Education Category	2.084	1.362	1.802
	(0.587)	(0.761)	(0.879)
Years of Education	12.125	9.631	11.410
	(2.494)	(1.774)	(2.832)
Married	0.664	0.885	0.767
	(0.472)	(0.319)	(0.424)
Num People in HH	2.717	3.570	3.107
	(1.272)	(1.680)	(1.597)
Young Children in HH	0.423	0.526	0.678
	(0.494)	(0.499)	(0.468)
Observations	29,708	2,839	396

Notes: This table summarises the average sample characteristics during the period 1992 - 1998 taken from the GSOEP. The first column uses a sample of German natives. The second column uses a sample of migrants who are eligible for citizenship. The third column uses a sample of migrants who are not eligible for citizenship.

Table 1.1 summarises the sample characteristics of the treatment as well as the two control groups in the period before the reform passed parliament in May 1999.

Immediately, we see that the two groups differ in terms of their economic situation. Households headed by migrants not eligible for citizenship have a monthly net income that, on average, is about 1,000€ less than that of native households. The associated household heads are less likely to be employed full time than either native or migrant household heads with access to citizenship and more likely to be employed part time than eligible migrant household heads. They do however have about two more years of education than migrant household heads with access to citizenship. Additionally, migrants without access to citizenship are more likely to have larger households with a greater number of underage children. This could be due at least partly to the fact that, on average, household heads without access to citizenship are just over 34 years old while the household heads from the native and eligible migrant group are about 40 and 44 years old respectively.

1.4.3 The Saving Rate Gap

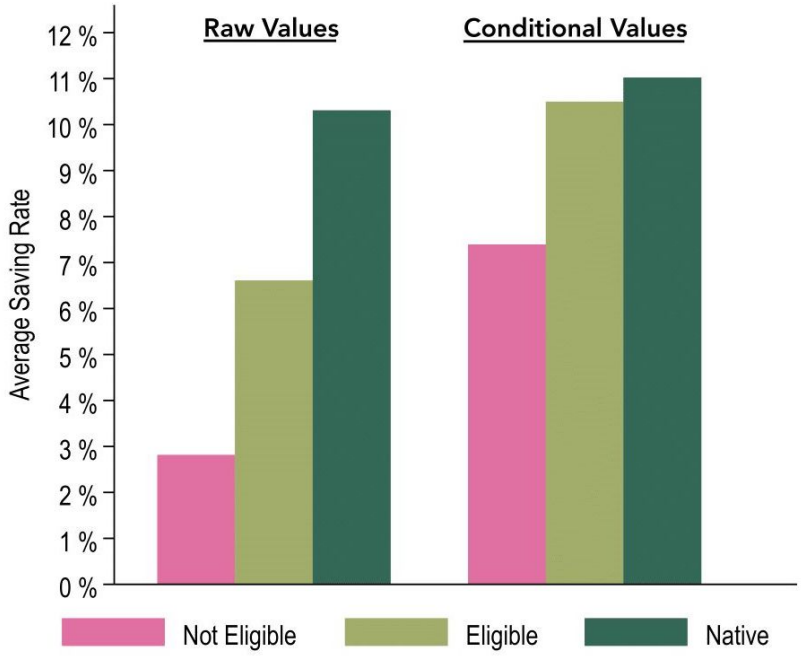
Result 1 *Migrants save 30% less than comparable natives if they do not have access to citizenship. If they do have access to citizenship, they save on par with them.*

Recalling the theoretical predictions from Section 1.2, the first thing we need to establish about migrants' saving choices under uncertainty, is whether in their choices, they react predominantly to uncertainty over future resources or uncertainty over future states of the world. If migrants are particularly uncertain about their future consumption levels compared to natives, they should save at higher rates. If instead uncertainty over their future location and quality of life is particularly consequential for their saving choices, they should save at lower rates. Both effects should be stronger for migrants without access to citizenship who face higher levels of uncertainty across dimensions.

Figure 1.1 plots saving rates of migrants with and without access to citizenship as well as natives. On the left hand side, it plots the raw saving rates, while on the right hand side, it plots the values conditional on the full set of controls. Comparing first the raw differences in saving, households headed by migrants without access to citizenship save a substantially lower fraction of their net income than households headed by either migrants with access to citizenship or natives. While native households save

about 10% of their net income, households headed by eligible migrants save about 6.6% of their net income and households headed by non-eligible migrants save only about 2.8% – less than a third of native households. Note that the saving rate includes savings not only in Germany, but anywhere in the world.

FIGURE 1.1: Saving Rate Gap Pre-Reform



Notes: This figure shows the saving rate gap between three groups. The saving rate of migrants not eligible for citizenship is captured in the pink bars, that of eligible migrants is shown in the light green bars, and that of natives in depicted in the dark green bars. The first three bars on the left-hand side show raw values. The second three bars on the right-hand side show residualised values. These are calculated by regressing the saving rate onto the full set of controls as well as fixed effects described in Section **??**. All calculations use data from the GSEOP pre-1999.

Of course, these raw differences reflect not only differences in legal status, but other characteristics as well. Conditioning on the full set of control variables described in the preceding section, we see that migrants with access to citizenship are no longer distinguishable from natives. Similarly, the gap between migrants without access to citizenship and the other two groups shrinks. However, even conditional on differential income levels, employment in full or part time, education, marital status, age, years worked in Germany, individuals living in the household and the set of fixed effects, migrants without citizenship *still* save about 30% less than comparable natives or

migrants with the right to citizenship. Uncertainty over their future appears to stifle rather than prompt migrants to save.

1.4.4 Closing the Gap

Result 2 *Gaining access to citizenship causally increases the saving rate of affected migrants by 2.5pp. This fully and permanently closes the previously detrimental saving rate gap.*

FIGURE 1.2: DID Pre/Post Residual Saving Rate



Notes: This figure shows the residualised saving rate of migrants who were not eligible for citizenship before the reform(pink), migrants who were eligible before the reform (light green), and natives (dark green) in the pre-reform (left-hand side) and post-reform (right-hand side) periods. The saving rate residual is calculated by regressing the saving rate onto the full set of controls as well as fixed effects described in Section 1.4.1. Caps represent 95% confidence intervals where standard errors are calculated by clustering at the household level.

Table 1.2 at the end of this subsection shows the results of the difference-in-difference estimation. In the post reform period, migrant households in the treatment group increase their saving rate by 2.48pp vis-à-vis natives — while the saving rate of migrant households in the control group does not change significantly. All controls have the expected sign. Household income and education exert a positive influence on the amounts as well as the fraction of income saved while being employed full time increases both saving as well as the propensity to remit money abroad. Also, the

more people live in a household, the less a household either saves or remits abroad. Appendix section 1.H additionally shows that these results are robust to all possible combinations of control variables and fixed effects.

The increase in the saving rate fully closes the previously detrimental gap between migrants without access to citizenship and natives, migrants with previous access. Figure 1.2 illustrates this point by plotting the residual saving rate of all three groups[37] in both the pre and the post reform period. Furthermore, the increase in the saving rate represents not only a temporary spike, but rather a permanent adjustment. Figure 1.3 plots the development of the saving rate residual of all three groups over time. Specifically, it plots the mean saving rate residual in each year by group and indicates trend lines. The graph illustrates several points. First, it highlights the parallel trends in the development of the saving rate residual in the lead up to the reform between migrants in the treatment and in the control group[38]. Second, it documents the stability of the impact of access to citizenship on saving rates. The residual saving rate of migrants who gained access to citizenship through the reform remains on par with that of natives and migrants who had access to citizenship before the reform even ten years after the reform.

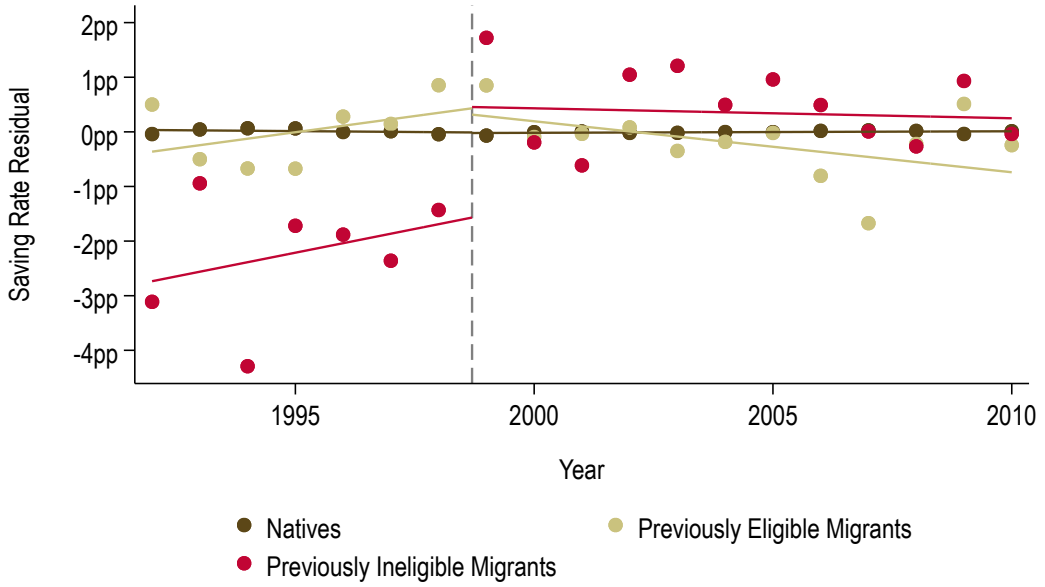
Thinking back to the theoretical predictions, this sizeable effect has several key implications. First, it provides further evidence for the crucial role of uncertainty over future states of the world in this context. Access to citizenship increases migrants' saving rate even when controlling for concurrent changes in labour market outcomes and income — the two likeliest drivers of potential changes in migrants' capacity to save. In the next section, I document in further detail how neither labour market outcomes, nor credit usage are changed by migrants' gaining access to citizenship. Absent any changes in capacity to save brought about by gaining access to citizenship, the only way in which it can lead to a sizeable *increase* in migrants' saving rates that closes a previously negative gap, is if it lowers migrants' uncertainty over their future

[37] To obtain the residual saving rate I regress the raw saving rate onto the full set of control variables and fixed effects laid out in section 1.4.1.

[38] Natives' saving rate is completely flat while migrants of both groups see a slight upwards trend in their saving rate residuals — highlighting the importance of using two control groups and not merely comparing migrants to natives. The uptick could derive from the anticipation of the introduction of the Euro from 1999 to 2000. It could also reflect household heads in both groups edging closer to becoming eligible to naturalise (and some in the control group even doing so) and increasing their saving rate in Germany in anticipation.

right to stay and the enjoyment they will derive from future consumption. The fact that gaining access to citizenship does not just decrease the residual gap but fully closes it suggests that uncertainty over future location and enjoyment derived from consumption is one of the most important channels in this setting, comparable in size to the impact of the entire income differential between migrants and natives. Furthermore, the effect shows that the impact of uncertainty is *malleable*. Providing migrants with access to citizenship appears to fully eliminate the uncertainty they feel about their future location and experienced utility — at least to the degree this uncertainty disincentives saving. That finding has important policy implications that I discuss in detail in Section 3.5.

FIGURE 1.3: Progression of the Mean Residual Saving Rate Over Time



Notes: This figure shows the evolution over time of the saving rate residual for migrants who were ineligible for citizenship in the pre-reform period (pink), migrants who were eligible for citizenship pre-reform (light green), and natives (green). Dots represent mean values for each group within a given year. Linear regression lines of best fit are plotted for each group allowing a break in 1999. The 1999 reform passing parliament is represented by a grey dashed vertical line. The saving rate residual is calculated by regressing the saving rate onto the full set of controls as well as fixed effects described in Section 1.4.1.

TABLE 1.2: Effect of the Citizenship Act on Saving & Remittances

	Saving Amount GER	Saving Rate GER	Remittance Amount	Remittance Rate
Migrant Treatment * Post Reform	59.35***	0.0248***	27.00*	0.00967
	(17.37)	(0.00577)	(15.19)	(0.00694)
Migrant Control * Post Reform	21.36	-0.00111	25.45**	0.0110**
	(19.84)	(0.00568)	(13.235)	(0.00614)
Years in Germany Squared	0.0632***	1.10e-05	-0.0160	-5.27e-06
	(0.0219)	(7.56e-06)	(0.0113)	(4.40e-06)
Age Squared	-0.0175	4.99e-06	0.0132	3.76e-06
	(0.0232)	(7.88e-06)	(0.0113)	(4.33e-06)
Employed Full Time	25.80**	0.0242***	1.878	0.000778*
	(10.84)	(0.00159)	(1.377)	(0.000444)
Employed Part Time	-3.255	0.00729***	1.101	0.000558
	(5.844)	(0.00161)	(1.123)	(0.000424)
Years FT Employment	-1.900	-0.000485	0.166	9.56e-05
	(1.369)	(0.000297)	(0.174)	(6.42e-05)
Years PT Employment	-1.333	3.55e-05	-0.345	-2.63e-05
	(1.738)	(0.000474)	(0.395)	(0.000136)
HH Net Income	0.140***	8.30e-06***	0.00218	-2.27e-07*
	(0.0213)	(1.43e-06)	(0.00198)	(1.23e-07)
Education Category	-7.533	0.00488*	-0.885	0.000245
	(10.94)	(0.00287)	(2.252)	(0.000854)
Years of Education	12.49***	0.00278**	0.821	0.000148
	(4.575)	(0.00109)	(0.733)	(0.000226)
Married	3.593	0.000855	1.454	0.00167
	(8.808)	(0.00190)	(2.157)	(0.00105)
Num People in HH	-46.59***	-0.00881***	-2.086	-0.000542**
	(9.766)	(0.000992)	(1.425)	(0.000230)
Young Children in HH	11.97	-0.00113	-1.908	-0.000797
	(7.820)	(0.00152)	(1.627)	(0.000553)
Observations	87,136	87,126	76,590	76,580
R-squared	0.113	0.031	0.008	0.013
State, Year & HH FE	YES	YES	YES	YES

Notes: This table shows the effects of citizenship eligibility on saving and remittance rates using quasi-experimental variation due to the 1999 citizenship act passing the German parliament. Natives form the omitted category. Column one shows results on the level of savings, column two on the saving rate, column three on remittances levels, and column four on the remittance rate. Regressions all include state, year and household fixed effects, and are estimated by OLS. Standard errors are given in parentheses and are clustered at the household level. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

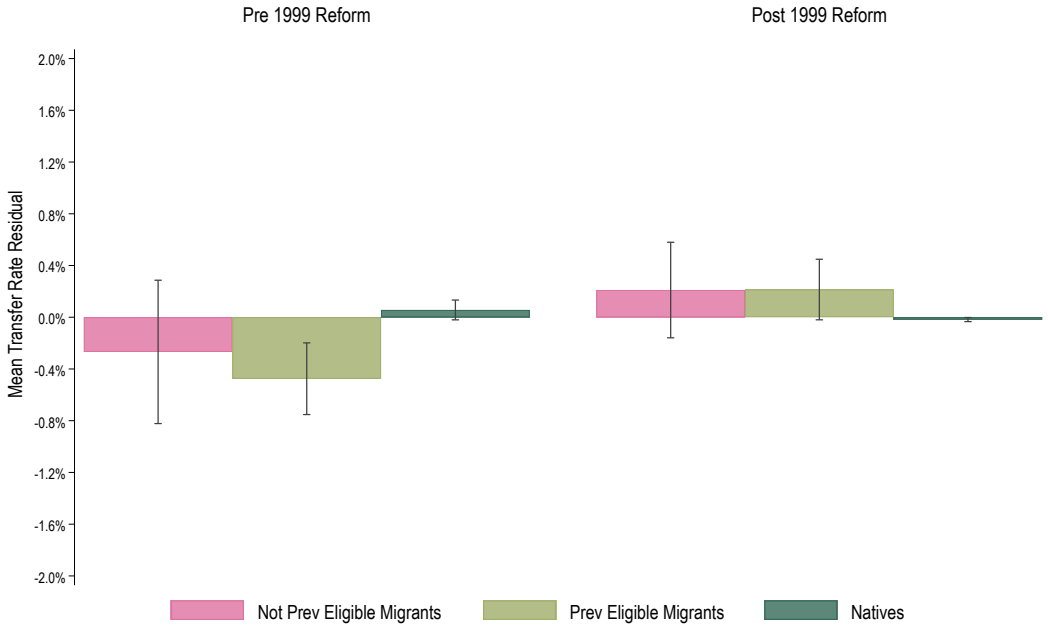
1.4.5 Impact on Transfers Abroad

Result 3 *Gaining access to citizenship does not affect transfers sent to individuals still residing in their country of origin.*

Table 1.2 as well as Figure 1.4 show that the transfers migrants send to individuals living abroad are not causally changed by gaining access to citizenship. While compared to natives, treated migrants increase their transfer amounts and rate slightly, the same is true for migrants in the control group. Thus, it appears that the increase opposite natives does not derive from changes in access to citizenship, but rather aggregate trends affecting migrants living in Germany regardless of their eligibility for citizenship. One likely explanation is that the introduction of the Euro changed migrants' economic calculus with transfers becoming relatively more or less expensive depending on preexisting currency differentials. This also highlights again the importance of having multiple control groups and not merely comparing migrants without access to citizenship to natives.

To test whether migrants shift their allocation of savings in the immigration country and abroad, I turn to the outcomes of the difference-in-difference analysis around the 1990 reform. Figure 1.G.1 in the Appendix plots the point estimates as well as the 95% confidence bands of the impact of gaining immediate access to citizenship relative to gaining the mere prospect of becoming eligible eventually. As before, migrants do not change their transfers to relatives, friends or other individuals living abroad. However, I do find some evidence of a slight shift from saving in the country of origin to saving in Germany. Migrants who gain immediate access to citizenship reduce their saving rate in their country of origin by about 1pp. As this variable is only available for a subset of migrants, the sample size is smaller than in the main analysis and thus the confidence bands larger. Yet this effect is still significant at the 10% level. As the main analysis identifies the impact on savings held in any country, the fact that migrants seem to reduce saving abroad by 1pp suggests that saving in Germany increased by not just 2.5pp but actually 3.5pp.

FIGURE 1.4: DID Pre/Post Residual Remittance Rate



Notes: This figure shows the residualised remittance rate for migrants who were ineligible for citizenship in the pre-reform period (pink), migrants who were eligible in the pre-reform period (light green), and natives (dark green) in the pre-reform (left-hand side) and post-reform (right-hand side) periods. The remittance rate residual is calculated by regressing the saving rate onto the full set of controls as well as fixed effects described in Section 1.4.1. Caps represent 95% confidence intervals where standard errors are calculated by clustering at the household level.

As shown in Figure 1.G.2 in the Appendix, the shift is driven by the extensive margin with migrants becoming 8pp less likely to save abroad at all. Thinking back to the theoretical predictions, this suggests that migrants are not only uncertain about their future location without access to citizenship, but also that investments are, at least to a degree, country specific. I investigate this and other channels further in the next section.

1.5 Mechanisms

1.5.1 Empirical Specification

In this section, I continue testing the different theoretical predictions of the life-cycle saving model by exploring which channels explain the large impact of access to citizenship on migrants' saving rates. To this end, I focus on the impact of changes in the

household head's eligibility over time on migrant households' saving and investment choices. This allows me to extend my main analysis in four ways. First, it allows me to exploit the variation introduced by both the Alien and the Citizenship Act. Second, it allows me to quantify the impact of changes in migrants' actual legal status, compared to the pooled effect of changes in legal status and the prospect thereof. And third, it allows me to distinguish between the impact of expected eligibility within a given regime and unexpected eligibility brought about by a regime change. This way, I can estimate whether eligibility influences behaviour even when uncertainty about the future is already less pronounced and gauge whether migrants adjust behaviour already in anticipation of becoming eligible. That is, I can get a better understanding of whether changes in pure legal status or uncertainty are the main driver of the observable changes. Finally, it allows me to test how eligibility impacts behaviour over time. Especially with regard to bigger investments such as housing, it is possible that migrants only react to eligibility with a delay.

The sample now consists of only households headed by migrants without access to citizenship at the beginning of the sample period in 1992. That is, only households headed by direct migrants below the age of 60 without German citizenship who are not married to a German citizen and did not become eligible to naturalise before 1992. The main sample consists of only migrants from non-EU countries, however I also test for differential effects of EU and non-EU migrants by repeating the main analyses for a sample of households headed by EU migrants. I regress the main outcomes on a binary variable indicating eligibility as well as the full set of controls and fixed effects detailed earlier. In a second specification, I split the eligibility dummy into two binary variables indicating whether an individual became eligible unexpectedly or expectedly. And in a third specification, I regress the outcomes of interest onto a set of dummy variables indicating whether a migrant has been eligible for 1-5, 6-10, 11-15, 16-20 or 21-25 years. I use five year bins rather than a continuous years of eligibility variable to increase power and allow for non-linear effects over time. The coefficients on each of the eligibility variables are identified by exploiting the discontinuities in eligibility created by the two reforms and comparing migrants of the same birth cohort who arrived in Germany in slightly different years, as well as migrants of the same arrival cohort who were born in different years. I describe the

identifying variation in detail in Section 1.E in the Appendix.

I estimate the following empirical specifications :

$$
\begin{aligned}
\mathrm{Y}_{it} &= \alpha + \omega * \mathrm{Eligible}_{it} + \theta * \mathrm{X}_{it} + \mathrm{Year}_t + \mathrm{State}_s + \mathrm{HH}_i + \epsilon_{it} \\
\mathrm{Y}_{it} &= \alpha + \delta_1 * \mathrm{EligibleExp}_{it} + \delta_2 * \mathrm{EligibleUnexp}_{it} + \theta * \mathrm{X}_{it} \\
&\quad + \mathrm{Year}_t + \mathrm{State}_s + \mathrm{HH}_i + \epsilon_{it} \\
\mathrm{Y}_{it} &= \alpha + \eta_1 * \mathrm{YearsEli_1_5}_{it} + \eta_2 * \mathrm{Yearseli_6_10}_{it} + \eta_3 * \mathrm{YearsEli_11_15}_{it} \\
&\quad + \eta_4 * \mathrm{YearsEli_16_20}_{it} + \eta_5 * \mathrm{YearsEli_21_25}_{it} + \theta * \mathrm{X}_{it} \\
&\quad + \mathrm{Year}_t + \mathrm{State}_s + \mathrm{HH}_i + \epsilon_{it}
\end{aligned}
\tag{1.9}
$$

The outcome and control variables as well as the fixed effects are defined as in Section 1.4.1 and standard errors are again clustered at the household level. The coefficient on the eligibility dummy ω measures the impact of becoming eligible to naturalise. δ_1 and δ_2 measure the impact of becoming eligible either expectedly or unexpectedly, and η_1, η_2, η_3, η_4 & η_5 measure the impact of having been eligible for 1-5, 6-10, 11-15, 16-20 or 21-25 years respectively.

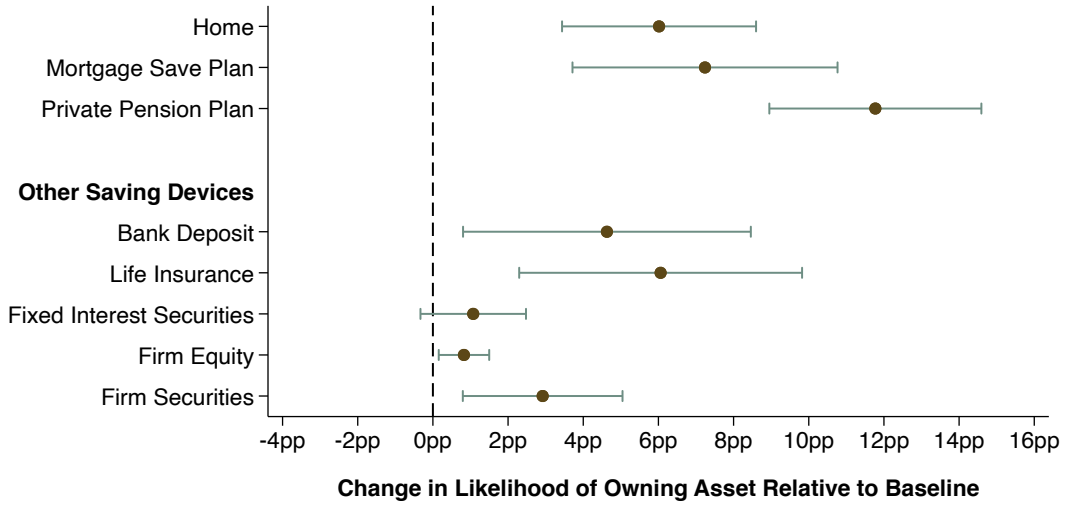
1.5.2 Long Term Investments

Result 4 *Gaining access to citizenship increases migrants' propensity to invest long term. The effect is particularly strong for illiquid, country specific assets with migrants becoming 6.5pp more likely to own their home, 7.8pp more likely to own a mortgage save plan and 12pp more likely to own a private pension plan. However, migrants also become 5pp more likely to open a bank deposit, that is to hold liquid assets.*

The increase in the saving rate following access to citizenship suggests that uncertainty over future location and utility derived from consumption is of greater consequence here than uncertainty over future consumption. If the preparatory indeed outweighs the precautionary saving motive in this context, the theory suggests that we should see not only a change in migrants' saving rate, but their means of saving

as well. Specifically, migrants should increase their investments in both liquid and illiquid assets but with a greater impact on illiquid saving technologies[39].

FIGURE 1.5: Effect of Becoming Eligible on Propensity to Own Different Assets



Notes: This figure shows the causal effect of citizenship eligibility on the likelihood of owning various saving technologies. Markers represent point estimates and caps indicate 95% confidence intervals. Standard errors are clustered at the household level.

This is exactly what I find empirically. Figure 1.5 plots the point estimates and confidence bands of the impact of eligibility for citizenship on the propensity of migrant households to tie up capital in various devices. In response to becoming eligible to naturalise, migrant households become 6.5pp more likely to own their home. Considering that among migrant households without access to citizenship only about 20% own their home, this represents a 30% increase in migrants' likelihood to own their home. Moreover, they become 7.8pp more likely to invest in a mortgage save plan, 12pp more likely to own a private pension plan[40], 5.2pp more likely to hold bank

[39] In contrast, if the precautionary saving motive were the dominant one in this context, we should see access to citizenship lead to a decrease in liquid and illiquid saving with a more pronounced impact on liquid savings

[40] In 2002, the German government introduced a private pension scheme (named after the federal minister of labour at that time, Walter Riester) which both directly subsidises private saving for old-age and allows individuals who pay into a private pension scheme to detract the amounts from their taxable income. Migrants can choose to have their Riester pension claims be either paid out to them or receive them abroad. However, in this case they have to *repay* the German government for both the subsidies and the tax breaks they were granted during the time they paid into the pension scheme.

deposits, 6pp more likely to own life insurance, 0.8pp more likely to own firm equity, and 3.1pp more likely to own firm securities.

The fact that investments in illiquid and in particular that investments in country specific investments increase most strongly adds further evidence that uncertainty over future states of the world is the dominant channel in this context. However, it also suggests that liquidation costs play a significant role in explaining existent saving differentials. If no assets had liquidation costs attached to them and access to citizenship would reduce uncertainty over future preferences only, the change should affect all asset classes equally. Similarly, if profitable assets were country specific with liquidation costs attached to them and uncertainty over the future would derive solely from uncertainty over future location with no bearing on preferences, we should see an increase in country specific investments financed out of a decrease in liquid savings. Only if migrants differ in their country preferences *and* saving technology is partly country specific do we see the empirical pattern we do. It also suggests that the welfare consequences of sustained uncertainty over future location can go even beyond inefficiently low saving rates. As laid out in Section 1.2, if liquidation costs and risk perceptions are high enough, migrants might invest in a country where they derive lower levels of enjoyment from consumption. These investment, if they are country specific, lock them into a specific path so that even if they gain certainty over their future right to stay, they would no longer be able to adjust paths without incurring prohibitive adjustment costs.

1.5.3 Resource Constraint

Result 5 *The increase in migrants' long term investments does not derive from an increase in resources they have at their disposal. Neither their household income, nor their labour market prospects or their credit uptake change in shift in response to becoming eligible for citizenship. Instead, access to citizenship prompts migrants to increase their savings in the lead up to investments.*

Migrants' increased propensity to invest long term after getting access to citizenship could reflect both changes in their willingness to save brought about by changes in uncertainty they encounter in their environment — or it could derive from changes

in their capacity to save. Long term investments, especially investments in housing, usually require high initial payments. If access to citizenship increases either migrants' income, most likely through improved labour market outcomes, or their access to credit, this could relax liquidity constraints they may have faced before and drive the increase in their propensity to invest. However, this is not what I find empirically. Table 1.F.2 in the Appendix Section 1.F reports the full results of regressing credit amounts being owed by a household in a given month, an indicator of having taken out any credit, household income, and an indicator for being in full time employment on the full set of controls. I find that becoming eligible for citizenship has no significant impact on either outcome. This is in line with recent evidence by Hainmueller et al. [2023] who document that migrants who gained access to citizenship through a New York city lottery see neither their income, access to credit or credit scores changed.

Instead, it appears that migrants build up liquidity through increased saving in the lead up to lumpy investments once they gain a reasonably secure pathway to citizenship. Figures 1.G.3 and 1.G.4 in the Appendix plot migrants' saving and investment choices in response to becoming eligible for citizenship separately by expected and unexpected access to citizenship. We see that while the increase in saving rate is driven almost exclusively by unexpected changes in access to citizenship, the impact on long term investments appears most pronounced for expected changes. This combination, in combination with the fact that eligible migrants do save higher fractions of their income in Germany, suggests anticipatory effects. Specifically, it seems as though migrants increase their saving rate not just once they become eligible, but as they approach eligibility. These anticipatory effects appear to only be of consequence close to the actual eligibility date, which is why an unexpected reduction in residency requirements can still have such a consequential effect. If more predictable changes in eligibility would allow migrants to build up more liquidity in anticipation, this would explain why the impact of eligibility on investments with high initial fixed costs such as housing are more pronounced for expected eligibility — in a situation where their capacity to save does not appear to be shifted by better income or credit access. However, the impact of expected eligibility on long term investments also highlights that migrants, even if they have built up sufficient liquidity, still hold off on consequential

investments until they are *actually* certain about their future prospects.

Finally, the importance of building up liquidity over time is captured in Figure 1.G.5 in the Appendix Section 1.G , which plots the impact of having been eligibility for different length of time on the probability of migrant households' owning their home. I find that the impact is strongest 6-10 years after gaining access to citizenship.

All of these findings underline the importance of accounting for uncertainty over future states of the world in this context. Gaining access to citizenship does not change the resources migrants have at their disposal. Instead, the empirical changes in their saving behaviour are in line with what theory predicts for the case where migrants face high levels of uncertainty about their future enjoyment of consumption in the immigration country as well as their right to stay and this uncertainty is lowered by giving them access to citizenship. The only remaining question is which of the two sources of uncertainty is driving the results.

1.5.4 Uncertainty over Preferences or Right to Stay

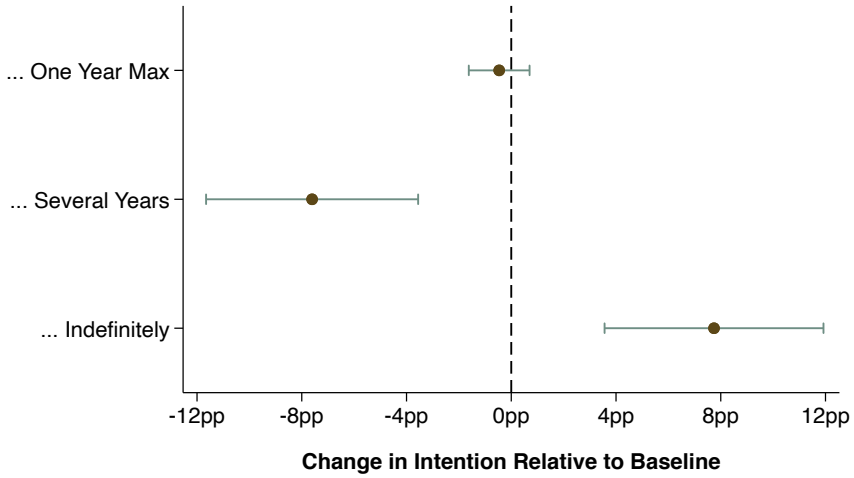
Result 6 *Neither EU migrants, nor temporary migrants are affected in their saving behaviour when gaining access to citizenship. Thus, the observable changes are driven by changes in uncertainty over future right to stay, rather than preferences.*

The evidence suggests that migrants increase their saving and long term investments in response to gaining access to citizenship because it lowers their uncertainty over the utility they will derive from consumption in the future, prompting risk averse agents to shift consumption forward. However, this change in uncertainty over future utility could derive from one of two sources. For one, it could be that migrants are uncertain about their future attachment to and preference for either country, and that access to citizenship lower this uncertainty through providing them with information about the quality of life they can expect in the immigration country. It could even be that access to citizenship is seen as a sign of goodwill that increases attachment to the immigration country. Both would lower uncertainty over future location by rendering migrants more certain over where they would *like* to retire. Or, it could be that migrants are uncertain over their future right to stay. Since the country migrants live in determines their enjoyment of consumption this too would translate into

uncertainty over future utility. However, in this case the effect of uncertainty would work through reducing uncertainty over where migrants will be *allowed* to retire.

If the first channel is the decisive one, we should see migrants who have a secure right to stay be as affected by gaining access to citizenship as those who do not. That is, we should see a similar effect of access to citizenship on migrants who moved to Germany from an EU country and thus have a secure right to stay regardless of German citizenship, and migrants who moved there from outside the EU. This is not what I find empirically. Figure 1.G.7 in the Appendix plots the results of the same regression of long term investments on eligibility as before, only for the sample of EU migrants. None of the investments are significantly affected. The same is true for their saving choices, which are plotted in Figure 1.G.6 in the Appendix, although the small sample size in that particular case makes it hard to gauge exact effect sizes.

FIGURE 1.6: Effect of Becoming Eligible on Intention to Stay



Notes: This figure shows the effects of citizenship eligibility on migrants' intention to stay. Graph shows treatment effects from specification 1 detailed in Section 1.5.1. Markers represent point estimates and caps represent 95% confidence intervals. Standard errors are calculated by clustering at the household level.

The non-effect of access to citizenship on EU migrants suggests that neither uncertainty over future country preference, nor a shift in preferences is driving the saving result, and that instead the results are driven by uncertainty over future location due to right to stay. An additional test, is to analyse the impact of access to citizenship on the intentions to stay of migrants. If access to citizenship brought about a shift

in migrants' preferences, this should affect temporary and permanent migrants alike. That is, we should see intentions to stay shift upwards along the entire distribution. Yet again this is not what I find in the data. Figure 1.6 depicts how migrants' expressed intention to stay in Germany for "one year maximally", "several years" or "indefinitely" changes in reaction to becoming eligible to naturalise. Migrants who only want to stay in Germany for maximally one more year do not appear to be affected in their plans by becoming eligible to naturalise. In contrast, migrants become 7.6pp less likely to indicate they want to stay in Germany for a set number of years in response to eligibility. This decrease is matched almost exactly by an increase in their propensity to say that they intend to stay in Germany indefinitely (7.7pp). It seems as though migrants adjust their plans to stay in Germany only if they were already planning to stay long term.

Finally, table 1.F.3 in the Appendix Section 1.F shows the impact of becoming eligible for citizenship on measures of feeling German and disadvantaged due to one's origin. Neither of the two dimensions are affected. Furthermore, worries about employment or one's future economic situation at large also do not change. This adds further credence to the interpretation that access to citizenship does not operate through shifts in income uncertainty and thus precautionary saving. Instead, the evidence suggests that the positive impact of access to citizenship on migrants' saving choices derives from a decrease in uncertainty about their future right to stay which translates into uncertainty over their location and thus both the marginal utility they can expect to derive from consumption in retirement as well as the extent to which they will be able to access resources. Gaining that certainty makes them more willing to give up consumption in the present to prepare for the future they now have good reason to hope will come to pass.

1.5.5 Direct Evidence from Interviews

The reduced form findings provide strong evidence for the importance of accounting for uncertainty over future states of the worl in the context of migrant saving choices. However, as for all revealed preference analyses, the interpretation relies on a correct grasp on all the constraints guiding migrants' saving choices. To make sure that I account for all relevant dimensions of their decision problem, I have conducted

semi-structured focus group interviews with 26 migrants living in different parts of Germany. The interviews provide me with direct information on migrants' economic situation, beliefs, preferences and subsequent plans for the future. In the following, I briefly sketch the characteristics of the interviewees, the recruitment strategy and the main themes that emerged in the interviews. A detailed description of the set of questions, the recruitment procedure and interview summaries are available from the author.

The migrants I interviewed all had a direct migration experience and moved to Germany at different points in between 1997 and 2022. They were aged between 23 and 54 years at the time of the interview. The countries of origin included Afghanistan, Bulgaria, Brazil, Egypt, India, Iraq, Kazakhstan, Lebanon, Macedonia, Peru, Russia, Syria, Taiwan, Thailand, Ukraine and Vietnam. 9 out of the 26 interviewees arrived in Germany as refugees. 18 of the interviewees were married, and 14 had children under the age of 18 living with them. 14 interviewees were employed full time, 5 were employed part time, 1 was studying full time, and 6 were not in the labour force with 5 of them working in the house and taking care of children full time. I recruited the interviewees by cooperating with the network of adult learning centres ("Volkshochschulen", VHS) that administer the official German language and integration courses. Schools across Northrhine-Westphalia and Berlin sent out invitations to participate in the interviews to migrants who were enrolled in any of their courses. German language as well as integration courses are mandatory for most migrants living in Germany, decreasing selection bias.

There were three main themes that emerged from the interviews. First, uncertainty over their future right to stay and location loomed large in individuals' minds. 20 out of 26 interviewees told me a that they had held off on or were still holding off on long term investments either in Germany or their home country because they did not know for certain where they would live long term. 14 individuals said that this uncertainty derived at least in part from them not having a permanent right to stay or access to citizenship. 8 out of 11 interviewees who had invested in a house or a pension plan in Germany by the time of the interview said they had waited until at least one person in their household had obtained German citizenship.

Second, entrepreneurship is an important dimension when analysing the saving

choices of displaced individuals in particular. 6 out of the 9 individuals with refugee experience I interviewed reported challenges in having their degrees recognised in Germany and finding employment. Hence, a majority of them tried to become self employed and open their own businesses. The subsequent investments they described were as country specific as any investment in housing or pension plans. However, they were also seen as non-optional by the interviewees who wanted to work and independently support their families financially. Hence, even though 5 of the interviewees in this group said they held off on other country specific investments due to uncertainty over the future, they still invested in overwhelmingly specific business capital. Refugees only make up a small minority of migrants in my GSOEP sample. Therefore, this dimension has little bearing on observable saving patterns in the reduced form. However, even there I find an increase in investments in firm capital as migrants gain access to citizenship.

Finally, access to credit is an important constraint on migrants' investments if they do not have a right to stay long term. 13 interviewees described challenges in making investments in assets such as vehicles, business capital or housing, due to an inability to obtain a loan on acceptable terms. In all cases, this was due to these interviewees not having a right to stay in Germany for more than 6 years when they applied for a loan. In Germany, the duration of a loan agreement is capped by the number of years, migrants have a right to stay in the country for by the time of their application. This means that migrants with an initially temporary right to stay will have to pay much higher rates of return on any loan they apply for than comparable natives. However, this effect is tied to the length of a migrants' right to stay, *not* their access to citizenship. In my main analysis, a majority of migrants have resided in the Germany long enough to have secured a long term right to stay, albeit one that is usually tied to employment and does not extend into retirement. Hence, in the reduced form analysis, credit uptake is not sigificantly affected by gaining access to citizenship. However, it does highlight an important dimension to examine going forward.

1.6 Aggregate Implications

This paper shows that migrants without access to citizenship save significantly less than natives and migrants *with* the right to citizenship. This residual gap can be closed completely by granting migrants easier access to citizenship, a change that is accompanied by a higher propensity to invest in housing as well as private pension plans in Germany specifically. These assets tend to make up the majority of individuals' retirement wealth (Poterba et al. [2011]) and have particularly high rates of return, granted that individuals can keep them to maturity (Choukhmane et al. [2022]). Thus, the observable changes imply that migrants will accumulate higher levels of retirement wealth when given access to citizenship. This effect is a particularly important amidst demographic change.

In Germany, more than 20% of the population are currently over the age of 65 and thus the retirement age. Over the next ten years, as the baby boomer generation retires, the share of people over the age of 65 is expected to rise to more than 26%. In comparison, the share of children and adolescents below the age of 18 is expected to remain at just 17%. Other Western countries face similar prospects. The impact of this process is expected to be so pronounced that Angela Merkel has declared demographic change to be one of the greatest force for "change in our social life as well as the personal lives of each individual in the first half of the 21. century".[41] In response, Germany, like many other Western countries, increasingly requires citizens to prepare for retirement privately, transitioning out of a fully funded and into a three pillar pension system. As a result, private wealth becomes an increasingly important predictor of precarity in old age. The substantial positive impact of access to citizenship on migrants' wealth accumulation rates, therefore significantly reduces their personal risk of poverty in old age. However, do these individual gains have aggregate implications for public spending as well?

If migrants choosing to stay in Germany will have a higher wealth stock by the time they retire, they are less likely to depend on government transfers during retirement. In general, there are different sources of state transfers that individuals can receive in retirement besides statutory pension payments, irrespective of whether they are a

[41] Original transcript of the speech given at the "Demograhic Summit can be found here.

German citizen or a non-German resident. Transfers are available to all individuals whose combined retirement income from the statutory and potential occupational and private pensions is less than the legal minimum below which individuals are determined to be "in need of help" (hilfebedürftig) — unless they have sufficient wealth. In practice, this currently means that an individual with a monthly net income of less than 893€ and not more than 5000€ in wealth would be eligible to receive "basic security in old age" (Grundsicherung im Alter) payments (pending case specific regulations). Furthermore, even if the requirements of "basic security in old age" payments are not met, individuals with a monthly net income of less than 432€ and less than 5000€ in wealth can receive "livelihood security" (Hilfe zum Lebensunterhalt) payments or housing assistance (Wohngeld) if the household net income is deemed insufficient to meet rent or upkeep costs (exact income cutoffs depend on number of people living in the household, area of residence, etc). In 2011, 12.7% of foreign citizens over the age of 65 living in Germany received "basic security in old age" transfer payments, compared to only 2% among German senior citizens Seils [2013]. That is 268,000 individuals. Amidst demographic and pension changes, this number has been steadily rising. In 2019, 33.14% of foreign citzens over the retirement age were at the risk of poverty in Germany, compared to 13.2% of senior citizens with a German passport.[42] This hints at the potentially large gains that can be made from granting migrants access to citizenship more swiftly.

From the perspective of the host society, another take away from the findings could be that migration should be limited to temporary migration. This is something the German government tried during the 1950s and 1960s with the "guest worker programme" where they recruited workers from Southern European states to come work in Germany for a set period of time. However, even though the programme was intended to be temporary, many migrants decided to stay in the Germany and over time moved their families over as well. Even though the German government stopped the programme in the 1970s and in the 1980s began to financially incentivise return migration, the number of migrants from Southern Europe working in Germany remained stagnant or, in the case of Turkish migrants for example, increased (Bundesanstalt für Arbeit [1990]). The historical experience suggests that it is difficult

[42]Numbers taken from the German statistical office.

to limit the duration of stay of migrants once they settle in a country. Furthermore, 36.5% of all direct migrants living in Germany have an EU passport. Thus, they have a permanent right to stay regardless of national legislation.

Finally, Germany, where in 2022 about 850,000 positions on average could not be filled due to a lack of workers[43], *needs* more migration to stabilise its workforce. And restrictive right to stay legislation makes a country a less attractive destination. Hence, it becomes ever more important to design immigration policy in a way that both makes a country an attractive destination for skilled foreign professionals — and enables long term economic and social integration. In recognition of this, the German government under Angela Merkel passed the "Skilled Immigration Act" in March 2020 which speeds up the recognition of foreign degrees as well as German language lessons among other things. And the current government under Olaf Scholz is planning to reduce residency requirements for naturalisation from 8 to 5 years within this legislative cycle.

1.7 Conclusion

In this paper, I show that uncertainty over their future right to stay depresses migrants' saving rates, and that this effect can be remedied by giving them access to citizenship. Migrants without access to citizenship save 30% less than comparable natives and migrants with a right to citizenship. This residual gap is closed completely when migrants gain access to citizenship which increases their saving rate across countries by 2.5pp. Ten years out, treated migrants still save on par with natives and other migrants. They do not decrease the transfers they send to family and friends living abroad, but some treated migrants shift savings from their home to the immigration country.

The increase in migrants' saving rates derives from the greater level of certainty they have over their future location. While standard economic theory predicts that saving increases in uncertainty over the future, I show that this prediction hinges on three implicit assumptions: that utility is state-independent, does not change over time in an unpredictable manner, and that resources are equally accessible in

[43] Number taken from the German statistical office.

any future state of the world. These assumption are unlikely to hold for migrants. I specify a life cycle saving model with uncertain retirement location and heterogeneous country preferences. The model predicts that uncertainty can decrease migrants' propensity to save whenever (i) they are uncertain about their future location and investments are partly country specific, (ii) they are uncertain about their future location and prefer living in one country over another, or (iii) they are uncertain about the evolution of their country preferences.

I then take the model to the data and test its predictions empirically. I find that migrants become more likely to invest in both liquid and illiquid assets upon gaining access to citizenship with a particularly strong effect on country specific investments in housing and pension plans. This increase does not stem from an increase in the resources migrants have at their disposal as neither their income nor their credit uptake rises. Instead, migrants save up to make these investments. Finally, the results are driven by migrants from non-EU countries and those who plan to stay long term. These results show that out of the three possible channels, the first two are empirically important in this context.

The findings of this paper have important conceptual implications. There are many situations in which individuals have to make forward looking choices without knowing either in which future state they will find themselves in or to which degree they will be able to access resources or enjoy consumption in this state. For example, this will be true whenever individuals are uncertain about their future family status, health status or the location they will live in. In all of these situations, individuals may have less incentive to save or even work due to a lower preparatory motive. This is an important insight for understanding dynamic choice under uncertainty.

Finally, the findings have immediate policy implications. Migrants hold substantially less wealth than natives in most Western countries and are at a greater risk of poverty in old age than natives. This inequality is set to increase amidst demographic change as private saving becomes an ever more important predictor of economic security in old age. Most factors shaping the wealth gap, for example discrimination in the labour market, are systemic and hard for policy makers to change. In citizenship legislation, this paper identifies a novel factor under direct government control which can decrease differences in migrants and natives wealth accumulation rates. This is

an important insight for governments trying to organise immigration in a way that allows migrants as well as the host society to thrive long term.

Appendix

1.A Formal Proofs

This section formally proves the key result: that uncertainty over future location reduces the saving rate of migrants who intend to stay in the immigration country. For brevity, I use a simplified two period version of the model. This does not affect the proof, but simplifies notation.

Location Choice. In the beginning of their life, migrants learn about their relative preference for their home country γ. Given that they know this as well as the rates of return in both countries q_2^I & q_2^H, and their income y, migrants have information on all relevant decision parameters already during their working age. Hence, they can perfectly predict whether or not they will choose to stay in retirement and the model can be solved through backwards induction. First, let us consider the case where migrants receive the same rate of return on the asset in both countries $q_2^I = q_2^H$. In this case, migrants can consume the same stock of wealth in either country in retirement: $c_2^I = c_2^H = c_2$. Consequently, in the situation without any risk, migrants with a preference for the immigration country gain more utility from consuming there, while for migrants with a preference for the home country the reverse is true:

$$\begin{aligned} u^I(c_2) > u^H(c_2) \quad \text{if } \gamma < 1 \\ u^H(c_2) > u^I(c_2) \quad \text{if } \gamma > 1 \end{aligned} \tag{1.10}$$

As a consequence, migrants with a preference for the immigration country always stay and migrants with a preference for their home country always leave. This decision does not change if $\theta > 0$. The risk reduces the expected utility from staying, but it is still always more than the expected utility of leaving for migrants who prefer the

immigration country. And always worse for migrants who prefer their home country:

$$\begin{aligned} &(1-\theta)\mathrm{u}^I(\mathrm{c}_2)+\theta\mathrm{u}^H(\mathrm{c}_2) > \mathrm{u}^H(\mathrm{c}_2) \;\text{ if } \gamma < 1 \\ &\mathrm{u}^H(\mathrm{c}_2) > (1-\theta)\mathrm{u}^I(\mathrm{c}_2)+\theta\mathrm{u}^H(\mathrm{c}_2) \;\text{ if } \gamma > 1. \end{aligned} \tag{1.11}$$

Now, let us turn to the situation where $\mathrm{q}_2^I > \mathrm{q}_2^H$. Migrants can now consume *more* in the immigration than in their home country during retirement: $\mathrm{c}_2^I = \mathrm{q}_2^I\mathrm{a}_1 > \mathrm{c}_2^H = \mathrm{q}_2^H\mathrm{a}_1$ for a given stock of wealth. This means that migrants with a preference for the immigration country still always want to stay. However, some migrants with a (weak) preference for the home country now also want to stay if the consumption utility surplus in the immigration country is great enough to compensate them for their "preference" loss:

$$\begin{aligned} \text{stay if:} \quad & \mathrm{u}^I(\mathrm{c}_2^I) \geq \mathrm{u}^H(\mathrm{c}_2^H) \\ \Leftrightarrow \quad & (2-\xi)\ \mathrm{u}(\mathrm{q}_2^I\mathrm{a}_1) \geq \xi\ \mathrm{u}(\mathrm{q}_2^H\mathrm{a}_1) \\ \Leftrightarrow \quad & \gamma = \frac{\xi}{(2-\xi)} \leq \frac{\mathrm{u}^{'}(\mathrm{q}_2^I\mathrm{a}_1)}{\mathrm{u}^{'}(\mathrm{q}_2^H\mathrm{a}_1).} \end{aligned} \tag{1.12}$$

This inequality either hinges only on the rates of return, or the level of savings also – depending on the assumed utility function. For utility functions where rates of return and level of savings are multiplicatively separable, the location decision hinges solely on the rate of return differential. For example, for Cobb Douglas utility of the form $\mathrm{u}(\mathrm{c}) = \mathrm{c}^\alpha$ where $0 < \alpha < 1$, the inequality simplifies to:

$$\text{stay if:} \quad \gamma = \frac{\xi}{(2-\xi)} \& < \frac{\mathrm{u}^{'}(\mathrm{q}_2^I\mathrm{a}_1)}{\mathrm{u}^{'}(\mathrm{q}_2^H\mathrm{a}_1)} \quad \Leftrightarrow \quad \gamma \& < (\frac{\mathrm{q}_2^I}{\mathrm{q}_2^H}). \tag{1.13}$$

In this case, migrants' location decision does not change if $\theta > 0$:

$$\begin{aligned} \text{stay if:} \quad & (1-\theta)\mathrm{u}^I(\mathrm{c}_2^I)+\theta\mathrm{u}^H(\mathrm{c}_2^H) \geq \mathrm{u}^H(\mathrm{c}_2^H) \\ \Leftrightarrow \quad & (1-\theta)\ (2-\xi)\mathrm{u}(\mathrm{q}_2^I\mathrm{a}_1)\ +\ \theta\ \xi\mathrm{u}(\mathrm{q}_2^H\mathrm{a}_1) \geq \xi\mathrm{u}(\mathrm{q}_2^H\mathrm{a}_1) \\ \Leftrightarrow \quad & (1-\theta)\ (2-\xi)\mathrm{u}(\mathrm{q}_2^I\mathrm{a}_1) \geq (1-\theta)\ \xi\mathrm{u}(\mathrm{q}_2^H\mathrm{a}_1) \\ \Leftrightarrow \quad & \gamma = \frac{\xi}{(2-\xi)} \leq \frac{\mathrm{u}^{'}(\mathrm{q}_2^I\mathrm{a}_1)}{\mathrm{u}^{'}(\mathrm{q}_2^H\mathrm{a}_1).} \end{aligned} \tag{1.14}$$

That is, the same migrants choose to either stay in or leave the immigration country

regardless of whether or not they face a risk in their right to stay. This is different, if for utility functions where q_2^I & q_2^H are not separable from a_1. There, the location and saving amount decision are interconnected. For example, for logarithmic utility of the form, $u(c) = \ln(c)$, the inequality becomes:

$$\text{stay if:} \qquad \gamma = \frac{\xi}{(2-\xi)} \& < \frac{u(q_2^I a_1)}{u(q_2^H a_1)} \qquad \Leftrightarrow \qquad \gamma \& < \frac{\ln(q_2^I a_1)}{\ln(q_2^H a_1)} \tag{1.15}$$

in the case without risk in right to stay. $\frac{u(q_2^I a_1)}{u(q_2^H a_1)}$ increases in a_1. So, if migrants own more than cutoff wealth $\bar{a}_1$, defined by $\gamma = \frac{u(q_2^I \bar{a}_1)}{u(q_2^H \bar{a}_1)}$, they want to stay in the immigration country, even if they have a (weak) relative preference for their home country. $\bar{a}_1$ will be lower, the higher q_2^I, the lower q_2^H or the lower γ is. Since migrants know q_2^I, q_2^H & γ from the beginning, they also know their personal cutoff wealth value $\bar{a}_1$. This means migrants' optimisation problem can still be solved through backwards induction. Only now they have to solve two optimisation problems when deciding where to stay and how much to save. Either they decide to save as much or more than the cutoff wealth value and stay in the immigration country. Or they decide to save less than the cutoff value and return to their home country. $\tilde{a}_1 \geq \bar{a}_1$ is the optimal saving amount if a migrant decides to save a lot and stay in the immigration country. That is, the amount which solves:

$$\begin{aligned} \max V(c_1, c_2) &= u^I(c_1^I) + \beta\, u^I(c_2^I) \\ &= u^I(y - a_1) + \beta\, u^I(q_2^I a_1). \end{aligned} \tag{1.16}$$

$\underset{\sim}{a}_1 < \bar{a}_1$ is the optimal saving amount if a migrant decides to save relatively less and return to their home country. That is, the amount which solves:

$$\begin{aligned} \max V(c_1, c_2) &= u^I(c_1^I) + \beta\, u^H(c_2^H) \\ &= u^I(y - a_1) + \beta\, u^H(q_2^H a_1). \end{aligned} \tag{1.17}$$

Knowing the optimal values for either location choice, migrants then compare and choose the path which generates the highest lifetime utility. That is, they choose to

stay, *if*:

$$\begin{aligned} & \mathrm{u}^I(\mathrm{y}-\tilde{\mathrm{a}}_1)+\beta\ \mathrm{u}^I(\mathrm{q}_2^I\tilde{\mathrm{a}}_1)\ \geq\ \mathrm{u}^I(\mathrm{y}-\underset{\sim}{\mathrm{a}}_1)+\beta\ \mathrm{u}^H(\mathrm{q}_2^H\underset{\sim}{\mathrm{a}}_1) \\ \Leftrightarrow \quad & \underbrace{\mathrm{u}(\mathrm{y}-\underset{\sim}{\mathrm{a}}_1)-\mathrm{u}(\mathrm{y}-\tilde{\mathrm{a}}_1)}_{\text{Cons utility in working age}}\ \leq\ \underbrace{\beta\ [\mathrm{u}(\mathrm{q}_2^I\tilde{\mathrm{a}}_1)-\gamma\ \mathrm{u}(\mathrm{q}_2^H\underset{\sim}{\mathrm{a}}_1)]}_{\text{Cons utility in retirement}} \end{aligned} \tag{1.18}$$

This decision is changed by the presence of risk in right to stay. For a given set of parameters, $\theta > 0$ decreases the number of migrants who would like to stay compared to the case where $\theta = 0$. This is because, for a given rate of return differential $\frac{\mathrm{q}_2^I}{\mathrm{q}_2^H}$, the cutoff value of γ beyond which migrants choose to return decreases:

$$\begin{aligned} & \mathrm{u}^I(\mathrm{y}-\tilde{\mathrm{a}}_1)+\beta\ [(1-\theta)\ \mathrm{u}^I(\mathrm{q}_2^I\tilde{\mathrm{a}}_1)+\theta\ \mathrm{u}^H(\mathrm{q}_2^H\tilde{\mathrm{a}}_1)]\ \geq\ \mathrm{u}^I(\mathrm{y}-\underset{\sim}{\mathrm{a}}_1)+\beta\ \mathrm{u}^H(\mathrm{q}_2^H\underset{\sim}{\mathrm{a}}_1) \\ \Leftrightarrow \quad & \underbrace{\mathrm{u}(\mathrm{y}-\underset{\sim}{\mathrm{a}}_1)-\mathrm{u}(\mathrm{y}-\tilde{\mathrm{a}}_1)}_{\text{Cons utility in working age}}\ \leq\ \underbrace{\beta\ [(1-\theta)\ \mathrm{u}(\mathrm{q}_2^I\tilde{\mathrm{a}}_1)+\theta\ \gamma\ \mathrm{u}(\mathrm{q}_2^H\tilde{\mathrm{a}}_1)-\gamma\ \mathrm{u}(\mathrm{q}_2^H\underset{\sim}{\mathrm{a}}_1)]}_{\text{Cons utility in retirement}} \end{aligned} \tag{1.19}$$

and:

$$\mathrm{u}(\mathrm{q}_2^I\tilde{\mathrm{a}}_1)-\gamma\ \mathrm{u}(\mathrm{q}_2^H\underset{\sim}{\mathrm{a}}_1)\ \geq\ (1-\theta)\ \mathrm{u}(\mathrm{q}_2^I\tilde{\mathrm{a}}_1)+\theta\ \gamma\ \mathrm{u}(\mathrm{q}_2^H\tilde{\mathrm{a}}_1)-\gamma\ \mathrm{u}(\mathrm{q}_2^H\underset{\sim}{\mathrm{a}}_1). \tag{1.20}$$

This means that if the rate of returns and level of savings are non-separable in the utility function, fewer migrants with a preference for their home country will choose to stay in the immigration country when there is risk in their right to do so. Specifically, only migrants with a weaker relative preference for their home country will choose to stay (i.e. a lower $\gamma > 1$). That is because it is costly to forego a lot of consumption and save high amounts during the working age. It is only profitable for migrants who suffer a smaller "preference" cost due to staying once risk in right to stay lower the expected purely economic returns during retirement.

Saving Choice. In anticipation of their location choice in retirement, migrants choose their optimal saving amount during their working age. In the case without risk to their right to stay, this yields the following optimality conditions: Migrants who want to stay $\mathrm{s} = 1$ save more, the higher the rate of return q_2^I and the patience

β:

$$\begin{aligned} &\frac{\delta \mathrm{V}(\cdot)}{\delta \mathrm{a}_1^I} = -\mathrm{u}'^I(\mathrm{c}_1^I) + \beta\ \mathrm{q}_2^I \mathrm{u}'^I(\mathrm{c}_2^I) \stackrel{!}{=} 0 \\ \Leftrightarrow\quad &\frac{\mathrm{u}'^I(\mathrm{c}_2^I)}{\mathrm{u}'^I(\mathrm{c}_1^I)} = \frac{1}{\beta \mathrm{q}_2^I} \quad \Leftrightarrow \quad \frac{\mathrm{u}'(\mathrm{c}_2^I)}{\mathrm{u}'(\mathrm{c}_1^I)} = \frac{1}{\beta \mathrm{q}_2^I} \end{aligned} \tag{1.21}$$

Migrants who want to leave, $\mathrm{s} = 0$ save more, the higher the rate of return q_2^H, the patience β, and the home country preference γ:

$$\begin{aligned} &\frac{\delta \mathrm{V}(\cdot)}{\delta \mathrm{a}_1^H} = -\mathrm{u}'^I(\mathrm{c}_1^I) + \beta\ \mathrm{q}_2^H \mathrm{u}'^H(\mathrm{c}_2^H) \stackrel{!}{=} 0 \\ \Leftrightarrow\quad &\frac{\mathrm{u}'^H(\mathrm{c}_2^H)}{\mathrm{u}'^I(\mathrm{c}_1^I)} = \frac{1}{\beta \mathrm{q}_2^H} \quad \Leftrightarrow \quad \frac{\mathrm{u}'(\mathrm{c}_2^H)}{\mathrm{u}'(\mathrm{c}_1^I)} = \frac{1}{\gamma\beta \mathrm{q}_2^H} \end{aligned} \tag{1.22}$$

In line with the literature, if $\mathrm{q}_2^I = \mathrm{q}_2^H$, migrants who plan to leave, save more:

$$\begin{aligned} &\mathrm{q}_2^I = \mathrm{q}_2^H \quad \Leftrightarrow \quad \frac{1}{\beta \mathrm{q}_2^I} = \frac{1}{\beta \mathrm{q}_2^H} \\ \Rightarrow\quad &\frac{\mathrm{u}'^I(\mathrm{c}_2^I)}{\mathrm{u}'^I(\mathrm{c}_1^I)} = \frac{\mathrm{u}'^H(\mathrm{c}_2^H)}{\mathrm{u}'^I(\mathrm{c}_1^I)} \quad \Leftrightarrow \quad \frac{\mathrm{u}'(\mathrm{c}_2^I)}{\mathrm{u}'(\mathrm{c}_1^I)} = \gamma\ \frac{\mathrm{u}'(\mathrm{c}_2^H)}{\mathrm{u}'(\mathrm{c}_1^I)} \\ \Leftrightarrow\quad &\frac{\mathrm{u}'(\mathrm{q}_2^I \mathrm{a}_1^I)}{\mathrm{u}'(\mathrm{y} - \mathrm{a}_1^I)} = \gamma\ \frac{\mathrm{u}'(\mathrm{q}_2^H \mathrm{a}_1^H)}{\mathrm{u}'(\mathrm{y} - \mathrm{a}_1^H)} \quad \Leftrightarrow \quad \frac{\mathrm{u}'(\mathrm{q}_2 \mathrm{a}_1^I)}{\mathrm{u}'(\mathrm{y} - \mathrm{a}_1^I)} = \gamma\ \frac{\mathrm{u}'(\mathrm{q}_2 \mathrm{a}_1^H)}{\mathrm{u}'(\mathrm{y} - \mathrm{a}_1^H)} \end{aligned} \tag{1.23}$$

since $\gamma > 1$ for migrants choosing to return, this implies:

$$\frac{\mathrm{u}'(\mathrm{q}_2 \mathrm{a}_1^I)}{\mathrm{u}'(\mathrm{y} - \mathrm{a}_1^I)} > \frac{\mathrm{u}'(\mathrm{q}_2 \mathrm{a}_1^H)}{\mathrm{u}'(\mathrm{y} - \mathrm{a}_1^H)} \quad \Rightarrow \quad \mathrm{a}_1^H > \mathrm{a}_1^I. \tag{1.24}$$

By the same logic, if $\mathrm{q}_2^I > \mathrm{q}_2^H$, only those migrants who plan to leave *and* for whom $\gamma > \frac{\mathrm{u}(\mathrm{q}_I^2 \mathrm{a}_1^I)}{\mathrm{u}(\mathrm{q}_H^2 \mathrm{a}_H^I)}$, save more than migrants planning to stay.

If migrants' face risk in their right to stay, the optimal saving choice of migrants intending to leave in retirement are unchanged:

$$\frac{\delta \mathrm{V}(\cdot)}{\delta \mathrm{a}_1^I} = -\mathrm{u}'^I(\mathrm{c}_1^I) + \beta\ [(1-\theta)\mathrm{q}_2^H \mathrm{u}'^H(\mathrm{c}_2^H) + \theta \mathrm{q}_2^H \mathrm{u}'^H(\mathrm{c}_2^H)] \stackrel{!}{=} 0 \quad \Leftrightarrow \quad \frac{\mathrm{u}'^H(\mathrm{c}_2^H)}{\mathrm{u}'^I(\mathrm{c}_1^I)} = \frac{1}{\beta \mathrm{q}_2^H}. \tag{1.25}$$

But the saving choice of migrants wanting to stay in the immigration country is affected:

$$\begin{aligned} & \frac{\delta \mathrm{V}(\cdot)}{\delta \mathrm{a}_1^I} = -\mathrm{u}'^I(\mathrm{c}_1^I) + \beta\ [(1-\theta)\mathrm{q}_2^I \mathrm{u}'^I(\mathrm{c}_2^I) + \theta \mathrm{q}_2^H \mathrm{u}'^H(\mathrm{c}_2^H)] \stackrel{!}{=} 0 \\ \Leftrightarrow \quad & \frac{\mathrm{u}'^I(\mathrm{c}_2^I)}{\mathrm{u}'^I(\mathrm{c}_1^I)} = \underbrace{\frac{1}{(1-\theta)} \frac{1}{\beta \mathrm{q}_2^I}}_{\mathrm{A}} - \underbrace{\frac{\theta}{(1-\theta)} \frac{\mathrm{q}_2^H}{\mathrm{q}_2^I} \frac{\mathrm{u}'^H(\mathrm{c}_2^H)}{\mathrm{u}'^I(\mathrm{c}_1^I).}}_{\mathrm{B}} \end{aligned} \tag{1.26}$$

A denotes the consumption utility migrants could get in the immigration country during retirement, adjusted for risk. And B denotes the consumption utility they could get in their home country during retirement, adjusted for risk and expressed in immigration country utils. To learn whether migrants intending to stay in the immigration country save more or less under risk, compare the Euler Equations in the two situations. In the situation without risk, migrants choose the saving amount that satisfies:

$$\frac{\mathrm{u}'^I(\mathrm{c}_2^{\star I})}{\mathrm{u}'^I(\mathrm{c}_1^{\star I})} = \frac{\mathrm{u}'^I(\mathrm{q}_2^I \mathrm{a}_1^{\star I})}{\mathrm{u}'^I(\mathrm{y} - \mathrm{a}_1^{\star I})} = \frac{1}{\beta \mathrm{q}_2^I} \tag{1.27}$$

where $\mathrm{a}_1^{\star I}$, $\mathrm{c}_1^{\star I}$ & $\mathrm{c}_2^{\star I}$ denote migrants' *optimal* saving and consumption levels. In the situation *with* risk, migrants choose the saving amount that satisfies:

$$\begin{aligned} \frac{\mathrm{u}'^I(\mathrm{c}_2^I)}{\mathrm{u}'^I(\mathrm{c}_1^I)} = \frac{\mathrm{u}'^I(\mathrm{q}_2^I \mathrm{a}_1^I)}{\mathrm{u}'^I(\mathrm{y} - \mathrm{a}_1^I)} &= \frac{1}{(1-\theta)} \frac{1}{\beta \mathrm{q}_2^I} - \frac{\theta}{(1-\theta)} \frac{\mathrm{q}_2^H}{\mathrm{q}_2^I} \frac{\mathrm{u}'^H(\mathrm{c}_2^H)}{\mathrm{u}'^I(\mathrm{c}_1^I)} \\ &= \frac{1}{(1-\theta)} \frac{1}{\beta \mathrm{q}_2^I} - \frac{\theta}{(1-\theta)} \frac{\mathrm{q}_2^H}{\mathrm{q}_2^I} \frac{\mathrm{u}'^H(\mathrm{q}_2^H \mathrm{a}_1^I)}{\mathrm{u}'^I(\mathrm{y} - \mathrm{a}_1^I)} \end{aligned} \tag{1.28}$$

where a_1^I, c_1^I & c_2^I denote the levels of saving and consumption migrants choose when they have to account for risk. Migrants save less under uncertainty if $\mathrm{a}_1^I < \mathrm{a}_1^{\star I}$. To find out whether this is the case, we rearrange the equation (22) so that $\frac{1}{\beta \mathrm{q}_2^I}$, the term independent of uncertainty, is isolated on one side:

$$\frac{1}{\beta \mathrm{q}_2^I} = (1-\theta)\ \frac{\mathrm{u}'^I(\mathrm{q}_2^I \mathrm{a}_1^I)}{\mathrm{u}'^I(\mathrm{y} - \mathrm{a}_1^I)} + \theta\ \frac{\mathrm{q}_2^H}{\mathrm{q}_2^I}\ \frac{\mathrm{u}'^H(\mathrm{q}_2^H \mathrm{a}_1^I)}{\mathrm{u}'^I(\mathrm{y} - \mathrm{a}_1^I)} \tag{1.29}$$

Now, we can equate terms 1.27 and 1.29, and rearrange to isolate the difference in utility differentials on the left hand side:

$$\frac{u'^I(q_2^I a_1^{\star I})}{u'^I(y-a_1^{\star I})} = (1-\theta)\ \frac{u'^I(q_2^I a_1^I)}{u'^I(y-a_1^I)} + \theta\ \frac{q_2^H}{q_2^I}\ \frac{u'^H(q_2^H a_1^I)}{u'^I(y-a_1^I)}$$

$$\Leftrightarrow \qquad \frac{u'^I(q_2^I a_1^{\star I})}{u'^I(y-a_1^{\star I})} - \frac{u'^I(q_2^I a_1^I)}{u'^I(y-a_1^I)} = \underbrace{\theta\ \left[\frac{q_2^H}{q_2^I}\ \frac{u'^H(q_2^H a_1^I)}{u'^I(y-a_1^I)} - \frac{u'^I(q_2^I a_1^I)}{u'^I(y-a_1^I)}\right]}_{m(\theta)}. \tag{1.30}$$

$a_1^I < a_1^{\star I}$ if $m(\theta) < 0$, that is if $\frac{q_2^H}{q_2^I}\ \frac{u'^H(q_2^H a_1^I)}{u'^I(y-a_1^I)} < \frac{u'^I(q_2^I a_1^I)}{u'^I(y-a_1^I)}$. If $q_2^I = q_2^H$ this always holds for migrants with a preference for the immigration country (who are the only migrants intending to stay if $q_2^I = q_2^H$):

$$u'^H(q_2^H a_1^I) < u'^I(q_2^I a_1^I) \quad \Leftrightarrow \quad \xi u'(q_2 a_1^I) < (2-\xi) u'(q_2 a_1^I) \quad \Leftrightarrow \quad \gamma < 1. \tag{1.31}$$

If $q_2^I > q_2^H$, this always holds for migrants who prefer the immigration country, as well as some migrants who prefer their home country (who might now also stay in the immigration country):

$$\frac{q_2^H}{q_2^I} u'^H(q_2^H a_1^I) < u'^I(q_2^I a_1^I) \quad \Leftrightarrow \quad \frac{q_2^H}{q_2^I}\ \xi\ u'(q_2^H a_1^I) < (2-\xi)\ u'(q_2^I a_1^I)$$

$$\Leftrightarrow \quad \frac{q_2^H}{q_2^I}\ \gamma\ u'(q_2^H a_1^I) < u'(q_2^I a_1^I) \quad \Leftrightarrow \quad \gamma\ < \underbrace{\frac{q_2^I}{q_2^H}\ \frac{u'(q_2^I a_1^I)}{u'(q_2^H a_1^I).}}_{>1} \tag{1.32}$$

This means that migrants who intend to stay in the immigration country save less if they face risk in their right to stay – both for $\gamma < 1$ and γ (weakly) > 1, while migrants who intend to leave the immigration country are unaffected by uncertainty. Furthermore, as $m(\theta)$ linearly increases in size (becomes more negative) in θ, migrants wanting to stay save less, the more risk they face. That is, they save less, the more likely they believe it is that the immigration country's government will ask them to leave after they stop working.

1.B Further Identification Examples

Think about Carl, born 1962 and migrated 1990 at age 28. He moves to Germany after the Alien Act established the age-dependent residency requirements and, upon arrival, expects to become eligible to naturalise in 2005, after 15 years of residence. However, when the Citizenship Act is enacted in 2000, he becomes eligible 5 years earlier as he immediately fulfills the newly reduced 8 year residency requirement. And Dolores, who was born in 1962 and migrated four years after Carl, in 1995. She expects to become eligible in 2010 and when she arrives. But she actually becomes eligible 7 years earlier, in 2003, in accordance with the newly shortened residency requirements of the Citizenship Act. Here again, two migrants with the same birth cohort become eligible at different points in time by virtue of being in different immigration cohorts. However, there can also be variation between people of the same immigration, but different birth cohorts. Take Emil, who comes to Germany in 1990 at age 8. He arrives the same year as Carl, but was born 20 years later than him. So unlike him, he expects to become able to naturalise in 1998, not 2005 – as Emil would fall under the adolescent regulations of the Alien Act and thus become eligible to naturalise after only 8 years of residence.

1.C Variable Definitions

My main outcome variables are the amounts households save in Germany as well as remit abroad — in absolute terms and as a fraction of the net household income available to them. The amounts are measured in euros per month adjusted for inflation while the saving and remittance rates reflect the fraction of the households' monthly net income spent on one or the other. The households' monthly net income as well as the amounts of money they save and remit are self reported measures that are elicited annually in the household questionnaire by professionally trained surveyors. To construct the measure of household remittances, I sum over the transfers made by each member of the household to recipients living abroad (parents, partners, children, other relatives and other people respectively). The saving measure has been available since 1992 and the remittance measure since 1996.

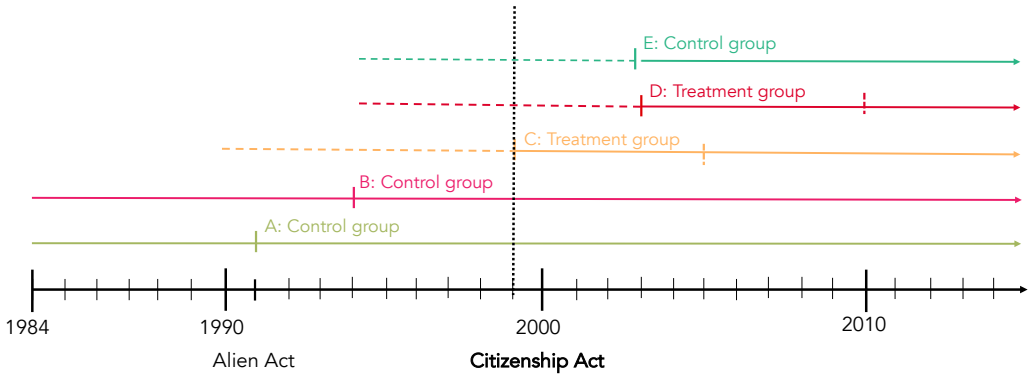
In addition to the main outcome variables, I also look at a number of other outcome variables that shed more light onto the motivation behind migrants adjusting their saving behaviour. Firstly, I look at migrants' intention to stay in Germany. In particular, I test the effect of eligibility on three binary variables indicating whether migrants say they want to stay one year maximally, several years or indefinitely in Germany. The variables are available from 1984 onwards and collected on the individual level. Secondly, I look at the propensity of households to save via different devices – namely, bank deposits, mortgage save plans, life insurance, fixed interest securities, unlisted or listed equity. I also look at their propensity to own the home they occupy. All of these binary variables are collected at the household level and are available from 1984 onwards with the exception of listed equity which was only added in 2000. Finally, I examine migrants' propensity to invest in a state subsidised, private pension plan ("Riesterrente"). This binary variable is available on the individual level. Individuals were first asked about paying into the scheme in 2004 and then again in 2006, 2007, 2010, 2013 & 2015.

The main explanatory variables I focus on are two sets of difference-in-difference estimates. One reflects being in the post-Citizenship Act era and either a migrant with or without access to citizenship prior to that. The other reflects changes in eligibility over time and whether or not those changes occured within a regime (expectedly) or

through a regime change (unexpectedly). I elaborate on how exactly these measures are constructed, how they identify the causal impact and which confounding factors I control for to ensure clean estimates in Sections 1.4 & 1.5.

1.D Treatment Group Assignment in the Difference-in-Difference Analysis around the 1999 Reform

FIGURE 1.D.1: Assignment to Treatment and Control Groups

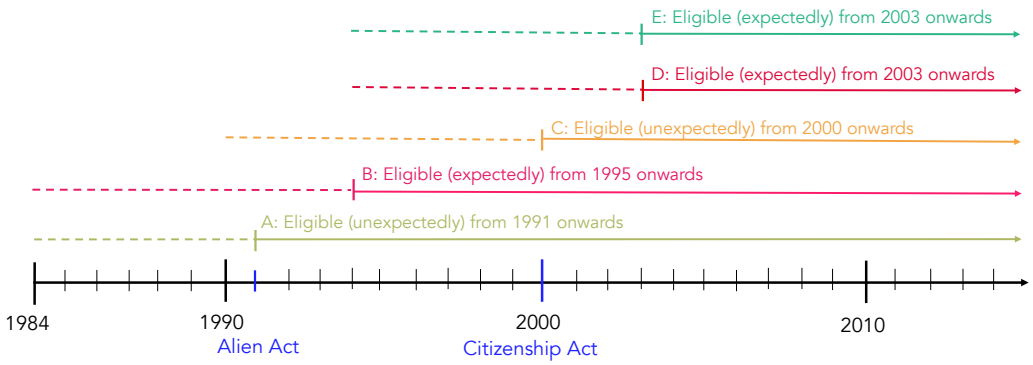


Notes: This graph depicts the core intuition behind the first identification strategy. Each line represents the changing location and legal status of a given migrant. Dashed horizontal lines denote a migrant who has moved to Germany but does not yet have access to citizenship. Solid lines denote migrants living in Germany with access to citizenship. On each line, the vertical tick denotes the point in time at which a migrant became eligible for citizenship. The black dashed vertical line indicates the passage of the Citizenship Act in the German parliament.

Figure 1.D.1 illustrates how migrants are categorised into either treatment or control group. Adult migrants who arrived in Germany before 1985 (such as migrants A & B) were eligible for citizenship before the reform passed and thus unaffected by the reduction in residency requirements. Similarly, adolescent migrants who became eligible for citizenship after 8 years of residency under the stipulations of the Alien Act (such as migrant E), were unaffected by the Citizenship reducing the requirements to 8 years for everyone. Thus, these migrants are classified as the control group. In contrast, adult migrants who arrived in Germany after 1985 would only become eligible after 2000 under the old regime and thus saw their citizenship prospects changed. Migrants who arrived before 1992 (such as migrant C) became immediately eligible for citizenship through the reform. Migrants who arrived after 1992 (such as migrant D) did not immediately become eligible, but saw their timeline of doing so drastically sped up. Both groups see the uncertainty over their future changed and are consequently classified as the treatment group. In Section 1.5, I analyse the impact of changes in actual legal status as a second step.

1.E Treatment Group Assignment Using Variation in Eligibility Over Time

FIGURE 1.E.1: Changes in Eligibility Over Time



Notes: This graph depicts the core intuition behind the second identification strategy. Each line represents the changing location and legal status of a given migrant. Dashed horizontal lines denote a migrant who has moved to Germany but does not yet have access to citizenship. Solid lines denote migrants living in Germany with access to citizenship. On each line, the vertical tick denotes the point in time at which a migrant became eligible for citizenship. The blue vertical ticks indicate the passage of the Alien and the Citizenship Act.

Figure 1.E.1 illustrates the identifying variation. Adult migrants who arrived in Germany before 1976 or adolescent migrants who arrived before 1983 become eligible immediately and unexpectedly through the passage of the Alien Act (such as migrant A). For these migrants, the dummy variables indicating eligibility and unexpected eligibility will switch to 1 thereafter while the dummy on expected eligibility will remain 0 throughout. From 1991 to 1995, the dummy indicating eligibility for 1 to 5 years will equal 1. From 1996 to 2001, the dummy indicating eligibility for 6 to 10 years will be equal 1, etc. Adult migrants who arrived in between 1976 and 1985 become eligible expectedly, within the 1990s citizenship regime (such as migrant B). In this case, the binary variables for eligibility and expected eligibility will equal 1 from 1995 onwards. Adult migrants who arrived between 1985 and 1992 became eligible unexpectedly through the passage of the Citizenship Act (such as migrant C). Adult migrants who arrived after 1992 became eligible unexpectedly under the new citizenship regime (such as migrant D, although many saw their expected lead up time drastically sped up in 2000). Adolescent migrants who arrived after 1983 always become eligible expectedly after 8 years (such as migrant E).

1.F Additional Result Tables

TABLE 1.F.1: Effect of the Citizenship Act on Saving & Remittances: Migrants Only

	Saving Amount GER	Saving Rate GER	Remittance Amount	Remittance Rate
Migrant Treatment * Post Reform	34.58**	0.0194***	1.697	0.000354
	(16.74)	(0.00627)	(14.42)	(0.00651)
Years in Germany Squared	0.0326	-5.95e-06	-0.0874***	-3.22e-05***
	(0.0651)	(1.94e-05)	(0.0274)	(1.16e-05)
Age Squared	-0.0318	-4.60e-06	0.000926	-1.99e-07
	(0.0314)	(1.05e-05)	(0.0179)	(6.65e-06)
Employed Full Time	-1.799	0.0224***	29.69***	0.0122***
	(27.28)	(0.00417)	(6.460)	(0.00317)
Employed Part Time	3.444	0.0136***	11.74*	0.00770*
	(17.85)	(0.00452)	(6.054)	(0.00421)
Years FT Employment	1.033	0.000251	1.861	0.000976*
	(2.936)	(0.000822)	(1.499)	(0.000536)
Years PT Employment	2.082	-0.00117	-0.775	0.000928
	(5.630)	(0.00225)	(4.048)	(0.00135)
HH Net Income	0.155***	1.69e-05***	-0.00356	-6.68e-06***
	(0.0442)	(3.00e-06)	(0.00331)	(1.73e-06)
Education Category	12.04	0.0106	2.105	-0.000874
	(22.72)	(0.00881)	(9.946)	(0.00431)
Years of Education	-1.588	-0.00186	-0.0153	-0.000504
	(8.243)	(0.00364)	(4.565)	(0.00176)
Married	-26.01	-0.00450	33.22**	0.0140**
	(24.63)	(0.00934)	(15.98)	(0.00706)
Num People in HH	-47.53***	-0.00974***	-0.498	-0.000404
	(17.64)	(0.00286)	(3.703)	(0.00157)
Young Children in HH	13.65	-0.000338	-15.84*	-0.00679*
	(20.76)	(0.00609)	(8.569)	(0.00390)
Observations	5,802	5,801	4,849	4,848
R-squared	0.150	0.061	0.034	0.036
State, Year & HH FE	YES	YES	YES	YES

Notes: This table shows the effects of citizenship eligibility on saving and remittance rates using quasi-experimental variation due to the 1999 citizenship act passing the German parliament. Migrants with access to citizenship form the omitted category. Column one shows results on the level of savings, column two on the saving rate, column three on remittances levels, and column four on the remittance rate. Regressions all include state, year and household fixed effects, and are estimated by OLS. Standard errors are given in parentheses and are clustered at the household level. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 1.F.2: Effect of Eligibility for Citizenship on Migrants' Resources

	Credit Amount	Credit At All	Net HH Income	Employed FT
Eligible	1.725	0.00895	-17.51	0.00582
	(2.845)	(0.0604)	(68.12)	(0.0377)
Years in GER Squared	-0.00275	-0.0000467	0.322	-0.000136
	(0.00785)	(0.000184)	(0.319)	(0.000132)
Age Squared	-0.00294	-0.000119	-0.363	-0.000466***
	(0.00960)	(0.000175)	(0.235)	(0.000104)
Employed Full Time	1.137	0.0766***	624.0***	
	(1.296)	(0.0266)	(41.65)	
Employed Part Time	0.724	0.0840**	308.8***	
	(1.471)	(0.0381)	(85.45)	
Years FT Employment	0.416	0.0142**	16.75	0.0444***
	(0.311)	(0.00696)	(11.91)	(0.00655)
Years PT Employment	0.149	0.0217**	-35.67	0.0525***
	(0.528)	(0.0109)	(23.97)	(0.0172)
HH Net Income	0.00208**	7.67e-08		
	(0.000993)	(0.0000111)		
Education Category	2.152	0.0782*	123.8	0.0572
	(2.172)	(0.0409)	(109.6)	(0.0486)
Years of Education	-0.0462	-0.0138	30.75	0.0132
	(0.880)	(0.0153)	(45.17)	(0.0197)
Married	2.445	0.0222	145.2**	-0.0298
	(2.596)	(0.0591)	(68.18)	(0.0487)
Num People in HH	0.202	-0.00347	329.4***	-0.00588
	(0.930)	(0.0147)	(26.58)	(0.0105)
Young Children in HH	-1.305	-0.0314	-247.9***	-0.0281
	(1.686)	(0.0376)	(63.84)	(0.0231)
State, Year & Individual FE	YES	YES	YES	YES
R2	0.501	0.507	0.661	0.578
N	3921	4071	7285	7511

Notes: The table shows the causal effect of citizenship eligibility on a household's resources. Column one shows results on total credit, column two an indicator for any credit, column three net household income, and column four an indicator of whether an individual within the household is employed full time. All columns include state, year, and household fixed effects. Standard errors in parentheses clustered at the household. Stars indicate significance level: *** p<0.01,** p<0.05,* p<0.1.

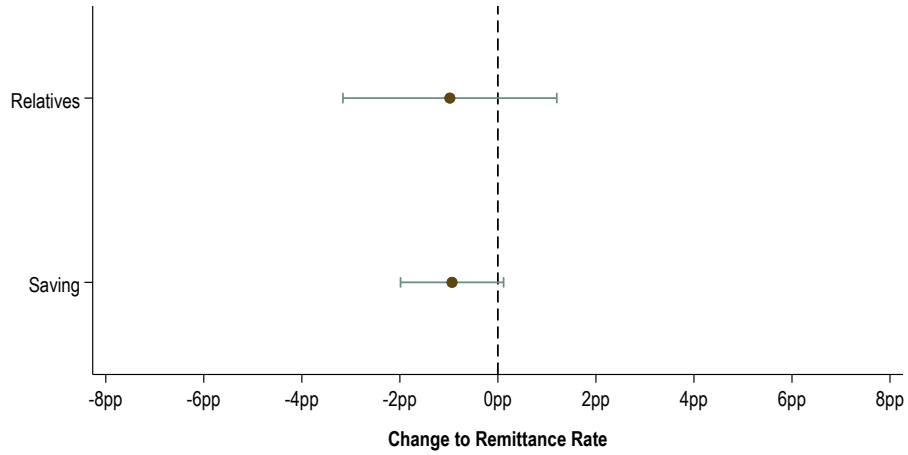
TABLE 1.F.3: Effect of Eligibility for Citizenship on Identity & Worries

	Disadv Origin	Worry Econ Sit	Worry Empl	Feel Ger
Eligible	0.0813	0.0936	0.0942	0.0487
	(0.110)	(0.0625)	(0.0825)	(0.104)
Years in GER Squared	0.000201	-0.0000593	-0.000146	-0.000361
	(0.000333)	(0.000209)	(0.000256)	(0.000513)
Age Squared	0.000328	-0.000145	-0.000426**	-0.000427
	(0.000247)	(0.000158)	(0.000215)	(0.000284)
Years FT Employment	0.00386	-0.00155	-0.0153	0.0229
	(0.00981)	(0.00822)	(0.0135)	(0.0152)
Years PT Employment	-0.00744	-0.00160	-0.00708	0.0183
	(0.0192)	(0.0152)	(0.0260)	(0.0339)
Education Category	-0.142**	0.00976	0.0170	0.288*
	(0.0667)	(0.0572)	(0.0759)	(0.155)
Years of Education	0.0514*	-0.0131	-0.0561**	0.00393
	(0.0284)	(0.0236)	(0.0272)	(0.0548)
Married	-0.0690	-0.131*	-0.149	-0.140
	(0.0760)	(0.0709)	(0.117)	(0.171)
Num People in HH	0.0243	0.00961	0.00652	0.0268
	(0.0188)	(0.0125)	(0.0185)	(0.0215)
Young Children in HH	0.0126	-0.00659	-0.0248	-0.161**
	(0.0484)	(0.0352)	(0.0464)	(0.0790)
State, Year & Individual FE	YES	YES	YES	YES
R2	0.475	0.438	0.397	0.600
N	3048	6199	5173	2840

Notes: This table shows the effect of citizenship eligibility on identity variables, and variables indicating the worries an individual has. Column one shows results on feeling disadvantaged due to one's origin. Column two shows results on whether individuals are worried about their economic situation. Column three shows results on whether individuals are worried about their employment situation. Column four shows results on whether an individual feels German. All regressions include state, year, and individual fixed effects. Standard errors are clustered at the household level.

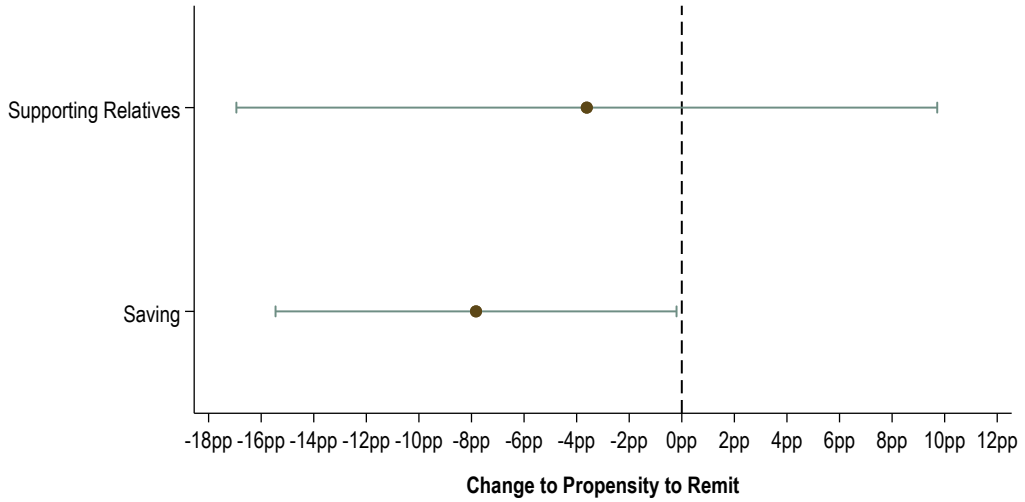
1.G Additional Result Figures

FIGURE 1.G.1: 1990 DID, Residual Remittance Rate



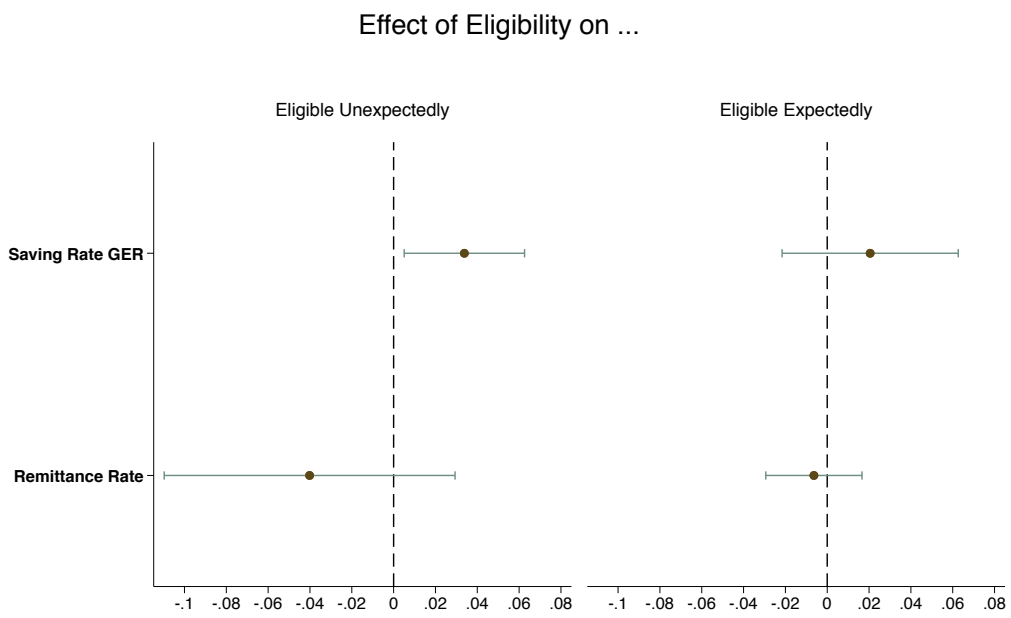
Notes: This graph shows the effects of eligibility due to the citizenship act reform on remittance rates of affected migrants. The first marker and corresponding 95% confidence interval shows the effects on remittances sent as transfers to relatives. The second marker and corresponding 95% confidence interval shows the effect on remittances sent as means of saving. Standard errors are calculated by clustering at the household level.

FIGURE 1.G.2: 1990 DID, Propensity to Remit



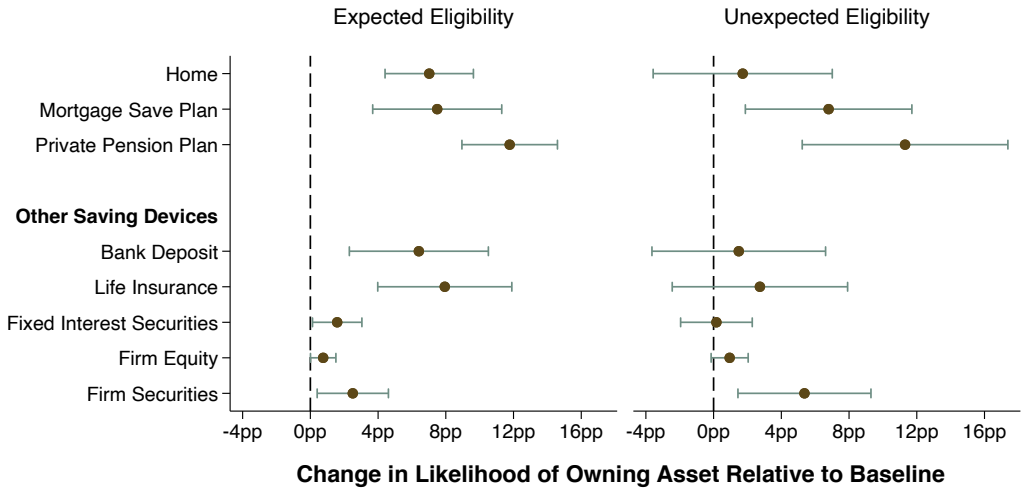
Notes: This figure depicts the effect of becoming eligible for citizenship relative to merely gaining the prospect of future eligibility on households' propensity to remit money for the purpose of supporting relatives and saving respectively. The variation stems from the Alien Act passing parliament in 1990. Markers indicate point estimates and caps represent 95% confidence intervals. Standard errors are clustered at the household level.

FIGURE 1.G.3: Effect of Becoming Eligible Expected or Unexpectedly On Saving and Remittances



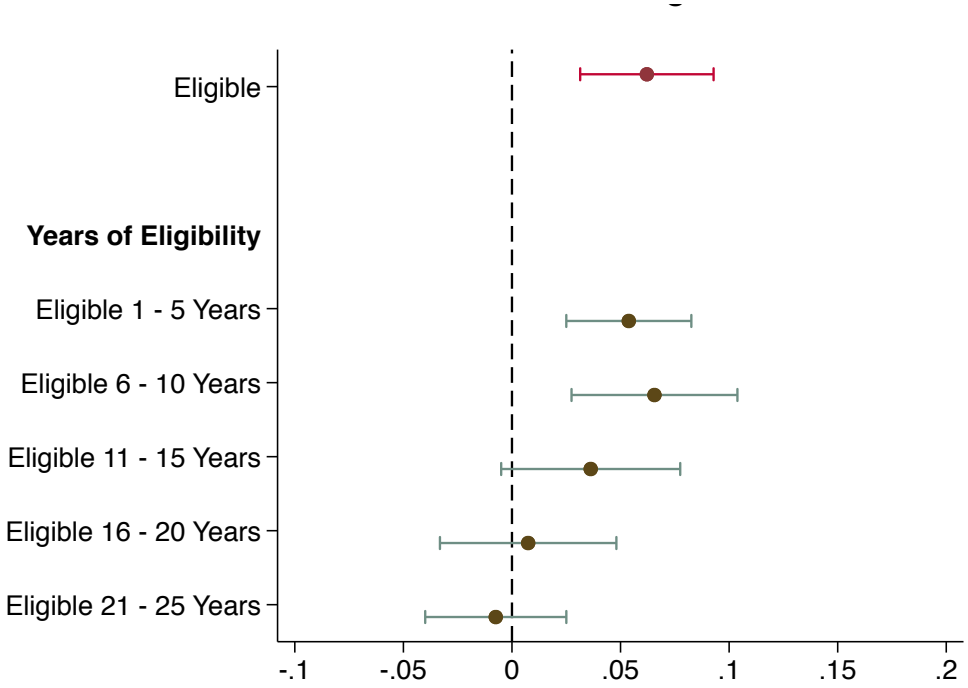
Notes: This figure shows the effect of citizenship eligibility on household saving and remittance rates separately for households that become eligible expectedly (right-hand side) and unexpectedly (left-hand side). Markers indicate point estimates and caps represent 95% confidence intervals. Standard errors are clustered at the household level.

FIGURE 1.G.4: Effect of Becoming Eligible Expected or Unexpectedly on Propensity to Own Different Assets



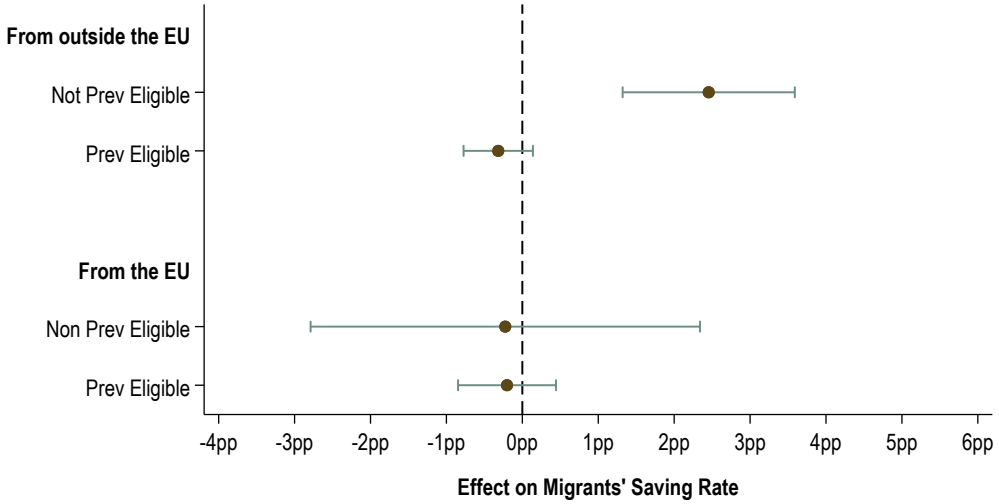
Notes: This figure shows the effect of citizenship eligibility on households' likelihood to own different saving technologies separately for households that become eligible expectedly (right-hand side) and unexpectedly (left-hand side). Markers indicate point estimates and caps represent 95% confidence intervals. Standard errors are clustered at the household level.

FIGURE 1.G.5: Effect of Eligibility and Years of Eligibility on Propensity to Own One's Home



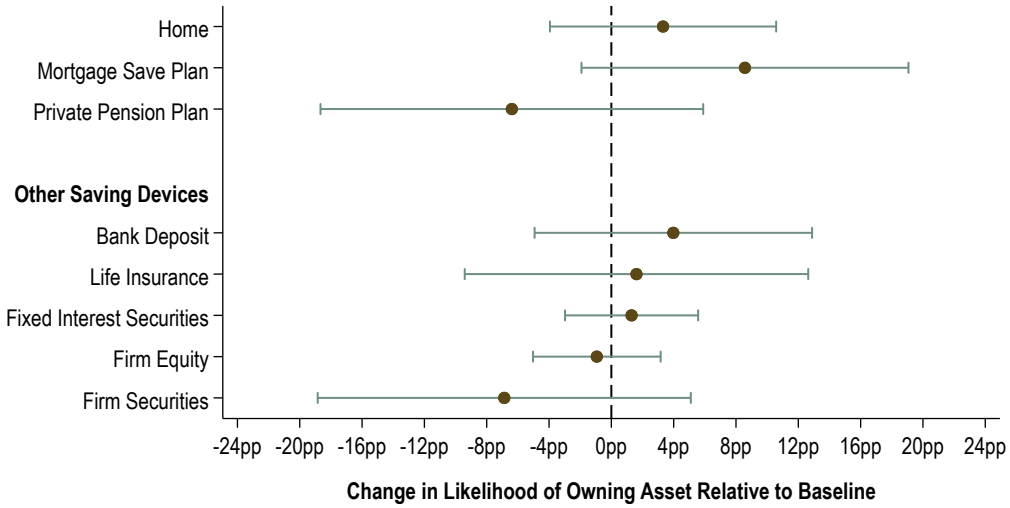
Notes: This figure shows the effect of eligibility and years of eligibility on the propensity to own one's own home. Markers represent point estimates and caps represent 95% confidence intervals. Standard errors are calculated by clustering at the household level.

FIGURE 1.G.6: Effect of Becoming Eligible on Saving Rates of EU Migrants



Notes: This figure is replicating Figure **??** for migrants from EU15 countries. Markers represent point estimates and caps represent 95% confidence intervals. Standard errors are calculated by clustering at the household level.

FIGURE 1.G.7: Effect of Becoming Eligible on Investment Choices of EU Migrants

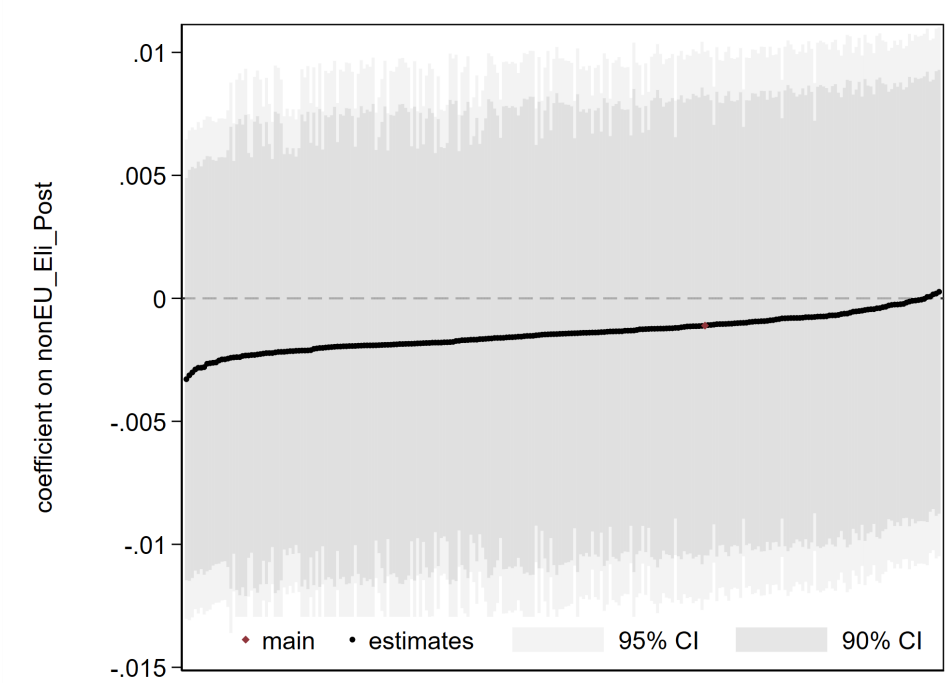


Notes: This figure is replicating Figure 1.5 for migrants from EU15 countries. Markers represent point estimates and caps represent 95% confidence intervals. Standard errors are calculated by clustering at the household level.

1.H Robustness of Main Results

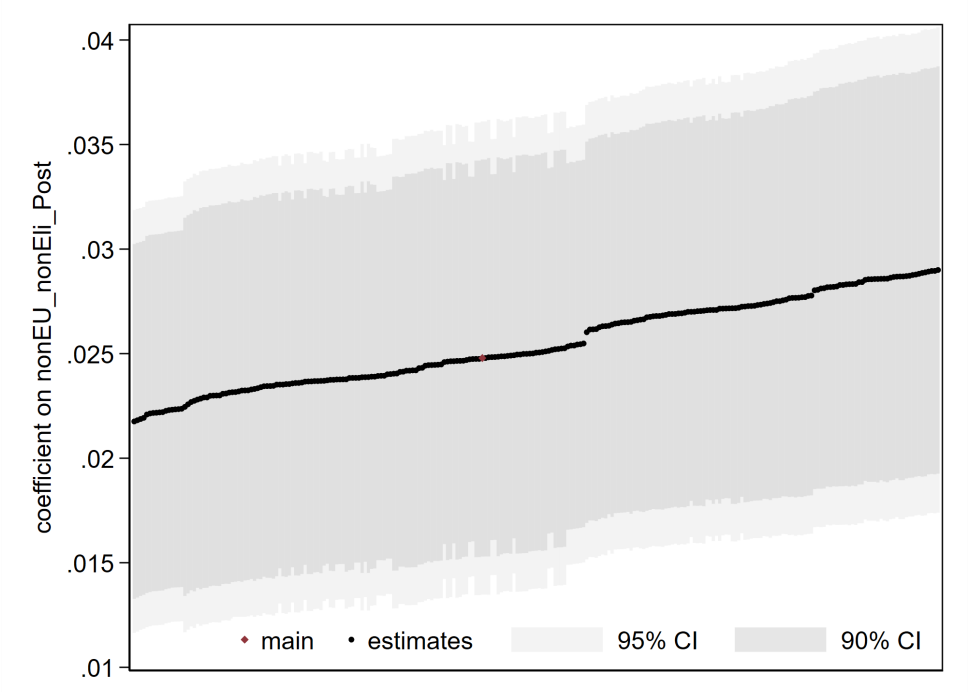
Figures in this section show the robustness of the main results in table 1.2 to permuting over all possible combinations of control variables (Figures 1.H.1 and 1.H.4) and fixed effects (Figures 1.H.3 and 1.H.2). To retain computational tractability these two sets of permutations are presented separately. In all cases, the placebo group presents no effect of citizenship on saving, and the treatment group a positive effect. This gives strong evidence to suggest that effects are not driven by the specific combination of control variables used. I additionally show that results are robust to removing outliers.

FIGURE 1.H.1: Robustness: Effect of Citizenship on Already Eligible Migrants' Saving Rate



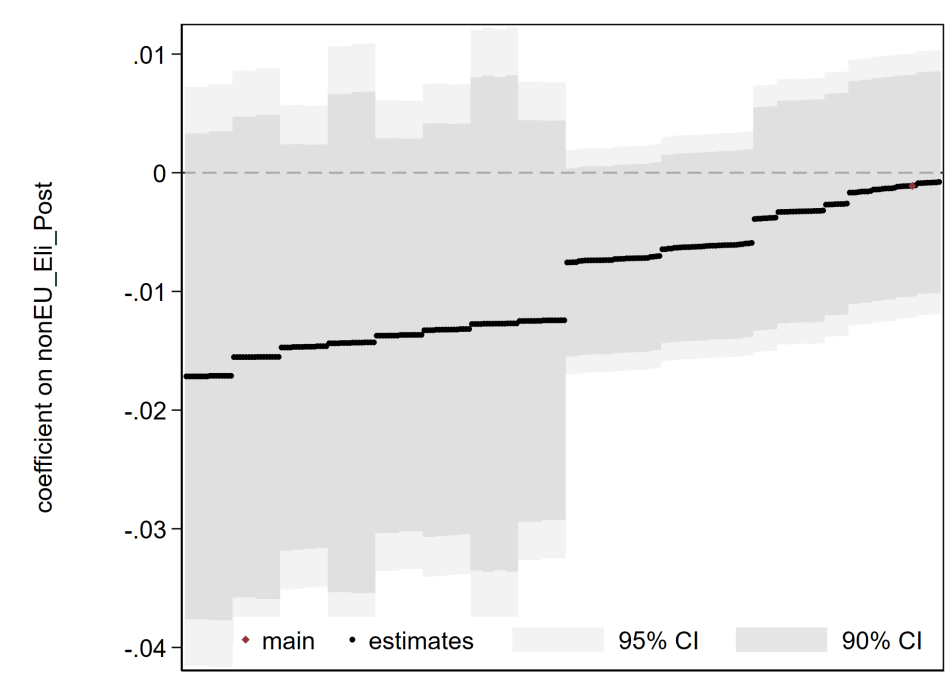
Notes: This figure shows the robustness of the main citizenship on savings rates results shown in column two of table 1.2 in the main paper. This figure focuses on the effect on the control group, migrants who were previously eligible. Each marker corresponds to a regression with a different set of controls, 95% and 90% confidence intervals are given in light and dark grey. The coefficient used in the main text is given in red. Each regression uses a different permutation over possible controls. These controls are: employed full time, employed part time, years full time employed, years part time employed, education level, years of education, marriage status, presence of children in the household. Each possible combination of these controls corresponds to one regression on the figure. All regressions also include household, year, and state fixed effects. All regressions include controls for years in Germany, age (squared), household income, and the number of people in the household.

FIGURE 1.H.2: Robustness: Effect of Citizenship on Not Already Eligible Migrants' Saving Rate



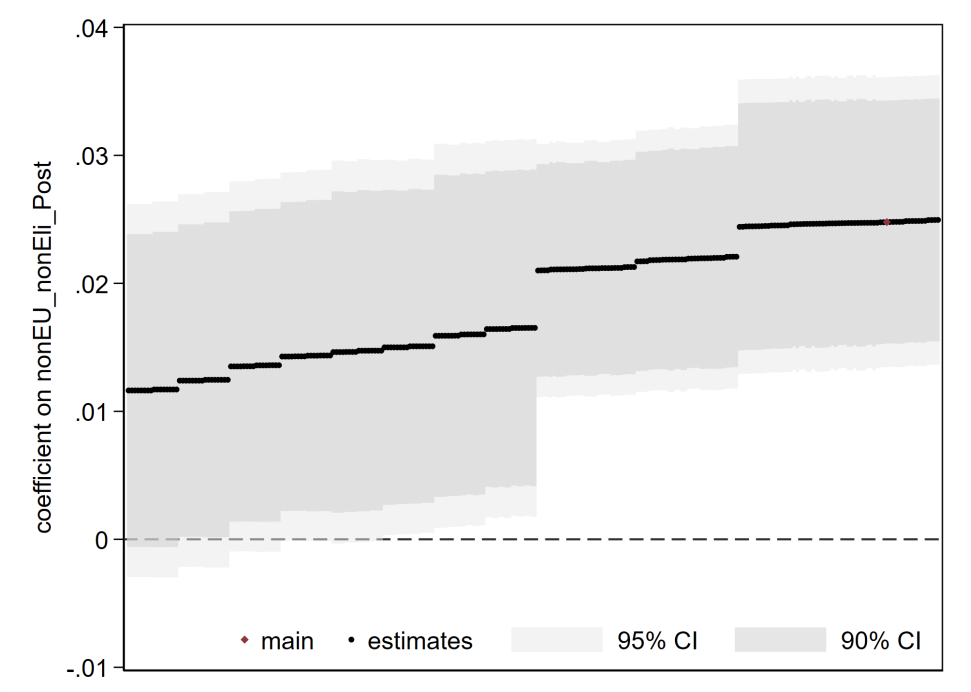
Notes: This figure shows the robustness of the main citizenship on savings rates results shown in column two of table 1.2 in the main paper. This figure focuses on the effect on the treatment group, migrants who were not previously eligible. Each marker corresponds to a regression with a different set of controls, 95% and 90% confidence intervals are given in light and dark grey. The coefficient used in the main text is given in red. Each regression uses a different permutation over possible controls. These controls are: employed full time, employed part time, years full time employed, years part time employed, education level, years of education, marriage status, presence of children in the household. Each possible combination of these controls corresponds to one regression on the figure. All regressions also include household, year, and state fixed effects. All regressions include controls for years in Germany, age (squared), household income, and the number of people in the household.

FIGURE 1.H.3: Robustness: Effect of Citizenship on Not Already Eligible Migrants' Saving Rate



Notes: This figure shows the robustness of the main citizenship on savings rates results shown in column two of table 1.2 in the main paper. This figure focuses on the effect on the control group, migrants who were previously eligible. Each marker corresponds to a regression with a different set of controls, 95% and 90% confidence intervals are given in light and dark grey. The coefficient used in the main text is given in red. Each regression uses a different permutation over possible fixed effects. These fixed effects are: year of immigrants, state, age, origin country, location in 1989, birth region, and an east/west dummy. Each possible combination of these fixed effects corresponds to one regression on the figure. All regressions also include all controls in the baseline regression as well as household and year fixed effects.

FIGURE 1.H.4: Robustness: Effect of Citizenship on Not Already Eligible Migrants' Saving Rate



Notes: This figure shows the robustness of the main citizenship on savings rates results shown in column two of table 1.2 in the main paper. This figure focuses on the effect on the treatment group, migrants who were not previously eligible. Each marker corresponds to a regression with a different set of controls, 95% and 90% confidence intervals are given in light and dark grey. The coefficient used in the main text is given in red. Each regression uses a different permutation over possible fixed effects. These fixed effects are: year of immigrants, state, age, origin country, location in 1989, birth region, and an east/west dummy. Each possible combination of these fixed effects corresponds to one regression on the figure. All regressions also include all controls in the baseline regression as well as household and year fixed effects.

Chapter 2

The Effect of Conduct Grades on Socio-Emotional Skill Accumulation

Socio-emotional skills are important predictors of life outcomes. Thus, it is worrying that, from an early age, we observe persistent gaps in socio-emotional skills between children from different backgrounds. We test whether grading primary school students on non-academic attainment, such as social conduct and work ethic, can affect socio-emotional skill formation and reduce these gaps. To this end, we exploit a natural experiment in Germany where various states introduced conduct grades at different points during the 2000s. Using current and archival legal texts, we construct a measure of standardised feedback on social and work behaviour in school. We find that receiving conduct grades in primary school causes students to be more open to experience and willing to take risks at age 17. Even more importantly, we find that students from more disadvantaged backgrounds grow up to be less neurotic and present biased adolescents if they received conduct grades in primary school – decreasing a detrimental existing gap. The findings provide suggestive evidence that a light touch schooling intervention can narrow socio-economic gaps by shifting the salience of and perceived returns to children's socio-emotional skills.

2.1 Introduction

Personality, preferences and socio-emotional skills such as patience, empathy or perseverance determine many outcomes in later life – ranging from educational attainment to labour market success or partnerships [e.g. Heckman et al., 2006, Deming, 2017]. The vast majority of job postings, irrespective of field, mention that successful candidates must demonstrate work ethic and social skills – be that through the ability to work in a team, the ability to work independently or the ability to organise oneself efficiently. This illustrates that on the labour market success is determined not only by cognitive but also socio-emotional skills. Thus, it is worrying that gaps in socio-emotional skills open up early in life between children depending on their background. Differences by gender and socio-economic status appear to be especially persistent [e.g. Cunha and Heckman, 2007, Heckman, 2008, Heckman et al., 2008, Heckman, 2013]. However, socioeconomic status does not seem to be the causal driving force behind these gaps. Instead, it appears to be a proxy for risky family environments which impede skill development [Heckman, 2008]. Various programmes have been launched to mitigate these gaps – for example through after-school support or mentoring initiatives. However, while these programmes have been shown to decrease gaps in socio-emotional skills [e.g. Heckman et al., 2008, Kosse et al., 2018], they also tend to be costly in terms of both resources and time. Thus, they are hard to scale up. In this paper, we investigate if grading students on not just academic attainment but also dimensions such as work ethic or social conduct can affect the formation of socio-emotional skills in children and thus represent a low-cost addition to more targeted and intensive interventions. Specifically, we examine whether grading children's behaviour in primary school can close some of the persistent gaps we observe between male and female children as well as children from high and low socioeconomic backgrounds once they grow up to be teenagers.

Grades on conduct could affect skill formation in different ways. Firstly, they could shift children's perception of their own strengths and weaknesses and prompt them to try and improve in non-academic dimensions such as time management. However, the effect here could go also in the opposite direction, as bad grades could

demoralise children and inhibit skill formation. Conduct grades could also affect parents' or teachers' perception of the children and subsequently shift their investments. A growing literature in economics highlights the importance of parental investments in the production of both cognitive and socio-emotional skills. The role that parental beliefs play in parents' investment decisions has also been the focus of recent studies aimed at explaining differences in parental time allocations across different groups [Attanasio, 2015]. So far, however, it still remains to be conclusively established which factors exactly shape parental investment choices. We propose that *salience* as well as perceived *returns to investments* could, at least partly, determine parents' (as well as teachers and children's) decision to invest in socio-emotional skills. Parents with limited resources, for example in terms of time they can spend with their children, may concentrate on fostering the skills they deem most important to their children's success later in life. Once children are in school, parents regularly get updates on their offspring's attainment in maths, foreign languages, etc. Thus, these academic dimensions become particularly salient. Moreover, the fact that the school system puts emphasis on children's progression in these subjects through grades may suggest to parents that a mastery of algebra may carry particularly high returns on investment. Consequently, parents with time and resource constraints may choose to focus on fostering academic rather than time- or conflict-management skills in their children. For example by helping them with their homework rather than explaining the values of prioritisation. However, if children are graded on *both* their academic *and* their non-academic achievements in school, this could potentially shift the salience of and perceived returns to socio-emotional skills – rendering parents more willing to invest in these skills (rather than cognitive skills exclusively). Analogously, teachers and children themselves might become more inclined to invest in socio-emotional skills if they are graded not only on academic attainment but, for example, social conduct and work ethic as well.

We study the impact of conduct grades on socio-emotional skill formation in the context of the German education system where education policy is decided largely at the state rather than the federal level. During the 1990s, close to no states were formally grading students on non-academic attainments in school. However, during the 2000s, many states introduced (and in two cases removed again) formal conduct

grades on at least two dimensions – work ethic and social conduct – with some giving grades on as many as seven dimensions. We collect legal documents on current and past school policies in each of the 16 German states dating back to the 1990s. We then use the information on shifts in different forms of feedback across states and time to construct measures of formal feedback on non-academic attainment. In our main analysis, we look at the effect of receiving *numeric* grades on conduct. In our robustness checks, we also examine the role of grade-like and standardised feedback in affecting the formation of socio-emotional skills. We exploit the exogenous time variation in conduct grades depending on the state a child grew up in, to identify the impact of conduct grades on the development of children's personality and socio-emotional skills. We focus on conduct grades in primary school as socio-emotional skills have been shown to be especially malleable at a young age. We use data from the German Socio-Economic Panel, a large representative survey of private households in Germany. For our main analysis, we concentrate on the repeated cross-section sample of 17 year-old children of SOEP respondents that were surveyed over the years 2000-2017. The adolescent survey includes comprehensive personality measures as well as measures of economic preferences and beliefs. We match this data with comprehensive information on the adolescents' household and background characteristics of their parents from the adult and household questionnaires. In our analysis, we focus on different measures of socio-emotional skills. First, we explore whether conduct grades affect personality by looking at the Big Five personality traits. The Big Five model is a psychological taxonomy that encapsulates the core of an individual's personality [Digman, 1990]. The five traits identified by this taxonomy are *Openness to Experience, Conscientiousness, Extraversion, Agreeableness* and *Neuroticism*. Recent research in economics demonstrates the existence of a strong link between the Big Five personality traits and socio-economic outcomes such as earnings, occupation, marriage, smoking and crime [see e.g. Heckman et al., 2006, Le Moglie et al., 2015]. Second, we focus on adolescents' economic preferences and look at risk tolerance, patience, trust, self-esteem and locus of control, which have been identified as important predictors of a vast array of later-life outcomes, from human capital investment to wages and occupational choice as well as crime and health [Heckman et al., 2006, Caliendo et al., 2015, Piatek and Pinger, 2016, Deckers et al., 2017b, Caliendo et al.,

2020].[1]

We find that, overall, receiving conduct grades renders children more open to experience and willing to take risk. More importantly, being graded on conduct during primary school decreases the gap between children from different socioeconomic backgrounds with regard to present bias as well as neuroticism. Girls and adolescents from a low socio-economic (SES) background who are graded on conduct display 0.2 standard deviation lower scores on neuroticism compared to their counterpart who did not receive conduct grades. Similarly, boys and low-SES adolescents score around 0.2 standard deviation lower on present bias at age 17 if they received conduct grades during primary school. However, several other coefficients for the effect of conduct grades on socio-emotional skills can only be estimated imprecisely. This is presumably due to the fact that we are confined to using high level variation, where we cannot observe the actual grades a student received. Nevertheless, this limitation makes the fact that we find significant effects for some dimensions ten years after the subjects attended primary school in some ways even more encouraging with regard to the potential of conduct grades to affect socio-emotional skill formation in a persistent way. Our results are robust to considering different forms of grading, which suggests that it is the information itself rather than the modality in which this is given that affects skills formation. Finally, we also examine the effect of receiving conduct grades in high school on socio-emotional skills at age 17. In this part of the analysis, we focus on the effect of being graded on conduct *only* during high-school, i.e., we compare students who received conduct grades in high-school only to those who were never graded on their conduct. Our coefficients for receiving conduct grades throughout high-school are imprecisely estimated, possibly due to the high-level variation in our treatment. Yet, it seems, as though receiving conduct grades throughout high school does not significantly alter students' personality or economic preferences at age 17.

We contribute to several strands of literature. Firstly, we contribute to the literature on socio-emotional skill formation. A growing literature in economics stresses the importance of socio-emotional skills for a variety of later life outcomes, e.g. Deming [2017], Kautz et al. [2014], Almlund et al. [2011], Carneiro et al. [2007]. With this,

[1] Locus of control is a psychological concept measuring the degree to which people believe that they, as opposed to external forces beyond their influence, have control over the outcome of events in their lives [Rotter, 1966].

there has been increased interest in the *formation* of these skills, and in particular how *parental investments* at different points in childhood translate to skills later in the child's life. Cunha and Heckman [2007] develop a framework where parental investments at a young age have the largest effect on children's future outcomes, especially if they are followed up on with investments during adolescence. They also stress that socio-emotional skills not only are relevant to attaining better outcomes later in life, but also foster cognitive development. This is in line with empirical evidence on better environments in childhood being more impactful the sooner a child is exposed to them [e.g. Chetty et al., 2016, Katz et al., 2001]. However, while several studies document that children from lower socioeconomic backgrounds have worse cognitive as well as non-cognitive outcomes [e.g. Deckers et al., 2017a, Heckman, 2013], it has yet to be established how *exactly* parental investments factor into this. So far, much of the literature focuses on parental income and education as inputs into the production function generating children's abilities. However, Heckman and Kautz [2013] identify two programs that taught parents behaviours enabling them to foster their children's socio-emotional and cognitive skills respectively. Specifically, the "Catch 'Em Being Good" is a curriculum designed to teach parents skills that may help them with their children's behavioural problems [Hawkins et al., 1991], while the "How to Help Your Child Succeed in School" is a curriculum designed to help parents foster their children's cognitive development [Abbott et al., 1998]. The former teaches parents skills such as reinforcement techniques, expectation setting, etc. – while the latter teaches parents how to create an environment conducive to learning [Lonczak et al., 2002].

Closer to this paper, another strand of the literature investigates how initiatives in and after school can shape children's skills and outcomes. For example, Heckman et al. [2008] and Heckman [2008] analyse the effect of the "Perry Pre-School Program" which ran between 1962 and 1967 and provided children from disadvantaged backgrounds with 2.5 hours of extra classroom sessions on weekdays plus a 1.5 hour home visit once a week during preschool. They find that even though participants' cognitive skills do not appear to be higher long term, their socio-emotional skills do improve – translating into better outcomes later in life such as probability to earn higher salaries or own their home. Kosse et al. [2018] show that pairing children from lower socioeconomic backgrounds with a mentor (normally university students volunteering for the

role) who once a week spends an afternoon with them can significantly improve the mentee's social skills. Alan and Ertac [2018] demonstrate that encouraging students to imagine their future selves through a structured curriculum makes them more patient in experimental tasks. Finally, Alan et al. [2019] show that training teachers to foster grit in their students renders them more likely to work towards acquiring task-specific skills, opt for harder yet more rewarding tasks and subsequently succeed in those. We contribute to this literature, by testing whether a more *light touch* intervention, receiving conduct grades in primary school, can contribute to lowering differences in socio-emotional skills. By showing that conduct grades can decrease the gap between children from different socioeconomic backgrounds with regard to neuroticism and present bias, we provide suggestive evidence for a low-cost, scalable addition to more intensive policy measures.

Furthermore, we contribute to the literature on how differences in the education system affect children's outcomes. A variety of studies show that smaller class sizes and differences in class composition can improve students' grades [e.g. Dobbelsteen et al., 2002, Angrist and Lavy, 1999]. Also, spending more time in school, either through longer school days or longer school years can improve students' academic achievements [e.g. Hincapie, 2016, Patall et al., 2010, Pischke, 2007]. The psychology literature also documents that delays in school start time can improve student' mental health [Owens et al., 2010]. Focusing on school reforms in Germany, several papers document that the G8 reform, reducing school time by one year for students in the highest track and compensating for that change with longer school days, significantly altered academic and non-academic outcomes. In particular, it increased repetition rates [Huebener and Marcus, 2015], reduced criminal behaviour [Westermaier, 2016], and rendered students less emotionally stable [Dahmann and Anger, 2014]. We contribute to this literature by documenting how schools employing conduct grades can impact skill formation in children. We are further planning to extend this work to trace the impact of the changes in the grading regimes on later life outcomes as well.

The rest of the paper is structured as follows. Section 2.2 explains the institutional context. Section 2.3 presents the data. Section 2.4 lays out the empirical strategy. Section 2.5 presents the main results and Section 2.6 concludes.

2.2 Institutional Context

The focus of this paper is Germany where the federal structure of the education system combined with a number of reforms to states' school policies allows us to isolate the effect of formalised feedback on the formation of socio-emotional skills in children.

In Germany, education policies are largely under the jurisdiction of the states rather than the federal government. This means that, while there is a shared education infrastructure in terms of Germany's three-tier education system as well as jointly agreed upon curricula, many details of schooling are decided at the state level. One area where states differ is in the use of *conduct grades*. After World War II, pupils in all German states received grades on their work ethic and social conduct in varying detail – irrespective of whether they grew up in Western Germany or the former GDR. However, while the East German states kept conduct grades as a part of their grading system until reunification, many Western German states started to remove them from transcripts during the 1970s. So, by the 1990s, most Western German states were no longer grading students' conduct – either in primary or high school (see Tables 2.3.1 & 2.3.2 for specifics). And almost all Eastern German states (with the exception of Saxony which retained a version of formalised feedback) followed suit after reunification. This consensus stayed in place for nearly a decade. However, starting with Saxony which reintroduced numerical grades for "conduct", "diligence", "participation" and "tidiness" in 1999, more and more states began to reintroduce conduct grades during the 2000s. These grades often comprise just two dimensions, work ethic and social conduct, but some states also opt for a more detailed breakdown. For example, schools in Brandenburg differentiate between attainments in independence, perseverance, diligence, motivation, responsibility, participation & conflict management. These grades are located prominently at the top of the transcript (hence the German word "Kopfnoten" meaning "head grades") and thus very salient to parents or other legal guardians. Students are graded on either a six or a four point scale, ranging from 6 ("ungenuegend", insufficient) or 4 ("aussreichend", sufficient) to 1 ("sehr gut", very good).

The introduction and removal of conduct grades are hotly debated issues in Germany. Two states, Northrhine-Westphalia and Bavaria, introduced conduct grades during the 2000s only to ban them again a few years later, after a change in the governing coalition. Opponents have argued that grading students on non-academic aspects of their performance in school may hinder them developing freely and that teachers may struggle with being objective. Advocates stress that they may lead students to acquire skills which are crucial for success in later life and informative for future employers. However, the validity of either of these claims has yet to be tested – which is one thing we aim to do in this paper.

2.3 Data

Our main data source is the German Socioeconomic-Panel (GSOEP), a large representative longitudinal dataset of private households in Germany.[2] The GSOEP was first launched in 1984 and has since been carried out annually. Each wave surveys adult members from approximately 15,000 households. Since 2000, the survey includes a separate module which is administered to 17-year-old members of GSOEP households in the year before they enter the adult panel. The adolescent data is therefore a repeated cross-section. The adolescent survey module contains questions on the child's personality traits and preferences, as well as their expectations for the future and ambitions. We merge data from the adolescent module with detailed information on household and parental characteristics from the adult and household modules.[3] In particular, we obtain detailed information on household income and household composition, region of residence and whether the household resides in a rural or urban area. We also collect information on the labour market status and educational attainment of the child's parents. Our estimation sample includes individuals for whom we have information on all the above characteristics and the outcome variables.

[2]The data version used in this paper is SOEPv34. DOI: 10.5684/soep.v34

[3]See Schupp and Wagner [2002], Goebel et al. [2019] for a description of the dataset.

2.3.1 Sample

Table 2.3.1 presents descriptive statistics for the sample of adolescent respondents to the GSOEP.[4] The sample is split evenly between boys and girls. Around 23% of children are living in a single parent household at the time of data collection, whilst 79% of the sample grew up primarily with both parents. 89% of the sample has siblings, and the average household monthly income is €1075. 36% of children live in rural areas at the time of data collection. Mothers of the adolescents in our sample are on average less educated than fathers: whilst 18% of children have mothers who completed the *Abitur* (the school-leaving degree that allows access to university education), the corresponding figure for fathers is 21%. 76% of adolescents have mothers who work either full-time or part-time. Finally, 30% of the sample is either a direct or indirect migrant.

Table 2.3.1: Sample Characteristics

	N	Mean	S.D.
Male	7789	0.500	0.500
Single parent household	7789	0.227	0.419
Grew up primarily with both parents	7636	0.791	0.407
Siblings	7408	0.895	0.307
Household monthly income (EUR)	7365	1074.898	605.116
Abitur - Mother	7657	0.183	0.386
Abitur - Father	7319	0.211	0.408
Mother in work	5970	0.760	0.427
Rural Area	7789	0.360	0.480
Migrant background	7789	0.306	0.461

Notes: Source: GSOEP, survey years 2000-2017. The sample includes respondents to the youth module. 'Single parent household' is a dummy variable indicating whether the adolescent is currently living with one parent only. 'Grew up with both parents' takes value one if the adolescent spend a minimum of 8 years in a household with both parents present. 'Abitur' indicates that parents have completed the high-school academic track. 'Migrant background' takes value one if the respondent has a direct of indirect migration background, and zero otherwise.

2.3.2 Treatment Variables

In our main analysis, we want to investigate if the plausibly exogenous variation in students receiving numerical conduct grades depending on the enrolment cohort

[4] All descriptive statistics and results are unweighted.

and state they grew up in affects the development of their personality and economic preferences. We further want to examine whether it is the *information itself* or the *grading* aspect that affects skill formation. Hence, we need detailed information on the education policies that were in place in each of the 16 different German states at any point over the last three decades. In order to achieve this aim, we collected current as well as archival legal documents that fall into any of the following categories: (i) education laws that detail grading schemes in place at specific points in time, (ii) reform proposals and decisions that record legal changes to grading schemes at different points in time, and (iii) sample transcripts for different school years that were issued by state governments and that teachers had to use at different points in time. An example of each of these documents can be found in Appendix Section 2.C.

From these documents, we categorise grading schemes into four different strands. Most states chose to give students numeric grades at the top of the transcript when reintroducing conduct grades (thereby choosing the "traditional" form). However, a few also opted for feedback in either grid or written form with clearly coded phrases. For example, Lower Saxony introduced a feedback scheme where from 2000 onwards, students' work & social conduct was commented on with one phrase each at the top of the transcript. However, these phrases were clearly mapped to grades at the bottom of the page, e.g. it said "deserves special recognition" translates to "1: very good". And a few states introduced formalised feedback in text or grid form either at the end of the transcript or in an extra sheet. For example, transcripts in Schleswig-Holstein contain a section for "interdisciplinary skills" at the end where teachers can give feedback on organisational skills, usage of methods, concentration, independence, engagement, team spirit & conflict management in a grid ranging from "secure" to "insecure" in five steps. Finally, a number of states ask teachers to write free texts on children's conduct. We do not consider this as formalised feedback on conduct, as in practice it may vary from a one liner to a paragraph even between students in the same class – and anecdotally we know that parents therefore but less emphasis on this form of feedback.

We further differentiate between students receiving conduct grades in primary and in high school. This is because, for one, primary and high school are governed by different laws and the grading approaches can vary between them. Furthermore,

in our main analysis we focus on primary school as socio-emotional skills have been shown to be particularly malleable at an early age and become more stable after puberty. Therefore, the effectiveness of conduct grades likely differs substantially between primary and high school. Finally, the impact of conduct grades in high school may be driven by demand side effects as they appear on transcripts student use to apply for apprenticeships and jobs after graduation.

Using all this information, we construct binary variables that indicate whether a child received numeric grades, grade-like feedback or standardised feedback in the latter two years of their primary school time (as children do not get any grades before), as well as their high school time. The variables will go to one whenever a child received conduct grades for the entirety of their respective school stint. The details of the variation of conduct grades in primary school can be found in Tables 2.3.1 & 2.3.2, while the details of the variation of conduct grades in high school can be found in Tables 2.D.1 & 2.D.2 in the Appendix.

2.3.3 Outcome Variables

Our main outcomes variable are the Big Five personality traits and economic preferences. Table 2.A.1 provides a description of the exact questions we use to construct these variables.

The Big Five. The Big Five model is a suggested taxonomy that psychologists have developed to encapsulate the core of an individual's personality. The five different personality traits are *Openness* (broadly capturing intellectual curiosity and aesthetic appreciation), *Conscientiousness* (adherence to social norms and diligence), *Extraversion* (whether one is typically outgoing), *Agreeableness* (ability to engage in harmonious relations with others) and *Neuroticism* (high levels of worry, stress or other negative emotions). These traits are partly heritable and thought to be most malleable during childhood and adolescence, whilst remaining largely stable thereafter [McCrae and Costa Jr, 2008, Caspi et al., 2005]. The adolescent module in the GSOEP contains sixteen questions that can be used to recreate the Big Five taxonomy through factor analysis. The Big Five traits are measured by asking respondents

FIGURE 2.3.1: Variation in Feedback on Conduct in Primary School

State	**Numerical Feedback**	**Gradelike Feedback**	**Text Feedback**	**No Standardised Feedback**
Schleswig-Holstein			1990s-present: formalised comments on behaviour in text or table form	
Hamburg				1990s-present
Lower Saxony		2000-present: clearly coded comments on 2 dimensions (map to grades)		1980s-2000
Bremen			2002-present: comments/ ticks on 2 dimensions; extra sheet	1980s-2002:
Northrhine-Westphalia	1950-1975 2007-2010: 6 dimensions in 2007, 3 thereafter			1976-2006 2011-present
Hessia	2000-present: 2 dimensions			1990s-2000
Rhineland-Palatinate				1990s-present
Baden-Wuerttemberg				1990s-present

Notes: In the time periods without standardised feedback students may have still received feedback from their teachers in the form of free texts. The time periods without standardised feedback may extend even further back – these are lower bounds for periods where legal documents ascertain that the state in question had no conduct grades.

FIGURE 2.3.2: Variation in Feedback on Conduct in Primary School

State	**Numerical Feedback**	**Gradelike Feedback**	**Text Feedback**	**No Standardised Feedback**
Bavaria	2005-2007: 7 dimensions			1990s-2004, 2008-present
Saarland	2000-present: 2 dimension			1990s-2000
Berlin			2005-present: comments/ ticks on 6 dimensions; extra sheet	1990s-2005
Brandenburg	1949-1990; 2006-present: 7 dimensions	2001-2005: clearly coded comments on 2 dimensions (map to grades)		1991-2000
Mecklenburg-Vorpommern	1949-1990 2008-present: 2 dimensions until 2013, 4 thereafter			1991-2008
Saxony	1949-1990 1999-present: 4 dimension	1991-1998: text grades on 2 dimensions		
Saxony-Anhalt	1949-1990 2003-present: 2 dimensions			1991-2002
Thuringia	1949-1990		2003-present: comments/ ticks on 6 dimensions; extra sheet	1991-2002

Notes: In the time periods without standardised feedback students may have still received feedback from their teachers in the form of free texts. The time periods without standardised feedback may extend even further back – these are lower bounds for periods where legal documents ascertain that the state in question had no conduct grades.

how much they identify or agree with different statements about themselves. All answers are elicited on a 7-point Likert scale, with 1 corresponding to ”Not at all” and 7 corresponding to ”Fully”. For the purpose of our analysis, we create standardised measures of the Big Five personality traits with mean zero and standard deviation of one for the full sample.

Preferences. Our variables capturing economic preferences include risk acceptance, present bias, trust, self-esteem and locus of control. Risk acceptance is calculated from categorical answers to the question “How willing are you to take risks?", on a scale from 0 (not at all) to 10 (very willing). This short survey item has been validated by Falk et al. [2013] and shown to correlate with experimentally elicited measures of risk tolerance. Our measure of patience is computed by combining answers to two questions about whether it is better to live for today without thinking of tomorrow, and sacrificing something today to enjoy it tomorrow. Answers to both questions are elicited on an 11-point Likert scale. We use factor analysis and compute an indicator for patience that has mean zero and standard deviation of one for the full sample. Our measure of trust combines information from three questions on weather one can trust people, one can rely on others and one can trust strangers. Answers were given on a categorical scale from 1 (low trust) to 7 (high trust), and we combine information from the three questions using factor analysis. Self-esteem is measured from answers to the question about whether the respondent has a positive attitude towards himself or herself, with answers given on a categorical scale from 1 (low self-esteem) to 7 (high self-esteem). Finally, we construct a measure of locus of control. This is a psychological concept originally attributed to Rotter [1966] that measures the extent to which individuals feel in control of their life, as opposed to their outcomes being predominantly the consequence of faith and luck. Locus of control is measured in the SOEP with a battery of ten questions about the relationship between one’s own behaviour and life outcomes. Answers are given on a 7-point Likert scale indicating the level of agreement of the respondent with the statements provided. We construct a standardised measure of locus of control with mean zero and standard deviation of one for the full sample. Higher scores indicate a more internal locus of control, i.e. that the respondent feels more in control of their own life.

Socioeconomic Gaps. Table 2.A.2 shows large heterogeneities in our outcome variables across the gender and socio-economic background dimension. Looking at the Big Five personality traits, we find that girls score higher than boys on all five traits. Turning to differences in personality by socio-economic background, our data show that low SES adolescents are less open, extraverted or agreeable and more neurotic than their high SES counterpart, but they are also more conscientious on average. Economic preferences also significantly vary across gender and SES. On average, boys are significantly more risk loving and present-biased than girls, but also have higher levels of trust in others and self-esteem and feel more in control of their lives. Turning to socio-economic gaps in preferences, we find that respondents from a low socio-economic background are more risk loving and present-biased, and have less trust in others relative to high-SES adolescents. Finally, low SES children feel like they have less control over their life events than high-SES respondents, despite showing similar levels of self-esteem.

2.4 Empirical Strategy

In our analysis, we exploit the plausibly exogenous variation in the introduction of conduct grades across states and over time in Germany to quantify the causal impact of increased salience of non-academic skills on socio-emotional outcomes. As described in Section 2.2, in some German states primary school children receive numerical grades or other forms of standardised feedback on their work ethic and social conduct, whilst in other states no such feedback on non-academic outcomes is given. Children usually receive grades for their conduct in years 3–4 of primary school.

Over the last two decades, most states have introduced changes to their conduct grade regime, both in terms of the way in which conduct grades are expressed and in terms of whether or not any feedback on non-academic outcomes is provided at all. In our main specification, we compare children who received conduct grades in years 3 and 4 of primary school to children who did not receive conduct grades or received them for only one year of their primary education. Further, our treatment variable

only refers to *numeric* grades for conduct. These are expressed on the same scale as grades for other academic subjects, and are therefore arguably easier to interpret for parents.[5] We use the adolescents' date of birth to identify their school cohort and assign a value for our treatment variable. Our baseline specification is:

$$y_{ist} = \alpha + \beta \text{ConductGrade}_{ist} + \gamma X_{ist} + \mu_s + \eta_t + \epsilon_{ist} \tag{2.1}$$

where $\text{ConductGrade}_{ist}$ is our treatment variable that takes value one if the child received numeric conduct grades throughout primary school and 0 otherwise, and y_{ist} is the outcome of interest of child i living in state s at time t. X_{ist} is a vector of individual and household characteristics, which includes the gender of the child, whether they have siblings, whether they grew up primarily with both parents, parental educational attainment, household income and an indicator for whether the mother is in work as well as indicators for living in a rural area and having a migration background. μ_s and η_t are state and year fixed effects, respectively, and ϵ_{ist} is the error term. The coefficient of interest is β, which captures the effect of receiving numeric conduct grades in years 3–4 of primary school on long-term socio-emotional outcomes at age 17. In our robustness checks, we use similar specifications to analyse the effect of receiving grade-like and standardised text feedback in years 3–4 of primary school. We estimate our main specification with ordinary least squares (OLS). In all specifications, we cluster standard errors at the year-state level, which is the level at which the exogenous variation in conduct grade policy takes place.

2.5 Results

In this section, we present results from our main specification as per equation 2.1, where we focus on the effect of receiving conduct grades in years 3 and 4 of primary school on long-term socio-emotional outcomes. We present results for the Big Five personality traits in Section 2.5.1, and for economic preferences in Section **??**. In both sections, we discuss results for the full sample, as well as separately by gender and SES. Section 2.5.3 examines the robustness of our results to different definitions

[5] In Saxony the grading scheme was differentiated in 1999 to encompass more facets of behaviour. However, since 1990 all students received conduct grades for work ethic and social conduct.

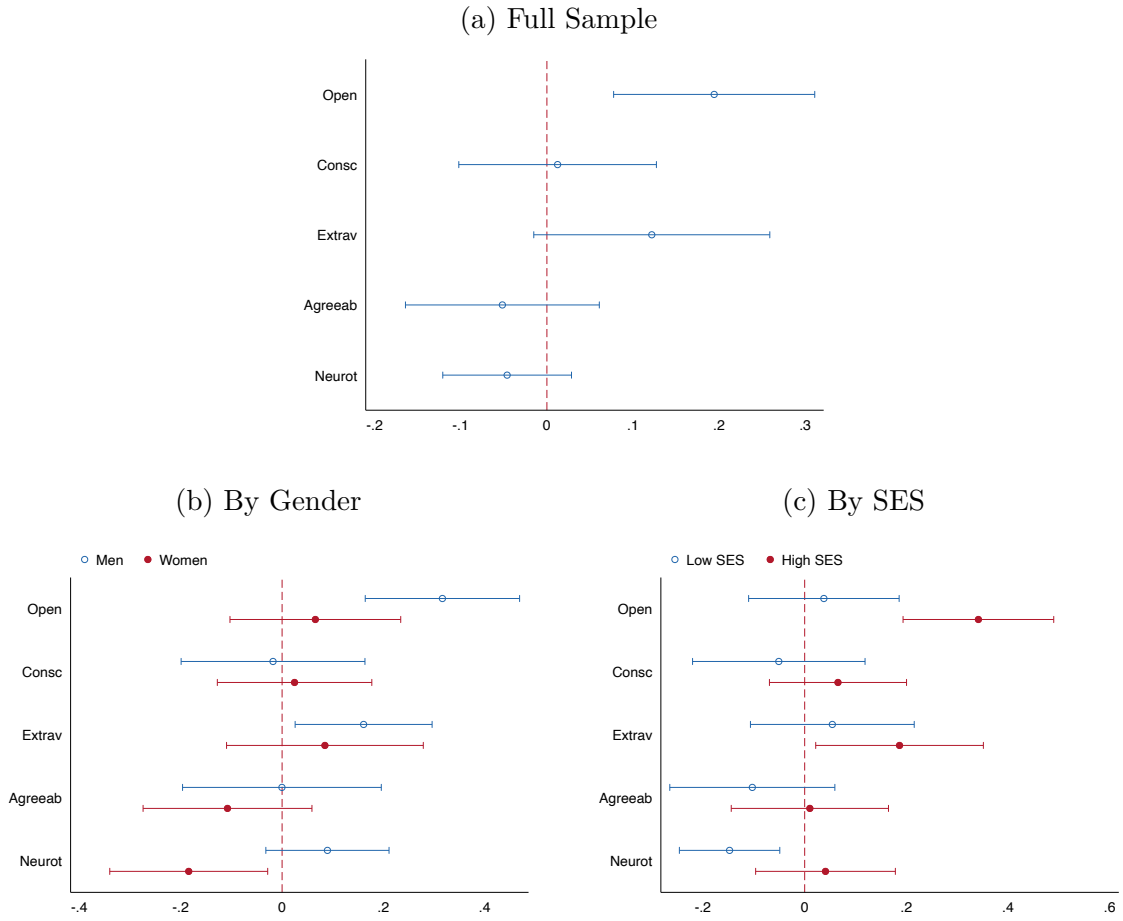
of the treatment variable. Finally, Section 2.5.4 expands the analysis to look at the effect of receiving conduct grades in high-school on the same set of socio-emotional outcomes.

2.5.1 Conduct Grades and Personality Traits

Result 1 *Receiving conduct grades in years 3 & 4 of primary school renders students more open to experience at age 17, an effect that is driven by male & high SES students. It further renders female students and low SES students less neurotic at age 17, reducing a detrimental gap.*

Panel (a) of Figure 2.5.1 shows results from our main difference-in-difference specification for the effect of conduct grades on the Big Five personality traits, estimated on the full sample of adolescents. Table 2.B.1 reports the complete set of coefficients. First, we confirm the existence of differences in personality traits at age 17 across several background characteristics. Gender differences are particularly stark, with boys being less conscientious, agreeable, extraverted and open, but also less neurotic than their female counterpart, everything else being equal. Such gender differences are sizeable: as an example, boys score almost half a standard deviation lower on the neuroticism index compared to girls, and about a quarter of a standard deviation lower on the agreeableness index. Having grown up primarily with both parents, as opposed to growing up with one parent, is also positively correlated with character traits such as conscientiousness and openness and reduces neuroticism, although it negatively correlates with extraversion. Household income is positively associated with extraversion and openness. Similarly, parental education is also an important predictor of openness. Turning to the effect of receiving conduct grades in years 3 and 4 of primary school, our results show that increasing the salience of socio-emotional skills increases openness to new experiences at age 17. We also note that, whilst the effect of conduct grades on the other personality traits is insignificant, there is evidence of a positive association between standardised grades for social and work ethic on conscientiousness and extraversion, and a negative correlation with neuroticism and agreeableness.

FIGURE 2.5.1: Conduct Grades in Primary School and the Big Five Personality Traits



Notes: The graph shows treatment effects from our baseline specification. Panel (a) shows results for the full sample. Panels (b) and (c) show results separately by gender and SES respectively). The caps represent 95% confidence intervals.

Table 2.B.4 explores heterogeneity in the impact of conduct grades on the Big Five personality traits along the gender and socio-economic dimension. Results are also graphically illustrated in Panels (b) and (c) of Figure 2.5.1. Looking at gender differences, we note that the positive effect of conduct grades on openness to new experiences is entirely driven by boys. Further, the coefficient for the effect of conduct grades on neuroticism is negative and significant for women. Given the existing higher levels of neuroticism among female adolescents compared to boys, this result suggests that the salience of conduct might positively contribute to reducing the gender gap in this dimension of personality. In Panels C and D of Table 2.B.4 we turn to differences

in the effect of receiving conduct grades during primary school across adolescents from different socio-economic backgrounds. For this purpose, we classify respondents as low SES if their household equalised monthly income is below €1065 and neither parent has completed their *Abitur*.[6]. Again, we find that the increase in openness that can be attributed to the introduction of conduct grades is confined to respondents from high socio-economic status. However, we note a significant decrease in neuroticism for low SES adolescents that can be attributed to the increased salience of socio-emotional skills. Given the importance of neuroticism as a predictor of labour market outcomes [see e.g. Goldberg et al., 1998, Fletcher, 2013], our evidence is indicative of the potential for a low-cost intervention to reduce socio-economic inequalities in the labour market.

2.5.2 Conduct Grades and Economic Preferences

Result 2 *Receiving conduct grades in years 3 & 4 of primary school renders students more willing to accept risks at age 17. It further renders male and low SES students less present biased at age 17, reducing a detrimental gap.*

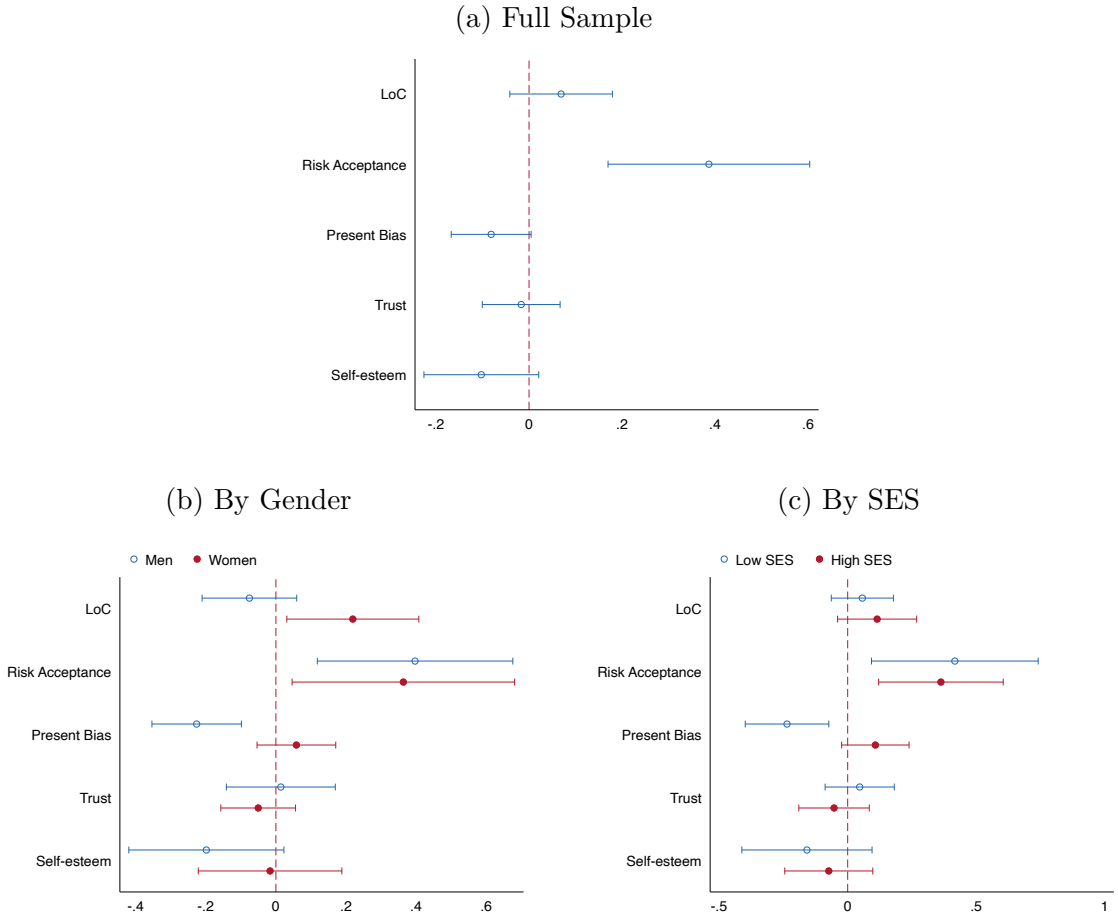
We now turn to examining the effect of receiving conduct grades through years 3 and 4 of primary school on adolescents' economic preferences. Panel (a) of Figure 2.5.2 shows estimates of our main coefficient of interest, for regressions on the full sample of adolescents, whilst Table 2.B.2 reports the complete set of coefficients. Our results show the existence of large differences in personality traits at age 17 across gender. Consistent with previous findings in the literature documenting gender differences in preferences, we show that boys are more likely to have an internal locus of control and higher self-esteem, but also more likely to be present biased and willing to accept higher risks [see e.g. Bleidorn et al., 2016, Falk et al., 2018].[7] For risk acceptance and self-esteem, these gender differences are particularly sizeable. Growing up with both parents also seems to be correlated with higher levels of internal locus of control and self-esteem, and lower risk acceptance and present bias. Finally, trust in others is

[6]With this classification, we follow Deckers et al. [2017a], Kosse et al. [2018] in their definition of SES as they too analyse the impact of SES on children's socio-emotional skills in Germany. €1065 corresponds to the 30% quantile of the German income distribution – calculated using the 2009 wave of the GSOEP and the cross-sectional weights provided therein. Finally, Abitur denotes the school-leaving degree qualifying for university studies.

[7]Differently from our results, Falk et al. [2018] document that women are less patient than men.

significantly and positively correlated with the educational levels of both parents, but negatively associated with being a first- or second-generation migrant. Turning to the effect of receiving conduct grades in years 3 and 4 of primary school, our results point to a positive, albeit insignificant, effect of receiving conduct grades on locus of control and a negative effect on present bias. However, conduct grades appear to significantly increase risk acceptance, and to reduce trust and self-esteem in the long-run.[8]

FIGURE 2.5.2: Conduct Grades in Primary School and Preferences



Notes: The graph shows treatment effects from our baseline specification. Panel (a) shows results for the full sample. Panels (b) and (c) show results separately by gender and SES respectively. The caps represent 95% confidence intervals.

[8]The latter two coefficients are not significantly distinguishable from zero.

Table 2.B.3 and Panels (b) and (c) of Figure 2.5.2 explore heterogeneity in the impact of conduct grades on preferences along the gender and socio-economic dimension. Looking at gender differences, we note that outcomes of male adolescents are more sensitive to the introduction of conduct grades in primary schools. In particular, we find a large and negative effect of conduct grades on present bias for boys, but not for girls, and larger treatment effects for self-esteem. Interestingly, girls' locus of control is positively associated with the introduction of conduct grades, whilst no significant effect can be detected for boys. Turning to differences in the effect of receiving conduct grades during primary school across adolescents from different socio-economic backgrounds, we find that present bias is significantly and negatively correlated with increased salience of work and social behaviour for the sub-sample of adolescents from a low socio-economic background. The effect of conduct grades for the other preferences we examine is similar across socio-economic groups.

2.5.3 Robustness

We conduct a number of robustness tests to check the validity of our results. First, as Saxony adopts a different grade scheme from other states, we construct a treatment variable for receiving numeric conduct grades in primary school that only takes value one for the states where the numeric scale was comparable. Results from this set of estimates are presented in Table 2.B.5 and Table 2.B.6. Results from this exercise confirm our main findings that the introduction of conduct grades has a positive effect on openness and extraversion for high-SES children and boys, and it reduces present bias for the same groups. Further, conduct grades are positively associated with risk tolerance, with the coefficients being of similar magnitudes both across gender and socio-economic status.

Second, in Tables 2.B.7 to 2.B.10 we assess whether our results survive when we consider all grade-like assessments printed at the top of the transcripts as well as all standardised assessments including assessments given at the end of the transcript or extra sheets, respectively. Firstly, turning to the measure including all grade-like assessments, we confirm that the estimated impact on personality traits is robust. Grade-like assessments, just as the purely numeric grades, increases openness to experience in the full sample (driven by male and high SES students) and reduces

neuroticism in female and low SES students[9]. With regard to economic preferences, we find that the more inclusive measure also leads to a significant increase in risk acceptance for the full sample. However, the decrease in present bias is now only significant for male students. Turning to the broader measure including all forms of standardised feedback, we again confirm the positive impact on openness to experience for male and high SES students as well as the negative impact on neuroticism for female and low SES students. We also confirm the positive impact on risk acceptance, now seemingly driven by women and low SES students in particular. Finally, the decrease in present bias does not appear to be robust to using the broadest possible definition of standardised feedback.

2.5.4 Conduct Grades in High School

Result 3 *Receiving conduct grades throughout high school does not appear to significantly alter students' personality or economic preferences at age 17.*

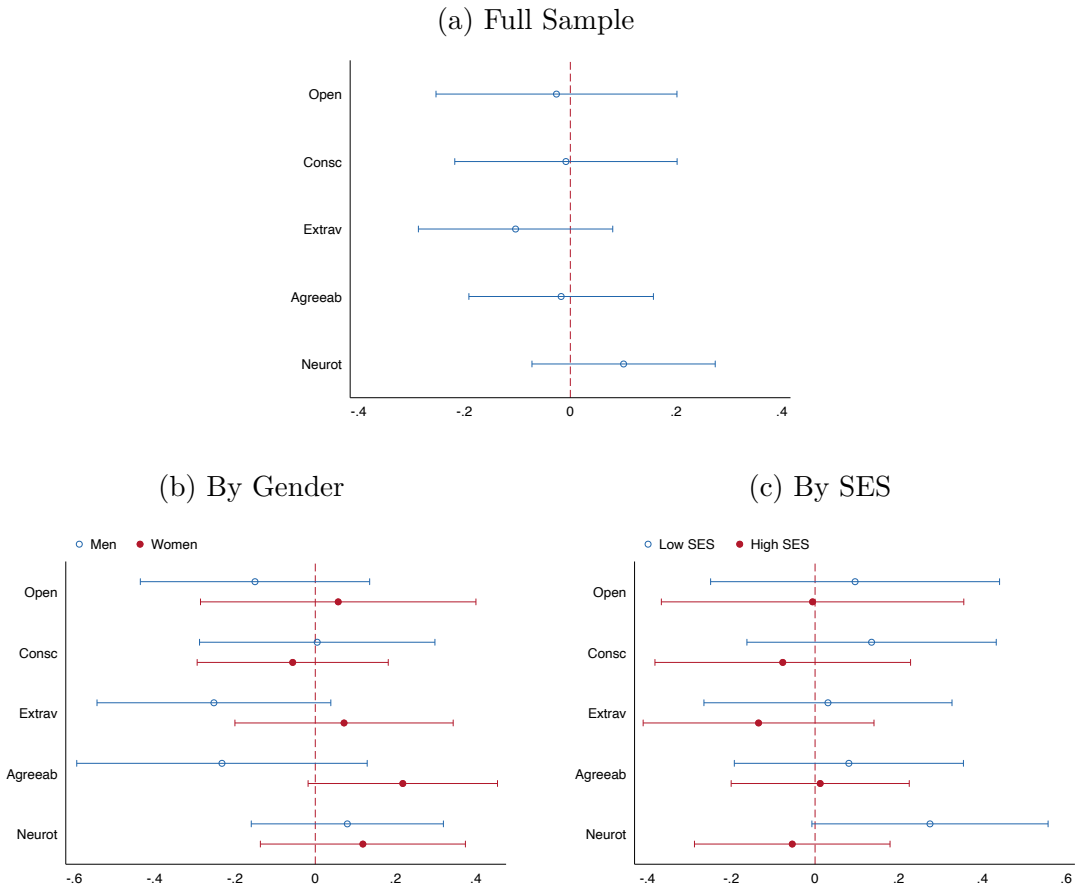
In addition to our main specification, where we analyse the impact of receiving conduct grades in primary school, we also test whether receiving conduct grades throughout high school affects both personality and preferences of students at age 17. The results are presented in Figures 2.5.3 & 2.5.4 – with panel (a) again documenting the results for the pooled sample, while panels (b) and (c) break down the overall effects by gender and socioeconomic status respectively.[10]

Overall, it appears as though conduct grades in high school do not affect either students' personality or their economic preferences at age 17. The only significant effect we find is a decrease in students' self-esteem – one that is particularly pronounced in male and low SES students. There is some movement along other dimensions as well, with male students seemingly becoming less extroverted and low SES students becoming more neurotic. However, none of these effects are statistically distinguishable from zero. This likely reflects at least partly the fact that personality and preferences are most malleable at a young age and stabilise after puberty [Caspi et al., 2005]. For many children, their personality and preferences are already fairly established by the time they start high school. Therefore, it is only plausible that conduct grades would

[9] Although the decrease is now significant even in the pooled sample.
[10] Tables 2.B.11 and 2.B.12 show the estimates from our OLS regressions.

have less of an effect – especially in the final years of high school. It could also hint at parents reacting more strongly to conduct grades than students themselves, which would be of greater importance at a young age.

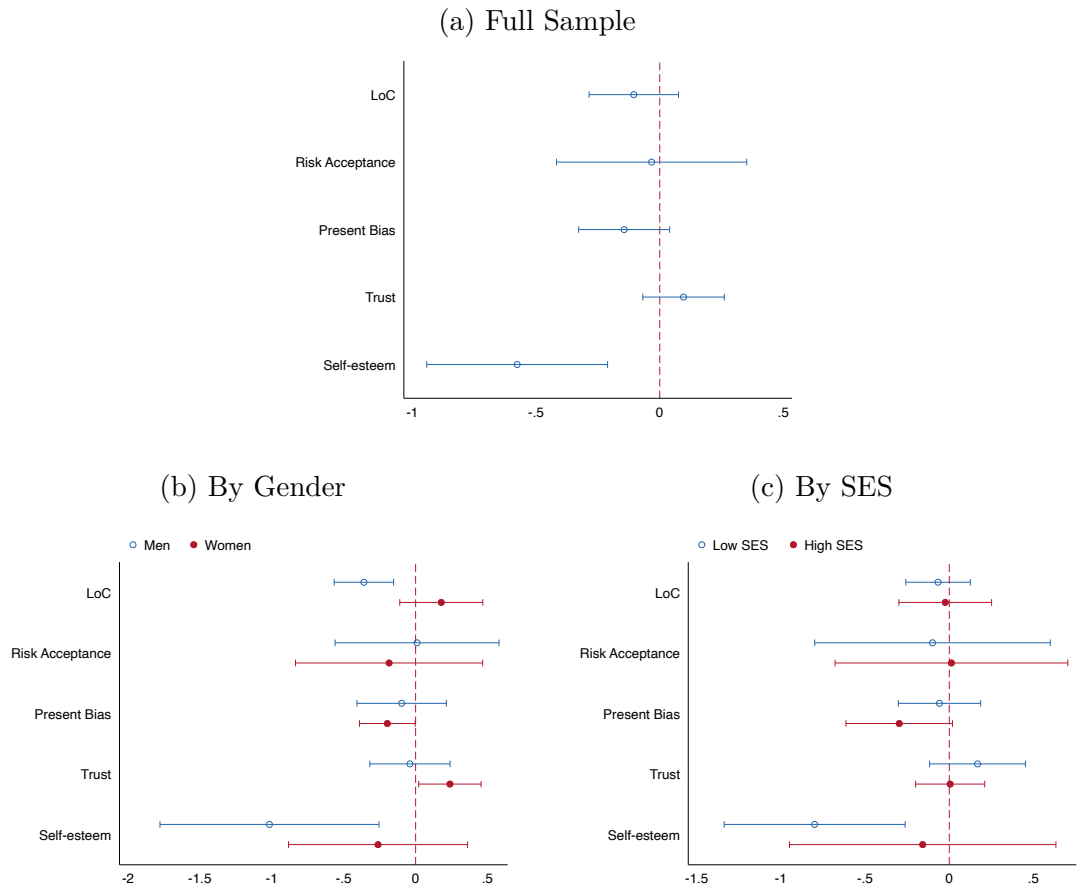
FIGURE 2.5.3: Conduct Grades in High School and the Big Five Personality Traits



Notes: The graph shows treatment effects from our baseline specification. Panel (a) shows results for the full sample. Panels (b) and (c) show results separately by gender and SES respectively. The caps represent 95% confidence intervals.

The results also point to a potential limitation of our study design. We are only able to use high-level variation at the state and year level and crucially do not observe the exact grades a student received. Consequently, we may be unable to estimate small effect sizes in particular with sufficient precision.

FIGURE 2.5.4: Conduct Grades in High School and Preferences



Notes: The graph shows treatment effects from our baseline specification. Panel (a) shows results for the full sample. Panels (b) and (c) show results separately by gender and SES respectively. The caps represent 95% confidence intervals.

2.6 Conclusion

Socio-emotional skills are important predictors of many outcomes in life. They are also shaped at an early age by the environment children encounter at home and in school. In this paper, we investigate if grading primary school students on non-academic dimensions such as work ethic and social conduct affects their socio-emotional skills in late adolescence. We find that, overall, conduct grades in primary school render students more open to experience and willing to take risks at age 17. More importantly though, we find that conduct grades effect women and students from disadvantaged backgrounds in a way that results in them scoring lower levels

of neuroticism at age 17. Since these two groups normally score higher than their male peers as well as those from more advantaged backgrounds, this decreases an existing and detrimental gap. Furthermore, we find that male students and those from more disadvantaged backgrounds become less present biased at 17 if they received conduct grades in primary school – again reducing an existing and detrimental gap. One potential channel that could explain the observable changes would be that conduct grades make children's socio-emotional skills more salient to parents, teachers and themselves and increase the perceived returns to investing in them. Thus, they might prompt all three groups to pay more attention to children's social or time management skills for example – and at least partly shift investments from fostering cognitive to socio-emotional skills. Finally, this paper also examines the impact that conduct grades in high school have on socio-emotional skill formation. It seems as though conduct grades have no significant impact on socio-emotional skills when they are given in high school. Instead the only significant impact we find is a decrease in self-esteem. This could reflect the fact that socio-emotional skills are most malleable at an early age, which is why conduct grades can no longer influence them during adolescence. However, most of the effects in high school can also only be estimated with relatively wide confidence intervals. This might highlight a limitation of the present study design. So far, we can only use high level variation, which may not be able to provide us with the level of precision needed to isolate smaller effect sizes. Future work should address this by analysing the impact of conduct grades at the individual level. For example, by comparing parental beliefs and children's socio-emotional skills before and after children receive conduct grades for the first time.

Appendix

2.A Data Description

Table 2.A.1: Outcome Variables

Variable	**Survey questions**
Openness (O)	I am original
	I value artistic expressions
	I have a lively imagination
	I like being introduced to new ideas
Conscientiousness (C)	I do things efficiently
	I am a thorough worker
	I tend to be lazy[a]
Extraversion (E)	I am communicative
	I am sociable
	I am reserved[a]
Agreeableness (A)	I am too coarse[a]
	I am of forgiving nature
	I am friendly
Neuroticism (N)	I often worry
	I am somewhat nervous
	I handle stress well[a]
Risk acceptance	Are you willing to take risks?
Present Bias	Live for today, do not think of tomorrow
	Do without today, enjoy tomorrow
Trust	One can trust people
	In these times one cannot rely on anybody else
	When dealing with strangers it is better to be cautious
Self-esteem	I have a positive opinion about myself
Locus of control	How my life goes depends on me
	Compared to others, I did not achieve what I deserve
	What a person achieves is a question of fate or luck
	Being involved can have an effect on social conditions
	Others have a controlling influence over my life
	One has to work hard in order to succeed
	I often doubt my own abilities when problems arise
	Opportunities are determined by social conditions
	Innate abilities are more important than effort
	I have little control over my life

a) Coding reversed.

TABLE 2.A.2: Summary Statistics of Outcome Variables by gender and SES

	Full sample		Men	Women	Low SES	High SES
	N	Mean [SD]	Mean [SD]	Mean [SD]	Mean [SD]	Mean [SD]
Openness	5346	-0.000	-0.112	0.113	-0.068	0.065
		[1.000]	[0.982]	[1.005]	(1.024)	[0.972]
Conscientiousness	5411	-0.000	-0.115	0.116	0.067	-0.064
		[1.000]	[0.990]	[0.997]	(1.012)	[0.984]
Extraversion	5408	0.000	-0.071	0.071	-0.006	0.006
		[1.000]	[0.982]	[1.013]	(0.983)	[1.016]
Agreeableness	5418	0.000	-0.107	0.108	-0.012	0.012
		[1.000]	[1.002]	[0.987]	(1.034)	[0.965]
Neuroticism	5421	0.000	-0.250	0.251	0.055	-0.053
		[1.000]	[0.953]	[0.983]	(0.991)	[1.006]
Risk Acceptance	5344	6.045	6.178	5.909	6.120	5.972
		[2.060]	[2.123]	[1.985]	(2.107)	[2.012]
Present Bias	5287	0.000	0.034	-0.034	0.011	-0.010
		[1.000]	[1.013]	[0.986]	(1.044)	[0.958]
Trust	5294	-0.000	0.049	-0.049	-0.125	0.116
		[1.000]	[0.971]	[1.026]	(1.003)	[0.983]
Self-Esteem	4286	5.295	5.529	5.065	5.303	5.288
		[1.396]	[1.278]	[1.467]	(1.402)	[1.387]
LoC	4972	0.000	0.048	-0.048	-0.099	0.091
		[1.000]	[1.010]	[0.988]	(1.041)	[0.952]

Notes: The table presents summary statistics for our dependent variables, as described in Table 2.A.1. Columns 1 and 2 show the number of observations and mean for the full sample. Columns 3 and 4 show statistics for male and female respondents, respectively. Column 5 and 6 show statistics for low- and high SES adolescents, respectively. Standard deviations are reported in squared brackets below the means.

2.B Additional Results

TABLE 2.B.1: Conduct Grades and the Big Five Personality Traits

	Conscientiousness	Neuroticism	Agreeableness	Extraversion	Openness
Conduct grades	0.0124	-0.0462	-0.0517	0.122	0.194***
	(0.0694)	(0.0452)	(0.0681)	(0.0829)	(0.0706)
Male	-0.195***	-0.480***	-0.249***	-0.137***	-0.198***
	(0.0300)	(0.0321)	(0.0371)	(0.0347)	(0.0333)
Grew Up Primarily With Both Parents	0.206***	-0.0987*	0.0917	-0.110**	0.0980*
	(0.0610)	(0.0519)	(0.0602)	(0.0526)	(0.0568)
Siblings	-0.0272	0.0860	0.0217	-0.0474	-0.0239
	(0.0656)	(0.0606)	(0.0645)	(0.0627)	(0.0625)
HH income (1000's)	-0.0304	0.0157	-0.0105	0.0595**	0.0573**
	(0.0337)	(0.0250)	(0.0299)	(0.0301)	(0.0269)
Abitur - Mother	-0.128***	-0.0479	0.0273	-0.0458	0.0972**
	(0.0464)	(0.0538)	(0.0509)	(0.0521)	(0.0473)
Abitur - Father	-0.0232	-0.0948*	-0.0256	-0.0500	0.176***
	(0.0497)	(0.0495)	(0.0485)	(0.0564)	(0.0425)
Mother in work	-0.0575	-0.0262	0.00502	0.0236	0.0125
	(0.0435)	(0.0395)	(0.0401)	(0.0466)	(0.0428)
Rural Area	-0.0636	-0.102**	-0.0182	0.102**	-0.0182
	(0.0402)	(0.0492)	(0.0408)	(0.0459)	(0.0523)
Migrant background	0.0707**	-0.0288	0.0408	0.133***	0.138***
	(0.0350)	(0.0406)	(0.0397)	(0.0332)	(0.0405)
Observations	3,325	3,331	3,333	3,328	3,286
R-squared	0.035	0.085	0.032	0.039	0.038
Year & State FE	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. Conduct grades takes value one if the child received conduct grades throughout primary school, and zero otherwise.

TABLE 2.B.2: Conduct Grades and Preferences

	Locus of Control	Risk Acceptance	Present Bias	Trust	Self Esteem
Conduct grades	0.0689	0.387***	-0.0814	-0.0168	-0.103
	(0.0667)	(0.131)	(0.0519)	(0.0506)	(0.0745)
Male	0.0829**	0.236***	0.0598**	0.0453	0.496***
	(0.0389)	(0.0693)	(0.0302)	(0.0315)	(0.0627)
Grew Up Primarily With Both Parents	0.198***	-0.186*	-0.210***	0.0525	0.158*
	(0.0632)	(0.103)	(0.0527)	(0.0492)	(0.0884)
Siblings	-0.0170	0.183	-0.0123	0.0686	-0.0306
	(0.0662)	(0.124)	(0.0587)	(0.0685)	(0.107)
HH income (1000's)	0.0771**	0.0676	0.0142	0.0540	0.0452
	(0.0301)	(0.0461)	(0.0353)	(0.0380)	(0.0518)
Abitur - Mother	0.0327	-0.199**	0.00212	0.204***	-0.127*
	(0.0497)	(0.0970)	(0.0464)	(0.0444)	(0.0761)
Abitur - Father	0.131***	-0.136	-0.0472	0.104**	0.0327
	(0.0440)	(0.0958)	(0.0489)	(0.0463)	(0.0834)
Mother in work	0.135***	0.133	0.0515	0.00946	0.0189
	(0.0456)	(0.0949)	(0.0403)	(0.0393)	(0.0718)
Rural Area	-0.0247	-0.0828	0.0451	-0.00735	0.0266
	(0.0461)	(0.0832)	(0.0489)	(0.0472)	(0.0720)
Migrant background	0.00535	0.168**	-0.0377	-0.153***	0.180**
	(0.0421)	(0.0808)	(0.0391)	(0.0419)	(0.0747)
Observations	3,180	3,292	3,323	3,323	2,298
R-squared	0.037	0.027	0.019	0.045	0.051
Year & State FE	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. Conduct grades takes value one if the child received conduct grades throughout primary school, and zero otherwise.

TABLE 2.B.3: Conduct Grades and Preferences – Heterogeneity

	Locus of Control	Risk Acceptance	Present Bias	Trust	Self Esteem
Panel A: Men					
Conduct grades	-0.0755	0.397**	-0.226***	0.0141	-0.198
	(0.0817)	(0.169)	(0.0772)	(0.0940)	(0.133)
Observations	1,594	1,666	1,671	1,671	1,134
R-squared	0.051	0.041	0.028	0.047	0.030
Panel B: Women					
Conduct grades	0.219*	0.364*	0.0585	-0.0505	-0.0166
	(0.114)	(0.192)	(0.0679)	(0.0644)	(0.123)
Observations	1,586	1,626	1,652	1,652	1,164
R-squared	0.047	0.031	0.023	0.070	0.045
Panel C: High SES					
Conduct grades	0.0569	0.417**	-0.236**	0.0467	-0.158
	(0.0730)	(0.196)	(0.0982)	(0.0813)	(0.153)
Observations	1,661	1,708	1,721	1,722	1,176
R-squared	0.043	0.049	0.030	0.038	0.074
Panel D: Low SES					
Conduct grades	0.115	0.362**	0.108	-0.0531	-0.0735
	(0.0929)	(0.146)	(0.0792)	(0.0828)	(0.103)
Observations	1,519	1,584	1,602	1,601	1,122
R-squared	0.055	0.041	0.028	0.044	0.059
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received conduct grades throughout primary school, and zero otherwise. Panels A and B restrict the sample to male and female respondents, respectively. Panels C and D restrict the sample to high- and low-SES individuals, respectively.

TABLE 2.B.4: Conduct Grades and the Big Five Personality Traits – Heterogeneity

	Conscientiousness	Neuroticism	Agreeableness	Extraversion	Openness
Panel A: Men					
Conduct grades	-0.0179	0.0896	-0.000335	0.161*	0.317***
	(0.110)	(0.0736)	(0.119)	(0.0818)	(0.0922)
Observations	1,673	1,675	1,675	1,674	1,659
R-squared	0.034	0.024	0.023	0.040	0.035
Panel B: Women					
Conduct grades	0.0246	-0.184*	-0.108	0.0845	0.0657
	(0.0922)	(0.0943)	(0.101)	(0.117)	(0.102)
Observations	1,652	1,656	1,658	1,654	1,627
R-squared	0.036	0.048	0.032	0.061	0.043
Panel C: High SES					
Conduct grades	0.0655	0.0409	0.0102	0.187*	0.342***
	(0.0815)	(0.0830)	(0.0935)	(0.0997)	(0.0896)
Observations	1,725	1,726	1,727	1,725	1,710
R-squared	0.033	0.099	0.048	0.051	0.050
Panel D: Low SES					
Conduct grades	-0.0509	-0.148**	-0.103	0.0544	0.0378
	(0.103)	(0.0597)	(0.0981)	(0.0974)	(0.0895)
Observations	1,600	1,605	1,606	1,603	1,576
R-squared	0.059	0.082	0.044	0.052	0.039
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received conduct grades throughout primary school, and zero otherwise. Panels A and B restrict the sample to male and female respondents, respectively. Panels C and D restrict the sample to high- and low-SES individuals, respectively.

TABLE 2.B.5: Numeric Conduct Grades and the Big Five Personality Traits

	Conscientiousness	Neuroticism	Agreeableness	Extraversion	Openness
Panel A: Full sample					
Conduct Grades	0.00645	-0.0382	-0.0813	0.132*	0.168**
	(0.0686)	(0.0515)	(0.0673)	(0.0756)	(0.0690)
Observations	3,325	3,331	3,333	3,328	3,286
R-squared	0.035	0.085	0.032	0.040	0.037
Panel B: Men					
Conduct Grades	-0.0260	0.0838	-0.0325	0.165**	0.290***
	(0.109)	(0.0720)	(0.112)	(0.0755)	(0.0880)
Observations	1,673	1,675	1,675	1,674	1,659
R-squared	0.034	0.024	0.023	0.041	0.035
Panel C: Women					
Conduct Grades	0.0274	-0.171*	-0.135	0.0957	0.0296
	(0.0889)	(0.0965)	(0.0987)	(0.111)	(0.101)
Observations	1,652	1,656	1,658	1,654	1,627
R-squared	0.036	0.048	0.032	0.062	0.043
Panel D: High SES					
Conduct Grades	0.0913	0.0293	0.0472	0.220**	0.323***
	(0.0777)	(0.0799)	(0.0888)	(0.0940)	(0.0876)
Observations	1,725	1,726	1,727	1,725	1,710
R-squared	0.033	0.099	0.048	0.052	0.050
Panel E: Low SES					
Conduct Grades	-0.0884	-0.113	-0.199*	0.0418	0.0104
	(0.104)	(0.0691)	(0.106)	(0.0897)	(0.0852)
Observations	1,600	1,605	1,606	1,603	1,576
R-squared	0.060	0.082	0.046	0.052	0.039
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received numeric conduct grades throughout primary school, and zero otherwise. Panel A presents results for the full sample. Panels B and C restrict the sample to male and female respondents, respectively. Panels D and E restrict the sample to high- and low-SES individuals, respectively.

TABLE 2.B.6: Numeric Conduct Grades and Preferences

	Locus of Control	Risk Acceptance	Present Bias	Trust	Self Esteem
Panel A: Full sample					
Conduct Grades	0.0372	0.338***	-0.0537	-0.0109	-0.103
	(0.0654)	(0.129)	(0.0506)	(0.0469)	(0.0745)
Observations	3,180	3,292	3,323	3,323	2,298
R-squared	0.037	0.027	0.019	0.045	0.051
Panel B: Men					
Conduct Grades	-0.109	0.333**	-0.195***	0.0214	-0.198
	(0.0803)	(0.167)	(0.0741)	(0.0851)	(0.133)
Observations	1,594	1,666	1,671	1,671	1,134
R-squared	0.051	0.041	0.028	0.047	0.030
Panel C: Women					
Conduct Grades	0.192*	0.328*	0.0860	-0.0437	-0.0166
	(0.110)	(0.185)	(0.0654)	(0.0620)	(0.123)
Observations	1,586	1,626	1,652	1,652	1,164
R-squared	0.046	0.031	0.023	0.070	0.045
Panel D: High SES					
Conduct Grades	0.0202	0.309	-0.195**	0.0559	-0.158
	(0.0712)	(0.190)	(0.0929)	(0.0772)	(0.153)
Observations	1,661	1,708	1,721	1,722	1,176
R-squared	0.042	0.048	0.029	0.038	0.074
Panel E: Low SES					
Conduct Grades	0.0905	0.385***	0.114	-0.0437	-0.0735
	(0.0900)	(0.144)	(0.0743)	(0.0752)	(0.103)
Observations	1,519	1,584	1,602	1,601	1,122
R-squared	0.055	0.041	0.028	0.044	0.059
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received numeric conduct grades throughout primary school, and zero otherwise. Panel A presents results for the full sample. Panels B and C restrict the sample to male and female respondents, respectively. Panels D and E restrict the sample to high- and low-SES individuals, respectively.

TABLE 2.B.7: Grade-like Feedback and the Big Five Personality Traits

	Conscientiousness	Neuroticism	Agreeableness	Extraversion	Openness
Panel A: Full sample					
Conduct Grades	-0.0357	-0.0884**	0.0134	0.0690	0.165***
	(0.0662)	(0.0442)	(0.0587)	(0.0715)	(0.0626)
Observations	3,325	3,331	3,333	3,328	3,286
R-squared	0.035	0.086	0.032	0.039	0.038
Panel B: Men					
Conduct Grades	-0.0712	0.0251	0.107	0.0762	0.240***
	(0.0860)	(0.0680)	(0.0876)	(0.0810)	(0.0879)
Observations	1,673	1,675	1,675	1,674	1,659
R-squared	0.034	0.023	0.023	0.039	0.034
Panel C: Women					
Conduct Grades	-0.0107	-0.186**	-0.0979	0.0569	0.0792
	(0.100)	(0.0848)	(0.0860)	(0.0970)	(0.0880)
Observations	1,652	1,656	1,658	1,654	1,627
R-squared	0.036	0.049	0.032	0.061	0.044
Panel D: High SES					
Conduct Grades	-0.0180	-0.0548	0.0127	0.161*	0.241***
	(0.0813)	(0.0726)	(0.0837)	(0.0880)	(0.0786)
Observations	1,725	1,726	1,727	1,725	1,710
R-squared	0.032	0.099	0.048	0.051	0.047
Panel E: Low SES					
Conduct Grades	-0.0531	-0.132**	0.0339	-0.0165	0.100
	(0.0932)	(0.0647)	(0.0876)	(0.0939)	(0.0792)
Observations	1,600	1,605	1,606	1,603	1,576
R-squared	0.059	0.083	0.043	0.052	0.039
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received grade-like conduct grades throughout primary school, and zero otherwise. Panel A presents results for the full sample. Panels B and C restrict the sample to male and female respondents, respectively. Panels D and E restrict the sample to high- and low-SES individuals, respectively.

TABLE 2.B.8: Grade-like Feedback and Preferences

	Locus of Control	Risk Acceptance	Present Bias	Trust	Self Esteem
Panel A: Full sample					
Conduct Grades	0.000	0.328***	-0.0110	-0.0163	-0.0542
	(0.0633)	(0.122)	(0.0599)	(0.0517)	(0.0697)
Observations	3,180	3,292	3,323	3,323	2,298
R-squared	0.036	0.027	0.018	0.045	0.051
Panel B: Men					
Conduct Grades	-0.157**	0.274*	-0.168**	0.00385	-0.131
	(0.0748)	(0.164)	(0.0842)	(0.0838)	(0.117)
Observations	1,594	1,666	1,671	1,671	1,134
R-squared	0.052	0.041	0.027	0.047	0.030
Panel C: Women					
Conduct Grades	0.148	0.393**	0.149**	-0.0255	0.0135
	(0.105)	(0.163)	(0.0652)	(0.0649)	(0.121)
Observations	1,586	1,626	1,652	1,652	1,164
R-squared	0.045	0.032	0.024	0.070	0.045
Panel D: High SES					
Conduct Grades	-0.0153	0.273	-0.0865	0.0460	-0.0988
	(0.0744)	(0.180)	(0.0950)	(0.0843)	(0.152)
Observations	1,661	1,708	1,721	1,722	1,176
R-squared	0.042	0.048	0.027	0.038	0.074
Panel E: Low SES					
Conduct Grades	0.0465	0.414**	0.0800	-0.0467	-0.0571
	(0.0833)	(0.174)	(0.0834)	(0.0716)	(0.101)
Observations	1,519	1,584	1,602	1,601	1,122
R-squared	0.054	0.042	0.027	0.044	0.059
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received grade-like conduct grades throughout primary school, and zero otherwise. Panel A presents results for the full sample. Panels B and C restrict the sample to male and female respondents, respectively. Panels D and E restrict the sample to high- and low-SES individuals, respectively.

TABLE 2.B.9: Standardised Feedback and the Big Five Personality Traits

	Conscientiousness	Neuroticism	Agreeableness	Extraversion	Openness
Panel A: Full sample					
Conduct Grades	-0.0156	-0.0567	0.0574	0.0891	0.166***
	(0.0633)	(0.0448)	(0.0566)	(0.0668)	(0.0603)
Observations	3,325	3,331	3,333	3,328	3,286
R-squared	0.035	0.085	0.032	0.039	0.038
Panel B: Men					
Conduct Grades	-0.00740	0.0425	0.148*	0.0989	0.236***
	(0.0850)	(0.0647)	(0.0812)	(0.0767)	(0.0847)
Observations	1,673	1,675	1,675	1,674	1,659
R-squared	0.034	0.023	0.024	0.040	0.034
Panel C: Women					
Conduct Grades	-0.0315	-0.139*	-0.0631	0.0660	0.0844
	(0.0940)	(0.0832)	(0.0858)	(0.0926)	(0.0851)
Observations	1,652	1,656	1,658	1,654	1,627
R-squared	0.036	0.048	0.031	0.061	0.044
Panel D: High SES					
Conduct Grades	0.0440	-0.0117	0.0609	0.160*	0.204**
	(0.0783)	(0.0719)	(0.0784)	(0.0875)	(0.0790)
Observations	1,725	1,726	1,727	1,725	1,710
R-squared	0.032	0.099	0.048	0.051	0.046
Panel E: Low SES					
Conduct Grades	-0.0920	-0.114*	0.0800	0.0124	0.132*
	(0.0865)	(0.0651)	(0.0864)	(0.0862)	(0.0772)
Observations	1,600	1,605	1,606	1,603	1,576
R-squared	0.060	0.082	0.044	0.052	0.040
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received standardised conduct grades throughout primary school, and zero otherwise. Panel A presents results for the full sample. Panels B and C restrict the sample to male and female respondents, respectively. Panels D and E restrict the sample to high- and low-SES individuals, respectively.

Table 2.B.10: Standardised Feedback and Preferences

	Locus of Control	Risk Acceptance	Present Bias	Trust	Self Esteem
Panel A: Full sample					
Conduct Grades	-0.00312	0.292**	0.0121	-0.00340	-0.0685
	(0.0607)	(0.119)	(0.0566)	(0.0491)	(0.0681)
Observations	3,180	3,292	3,323	3,323	2,298
R-squared	0.036	0.027	0.018	0.045	0.051
Panel B: Men					
Conduct Grades	-0.140*	0.204	-0.0975	-0.00858	-0.106
	(0.0715)	(0.163)	(0.0811)	(0.0790)	(0.110)
Observations	1,594	1,666	1,671	1,671	1,134
R-squared	0.052	0.040	0.026	0.047	0.029
Panel C: Women					
Conduct Grades	0.118	0.404**	0.130**	0.0145	-0.0339
	(0.102)	(0.157)	(0.0647)	(0.0649)	(0.119)
Observations	1,586	1,626	1,652	1,652	1,164
R-squared	0.045	0.032	0.024	0.070	0.045
Panel D: High SES					
Conduct Grades	0.00827	0.222	-0.0835	0.00118	-0.108
	(0.0713)	(0.176)	(0.0891)	(0.0820)	(0.146)
Observations	1,661	1,708	1,721	1,722	1,176
R-squared	0.042	0.048	0.027	0.037	0.074
Panel E: Low SES					
Conduct Grades	0.0177	0.366**	0.115	0.0322	-0.0852
	(0.0820)	(0.170)	(0.0789)	(0.0677)	(0.0995)
Observations	1,519	1,584	1,602	1,601	1,122
R-squared	0.054	0.041	0.028	0.044	0.059
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received standardised conduct grades throughout primary school, and zero otherwise. Panel A presents results for the full sample. Panels B and C restrict the sample to male and female respondents, respectively. Panels D and E restrict the sample to high- and low-SES individuals, respectively.

TABLE 2.B.11: Conduct Grades in High School and the Big Five Personality Traits

	Conscientiousness	Neuroticism	Agreeableness	Extraversion	Openness
Panel A: Full sample					
Conduct Grades	-0.00830	0.101	-0.0174	-0.104	-0.0262
	(0.127)	(0.105)	(0.106)	(0.111)	(0.138)
Observations	3,325	3,331	3,333	3,328	3,286
R-squared	0.035	0.085	0.032	0.039	0.035
Panel B: Men					
Conduct Grades	0.00485	0.0798	-0.232	-0.252	-0.150
	(0.177)	(0.145)	(0.219)	(0.176)	(0.173)
Observations	1,673	1,675	1,675	1,674	1,659
R-squared	0.034	0.023	0.024	0.040	0.029
Panel C: Women					
Conduct Grades	-0.0562	0.119	0.218	0.0716	0.0571
	(0.144)	(0.154)	(0.143)	(0.164)	(0.207)
Observations	1,652	1,656	1,658	1,654	1,627
R-squared	0.036	0.046	0.032	0.061	0.043
Panel D: High SES					
Conduct Grades	-0.0772	-0.0545	0.0123	-0.135	-0.00596
	(0.185)	(0.141)	(0.129)	(0.167)	(0.218)
Observations	1,725	1,726	1,727	1,725	1,710
R-squared	0.032	0.099	0.048	0.049	0.042
Panel E: Low SES					
Conduct Grades	0.135	0.274	0.0807	0.0308	0.0956
	(0.180)	(0.171)	(0.165)	(0.179)	(0.209)
Observations	1,600	1,605	1,606	1,603	1,576
R-squared	0.060	0.083	0.043	0.052	0.039
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received numeric conduct grades in high school, and zero otherwise. Panel A presents results for the full sample. Panels B and C restrict the sample to male and female respondents, respectively. Panels D and E restrict the sample to high- and low-SES individuals, respectively.

TABLE 2.B.12: Conduct Grades in High School and Preferences

	Conscientiousness	Neuroticism	Agreeableness	Extraversion	Openness
Panel A: Full sample					
Conduct Grades	-0.105	-0.0331	-0.144	0.0962	-0.577**
	(0.109)	(0.233)	(0.111)	(0.0996)	(0.221)
Observations	3,180	3,292	3,323	3,323	2,298
R-squared	0.037	0.025	0.019	0.045	0.052
Panel B: Men					
Conduct Grades	-0.360***	0.0101	-0.0970	-0.0394	-1.016**
	(0.125)	(0.345)	(0.188)	(0.169)	(0.459)
Observations	1,594	1,666	1,671	1,671	1,134
R-squared	0.053	0.039	0.025	0.047	0.034
Panel C: Women					
Conduct Grades	0.178	-0.184	-0.197*	0.239*	-0.261
	(0.175)	(0.394)	(0.117)	(0.131)	(0.375)
Observations	1,586	1,626	1,652	1,652	1,164
R-squared	0.044	0.029	0.023	0.071	0.045
Panel D: High SES					
Conduct Grades	-0.0667	-0.0998	-0.0584	0.168	-0.800**
	(0.116)	(0.423)	(0.148)	(0.172)	(0.324)
Observations	1,661	1,708	1,721	1,722	1,176
R-squared	0.042	0.047	0.026	0.038	0.076
Panel E: Low SES					
Conduct Grades	-0.0242	0.0127	-0.297	0.00511	-0.158
	(0.167)	(0.418)	(0.191)	(0.124)	(0.477)
Observations	1,519	1,584	1,602	1,601	1,122
R-squared	0.054	0.039	0.028	0.044	0.059
Year & State FE	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes

Notes: OLS regressions. Standard errors in parentheses. * p< 0.1, ** p< 0.05, *** p< 0.01. The dependent variable are reported at the top of each column. All regressions include state and year fixed effects. In all regressions we additionally control for gender (if applicable), household monthly income, whether the respondent grew up primarily with both parents, whether they have siblings, the educational attainment of their parents, whether their mother is in work, as well as indicators for living in a rural area and having a migration background. Conduct grades takes value one if the child received numeric conduct grades in high school, and zero otherwise. Panel A presents results for the full sample. Panels B and C restrict the sample to male and female respondents, respectively. Panels D and E restrict the sample to high- and low-SES individuals, respectively.

2.C Examples of Legal Documents

FIGURE 2.C.1: Current Sample Transcript (1.Page) for Primary Schools in Mecklenburg-Vorpommern

Anlage 2 Grundschule, Jahrgangsstufen 2 bis 4 und Diagnoseförderklasse 2 Seite 1

_____
Name der Schule/Schulort

Zeugnis
der Grundschule
über das 1. Schulhalbjahr/Schuljahr ____/____

_____ geb. am: _____ Klasse: ____
Vorname und Name

Arbeitsverhalten

Fleiß _____

Zuverlässigkeit _____

Sozialverhalten

Umgangsformen _____

Teamfähigkeit _____

Vermerke:
_____
_____
_____
_____
_____
_____
_____
_____
_____
_____
_____
_____

Anlage 1 Grundschule Jahrgangsstufe 1 und Diagnoseförderklassen 0 und 1

_____
Name der Schule/Schulort

Zeugnis
der Grundschule
über das 1. Schulhalbjahr/Schuljahr ____/____

_____ geb. am: _____ Klasse: ____
Vorname und Name

Lernentwicklungsbericht
(Leistungsstand sowie Arbeits- und Sozialverhalten)

_____
_____
_____
_____
_____
_____
_____
_____
_____
_____
_____
_____

Vermerke:
_____
_____

Fehltage: _____ davon entschuldigt: _____

_____
Ort, Datum

_____ Stempel/Siegel _____
Schulleiterin/Schulleiter Klassenlehrerin/Klassenlehrer

Empfangsbestätigung:

_____ _____
Ort, Datum Erziehungsberechtigte

FIGURE 2.C.2: Excerpt of Current Legislation on Primary School Grading in Saxony-Anhalt

„Haftungsausschluss: Der vorliegende Text dient lediglich der Information. Rechtsverbindlichkeit haben ausschließlich die im Gesetz- und Verordnungsblatt des Landes Sachsen-Anhalt (GVBl. LSA) oder im Schulverwaltungsblatt des Landes Sachsen-Anhalt (SVBl. LSA) veröffentlichten Texte."

7.2 Der Bewertungsbereich Sozialverhalten umfasst unter anderem Hilfsbereitschaft, Zivilcourage, Verhalten bei Konflikten, Rücksichtnahme, Toleranz, Gemeinsinn, Beherrschtheit, die Fähigkeit zur Selbsteinschätzung, Kooperationsbereitschaft und Teamfähigkeit, Verantwortungsbereitschaft, Pünktlichkeit, Zuverlässigkeit sowie das Einhalten von Regeln und Absprachen.

7.3 Der Bewertungsbereich Lernverhalten umfasst unter anderem Lernbereitschaft, Zielstrebigkeit, Ausdauer, Aufmerksamkeit, Regelmäßigkeit beim Erfüllen von Aufgaben, Initiative, Beteiligung im Unterricht, Selbstständigkeit, Kreativität, Sorgfalt und das Bereithalten notwendiger Unterrichtsmaterialien.

7.4 Die Bewertung erfolgt mit folgenden Noten:

„sehr gut" (1);
„gut" (2);
„befriedigend" (3);
„ausreichend" (4);
„mangelhaft" (5).

Dabei liegen den Noten folgende Definitionen zu Grunde:

a) Die Note „sehr gut" ist zu erteilen, wenn die aufgeführten Kriterien vorbildlich ausgeprägt sind.
b) Die Note „gut" ist zu erteilen, wenn die aufgeführten Kriterien deutlich ausgeprägt sind.
c) Die Note „befriedigend" ist zu erteilen, wenn die aufgeführten Kriterien durchschnittlich ausgeprägt sind.
d) Die Note „ausreichend" ist zu erteilen, wenn die aufgeführten Kriterien schwach ausgeprägt sind.
e) Die Note „mangelhaft" ist zu erteilen, wenn die aufgeführten Kriterien unzureichend ausgeprägt sind.

7.5 Die Benotung des Sozialverhaltens und des Lernverhaltens sowie die Beurteilung gemäß Nummer 8.1 werden durch die Klassenleiterin oder den Klassenleiter oder die Lerngruppenleiterin oder den Lerngruppenleiter nach Beratung mit den in der jeweiligen Klasse/Lerngruppe tätigen Lehrkräften erstellt.

8. Beurteilung auf Zeugnissen

8.1 Im Zeugnis ist die Leistungs- und Verhaltensentwicklung der Schülerin oder des Schülers in einer kurzen verbalen Beurteilung auszuweisen. Die Aussagen müssen dem Ziel einer ermutigenden Erziehung dienen und Informationen für die Förderung der Schülerinnen und Schüler beinhalten.

8.2 Zum Halbjahr kann anstelle dieser Beurteilung auch ein Beratungsgespräch mit den Erziehungsberechtigten geführt werden.

9. Inkrafttreten

Dieser RdErl. tritt am 1.8.2010 in Kraft.

FIGURE 2.C.3: Excerpt of 1999 Reform of Primary School Grading Legislation in Saxony

Änd. Schulordnungen

Verordnung
des Sächsischen Staatsministeriums für Kultus
zur Einführung der Kopfnoten Betragen, Fleiß, Mitarbeit und Ordnung und zur Änderung der Oberstufen- und Abiturprüfungsverordnung

Vom 6. Juli 1999

Aufgrund von § 62 Abs. 1 des Schulgesetzes für den Freistaat Sachsen (SchulG) vom 3. Juli 1991 (SächsGVBl. S. 213), zuletzt geändert durch Artikel 1 des Gesetzes vom 29. Juni 1998 (SächsGVBl. S. 271), wird verordnet:

Artikel 1
Änderung der Schulordnung Grundschulen

Die Verordnung des Sächsischen Staatsministeriums für Kultus über Grundschulen im Freistaat Sachsen (Schulordnung Grundschulen – SOGS) vom 2. Mai 1994 (SächsGVBl. S. 1117), geändert durch § 8 Abs. 1 der Verordnung vom 3. März 1999 (SächsGVBl. S. 153, 154), wird wie folgt geändert:

1. Die Inhaltsübersicht wird wie folgt geändert:
   a) In der Überschrift des Fünften Abschnitts werden die Worte „Verhalten und Mitarbeit“ durch die Angabe „Betragen, Fleiß, Mitarbeit und Ordnung“ ersetzt.
   b) Die § 16 betreffende Angabe wird wie folgt gefasst:
   „§ 16 Beurteilung und Bewertung von Leistungen, Betragen, Fleiß, Mitarbeit und Ordnung“.
   c) Die § 20 betreffende Angabe wird wie folgt gefasst:
   „§ 20 aufgehoben“.
   d) Die § 21 betreffende Angabe wird wie folgt gefasst:
   „§ 21 aufgehoben“.
2. In der Überschrift des Fünften Abschnitts werden die Worte „Verhalten und Mitarbeit“ durch die Angabe „Betragen, Fleiß, Mitarbeit und Ordnung“ ersetzt.
3. § 16 wird wie folgt geändert:
   a) In der Überschrift werden die Worte „Verhalten und Mitarbeit“ durch die Angabe „Betragen, Fleiß, Mitarbeit und Ordnung“ ersetzt.
   b) In Absatz 1 Satz 2 werden die Worte „sowie seine Leistungsbereitschaft“ gestrichen.
   c) Die Absätze 6 bis 8 werden wie folgt gefasst:
   „(6) Betragen umfasst Aufmerksamkeit, Hilfsbereitschaft, Zivilcourage und angemessener Umgang mit Konflikten, Rücksichtnahme und Toleranz und Gemeinsinn, Selbsteinschätzung. Fleiß umfasst Lernbereitschaft, Zielstrebigkeit, Ausdauer, Regelmäßigkeit beim Erfüllen von Aufgaben. Mitarbeit umfasst Initiative, Kooperationsbereitschaft und Teamfähigkeit, Beteiligung im Unterricht, Selbständigkeit, Kreativität, Verantwortungsbereitschaft. Ordnung umfasst Sorgfalt, Pünktlichkeit, Zuverlässigkeit, Einhalten von Regeln und Absprachen, Bereithalten notwendiger Unterrichtsmaterialien.

   (7) Betragen, Fleiß, Mitarbeit und Ordnung des Schülers werden mit folgenden Noten bewertet:

   „sehr gut“ (1);
   „gut“ (2);
   „befriedigend“ (3);
   „ausreichend“ (4);
   „mangelhaft“ (5).

   Verbale Einschätzungen ergänzen und präzisieren diese Bewertung. Alle diese Aussagen müssen dem Ziel einer ermutigenden Erziehung dienen und Informationen für die Förderung des Schülers beinhalten. Das allgemeine Persönlichkeitsrecht des Schülers und das Erziehungsrecht der Erziehungsberechtigten sind zu achten.

   (8) Den Noten gemäß Absatz 7 liegen folgende Definitionen zu Grunde:

   1. Die Note *„sehr gut“* ist jeweils zu erteilen, wenn Betragen oder Fleiß oder Mitarbeit oder Ordnung des Schülers vorbildlich ausgeprägt ist.
   2. Die Note *„gut“* ist jeweils zu erteilen, wenn Betragen oder Fleiß oder Mitarbeit oder Ordnung des Schülers stark ausgeprägt ist.
   3. Die Note *„befriedigend“* ist jeweils zu erteilen, wenn Betragen oder Fleiß oder Mitarbeit oder

2.D Description of the High School Variation

FIGURE 2.D.1: Variation in Feedback on Conduct in High School

State	Numerical Feedback	Gradelike Feedback	Text Feedback	No Standardised Feedback
Schleswig-Holstein			At least 1990s-present: formalised comments on behaviour in text or table form	
Hamburg				1990s-present
Lower Saxony		2000-present: clearly coded comments on 2 dimensions (map to grades)		1980s-2000
Bremen			2002-present: comments/ ticks on 2 dimensions; extra sheet	1980s-2002
Northrhine-Westphalia	1950-1975 2007-2010: 6 dimensions in 2007, 3 thereafter			1976-2006
Hessia	2000-present: 2 dimensions			1990s-2000
Rhineland-Palatinate	1989-present: 2 dimensions			

Notes: In the time periods without standardised feedback students may have still received feedback from their teachers in the form of free texts. The time periods without standardised feedback may extend even further back – these are lower bounds for periods where legal documents ascertain that the state in question had no conduct grades.

FIGURE 2.D.2: Variation in Feedback on Conduct in High School

State	Numerical Feedback	Gradelike Feedback	Text Feedback	No Standardised Feedback
Bavaria			At least 1990s-2000: Coded comments on participation and conduct	
Saarland	2000-present: 2 dimension			1990s-2000
Berlin			2005-present: comments/ ticks on 6 dimensions; extra sheet	1990s-2005
Brandenburg	1949-1990; 2006-present: 7 dimensions	2001-2005: clearly coded comments on 2 dimensions (map to grades)		1991-2000
Mecklenburg-Vorpommern	1949-1990 2008-present: 2 dimensions until 2013, 4 thereafter			1991-2008
Saxony	1949-1990 1999-present: 4 dimension	1991-1998: text grades on 2 dimensions		
Saxony-Anhalt	1949-1990 2003-present: 2 dimensions			1991-2002
Thuringia	1949-1990 2003-today: 2 dimensions; year 7-10 only			1991-2002

Notes: In the time periods without standardised feedback students may have still received feedback from their teachers in the form of free texts. The time periods without standardised feedback may extend even further back – these are lower bounds for periods where legal documents ascertain that the state in question had no conduct grades.

Chapter 3

Political Participation Under Uncertain Democratic Norms

A functioning democracy requires debate over political outcomes but consensus over the norms guiding said debate. Yet, polarisation on issues is increasingly accompanied by public disagreement over norms as well, such as voting systems, limits of legitimate protest, or term limits for Supreme Court justices. We study how individuals' willingness to come out in support of a given norm is shaped by their beliefs over the degree to which others agree with the norm and are certain about their stance. We focus on the context of Brexit and the question of the legitimacy of a second referendum. We experimentally vary participants' beliefs over the fraction of the population that agrees with and is certain about the appropriateness of a repeat referendum. We find that respondents become significantly more likely to donate to and tweet about campaigns lobbying for or against a repeat referendum if they are informed that a higher share of the population is uncertain about which behaviour conforms to democratic norm. This is due to Leave (Remain) supporters becoming more likely to campaign for (against) a repeat referendum after learning that other Leave (Remain) supporters are uncertain about their views. We find evidence that results derive from social image concerns and strategic considerations rather than social learning. The results imply that there is scope for governments to improve consensus over norms guiding political debate through appropriate information campaigns.

3.1 Introduction

Political norms define the rules of the game within which politicians and voters try to shape outcomes. Political competition and debate over issues are vital for democracies to flourish. However, for them to *function*, a majority of the population needs to be in agreement over the norms governing these debates. Thus, it is worrying that across countries the two debates, on issues and on how decisions should be taken, are becoming increasingly entwined. For example, in the US, views on the appropriateness of term limits for Supreme Court justices or the abolition of the filibuster in the Senate appear to be tightly linked to individuals' takes on the court's recent rulings or the legislation they believe the Senate should or should not pass. In the UK, the same is true for people's views on acceptable means of protest and the urgency of climate protection, or the legitimacy of a repeat referendum on the country's relationship to the European Union and the preferred outcome of such a vote.

At the same time, while most public opinion polls reports the degree to which different groups support or oppose particular norms, they rarely mention how certain the average person is about their stance. Since opinions on political outcomes and norms are likely positively correlated for the majority of people, this may create a situation in which views on democratic norms are seen as increasingly politicised – discouraging those in the minority who support or oppose norms that may work against their political interests from voicing these views, furthering polarisation.

In this paper, we ask how information on other people's uncertainty over political norms affects individuals' willingness to campaign for (or against) them. We also explore whether individuals react differently to information about people who share or oppose their beliefs on political outcomes. Finally, we examine if such information renders individuals more likely to campaign for norms that "align" with their political leaning – or those that do not. Specifically, we explore these questions in the context of a repeat referendum on the United Kingdom leaving the European Union. In the years after the 2016 referendum when 51.9% of the UK population voted to leave the European Union, there has been an intense debate, over whether or not it would be good for the UK to leave the EU. However, there was an equally intense debate about whether democratic norm dictated to hold another referendum with all information

about the withdrawal agreement available – or forbade it on the grounds of respect for the outcome of a democratic vote. We conduct an experiment in the lead up to the 2019 general election when the debate and ambiguity both over Brexit and the validity of another referendum was at its peak and after which both questions were effectively settled. In it, we test how information on the share of people who believe a repeat referendum would be appropriate as well as are uncertain about this stance shapes participants' willingness to campaign either for or against it.

We recruit 4,295 individuals representative of the UK population in terms of income, region, gender, education and age in November 2019 through the online panel provider Luc.id. We measure respondents' beliefs about the share of Remain supporters as well as Leave supporters agreeing with the appropriateness of a repeat referendum. We also measure their beliefs about the share of Remain and Leave supporters who are uncertain about the appropriateness. Thereafter, we give respondents different pieces of truthful information about these fractions based on a pilot survey with 644 participants we ran two weeks prior to the main experiment. After the treatment, we elicit respondents' willingness to donate to either the "People's Vote" or the "Better Off Out" campaign. While the former group was lobbying for another referendum, the latter was opposing it – both citing democratic norm. We also test participants' willingness to tweet a message endorsing or opposing a repeat referendum tagging either campaign. Moreover, we elicit respondents' intention to vote in the upcoming election where different parties championed or opposed a repeat referendum in their platforms. Finally, we measure a variety of associated beliefs to identify the mechanisms driving any potential shifts in behaviour. Information on other people's support for or uncertainty over a given norm, could shift individuals' willingness to politically engage through three distinct channels: social learning about the merits/flaws of the norm, a shift in the perceived signalling value/social cost of supporting a given norm, or a shift in the perceived malleability of others' beliefs and thus the potential returns to political action. To disentangle the three, we collect data on participants' private support for and agreement with the legitimacy of another referendum, their beliefs on others' assessment of it, their beliefs about the likelihood of Brexit in case of another referendum and the consequences of Brexit, as well as their beliefs about other people's perception of individuals who publicly

campaign for or against another referendum plus their beliefs about the effectiveness of campaigning. We run both OLS as well as instrumental variable regressions to determine the causal effect that seeing higher levels of agreement with the appropriateness of another referendum as well as uncertainty thereover has on the different outcome variables.

We find that respondents in general become 10% more likely to donate to both the People's Vote as well as the Better Off Out campaign if they learn that supporter of the Leave as well as the Remain campaign are relatively more uncertain about their views on the legitimacy of a repeat referendum. This is driven entirely by them learning that people in their *own* political camp are more uncertain and subsequently donating to the campaign that is less popular in their reference group. That is, Leave supporters become 21% more likely to donate to the People's Vote campaign (relative to the Leave baseline of 15%) if they learn that a high fraction of Leave supporters are uncertain. Similarly, Remain supporters become 25% more likely to donate to the Better Off Out campaign (relative to the Remain baseline of 12%) if they learn that a higher fraction of Remain supporters is uncertain. Learning about uncertainty among other Leavers further increases Leave supporters' willingness to tweet their support for the "People's Vote" by 135% on a baseline value of about 2%. Additionally, seeing high shares of uncertainty among both Leave and Remain supporters decreases participants' intention to vote for the Brexit Party by 29% on a general baseline of 5.5%, a result that is driven by Leave supporters. Although participants update their beliefs about the support for and uncertainty over another referendum in the population in line with expectations, the treatment does not affect their private beliefs. Likewise, their beliefs about the likelihood and consequences of Brexit are largely unaffected. Hence, the results do not appear to be driven by social learning. In contrast, learning that other people are relatively more uncertain does reduce the social benefits that are associated with supporting a norm that is more popular in a subgroup, as well as the costs of going against it. That is, participants think that people who support another referendum are seen more positively by Leave supporters and less positively by Remain supporters. Similarly, they think that opposing a second referendum is seen less positively by Leave supporters and more positively by Remain supporters. Thus, social image concerns do appear to play a

role. Finally, learning that Remain and Leave supporters are more uncertain about their views leads participants to believe that both public support for or opposition to another referendum would be substantially more effective in convincing others. Thus, strategic considerations about the impact of one's own actions appear to be another channel through which the intervention works.

Our results imply that people become more willing to campaign for a norm that is at odds with their preference over outcomes if they learn that others are uncertain about their stance on said norm. The information does not shift private beliefs about either the norm or the outcome, but beliefs over others' reactions to the campaigning. This implies that the results are likely driven by people whose views on political outcomes (the UK leaving or remaining in the EU) diverged from their view on the norm that should produce this outcome (the UK implementing the results of the 2016 referendum or having another one) already before the intervention. Learning that a higher fraction of people are uncertain appears to have lowered the social costs associated with going against their political camp's majority opinion on the norm. Consequently, our results imply that policy makers interested in reducing polarisation on democratic norms should launch information campaigns that highlight not only the level of support or opposition that specific norms command, but also the certainty people attach to their assessments.

We contribute to several active strands of literature. First, we contribute to the literature on social norms and how they shape economic as well as political behaviour (e.g. [Bénabou and Tirole, 2006, Krupka and Weber, 2009, DellaVigna et al., 2016, Acemoglu and Jackson, 2017, Bursztyn and Jensen, 2017, Perez-Truglia and Cruces, 2017, Bénabou et al., 2018, Karing, 2018, Bursztyn et al., 2020a,b, Ali and Bénabou, 2020, Andreoni et al., 2021, Bursztyn and Yang, 2022, Bursztyn et al., 2022, 2023a]). This literature shows that concerns over social norms and image concerns (e.g. [Bénabou and Tirole, 2006]) shape labour market behaviour (e.g. [Bursztyn et al., 2020b]), health behaviour (e.g. [Karing, 2018, Bursztyn et al., 2022]), voting behaviour (e.g. [DellaVigna et al., 2016, Perez-Truglia and Cruces, 2017]), and hate speech (e.g. [Bursztyn et al., 2020a]). It also documents that light touch interventions can shift beliefs over appropriate behaviour and subsequent action. For example, [Bursztyn et al., 2020b] show that in Saudi Arabia men's beliefs over the acceptability

of women working outside the household are shifted when they are informed over true levels of support for this notion from fellow men. We contribute to this literature by providing evidence that in the context of democratic norms, information on other people's *uncertainty* over a specific norm, rather than level support for it, prompts people to become more politically active. In this context, we also provide new descriptive measures of perceived uncertainty about norms. This directly relates to the literature on the measurement of social norms [Krupka and Weber, 2013] and the measurement of uncertainty [Manski, 2004, Enke and Graeber, 2019].

Second, we contribute to the literature on the determinants of political participation (e.g. DellaVigna et al. [2016], Acemoglu et al. [2018], Passarelli and Tabellini [2017], Perez-Truglia [2018], Cantoni et al. [2019], Enikolopov et al. [2020], Manacorda and Tesei [2020], Bursztyn et al. [2021], Hager et al. [2022a,b, 2023]). This literature explores how peer effects shape party support, protesting, or political campaigning. Other people's political activity can act as a substitute and thus discourage individuals from becoming politically active themselves (e.g. [Cantoni et al., 2019, Hager et al., 2023]). On the other hand, individuals are become more politically active when surrounded by like-minded peers (e.g. [Perez-Truglia, 2018]). We provide novel evidence on the relative importance of different social groups in shaping individual's behaviour by providing people with information on the views of both people from their own and the competing political camp. This allows us to disentangle the relative importance of ingroup and outgroup members in individuals' choice to politically engage. Our findings imply that individuals react more to ingroup members, but that learning about their uncertainty renders them more inclined to campaign for the preferred norm of the outgroup – decreasing conformity effects. We also show that people can be mobilised by information about democratic norms, i.e. the rules governing the process adjudicating issues, rather than information about political issues themselves.

Third, our results have implications for the growing literature on the impact of information campaigns and news media on political thoughts and behaviour (e.g. [DellaVigna and Kaplan, 2007, Bursztyn et al., 2019, Barrera et al., 2020, Pennycook et al., 2020, Chopra et al., 2022, Galasso et al., 2022, Bursztyn et al., 2023b]). The literature documents a strong impact of slant (e.g. [DellaVigna and Kaplan, 2007])

and presentation (e.g. [Bursztyn et al., 2023b]) of news media as well as fake news in information campaigns (e.g. [Barrera et al., 2020]). In exploring correctives to this, most of the focus lies on fact checking interventions (e.g. [Chopra et al., 2022, Galasso et al., 2022]). With this paper, we provide suggestive evidence that information campaigns which highlight the uncertainty that people attach to their views can activate voters and decrease political polarisation in cases where norms are ambiguous.

The rest of the paper is structured as follows: Section 3.2 describes the empirical design of our experiment. That is, the institutional context (3.2.1), the experimental design (3.2.2), the data (3.2.3) and the empirical specifications (3.2.4) we use. Section 3.3 details our main results, both in the reduced form (3.3.1 and 3.3.2) and using an instrumental variable approach (3.3.3). Section 3.4 explores the mechanisms driving the change we see in political participation. Section 3.5 discusses the policy implications of the results, and Section 3.6 concludes.

3.2 Empirical Design

3.2.1 Institutional Context

The United Kingdom joined the European Communities, the precursor organisation of the European Union, in 1972. From the start, the membership was hotly debated in the UK. In 1975, the country held it's first referendum on whether the UK should remain in the EC. At that point, the Yes campaign won with 67.2% majority. However, throughout the next decades, whether and how closely the UK should tie itself to the European Union remained a point of contention, with "Euroscepticism" running particularly high in the early 2010s as the UK saw increased immigration from the EU.

In 2015, David Cameron, the prime minister from the Conservative Party, announced plans for another referendum on whether or not the UK should remain a member of the European Union, or leave it. His reasoning was that even though he himself was in favour of the UK remaining in the European Union, democratic norm dictated hearing from the public. The vote was held on the 23^{rd} June 2016 and the Leave campaign won by a margin of 51.9% to 48.1%. 72.2% of the UK's voting age

population voted in the referendum. The vote had immediate and far reaching consequences for the political leadership, economy and society in the UK. David Cameron resigned the day after the vote and was replaced by Theresa May. The pound sterling fell by 8% against the US dollar. And almost immediately public debate over whether the result should be implemented or not broke out.

In the lead up to the referendum, the "Vote Leave" campaign claimed that Britain contributed 350 million pound each week to the EU budget and promised that this money would be spent on the NHS if the UK left the European Union. This pledge was disowned the morning after the Brexit vote by Nigel Farage, one of the leaders of the UKIP party and Leave campaign. Examples like this stoked ire from supporters of the Remain campaign. They argued that the public had been misled about the economic consequences of Brexit and that therefore it was incumbent on policy makers to hold another vote or disregard the results of the 2016 one – appealing to the 2016 High Court's ruling that the results were not legally binding due to the constitutional principles of parliamentary sovereignty.[1]. On the other hand, Leave supporters too appealed to political principles, arguing that to question the outcome of a democratic vote was to go against the bedrock of democracy. Many further expressed the view that Remain supporters were motivated less by qualms over the process leading up to the vote, but more by their disagreement with its outcome.

Another point of contention was the deal under which the United Kingdom would leave the European Union. During the campaign, "Vote Leave" stated that if the UK were to leave the European Union, the deal they would negotiate with the EU would ensure tariff- and bureaucracy free trade with the EU and no changes at the Northern Irish border. However, as the UK government went into negotiations with the EU, it became clear that several of these promises could not be upheld, with the Northern Irish border becoming a particularly contested issue. The withdrawal agreement negotiated by Theresa May's government was rejected three times by parliament in two years, eventually leading to her resignation in summer 2019. She was replaced by Boris Johnson who renegotiated the Northern Ireland protocol. However, since this new deal still did not receive support from a majority in parliament, Boris Johnson called a general election for 12^{th} December 2019.

[1]The exact ruling can be found here.

In autumn 2019 when the snap election was announced, tensions on Brexit and the possibility of a second[2] referendum had been running high for three years. In April 2018, the People's Vote campaign had formed, demanding a repeat referendum be held where the choice was between the withdrawal agreement and the UK remaining in the European Union. They argued that not only had the Leave campaign misled the public, but also that only an informed choice that presented voters with the actual withdrawal agreement that would be implemented when the UK left the EU would comply with democratic principles. The campaign gained the support of prominent political figures such as the mayor of London Sadiq Khan and People's Vote marches in March and October 2019 were attended by a million people each. In response to this, pressure groups such as "Leave Means Leave" or "Better Off Out" formed and lobbied for the UK to leave the EU with or without a deal. They argued that in a democracy, the government had to respect the will of the people and that the people had spoken. This sentiment was echoed by a majority of the (more conservative skewing) British print outlets that insisted the government had to implement the majority's vote and termed public figures arguing for a new referendum "Remoaners".

The split in UK society on the questions of Brexit and a new referendum was also reflected in the platforms of the major political parties in the lead up to the general election. The Conservatives campaigned on leaving the European Union under the terms of Boris Johnson's withdrawal agreement. The Brexit party campaigned on leaving the European Union without a deal. The Liberal Democrats campaigned on remaining in the European Union without another referendum. And the Labour Party campaigned on another renegotiation of the withdrawal agreement that would then be put up for a vote in another referendum alongside the option to remain in the European Union. Crucially, Brexit and the possibility of a repeat referendum were decisive factors in the election campaign and the major parties had clear and distinguishable positions on both.

As a result, when we ran our experiment in November 2019, there was *substantial* disagreement over whether supporting a repeat referendum was breaching democratic

[2]Technically, another referendum on the UK remaining or leaving the European Union would have been the *third* referendum of this kind, as the 1975 one was the first. However, in the public discourse at the time, the 1975 referendum did not play much of a role and people kept talking about a second vote, taking the 2016 one as the first they were referring to. We will follow this convention in this paper.

norms by questioning the outcome of a legitimate vote – or whether opposing one was breaching a democratic norm by disregarding a potential change in the will of the people after a withdrawal agreement had been reached and both alternatives clarified. Both viewpoints had vocal public supporters and were represented in electoral platforms. Furthermore, there was substantial *uncertainty* about how widespread either belief was in the population, as the last general vote had been in 2017, which was a change of government and several renegotiations ago and had resulted in an almost tie between the Conservatives and Labour. The situation thus provided a rare situation in which majority opinion on a democratic norm was both genuinely contested and uncertain.

3.2.2 Experimental Design

We conduct our experiment in November 2019, before the general election that settled the question of whether a second referendum would be held on the UK leaving the European Union. We run two surveys, both times collaborating with the online survey provider Luc.id (now part of Cint) to recruit samples representative of the UK population in terms of age, gender, region, and income. In the first survey, the pilot, we recruit 644 participants. The main objective of the pilot is to obtain detailed measures of respondents' beliefs about the appropriateness of a second referendum on Brexit as well as their uncertainty about their view for the information treatments in the main survey. In said main survey, we recruit 4295 participants who, over the course of the survey, are shown different pieces of information on others' agreement with and uncertainty about the appropriateness of a second referendum. We subsequently measure how this information shifts their political beliefs as well as willingness to campaign. The questionnaires for both the pilot and the main survey can be accessed online, under https://hannahzille.github.io/hannahzillessen.com/Pre_survey.pdf and https://hannahzille.github.io/hannahzillessen.com/Brexit_main.pdf respectively.

Pilot survey. The pilot survey consists of two sections. After passing a short attention check, participants begin the survey by completing demographic questions about their age, gender, employment status, education and region of residence. We also ask them about their political views, such as their vote in the 2016 referendum

on the UK leaving the European Union as well as their current stance on this. Then, we elicit respondents' views on the appropriateness of a second referendum using two different scales. Specifically we ask all respondents to what degree they agreed with the statement that "Having a second referendum on Brexit is appropriate." and then, how uncertain they are about their view on whether having a second referendum is appropriate. Half of the participants are asked to indicate their agreement and uncertainty on a 5-point Likert with "Agree", "Somewhat agree", "Neither agree, nor disagree", "Somewhat disagree" and "Disagree" for agreement, and "Very uncertain", "Somewhat uncertain", "Neither uncertain, nor certain", "Somewhat certain" and "Very certain" for uncertainty. The other half can only choose between "Agree" and "Disagree" as their answer to the first question, and "Very uncertain" and "Very certain" for the latter one. Participants are randomly assigned to either the coarse or detailed answer scale. As a result of this variation in scales, we obtain different truthful estimates about the share of respondents that agreed with the appropriateness of a second referendum as well as were very uncertain. In the sample of respondents who can use a detailed scale, 3% of Leavers indicated that they "agree' with the notion that a second referendum was appropriate with 14% saying they are "very uncertain" about their view. Among Remainers, 50% choose "agree" and 3% say they are "very uncertain". In contrast, in the sample of participants that are given the coarse scale, 15% of Leavers agree with the appropriateness of a second referendum and 41% indicate that they are very uncertain. In the group of Remainers, 91% agree with the appropriateness of a second referendum while 25% state that they are very uncertain about their view. All eight numbers reflect true agreement and uncertainty shares in the population and as such we can use them to construct varying information treatments in the main survey. They are summarised in Table 3.2.1.

Main survey. The main survey is conducted 10 days after the pilot survey and consists of three parts. We first measure basic demographics (again age, gender, employment status, education and region of residence), as well as political attitudes. In particular, we ask respondents which political party (if any) they are closest to, how they voted in the 2016 referendum on Brexit, and whether, at the time of the survey, they are in favour of the UK leaving the European Union. We also ask them

about how closely they followe politics, what share of their reference group supports the UK leaving the European Union and to what degree they identify with either the Remain or the Leave campaign.

Next, we elicit respondents' beliefs about the degree to which Leavers and Remainers agree with the notion that a second referendum on the UK leaving the European Union would be appropriate, as well as the uncertainty that members of both groups attach to their view. Before we ask participants about their priors, we provide them with background knowledge on the source of information. Specifically, we inform respondents that we conducted "a large survey with a representative sample of the UK population in November 2019 to understand people's views on a second referendum on Brexit". We further tell them that we asked respondents whether they agreed "that having a second referendum on Brexit is appropriate" as well as how uncertain they were about their view. We then ask participants what fraction of respondents from our pilot who support Leave agree that having a second referendum on Brexit would be appropriate. We also ask them to estimate the fraction of respondents from our pilot who support Leave, that are very uncertain about their view. Finally, we ask the participants to estimate the same two numbers for Remain supporters. We randomise whether questions about Remainers or Leavers are asked first.

Thereafter, all of our respondents receive information about the true shares of Leave and Remain supporters that agree with the appropriateness of a second referendum as well as claim to be "very uncertain" about it. Participants see two treatment screens, one with information on Leavers and one with information on Remainers. On these screens they are reminded of their own guess in the previous section and then given the fractions from the pilot survey. All of our respondents receive information about Leave and Remain supporters in the same order that prior beliefs are elicited. For each group, respondents are show both text and a graphical representation of the information. Figure 3.A.1 in the Appendix depicts an example of a treatment screen covering the Remain sample.

Crucially, participants do not all see the same information. Instead, we use the variation in the fractions of respondents agreeing with the appropriateness and stating to be very uncertain about their views induced by the differential scales in the pilot survey. As detailed above, seeing the 2-point rather than the 5-point Likert

Table 3.2.1: Treatment Overview

	Leavers		**Remainers**	
	Agreement	Uncertainty	Agreement	Uncertainty
High Level	15%	41%	91%	25%
Low Level	3%	14%	50%	3%

scale results in much larger fractions of pilot participants indicating that they agree with the appropriateness as well as are very uncertain. The exact numbers for each dimension are summarised again in Table 3.2.1. Crucially, the shares from both the 2-point as well as the 5-point Likert scale arm represent accurate information about shares of respondents in agreement and very uncertain. Hence, for each of the four different dimensions (relating to the columns in Table 3.2.1) we cross-randomise whether respondents see the high or the low number – generating causal evidence on the impact of seeing both high levels of (dis)agreement with a certain norm as well as (un-)certainty about this from different groups.

In the final part of the survey, we measure outcomes. Straight after seeing the information treatment our respondents are asked about their support for a second referendum (note, *not* their take on its appropriateness) and their certainty about their view (both on a 5-point Likert-scale). Thereafter, we provide all of our respondents with information about the People's Vote and the Better Off Out campaigns, which lobby for and against a second referendum on Brexit respectively. For each of the campaigns, we ask participants whether they would like us to donate one pound to their efforts on their behalf. We also tell our respondents that we will randomly pick one in three participants and implement their answer to this question. To further measure people's willingness to publicly endorse or oppose a second referendum, we tell them that both campaigns encourage people to publicly endorse or oppose a second referendum on social media. We then ask participants whether they themselves would like to tweet about either their endorsement of or opposition to a second referendum. Respondents who indicate that they would like to share a tweet, are shown a link which includes an endorsement or opposition message. We measure their willingness to actually tweet via their propensity to click on the link as well as publicly share the message. To gauge the impact of the information on political behaviour

we also elicit participants' intention to vote in the upcoming general election as well as a hypothetical second referendum. To close out the survey, we ask respondents a series of manipulation check and mechanism questions to explore how the treatment information affects individuals' beliefs. For example, we ask them about the fraction of the population they believe supports Brexit; the consequences, Brexit would have for them personally as well as society at large; how different parts of the population perceive people who publicly support or oppose a second referendum; and how effective they believe public support of or opposition to a second referendum is at convincing others.

3.2.3 Data

Sample. Table 3.2.2 displays summary statistics for the main survey sample. Our sample is demographically in line with the overall UK population. 45% of respondents are in full-time employment and 5% are students. However, there are slightly more Remain supporters in our data set compared to the general population in December 2019. 55% of respondents support remaining in the European Union while only 45% voted to remain in the 2016 referendum. Table 3.2.3 shows that pre-determined characteristics are balanced between treatment arms, suggesting that the randomisation worked as intended.

Treatment Variables. The main treatment variables in the paper derive directly from the treatment information described in the previous section. We code up two dummy variables, "High Level Remain" and "High Level Leave" which go to one whenever participants see the higher of the two numbers on the fraction of Remainers or Leavers agreeing with the appropriateness of a second referendum respectively. Similarly, we code up two dummy variables that go to one whenever participants see the higher of the two numbers describing the fraction of Remainers or Leavers saying they are very uncertain about their view, "High Uncertainty Remain" and "High Uncertainty Leave". In the following, we will refer to these treatments by their variable names, that is, "High Level" for participants seeing a higher fraction agreeing with the appropriateness of a second referendum and "High Uncertainty" for them seeing a higher fraction of people stating they are very uncertain. From these four variables,

we code up three complimentary treatments. First, we generate two binary variables that go to one if participants see (i) high levels for both Leavers and Remainers, and (ii) high uncertainty for both Leavers and Remainers. We refer to these as "High Level Pooled" and "High Uncertainy Pooled". We further generate two dummy variables that go to one whenever participants see (i) high levels of agreement from members of their own political camp, and (ii) high uncertainty from members of their own political camp. That means, they go to one whenever Leavers see information that a higher fraction of Leavers agrees with the appropriateness of another referendum/ is uncertain about that stance – or Remainers see the same about a high fraction of Remainers. We denote these variables as "High Level Own" and "High Uncertainty Own". Finally, we analogously code up two variables indicating that participants see (i) high levels of agreement and (ii) uncertainty from members of the *opposed* political camp which we refer to as "High Level Other" and "High Uncertainty Other".

Outcome Variables. Our main outcome of interest is participants' willingness to politically campaign, captured by their willingness to (a) donate to either the People's Vote or Better Off Out campaign, and (b) tweet about their support for or opposition to a second referendum citing the respective campaign. The first is captured by two dummy variables going to one if a participant opted to donate to (i) the People's Vote or (ii) the Better Off Out campaign.[3] The second is captured by two further dummy variables indicating whether participants clicked on the link to share either (i) the pre-written message in support of a second referendum, or (ii) in opposition to it.[4]. In the interest of better understanding how the information impacts political activity more broadly, we also measure how it impacts participants' plans to vote in the upcoming election. Specifically, we focus on five dummy variables each going to one if a respondent indicates they plan on voting for (i) the Conservatives, (ii) Labour, (iii) the Liberal Democrats, (iv) the Brexit Party, or (v) not at all. Finally, to untangle the mechanisms driving the main outcomes, we also measure a variety of

[3]We focus on participants' willingness to donate at all, i.e. publicly or privately in the main body of the paper. Our research design allows us to distinguish between public and private donations, but the results do not appear to be driven by either of the dimensions specifically.

[4]We focus on participants' *actually* clicking the share link rather than them indicating that they would be willing to do so in the main body of the paper. However, both measures result in qualitatively and quantitatively similar results.

related beliefs. All of these are measured either in percentage shares or categorical variables that will be detailed as we go through them in Section 3.4.

TABLE 3.2.2: Summary Statistics

	Mean	SD	Median	Min.	Max.	Obs.
Pre-determined variables						
Female	0.49	0.50	0.00	0	1	4295
Age	3.40	1.57	3.00	1	6	4295
Some Tertiary Education	0.42	0.49	0.00	0	1	4295
Employed FT	0.45	0.50	0.00	0	1	4295
Student	0.05	0.22	0.00	0	1	4295
Voted Remain in 2016	0.45	0.50	0.00	0	1	4275
Voted Leave in 2016	0.39	0.49	0.00	0	1	4275
Support Remain Now	0.55	0.50	1.00	0	1	4255
Prior: Remain, Level	65.64	24.31	70.00	0	100	4179
Prior: Remain, Uncert	37.55	25.15	35.00	0	100	4179
Prior: Leave, Level	28.91	25.17	20.00	0	100	4182
Prior: Leave, Uncert	35.15	26.01	30.00	0	100	4182
Outcomes						
Donate Pro Ref	0.36	0.48	0.00	0	1	4116
Donate Anti Ref	0.30	0.46	0.00	0	1	4118
Tweet Pro Ref	0.05	0.22	0.00	0	1	1977
Tweet Anti Ref	0.03	0.18	0.00	0	1	1977
Private Support	0.53	0.50	1.00	0	1	4139
Private Uncert	1.96	1.24	1.00	1	6	4139

Notes: This table shows the summary statistics for the full sample of the main survey. The sampling is described in section 3.2.2. The first panel shows the pre-determined variables, while the second panel shows the outcomes. Column one shows the mean of each variable, column two its standard deviation, column three the median, columns four and five the minimum and maximum value the variable can take, and column six the number of observations available for each.

Table 3.2.3: Balance Between Treatment Arms

	Mean Outcomes				P-Value
	High Level Remain	High Uncert Remain	High Level Leave	High Uncert Leave	
Pre-determined variables					
Female	0.488	0.499	0.490	0.488	0.721
Age	3.407	3.416	3.403	3.382	0.882
Tertiary Education	0.418	0.423	0.421	0.419	0.990
Employed FT	0.456	0.454	0.446	0.448	0.806
Student	0.052	0.046	0.055	0.055	0.300
Voted Remain in 2016	0.440	0.451	0.437	0.445	0.183
Voted Leave in 2016	0.402	0.393	0.405	0.390	0.335
Support Remain Now	0.533	0.551	0.539	0.545	0.216
Prior: Remain, Level	64.887	65.035	65.709	65.456	0.149
Prior: Remain, Uncert	38.190	37.462	38.015	38.080	0.175
Prior: Leave, Level	28.339	29.595	29.488	28.985	0.112
Prior: Leave, Uncert	35.070	35.592	36.248	35.374	0.060

Notes: This table displays balance statistics for the full sample of the main survey. The sampling is described in section 3.2.2. Columns one to four show the mean of each pre-determined variable for each of the four treatments. Column five shows the p-value based on a test of joint insignificance of the four treatment dummies used in specification 3.3.

3.2.4 Empirical Strategy

Reduced Form Regressions. To test which impact information on the degree to which others agree with a certain democratic norm, in this case the (in)appropriateness of a second referendum on the UK leaving the European Union, as well as the uncertainty they attach to these views has on people's political views and participation, we use three complementary empirical specifications. All specifications have been pre-specified.[5]

First, we want to learn whether information about the level of agreement with a certain democratic norm as well as uncertainty about it in the general population changes people's beliefs about the norm as well as willingness to become politically active. Hence, we run the following OLS regression:

[5]The full pre-analysis plan can be found in the AEA RCT Registry under https://doi.org/10.1257/rct.5132-1.0.

$$\mathrm{Y}_i = \beta_0 + \beta_1 \mathrm{HighLev}_{pooled,i} + \beta_2 \mathrm{HighUnc}_{pooled,i} + \phi \mathrm{X}_i + \varepsilon_i \tag{3.1}$$

where Y_i denotes the outcome of interest and X_i a vector of control variables. Specifically, we control for participants' gender, age, employment status, education, frequency of consuming news and talking about politics, vote in the 2016 referendum, support for Brexit at the time of the survey and associated uncertainty, region of residence and share of their reference group supporting Brexit. As our sample is balanced across treatment arms, these controls are not necessary for identification and only serve to tighten our estimates. $\mathrm{HighLev}_{pooled,i}$ denotes the first treatment variable in this specification, indicating that participants saw (relatively) high levels of agreement with the appropriateness of another referendum for *both* Leavers and Remainers. Similarly, the second treatment variable, $\mathrm{HighUnc}_{pooled,i}$ indicates participants saw high levels of uncertainty for both Leavers and Remainers. The omitted category is made up of participants who saw the lower of two numbers for both the level of agreement as well as the uncertainty of Leavers and Remainers alike. That is, they received information that relatively low shares of Leavers and Remainers agree with the appropriateness of a second referendum and are relatively certain about this take.

Next, we want to disentangle whether people react differently to information about others who share their political beliefs and those who hold contrary ones. Concretely, in this setting we test whether the reactions of Leavers/Remainers differ in response to seeing information about other Leavers/Remainers by running the following regression:

$$\begin{aligned} \mathrm{Y}_i = \quad & \gamma_0 + \gamma_1 \mathrm{HighLev}_{own,i} \gamma_2 \mathrm{HighUnc}_{own,i} + \\ & + \gamma_3 \mathrm{HighLev}_{other,i} + \gamma_4 \mathrm{HighUnc}_{other,i} + \phi \mathrm{X}_i + \varepsilon_i \end{aligned} \tag{3.2}$$

where Y_i again captures the outcome of interest and X_i a vector of control variables, but the treatments have changed. $\mathrm{HighLev}_{own,i}$ and $\mathrm{HighUnc}_{own,i}$ indicate that a participant saw high levels of agreement with the appropriateness of a second referendum and uncertainty about this stance from members of their own political camp. For example, $\mathrm{HighUnc}_{own,i}$ goes to one whenever a participant supporting Leave saw

the high uncertainty treatment for Leavers, or a Remain leaning participant for Remainers. Similarly, $\text{HighLev}_{other,i}$ and $\text{HighUnc}_{other,i}$ indicate that a participant saw high levels of agreement with the appropriateness of a second referendum and uncertainty respectively from members of the *opposing* political camp. That is, they capture the information that Leave leaning participants saw about Remainers and vice versa.

Finally, we want to learn in which political direction information about others' agreement with a democratic norm and uncertainty thereof impacts people's political beliefs and actions, and whether information is more potent when it aligns with the more or less favoured view. For example, does information about Leavers or Remainers being uncertain render Remain supporters more likely to donate to the People's Vote or Better Off Out campaign? To this shed light onto this question, we run the following specification:

$$\begin{aligned} \text{Y}_i = \quad & \delta_0 + \delta_1 \text{HighLev}_{leave,i} + \delta_2 \text{HighUnc}_{leave,i} \\ & + \delta_3 \text{HighLev}_{remain,i} + \delta_4 \text{HighUnc}_{remain,i} + \phi \text{X}_i + \varepsilon_i \end{aligned} \tag{3.3}$$

where Y_i and X_i are defined as before. Alas, the treatment variables now indicate that participants saw high levels of agreement with the appropriateness of a second referendum and uncertainty about that assessment from Leavers and Remainers (irrespective of partipants' own political leaning). As pre-specified, we run this regression separately for Leavers and Remainers when looking at the main outcomes and for the pooled sample when we look at mechanisms.

Instrumental Variable Regressions. The reduced form regressions cleanly identify the average treatment effects of seeing relatively higher or lower levels of agreement and uncertainty. However, in practice, we might think that such information will have a different impact depending on whether it lies above or below people's *prior*. Seeing that 41% of Leavers are uncertain about their views on the appropriateness of a second referendum will likely have a different effect on people who previously thought the real number was 20% – or 60%. While both individuals would have seen

the relatively higher of the two uncertainty numbers for Leavers, only one of them will have learned that Leavers are *more* uncertain about their view than they previously thought. So, while the reduced form analysis allows us to identify the impact of seeing higher the higher of two pieces of treatment information, it does not allow us to capture the impact of seeing higher levels relative to people's previous beliefs. In order to do this, we can compute dummy variables which indicate whether participants are shown treatment information that exceeds their priors. Unfortunately, if we were to regress our outcomes of interest directly onto this set of binary variables, we would run into endogeneity problems. This is because the likelihood of participants seeing information that surpasses or falls short of their expectations is inextricably linked to precisely those personal expectations – which in turn shape political participation. However, the likelihood of seeing higher than expected levels of agreement as well uncertainty from Leavers/Remainers is also strongly predicted by the treatment arm participants were randomised into. No matter their prior, the likelihood of a participant seeing information that surpasses their expectation for a specific dimension and group is larger, if they were randomised into seeing the high level treatment for that dimension and group. At the same time, the treatment assignment is uncorrelated with participants' beliefs and political participation. Hence, the treatment variation provides us with a valid and relevant instrument to estimate the local average treatment effect of participants seeing higher than expected levels of agreement with the appropriateness of a second referendum or uncertainty about that notion on their political beliefs and participation. Specifically, we use a two-stage-least squares approach with the following first stage regressions:

$$\mathrm{T}_{higher,i} = \beta_0 + \eta \mathrm{T}_{high,i} + \phi \mathrm{X}_i + \varepsilon_i \tag{3.4}$$

where $\mathrm{T}_{higher,i}$ indicates that participants saw treatment information that surpassed their prior in a given treatment arm and $\mathrm{T}_{high,i}$ denotes the dummy variable for receiving the high level treatment in that arm. The second stage regressions replicate specifications 3.1, 3.2 and 3.3, but substitute in the fitted values from the first stage regression 3.4 for the treatment indicators.

3.3 Impact on Political Participation

There are different ways in which information on the fraction of Leave and Remain supporters that deem a repeat referendum appropriate and/or are uncertain about their stance could impact participants' willingness to become politically active. First, it could be that the information prompts them to update their beliefs about the true appropriateness of another referendum as well as potentially Brexit. In this case, we would expect that participants become more willing to campaign for another referendum if they see high levels of agreement with its appropriateness – and less likely if they do not. We would further expect these effects to to be larger if participants see (dis-)agreement among both Leavers and Remainers, and stronger whenever they see information that a higher fraction of the population is certain. Second, it could be that participants update their beliefs about the social benefits and costs associated with conforming to or going against the majority opinion in their reference group on having another referendum. That is, participants who see that a high fraction of people who share their beliefs on political outcomes (remain in/leave the EU) agrees with the appropriateness of a repeat referendum might take away that there is signalling value in campaigning for it. This is especially true if they also receive information that their reference group is relatively certain about that stance. The same would hold in the other direction if participants see that only a low fraction of people in their reference group agrees. In all of these cases information on certainty would act as weighting function. However, it could also be that information on uncertainty shapes political participation independently of information on level agreement. For one, it could be that learning that others are uncertain weakens both the social benefits and costs associated with conforming to or going against the majority opinion in one's reference group. Specifically, it could be that participants who disagree with most of the people in their reference group on the appropriateness of another referendum and learn that their peers are relatively more uncertain feel more empowered to campaign for the stance that is less popular in their reference group. Third, it could be that participants believe their actions have more impact if they learn others are more uncertain about their views. In this case, we would expect information on high levels of

uncertainty among either group to raise participants' willingness to politically campaign, with a potentially more pronounced effect on information on the "opposing" camp. Depending on the presence and relative strength of these channels, the treatment information could either narrow or increase polarisation on the appropriateness of a repeat referendum.

3.3.1 Impact on Political Campaigning

Result 1 *Participants become more willing to campaign both for and against a repeat referendum if they learn that a relatively high share of the population is uncertain about the appropriateness of such a referendum. Learning that a relatively high fraction agrees with the appropriateness does not affect willingness to campaign.*

The first question we want to answer is whether people become more or less inclined to campaign for another referendum if they learn that others are relatively more uncertain about the appropriateness of such a referendum. To this end, we regress participants' choice to donate to the People's Vote or Better Off Out campaign, as well as their choice to tweet in support of either of them onto two dummy variables. The first takes value one if they see high levels of agreement with the notion that a second referendum would be appropriate for both Leavers and Remainers. The second takes value one analogously if they see high levels of uncertainty about this stance for both groups (Specification 3.1).

We find that information on others' *level* of agreement does not affect participants' political participation. Table 3.3.1 shows that participants' willingness to donate to either campaign is not affected by learning that a higher share of both Leavers and Remainers agrees with the notion that a second referendum would be appropriate. Similarly, their willingness to tweet is unaffected by level information. In contrast, if participants learn that both Leavers and Remainers attach relatively high uncertainty to their views, they become 2.8pp more likely to donate to both the People's Vote as well as the Better Off Out campaign. This represents an increase of close to 10% on a willingness to donate of about 33% among participants who see low levels of agreement and uncertainty for at least one group.

TABLE 3.3.1: Campaigning, Pooled

	Donate Pro Ref	Donate Anti Ref	Tweet Pro Ref	Tweet Anti Ref
High Level	0.010	0.005	0.002	-0.007
	(0.016)	(0.015)	(0.012)	(0.009)
High Uncert	0.028*	0.028*	-0.003	0.002
	(0.016)	(0.015)	(0.011)	(0.009)
Observations	4116	4118	1977	1977
Control Mean	.3554422	.3016027	.0490642	.0343955

Notes: This table shows the effects of learning that a high fraction of both Leavers and Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on participants' willingness to campaign. Columns one and two show the impact on participants' willingness to donate to the People's Vote and Better Off Out campaigns. Columns three and four shows the impact on their willingness to tweet their support for or opposition to a repeat referendum. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Result 2 *Participants become more politically active when they learn that people who share their preference over political outcomes are relatively more uncertain about the appropriateness of another referendum. Neither information on the degree to which people agree with the appropriateness of a second referendum, nor information on how uncertain people on the other side of the political spectrum are, changes participants' behaviour.*

Next, we ask whether people react more to information about the beliefs and uncertainty of those who *share* their preference over political outcomes, or of those who campaign for opposing outcomes. This is, whether Remainers react more to information about other Remainers or Leavers and, similarly, whether Leavers react more to information about other Leavers or Remainers. We regress participants' decision to donate as well as tweet onto four dummy variables indicating whether they see high

levels of agreement from their own as well as the opposing political camp as well as high levels of uncertainty among either group (Specification 3.2).

TABLE 3.3.2: Campaigning, Own/Other

	Donate Pro Ref	Donate Anti Ref	Tweet Pro Ref	Tweet Anti Ref
High Level Own	0.001	0.006	-0.000	-0.006
	(0.014)	(0.013)	(0.010)	(0.008)
High Uncert Own	0.023*	0.023*	0.018*	-0.001
	(0.014)	(0.013)	(0.010)	(0.008)
High Level Other	0.006	-0.003	-0.001	0.008
	(0.014)	(0.013)	(0.010)	(0.008)
High Uncert Other	0.019	0.007	-0.013	0.004
	(0.014)	(0.013)	(0.010)	(0.008)
Observations	4116	4118	1977	1977
Control Mean	.3554422	.3016027	.0490642	.0343955

Notes: This table shows the effects of learning that a high fraction of people in one's own as well as the opposed political camp agrees with the appropriateness of a repeat referendum and is uncertain about this stance on participants' willingness to campaign. Columns one and two show the impact on participants' willingness to donate to the People's Vote and Better Off Out campaigns. Columns three and four shows the impact on their willingness to tweet their support for or opposition to a repeat referendum. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

We find that the impact of uncertainty information on participants' propensity to donate and tweet is driven entirely by them learning that members of their own political camp are uncertain about whether a second referendum would be appropriate. Table 3.3.2 shows that when participants learn that people who share their political beliefs are relatively more uncertain, they become 2.3pp more likely to donate to the People's Vote and the Better Off Out campaign. Moreover, Table 3.3.2 also documents that participants become 1.8pp more likely to tweet in support of

the People's Vote campaign if they learn that their own political camp is uncertain. This represents a 36.7% increase on the 4.9% likelihood to tweet for the People's Vote campaign among participants who see low figures on all four dimensions. Just as before, information on the degree to which people in either camp agree with the appropriateness of a second referendum has no significant impact on participants' political participation.

TABLE 3.3.3: Campaigning, Leavers

	Donate Pro Ref	Donate Anti Ref	Tweet Pro Ref	Tweet Anti Ref
High Level Remain	0.002	-0.017	0.012	0.022
	(0.016)	(0.023)	(0.009)	(0.020)
High Uncert Remain	0.008	-0.021	0.002	0.014
	(0.016)	(0.023)	(0.010)	(0.020)
High Level Leave	0.013	0.011	0.005	-0.019
	(0.016)	(0.023)	(0.010)	(0.020)
High Uncert Leave	0.032**	0.021	0.023**	0.000
	(0.016)	(0.023)	(0.010)	(0.020)
Observations	1857	1860	758	758
Control Mean	.1513193	.5204301	.0171504	.0870712

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on willingness to campaign of Leave supporters. Columns one and two show the impact on participants' willingness to donate to the People's Vote and Better Off Out campaigns. Columns three and four shows the impact on their willingness to tweet their support for or opposition to a repeat referendum. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Result 3 *When Leave supporters receive information that a large share of other Leavers are relatively uncertain about whether a repeat referendum would be appropriate, they become more likely to donate to and tweet in support of the People's Vote*

campaign. When Remain supporters receive information that a large share of other Remainers or Leavers are uncertain, they become more likely to donate to the Better Off Out campaign.

TABLE 3.3.4: Campaigning, Remainers

	Donate Pro Ref	Donate Anti Ref	Tweet Pro Ref	Tweet Anti Ref
High Level Remain	-0.007	0.001	-0.001	-0.003
	(0.021)	(0.014)	(0.015)	(0.002)
High Uncert Remain	0.010	0.030**	0.012	0.000
	(0.021)	(0.013)	(0.014)	(0.002)
High Level Leave	0.007	0.007	-0.013	0.000
	(0.021)	(0.013)	(0.014)	(0.003)
High Uncert Leave	0.029	0.029**	-0.022	-0.000
	(0.021)	(0.013)	(0.014)	(0.002)
Observations	2259	2258	1219	1219
Control Mean	.5232404	.1213463	.0689089	.0016407

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on willingness to campaign of Remain supporters. Columns one and two show the impact on participants' willingness to donate to the People's Vote and Better Off Out campaigns. Columns three and four shows the impact on their willingness to tweet their support for or opposition to a repeat referendum. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Finally, we want to know whether people become more likely to campaign *for* or *against* their previously held beliefs if they learn that people in their own or the opposing political camp are uncertain about the appropriateness of a second referendum. Therefore, we split the full sample into two: Leave and Remaine supporters. We then regress participants' willingness to donate and tweet onto four dummies indicating whether they saw high levels of agreement with the notion that a second

referendum would be appropriate from Leavers and Remainers, as well as high levels of uncertainty for the respective groups (specification 3.3).

We find that, in both groups, participants react most strongly to high levels of uncertainty from people in their own political camp – and that this motivates them to politically campaign for the norm that is less aligned with their preference over outcomes. Specifically, Leave supporters become 3.2pp more likely to donate to the People's Vote campaign if they learn other Leavers are very uncertain about their views (Table 3.3.3). They also become 2.3pp more likely to tweet their support of a People's Vote. These effects constitute a 21.2% and 135% increase on the 15.1% and 1.71% means of Leavers who see low numbers on all four dimensions respectively. Similarly, Remain supporters who learn that other Remainers are relatively more uncertain become 3pp more likely to donate to the Better Off Out campaign. This represents a 24.7% increase on the 12.1% of Remainers who see low numbers on all dimensions. However, Remain supporters also become 2.9pp more likely to donate to the Better Off Out campaign if they learn that *Leavers* are relatively more uncertain. Their Twitter behaviour is unaffected and neither Remainers nor Leavers react strongly to level information about the degree to which either Remainers or Leavers agree with the appropriateness of a second referendum.

3.3.2 Impact on Voting Intentions

Result 4 *Participants become less likely to vote for the Brexit Party if they learn both Leavers and Remainers are relatively more uncertain about their views on the appropriateness of a repeat referendum. This is driven by information about members of the opposing political camp.*

Next, we want to find out how information on other people's stance on the appropriateness of a repeat referendum as well as their uncertainty about this stance affects participants' plans to vote in the upcoming general election. As laid out in section 3.2.1, the possibility of another referendum was a hotly debated topic in the lead up to the election. The Conservative Party campaigned for leaving the EU with Boris Johnson's deal and without a second referendum. The Brexit Party campaigned for leaving the European Union without a deal and without a second referendum.

Labour campaigned for renegotiating the deal with the European Union and for having another referendum where voters could choose between remaining in the EU or leaving it with the new deal. The Liberal Democrats campaigned for remaining in the European Union without a second referendum.

TABLE 3.3.5: Plans to Vote

	Conservatives	Liberal Dem	Labour Party	Brexit Party	Abstaining
High Level	-0.012	-0.012	-0.002	-0.002	0.011
	(0.013)	(0.011)	(0.015)	(0.008)	(0.008)
High Uncert	-0.002	-0.015	-0.001	-0.016**	0.012
	(0.013)	(0.010)	(0.015)	(0.007)	(0.008)
Observations	4255	4255	4255	4255	4255
Control Mean	.2610012	.1180442	.3201397	.0551804	.0572759

Notes: This table shows the effects of learning that a high fraction of both Leavers and Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on participants' voting intentions. Columns one to four show the impact on participants' intent to vote for the Conservatives, Liberal Democrats, Labour and the Brexit Party in the 2019 election respectively. Column five shows the impact on their intent to abstain from the election. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Table 3.3.5 shows that if participants learn that a large share of both Leavers and Remainers are relatively more uncertain about their views on a second referendum, they become 1.6pp less likely to say they plan to vote for the Brexit Party – a 29% decrease relative to the group that saw only low levels. Table 3.3.6 shows that this result is driven by information about people in the politically opposed camp. It also shows that both high uncertainty and high levels of agreement with the appropriateness of a second referendum from the opposed camp reduce participants' stated intent to vote for the Liberal Democrats, by 1.6pp and 2.4pp respectively. Tables 3.B.1 and 3.B.2 in the Appendix show that the impact on (not) wanting to vote for the Brexit

Party is driven entirely by Leavers, while the impact on the Liberal Democrats is driven overwhelmingly by Remainers. One possible interpretation is that both the Brexit Party's as well as the Liberal Democrats' election platforms were seen as the far end of the Leave/Remain spectrum respectively. Having learned that people on the respective other side of this divide are less set in their beliefs about what would be appropriate to do, participants might be less inclined to vote for parties that run on more "extreme" platforms.

TABLE 3.3.6: Plans to Vote, Own/Other

	Conservatives	Liberal Dem	Labour Party	Brexit Party	Abstaining
High Level Own	-0.000	0.010	-0.013	-0.009	0.009
	(0.012)	(0.009)	(0.013)	(0.007)	(0.007)
High Uncert Own	-0.002	0.000	-0.008	-0.006	0.006
	(0.012)	(0.009)	(0.013)	(0.007)	(0.007)
High Level Other	0.002	-0.024**	0.001	0.009	0.009
	(0.012)	(0.009)	(0.013)	(0.007)	(0.007)
High Uncert Other	0.002	-0.016*	0.008	-0.017**	0.010
	(0.012)	(0.009)	(0.013)	(0.007)	(0.007)
Observations	4255	4255	4255	4255	4255
Control Mean	.2610012	.1180442	.3201397	.0551804	.0572759

Notes: This table shows the effects of learning that a high fraction of people in one's own as well as the opposed political camp agrees with the appropriateness of a repeat referendum and is uncertain about this stance on participants' voting intentions. Columns one to four show the impact on participants' intent to vote for the Conservatives, Liberal Democrats, Labour and the Brexit Party in the 2019 election respectively. Column five shows the impact on their intent to abstain from the election. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

It is interesting that while political campaigning is affected mainly by information on people on one's own political camp being uncertain, voting behaviour reacts more

strongly to information about the other camp. Yet, all main results have in common that information on other people being relatively more uncertain decreases polarisation on whether another referendum would be appropriate. Participants on either side of the Leave/Remain debate become more likely to campaign for the stance that is less popular in their respective reference group. At the same time, they also become less likely to vote for parties which are at either end of the political spectrum with regard to another referendum and Brexit.

3.3.3 LATE Estimates

Randomising whether participants see the higher of two numbers on Leavers' and Remainers' levels of agreement with a second referendum being appropriate as well as their associated uncertainty provides us with clean reduced form estimates on the average treatment effect of seeing relatively high agreement as well as uncertainty. However, we might think that what matters most for people's individual choice to become politically active is whether they receive information that *shifts their priors*. A participant who initially believes that 50% of Leavers are uncertain about the appropriateness of a second referendum will likely react differently to being told that the true number is 41% (the high uncertainty treatment) than someone who initially believes that 10% are uncertain. Even though both participants see the "high uncertainty" information, only the latter learns that Leavers might be *more* uncertain than previously thought.

A regression of the outcomes of interest on dummy variables indicating whether participants see a higher level of agreement or uncertainty for either Leavers or Remainers than they anticipated would suffer from endogenous right side variables. This is because people's priors are correlated with their political participation. However, we can use the exogenous variation in treatment information to instrument for whether participants see their priors surpassed. Tables 3.C.1 to 3.C.8 in the Appendix depict the estimates of two-stage least squares regressions of the outcomes of interest in this section on dummy variables indicating whether participants see higher than expected levels of agreement as well as uncertainty for (i) both Leavers and Remainers, (ii) members of their own political camp as well as the opposing one, and (iii) Leavers and Remainers separately. These dummy variables are instrumented for with their

respective reduced form counterparts. The tables thus mirror the ones in this section, but depict the local average rather than the average treatment effect.

We find that all the results are unchanged in direction and significance. However, the quantitative magnitude of the effects substantially increases. For example, seeing high levels of uncertainty for both Leavers and Remainers increases participants' willingness to donate to the People's Vote and Better Off Out campaign by 2.8pp in the reduced form. In contrast, seeing *higher* levels of uncertainty than previously expected for both Leavers and Remainers increases participants' willingness to donate to either campaign by 11pp. This constitutes an increase of 30.9% in willingness to donate to the People's Vote Campaign and a 36.5% increase in willingness to donate to Better Off Out compared to participants who saw numbers that were lower than or equal to what they expected. The same is true for all the other results in this section, highlighting that it is the participants who saw their priors changed that drive the outcomes. These findings make intuitive sense and highlight the magnitude of the impact that information on uncertainty can have on those who think most people are secure in their views.

3.4 Mechanisms

There a different channels through which information on other people's views about perceived democratic norms, in this case the appropriateness of a repeat referendum about Brexit, could impact participants' willingness to politically engage and vote. First, it could be that participants update their beliefs about the appropriateness and desirability of a second referendum as well as the desirability and likelihood of leaving the European Union itself, i.e. *social learning*. It could also be that they update their beliefs about the social perception of supporting or opposing a second referendum, the social penalties and benefits assigned to either, and subsequently update what they want to *signal* to their reference group as well as society at large. Finally, it could be that participants update their beliefs about the malleability of other people's beliefs and the effectiveness of political campaigning thereby changing their *strategic considerations*.

3.4.1 Social Learning

Result 5 *Participants update their beliefs about the share of the population agreeing with a second referendum being appropriate as well as supporting it in line with expectations. However, their private beliefs about the appropriateness of the referendum as well as their support for it are unaffected, making social learning an unlikely channel for the treatment.*

The first channel through which information on other people's views on and uncertainty about the appropriateness of a second referendum could affect participants' political participation is social learning. That is, it might lead them to update their beliefs about the existence, strength and merits of the democratic norm of a repeat referendum on leaving the European Union being appropriate.

Beliefs about the appropriateness of a second referendum. The first condition that needs to be fulfilled in order for social learning to be a viable channel is that our treatment has to be effective in shifting participants' beliefs about the degree to which Leavers and Remainers agree with a second referendum being appropriate as well as their associated uncertainty. Hence, we need to check whether participants understand and trust the information they have been provided with.

We proceed as follows: after seeing the treatment, participants are asked to what extent, on a 5-point Likert scale ranging from "Agree" (1) to "Disagree" (5), they agree with the following statements: "A large share of Remain supporters think that having a second referendum is appropriate.", "Some Leave supporters think that having a second referendum is appropriate.", "A large share of Leave supporters think that having a second referendum is appropriate.", 'A large share of Remain supporters are very uncertain about their view on the appropriateness of having a second referendum.", "Some Leave supporters are very uncertain about their view on the appropriateness of having a second referendum.", and "A large share of Leave supporters are very uncertain about their view on the appropriateness of having a

second referendum.".[6]

TABLE 3.4.1: Manipulation Checks: Appropriateness of Repeat Referendum

	Remain Level High	Remain Uncert High	Leave Level Medium	Leave Level High	Leave Uncert Medium	Leave Uncert High
High Level Remain	-0.363***	0.224***	0.062	0.092**	0.060	0.055
	(0.035)	(0.036)	(0.044)	(0.038)	(0.041)	(0.037)
High Uncert Remain	0.069**	-0.285***	-0.078*	-0.019	-0.176***	-0.091**
	(0.035)	(0.036)	(0.044)	(0.038)	(0.041)	(0.037)
High Level Leave	-0.002	-0.010	-0.175***	-0.207***	-0.071*	-0.102***
	(0.035)	(0.036)	(0.044)	(0.038)	(0.041)	(0.037)
High Uncert Leave	0.024	-0.185***	-0.038	-0.082**	-0.231***	-0.466***
	(0.035)	(0.036)	(0.044)	(0.038)	(0.041)	(0.037)
Observations	4072	4072	3270	4072	3270	4072
Control Mean	1.96832	3.289047	2.799694	3.889489	2.874006	3.360756

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on beliefs about shares of the population that agree with the appropriateness and are uncertain. Columns one & two show the impact on participants' agreement with the notion that a large share of Remainers agrees with the appropriateness and is very uncertain. Columns three and four show the same for Leavers. Columns five and six repeat the exercise for Leavers, but now asking about agreement with the statement that "some" Leavers agree with the appropriateness, are very uncertain. The outcomes are categorical variables each coded on a 5-point Likert scale ranging from "Agree" (1) to "Disagree" (5). All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Table 3.4.1 documents in which direction and to what degree participants update their priors after seeing the treatment information. It displays the coefficients on each of the four treatment indicators in a regression of the agreement scores onto four dummy variables indicating seeing high levels of agreement as well as uncertainty among Leavers and Remainers. That is, Specification 3.3 for the full sample. For brevity, we will focus on this specification throughout this section. All split sample regressions for Leavers and Remainers separately can be found in the Appendix. We

[6]For Leavers, we include both a statement mirroring the statement for the Remainers (mentioning "a large share"), as well as a statement mentioning "some" of them agreeing and being uncertain. This is because even in the "high" level treatment arm, it is not a *large* share of Leavers that believes a second referendum would be appropriate (15%).

find that participants indeed understand the treatment information and update their priors in the expected directions for all four pieces of treatment information. Upon seeing information that a relatively higher share of Remainers/Leavers agree with the appropriateness of a second referendum, participants are significantly more likely to agree with these statements in the manipulation checks too (for the Leavers checks, this is true both for the "large share" as well as "some" option). Similarly, participants become significantly more likely to agree with Leavers/Remainers being uncertain if they have been shown relatively higher levels of uncertainty among those groups.

TABLE 3.4.2: Manipulation Checks: Private Assessment of Appropriateness

	Conform With Law	Conform With Custom	Appropriateness	Uncertainty Appr
High Level Remain	-0.044	-0.055	-0.023	-0.010
	(0.036)	(0.036)	(0.035)	(0.035)
High Uncert Remain	-0.041	-0.053	-0.003	0.051
	(0.036)	(0.035)	(0.035)	(0.035)
High Level Leave	-0.028	-0.030	-0.025	0.016
	(0.036)	(0.036)	(0.035)	(0.035)
High Uncert Leave	-0.001	0.004	-0.017	0.049
	(0.036)	(0.036)	(0.035)	(0.035)
Observations	4039	4039	4071	4071
Control Mean	2.819262	3.11488	2.917711	2.086957

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on private beliefs about the appropriateness of another referendum. Columns one to three show the impact on participants' agreement with the notion that another referendum would "conform with UK laws", "conform with UK customs" and "be appropriate". The outcomes are categorical variables each coded on a 5-point Likert scale ranging from "Agree" (1) to "Disagree" (5). Column four shows how uncertain they are about their stance on appropriateness, coded on a Likert scale ranging from "Very certain" (1) to "Very uncertain" (6). All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Table 3.4.2 displays the estimates of a repeat regression with participants' *own* views on the appropriateness of a second referendum and uncertainty thereof on the left hand side. It also displays further information on whether the treatment prompted participants to update their beliefs about a repeat referendum conforming with existing laws as well as custom in the UK. As can be seen in Table 3.4.2, even though participants do update their beliefs about the degree to which others agree with a second referendum being appropriate, this does not change their private beliefs about its appropriateness or legality. It also does not impact how certain they are about their stance on the norm. It thus appears unlikely that the results are driven by social learning about the norm.

TABLE 3.4.3: Manipulation Checks: Support for Repeat Referendum

	Leave Supp	Leave Unc	Remain Supp	Remain Unc	RefGr Supp	RefGr Unc	Own Supp	Own Unc
High Level Remain	-0.842	-0.006	13.226***	-0.231**	1.355*	0.025	-0.002	-0.016
	(0.671)	(0.085)	(0.733)	(0.090)	(0.761)	(0.090)	(0.011)	(0.033)
High Uncert Remain	1.957***	-0.029	-1.608**	0.167*	0.162	0.039	0.002	0.019
	(0.672)	(0.085)	(0.732)	(0.090)	(0.763)	(0.090)	(0.011)	(0.033)
High Level Leave	4.925***	0.029	0.361	-0.085	1.202	0.082	0.015	0.029
	(0.671)	(0.085)	(0.732)	(0.090)	(0.763)	(0.090)	(0.011)	(0.033)
High Uncert Leave	2.125***	-0.032	1.056	0.014	-0.771	0.094	0.005	0.058*
	(0.672)	(0.085)	(0.729)	(0.090)	(0.763)	(0.090)	(0.011)	(0.033)
Observations	4064	4064	4066	4066	4059	4059	4139	4139
Control Mean	25.05463	4.033219	61.45303	4.205607	42.01552	3.918206	.5320126	1.961343

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on beliefs about support for another referendum. Columns one, three & five show the impact on participants' beliefs about the share of Leavers, Remainers and their reference group supporting another referendum. Columns two, four & six show how uncertain they believe each group is, coded on a Likert scale ranging from "Very certain" (1) to "Very uncertain" (6). Columns seven and eight show the impact on participants' private support, coded as a binary variable, and uncertainty, coded as before. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Beliefs about support for a second referendum. Next, we explore whether information on others' agreement with the appropriateness of a second referendum as well as the uncertainty they attach to this view changes participants' beliefs about their *support* for it. Note that individuals' judgement of the appropriateness of a second referendum can be distinct from their support of it. While the former captures the democratic norm, i.e. whether someone believes a second referendum should happen in a democratic system, the latter captures whether they themselves would like it to happen.

To capture beliefs about support, we ask participants about the share of current Leavers and Remainers as well as their family, friends and colleagues that support a second referendum. We also ask them about each group's uncertainty on a 6-point Likert scale ranging from "Very certain" (1) to "Very uncertain" (6). Table 3.4.3 displays the estimates from regressing these outcomes onto the four treatment dummies. We find that all treatments, but in particular the level information shifts perceived support for a repeat referendum significantly. Participants who see the high number for the share of Remainers who agree a second referendum would be appropriate, believe that the share of Remainers supporting it is 13.2pp higher than the 61% estimate of participants who saw only low numbers. Similarly, participants believe that the share of Leavers supporting a second referendum is 4.9pp higher than the 25% base level if they receive the high level treatment for this group. Both constitute an increase of about 20%. Seeing high levels of uncertainty for Leavers/Remainers also increases perceived support of Leavers by 2pp and 2.1pp respectively. Interestingly, while the treatment leads participants to update their beliefs about support for a second referendum in the general population, it appears to have only minimal impact on beliefs about participants' reference group. This is in line with participants having stronger and thus less malleable priors on the views of their reference group. Tables 3.B.3 and 3.B.4 in the Appendix show that this is driven entirely by Remainers learning about other Remainers.

Focusing now on participants' *private* support for a second referendum, Table 3.4.3 also shows that, again, neither information on high levels of agreement nor uncertainty changes participants' private support substantially in the pooled sample.

Only information on Leavers being relatively more uncertain appears to render participants slightly more uncertain about their support themselves. Tables 3.B.3 and 3.B.4 in the Appendix show that this is not driven by either of the subgroups. This is further evidence, that the treatment does not work through social learning.

Beliefs about Brexit. Finally, it could be that participants update their beliefs about the outcome and merits of a second referendum, i.e. the likelihood and consequences of the United Kingdom leaving the European Union. To get at this, we first ask participants about the share of the UK population they believe supported remaining in the European Union at the point of the survey as well the probability that the Remain campaign would win a second referendum. We also ask them what they think the consequences of Brexit would be for the UK economy, society as well as them personally on a 7-point Likert scale ranging from "Very positive" (1) to "Very negative" (7).

TABLE 3.4.4: Beliefs about Brexit

	Share Pro Remain	Prob Remain Win	Personal Cons	Economic Cons	Societal Cons	Soc Cons, Uncert
High Level Remain	1.277***	0.941*	0.043	0.031	0.069*	-0.020
	(0.421)	(0.560)	(0.039)	(0.041)	(0.040)	(0.037)
High Uncert Remain	0.320	-0.250	-0.055	0.005	-0.013	0.032
	(0.422)	(0.558)	(0.039)	(0.041)	(0.040)	(0.037)
High Level Leave	0.693	0.952*	0.062	0.014	0.052	0.019
	(0.422)	(0.559)	(0.039)	(0.041)	(0.040)	(0.037)
High Uncert Leave	0.332	0.799	-0.053	0.004	0.002	0.107***
	(0.420)	(0.558)	(0.039)	(0.041)	(0.040)	(0.037)
Observations	4068	4039	4037	4037	4037	4037
Control Mean	50.11013	51.09854	4.465445	4.597969	4.443399	2.747833

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on beliefs about another referendum as well as Brexit. Columns one and two show the impact on participants' beliefs about the share of the population currently supporting Remain and likelihood of Remain winning a repeat referendum. Columns three to four show their assessment of the consequences of Brexit for the UK economy, them personally as well as society coded on a 7-point Likert scale ranging from "Very positive" (1) to "Very negative" (7). Column six shows how uncertain they are about their stance, coded on a 5-point Likert scale ranging from "Very Uncertain" (1) to "Very Certain" (5). All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Table 3.4.4 depicts the results of a regression of these outcomes onto the four treatment variables. We find that participants who see high levels of agreement with the notion that a second referendum would be appropriate believe that a larger fraction of the UK population wanted to remain in the European Union at the time of the survey and that the Remain campaign would have been more likely to win a repeat referendum. With regard to the consequences of Brexit, seeing high levels of agreement from Remainers makes participants slightly more pessimistic about the consequences of Brexit for UK society, while seeing high levels of uncertainty from Leavers makes them more uncertain about societal consequences. Tables 3.B.5 and 3.B.6 in the Appendix show that the first result is driven by Remainers while the

latter is driven by Leavers. They also show that Leavers in particular become more sceptical about the consequences of Brexit on their own life if they see other Leavers agreeing more with the appropriateness of a second referendum. And that Remainers think that the consequences of Brexit on all three dimensions would be more positive if they see high levels of uncertainty from other Remainers. However, in general the treatment information does not appear to alter participants' judgement of the consequences of Brexit substantially.

On a related note, participants might update their own intentions to vote in a hypothetical second referendum if the treatments changed their beliefs about the consequences of Brexit. Table 3.4.5 depicts the impact of the four treatment variables on participants stating that they would vote to remain or leave the European Union in another referendum on Brexit as well as abstain from such a vote. It also shows the impact on them stating that they would accept the results of a second vote. We see that intentions are not drastically changed, but that participants become 1.8pp more likely to plan to vote Leave if they learn that a relatively high share of Remainers are uncertain about the appropriateness of a second referendum. At the same time, they become 1.7pp less likely to want to vote Leave if they see relatively high levels of agreement among Leavers with the appropriateness of the referendum. Tables 3.B.7 and 3.B.8 in the Appendix show that the first result is driven by Leavers while the second is driven by Remainer – in line with expectations.

TABLE 3.4.5: Plans for a Potential Repeat Referendum

	Vote Remain	Vote Leave	Abstain	Accept Result
High Level Remain	-0.010	0.010	-0.001	-0.009
	(0.007)	(0.008)	(0.008)	(0.013)
High Uncert Remain	-0.009	0.018**	-0.009	-0.010
	(0.007)	(0.008)	(0.008)	(0.013)
High Level Leave	0.007	-0.017**	0.010	0.002
	(0.007)	(0.008)	(0.008)	(0.013)
High Uncert Leave	0.005	-0.001	-0.004	-0.017
	(0.007)	(0.008)	(0.008)	(0.013)
Observations	4039	4039	4039	4039
Control Mean	.5337955	.3946521	.0715524	.7172567

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on intentions for a potential repeat referendum. Columns one and two show the impact on intention to vote Remain or Leave in such a referendum respectively. Column three shows intention to abstain, and column four shows likelihood to acccept the result. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Overall it appears that the treatments did shift participants' beliefs about the degree to which the public agreed with the appropriateness of a second referendum as well as it's support for it. They also moved participants' beliefs about the share of the UK population supporting Brexit as well as the likelihood of the remain campaign winning a second referendum. However, this did not shift their private beliefs about either the appropriateness of a second referendum or their support for it. It also did not substantially alter their beliefs about the consequences of Brexit, the subject of the

referendum. Thus, it does not appear that social learning is the predominant channel through which the information in the experiment affected political participation.

3.4.2 Signalling

Result 6 *Learning that a larger fraction of Leavers (Remainers) are uncertain about the appropriateness of a repeat referendum leads participants to believe that they perceive public support (opposition) of another referendum less negatively and public opposition (support) to it as less positively.*

The next channel through which the treatment information could impact participants' willingness to become politically engaged is signalling considerations. As shown in the previous section, the information provided does shift beliefs about the support that both the appropriateness of a second referendum as well as the referendum itself enjoy in British society. Thus, it could also shift how participants believe the public views people who either support or oppose another referendum. Furthermore, learning about the uncertainty others feel about their views might shift how much participants believe they will be penalised if they divert from the norm in society and/or their reference group.

To explore the degree to which social considerations drive the main results, we ask participants how (i) the general UK population, (ii) their family, friends & colleagues, (iii) current Remainers; as well as (iv) current Leavers view people who either publicly support of oppose a second referendum. Participants can indicate their assessment on a 5-point Likert scale ranging from "Negatively" (1) to "Positively" (5). The first four columns of Table 3.4.6 show the results for the perception of supporters while the latter four columns show the results for public opposition. We see that the level information affects beliefs about perception in the expected directions. Seeing a relatively high level of agreement among Remainers and/or Leavers with the notion that a second referendum would be appropriate leads participants to believe that the general UK population, their reference group and Remainers would perceive people who publicly campaign for a referendum more positively and those who oppose it more negatively. Interestingly, the effects reverse in sign when participants see high

levels of agreement from Remainers and are asked about the perception of Leavers, consistent with backlash effects.

TABLE 3.4.6: Beliefs about Perception of Supporters & Opponents of a Referendum

	Public Support				Public Opposition			
	Population	Ref Group	Remainers	Leavers	Population	Ref Group	Remainers	Leavers
High Level Remain	0.060*	0.048	0.181***	-0.067*	-0.029	-0.087***	-0.142***	0.124***
	(0.031)	(0.030)	(0.032)	(0.034)	(0.031)	(0.032)	(0.035)	(0.037)
High Uncert Remain	0.046	0.051*	-0.073**	0.043	0.030	0.012	0.067*	-0.004
	(0.031)	(0.030)	(0.033)	(0.034)	(0.031)	(0.032)	(0.035)	(0.038)
High Level Leave	0.044	0.038	0.010	0.096***	0.011	0.005	0.026	-0.050
	(0.031)	(0.030)	(0.033)	(0.034)	(0.031)	(0.032)	(0.035)	(0.037)
High Uncert Leave	0.038	-0.015	-0.043	0.068**	-0.037	-0.017	0.033	-0.124***
	(0.031)	(0.030)	(0.033)	(0.034)	(0.031)	(0.032)	(0.035)	(0.037)
Observations	4047	4047	4047	4047	4047	4047	4047	4047
Control Mean	2.968125	3.136644	3.800593	2.26044	3.115394	3.081295	2.529528	3.550284

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on beliefs about the perception of public support for/ opposition to it. Columns one to four show the impact on participants' beliefs on how the general population, their reference group, Remainers and Leavers view supporters of another referendum. Columns five to eight do the same for opponents of another referendum. The outcome is a categorical variable indicating whether participants think a specific group views opposition "Negatively" (1) or "Positively" (5) on a 5-point Likert scale. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Crucially, receiving information about Leavers and/or Remainers being more uncertain does appear to weaken both the social penalties and benefits associated with going against the more popular norm in either group. Seeing high levels of uncertainty among Remainers leads participants to think that Remainers perceive supporters of a second referendum more negatively and opponents of it more positively. On the other hand, seeing high levels of uncertainty among Leavers results in participants thinking that Leavers see supporters of another referendum more positively while judging

opponents more negatively. Tables 3.B.9 and 3.B.10 in the Appendix document that these patterns hold for the subsamples of Leavers and Remainers although the results are particularly pronounced for Remainers.

These results are in line with Remain leaning participants becoming more likely to campaign for the Better Off Out campaign when they learn that other Remainers are more uncertain, as well as Leave leaning participants becoming more likely to donate to and tweet about the People's Vote campaign after learning that other Leavers are relatively more uncertain. Participants who support Brexit but believe that a second *referendum* should be held to give everyone an informed say might take the information on other Leavers being uncertain about their view as a sign that they would face a lower social penalty from their peers when voicing their support for a "People's Vote". Similarly, participants who oppose Brexit but believe that a second referendum would violate democratic norms by questioning a public vote might be more willing to campaign for "Better Off Out" once they learn that other Remainers might not judge them as harshly as they previously may have thought for doing so. Thus, the results provide evidence that the treatment works at least in part through shifting participants' signalling concerns.

3.4.3 Strategic Considerations

Result 7 *Upon learning that a higher fraction of Leavers and Remainers are uncertain about their views, respondents believe that both public support for as well as opposition to a repeat referendum would be more effective in convincing others to join a respective cause.*

Finally, information about people's level of agreement with a repeat referendum being appropriate as well as their uncertainty about this take could change how malleable participants believe other people's views to be. If that is the case, they might become more or less inclined to become politically involved with the goal of convincing others. To test for this channel, we ask participants directly whether they believe publicly supporting or opposing a second referendum would be effective in convincing others to join them. They can express their (dis)agreement on a 5-point Likert scale ranging from "Agree" (1) to "Disagree" (5). Table 3.4.7 displays the results of a regression

of these two outcomes on the four treatment variables. We find that seeing higher levels of uncertainty, either for Leavers or Remainers, renders participants significantly more likely to agree with the notion that both public support or opposition of a second referendum would be an effective tool to convince others. Thus, it appears that information about other people being relatively more uncertain does lead people to believe that their views and the democratic norm are more malleable. Tables 3.B.11 and 3.B.12 in the Appendix show that these results hold for both Leavers and Remainers but are particularly pronounced for the former group.

TABLE 3.4.7: Beliefs about Effectiveness

	Public Support Effective Tool	Public Opposition Effective Tool
High Level Remain	-0.022	-0.010
	(0.034)	(0.033)
High Uncert Remain	-0.095***	-0.079**
	(0.034)	(0.033)
High Level Leave	-0.070**	-0.008
	(0.034)	(0.033)
High Uncert Leave	-0.109***	-0.096***
	(0.034)	(0.033)
Observations	4041	4041
Control Mean	3.002722	3.000495

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on beliefs about the effectiveness of public campaigning. Columns one shows the impact on participants' beliefs about public support and column two about public opposition. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

It is interesting to find a strong effect of uncertainty on the perceived effectiveness of campaigning here, given that in Section 3.3.1 we do not find that participants

become more likely to campaign for the norm that is more popular among people who share their preference over outcomes if they learn that others are uncertain. One reason could be that participants whose beliefs on the appropriateness of a second referendum align with their preference over the outcome as well as the stance of others who share that preference are generally more vocal about their stance. At the same time, they may be less inclined to tweet about or donate to campaigns beyond their ongoing efforts. In contrast, people whose beliefs on the appropriateness of a second referendum diverge from their personal desire to see one, may be more likely to have previously held off on campaigning for fear of punishment by their reference group (see preceding section). Coming from a lower baseline, this group might be more likely to react to the information treatments, with effectiveness considerations further motivating them to campaign for the non-obvious outcome.

Taken together the results in this section suggest that the treatment effects are driven by a shift in signalling as well as strategic considerations. Social learning does not appear to be important.

3.5 Discussion

We find that individuals who learn that others in their own political camp are relatively more uncertain about the validity of a political norm become more willing to campaign for the interpretation that is less popular amongst people who share their preference over outcomes. This appears driven not by social learning about the merits or flaws of the norm, but rather reduced stigma expected from going against the norm interpretation of one's own group. Furthermore, strategic considerations appear to play into it as well with people trying to convince others of their point of view.

The combination of results we see suggests that our findings are driven by people who are divided in their beliefs about the political outcome they desire and the norm that should be the guiding principle leading to it. That is, people who would like the UK to leave the European Union but believe that still a repeat referendum would be appropriate, and people who would like the UK to remain in the European Union but believe that having another referendum would be inappropriate. People in the

former group could be motivated by the amount of new information that emerged about the conditions and consequences of Brexit since 2016, while people in the former group might argue that it is a democratic principle not to question the outcomes of a legitimate vote. Learning that fellow Leavers or Remainers are relatively uncertain about their stance could make people in this situation believe that they will face fewer negative repercussions for supporting a referendum position that clashes with the one endorsed by a majority of people who share one's views on the ideal *outcome* of such a referendum. It also increases the perceived returns to campaigning as public statements are perceived as more effective in convincing peers.

The public divide in the UK about the validity of a repeat referendum, a norm, alongside the merits of leaving or remaining in the European Union, an outcome, is not a unique political situation. Instead, there is an increasing number of examples, across countries, where political ends are discussed alongside political means. In this context, partisanship becomes increasingly important in generating public support for either. For example, there is an ongoing debate in the UK about the scope of acceptable protests in a democracy, that is closely linked to endeavours of climate activists such as the "Last Generation" and thus people's views on the urgency of their motivation, i.e. a push for more climate protection. Similarly, in the US, publicly expressed opinions on whether or not there should be term limits for Supreme Court justices or an abolition of the Filibuster in the Senate appear closely linked to individuals' political affiliation and beliefs about the courts' (recent) rulings as well as the Senate's legislative track record.

Political coverage in all of these situations tends to report the aggregate levels of support that certain norms (or reform proposals) have in the general population as well as subgroups – but not how certain people are about their views. For a majority of people, views on certain norms as well as political outcomes will somewhat overlap insofar as they derive from an overarching worldview. This in turn can make it seem as though support for norms follows partisan divides – especially if people's certainty about their views are not reported. In such a situation, people may feel that if they support or oppose a certain outcome, they should also support or oppose certain norms, furthering polarisation. Yet, in reality, many people will hold complex views that do not perfectly align on multiple dimensions. Supporters of climate

protection legislation may think that certain protest measures such as widespread traffic disruption breach the limits of acceptable protest. On the other hand believers in a non-interventionary state may be opposed both to climate protection legislation and law enforcement moving people that are non-violently protesting. It is in the interest of stable democracies to encourage people with such "split allegiances" to voice their political views. This is because even though public debate about political outcomes is necessary in a democracy, so is a certain level of agreement about the norms regulating this debate.

Our results imply that political coverage on public opinion polls could help decrease some of the polarisation in support for or opposition to certain political norms by reporting the uncertainty people attach to their views along with their level of (dis-)agreement.

3.6 Conclusion

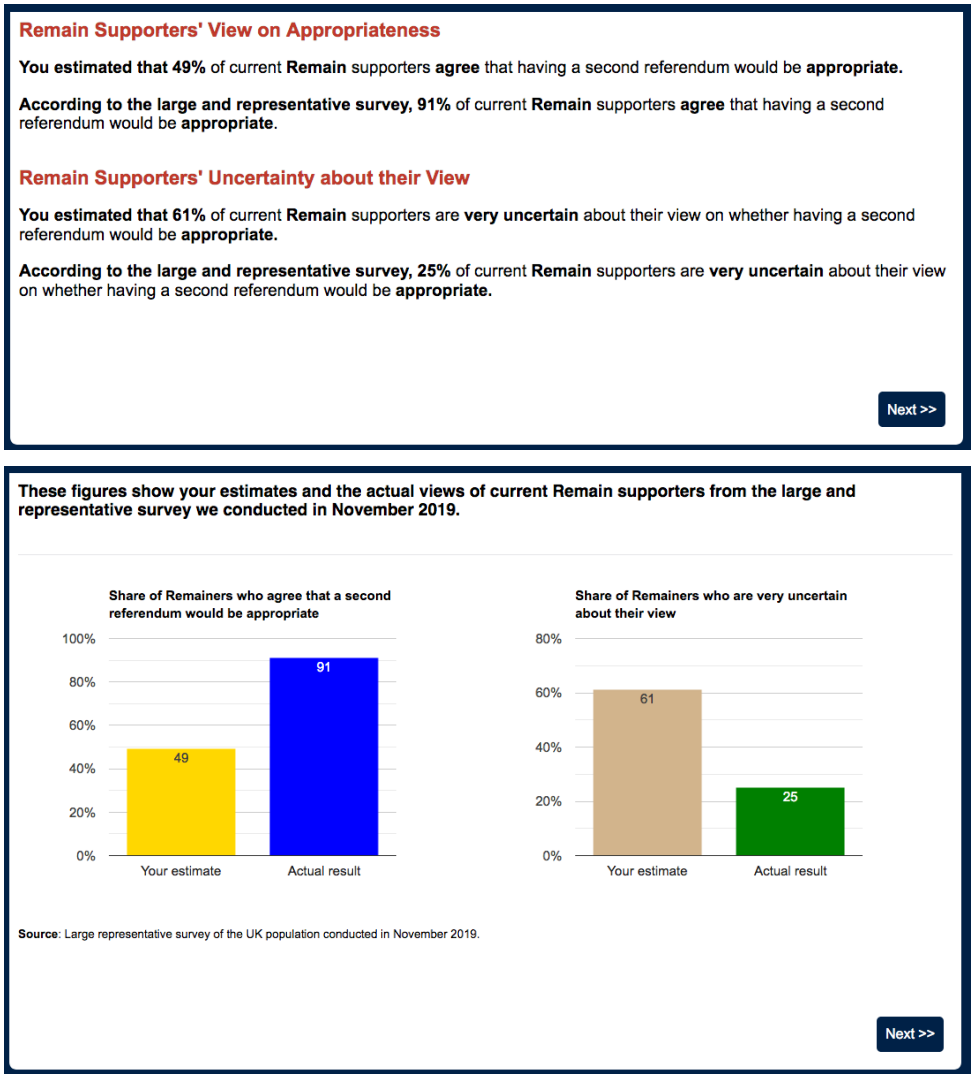
In this paper, we explore how information about the degree to which other people agree with a specific norm as well as the certainty they attach to this assessment shifts individuals' willingness to campaign for or against it in a situation where the rules of the game are ambiguous. We focus on the debate about a repeat referendum on Brexit in the weeks leading up to the UK's general election in 2019. At this point, the public was divided not only in their views on Brexit but also the legitimacy of having a repeat referendum with both supporters and opponents of such a vote citing democratic norms as the main reason. We run an online experiment with 4000 participants who we randomly show different pieces of information about the fractions of Leave and Remain supporters who agree with the appropriateness of a second referendum as well as are "very uncertain" about their view. We find that participants who see that a high fraction of both Leave and Remain supporters are uncertain about their view become on average 10% more likely to donate to both the People's Vote campaign which lobbies for a repeat referendum and the Better Off Out campaign that tries to prevent it. This is driven by Remainers becoming more likely to donate to the Better Off Out campaign if they learn that other Remainers are uncertain, and Leavers becoming more likely to donate to the People's Vote campaign if they learn that other Leavers

are uncertain. Leave supporters also become more likely to tweet their support for a repeat referendum if their learn that other Leavers are uncertain. Participants do not update their private beliefs about the legitimacy or merits of another referendum, but they do believe that Leavers and Remainers judge people who campaign for or against a second referendum respectively less harshly if they see that a large fraction of either group is uncertain. They also believe that campaigning is more effective in convincing others in this case. Thus, the results appear driven not by social learning but instead social image consideration and perceived returns to campaigning. Our results provide experimental evidence that public information campaigns or political coverage that highlight not only the level of support or opposition that a given norm commands among different groups, but also the uncertainty people attach to it, might reduce polarisation in public opinion. Going forward, it is important to examine how such interventions fare outside of an experimental setting and how they can affect not only willingness to donate to or tweet about a campaign but volunteering or running for office as well.

Appendix

3.A Additional Figures

FIGURE 3.A.1: Example Treatment Screen



3.B Additional Reduced Form Tables

TABLE 3.B.1: Plans to Vote, Leavers

	Conservatives	Liberal Dem	Labour Party	Brexit Party	Abstaining
High Level Remain	-0.002	-0.007	-0.010	0.017	0.001
	(0.022)	(0.007)	(0.015)	(0.014)	(0.011)
High Uncert Remain	0.000	0.001	0.020	-0.037**	0.008
	(0.022)	(0.007)	(0.015)	(0.014)	(0.011)
High Level Leave	-0.019	0.007	0.004	-0.010	0.011
	(0.022)	(0.007)	(0.015)	(0.014)	(0.011)
High Uncert Leave	-0.019	-0.008	0.013	-0.014	0.008
	(0.022)	(0.007)	(0.015)	(0.014)	(0.011)
Observations	1922	1922	1922	1922	1922
Control Mean	.5015609	.0234131	.1316337	.116025	.0686785

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on the voting intentions of Leave supporters. Columns one to four show the impact on participants' intent to vote for the Conservatives, Liberal Democrats, Labour and the Brexit Party in the 2019 election respectively. Column five shows the impact on their intent to abstain from the election. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.2: Plans to Vote, Remainers

	Conservatives	Liberal Dem	Labour Party	Brexit Party	Abstaining
High Level Remain	0.016	0.010	-0.027	-0.006**	0.008
	(0.010)	(0.016)	(0.020)	(0.003)	(0.008)
High Uncert Remain	0.016	0.004	-0.023	0.002	0.002
	(0.010)	(0.016)	(0.020)	(0.003)	(0.008)
High Level Leave	0.003	-0.038**	0.016	0.002	0.014*
	(0.010)	(0.016)	(0.020)	(0.003)	(0.008)
High Uncert Leave	0.000	-0.029*	0.001	-0.000	0.010
	(0.010)	(0.016)	(0.020)	(0.003)	(0.008)
Observations	2333	2333	2333	2333	2333
Control Mean	.0672953	.1980283	.4809258	.0060009	.0488641

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on the voting intentions of Remain supporters. Columns one to four show the impact on participants' intent to vote for the Conservatives, Liberal Democrats, Labour and the Brexit Party in the 2019 election respectively. Column five shows the impact on their intent to abstain from the election. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.3: Manipulation Checks: Support for 2nd Referendum, Leavers

	Leave Supp	Leave Unc	Remain Supp	Remain Unc	RefGr Supp	RefGr Unc	Own Supp	Own Unc
High Level Remain	-1.285	-0.072	12.855***	-0.186	-0.045	-0.081	-0.006	0.094*
	(1.052)	(0.131)	(1.167)	(0.131)	(1.017)	(0.132)	(0.015)	(0.049)
High Uncert Remain	1.177	0.118	-0.349	0.242*	1.649	0.143	0.003	-0.012
	(1.058)	(0.132)	(1.166)	(0.131)	(1.025)	(0.133)	(0.015)	(0.049)
High Level Leave	5.257***	0.028	0.537	0.073	2.502**	0.137	0.031**	0.031
	(1.060)	(0.131)	(1.174)	(0.131)	(1.028)	(0.133)	(0.015)	(0.049)
High Uncert Leave	2.698**	-0.193	0.169	0.028	-0.159	0.220*	0.014	0.046
	(1.061)	(0.132)	(1.170)	(0.131)	(1.021)	(0.133)	(0.015)	(0.050)
Observations	1826	1826	1826	1826	1824	1824	1871	1871
Control Mean	25.15225	3.885542	59.20099	4.205367	23.68366	3.774671	.1437734	1.81721

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Leave supporters' beliefs about support for another referendum. Columns one, three & five show the impact on participants' beliefs about the share of Leavers, Remainers and their reference group supporting another referendum. Columns two, four & six show how uncertain they believe each group is, coded on a Likert scale ranging from "Very certain" (1) to "Very uncertain" (6). Columns seven and eight show the impact on participants' private support, coded as a binary variable, and uncertainty, coded as before. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.4: Manipulation Checks: Support for 2nd Referendum, Remainers

	Leave Supp	Leave Unc	Remain Supp	Remain Unc	RefGr Supp	RefGr Unc	Own Supp	Own Unc
High Level Remain	-0.588	0.051	13.716***	-0.276**	2.808***	0.116	0.007	-0.111**
	(0.859)	(0.111)	(0.927)	(0.124)	(1.081)	(0.122)	(0.014)	(0.044)
High Uncert Remain	2.318***	-0.137	-2.502***	0.102	-1.467	-0.063	-0.003	0.041
	(0.862)	(0.111)	(0.926)	(0.124)	(1.083)	(0.122)	(0.015)	(0.044)
High Level Leave	4.593***	0.025	0.167	-0.226*	-0.289	0.027	-0.004	0.026
	(0.858)	(0.111)	(0.923)	(0.124)	(1.084)	(0.121)	(0.014)	(0.044)
High Uncert Leave	1.637*	0.098	1.709*	-0.007	-1.546	-0.007	-0.004	0.070
	(0.857)	(0.111)	(0.918)	(0.124)	(1.083)	(0.121)	(0.014)	(0.044)
Observations	2238	2238	2240	2240	2235	2235	2268	2268
Control Mean	24.97498	4.153709	63.28884	4.205804	56.97629	4.035347	.8522928	2.080247

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Remain supporters' beliefs about support for another referendum. Columns one, three & five show the impact on participants' beliefs about the share of Leavers, Remainers and their reference group supporting another referendum. Columns two, four & six show how uncertain they believe each group is, coded on a Likert scale ranging from "Very certain" (1) to "Very uncertain" (6). Columns seven and eight show the impact on participants' private support, coded as a binary variable, and uncertainty, coded as before. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.5: Beliefs about Brexit, Leavers

	Share Pro Remain	Prob Remain Win	Personal Cons	Economic Cons	Societal Cons	Soc Cons, Uncert
High Level Remain	0.724	-0.129	0.044	0.072	0.078	0.019
	(0.645)	(0.915)	(0.055)	(0.063)	(0.060)	(0.050)
High Uncert Remain	1.382**	0.174	-0.058	0.095	0.089	0.052
	(0.648)	(0.925)	(0.056)	(0.063)	(0.060)	(0.050)
High Level Leave	1.408**	2.297**	0.103*	0.047	0.061	-0.032
	(0.653)	(0.927)	(0.056)	(0.063)	(0.060)	(0.051)
High Uncert Leave	-0.159	0.542	0.070	0.050	0.104*	0.154***
	(0.647)	(0.922)	(0.055)	(0.063)	(0.060)	(0.050)
Observations	1828	1811	1811	1811	1811	1811
Control Mean	44.55525	42.08283	3.31143	3.09111	2.912203	2.735505

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Leave supporters' beliefs about another referendum as well as Brexit. Columns one and two show the impact on participants' beliefs about the share of the population currently supporting Remain and likelihood of Remain winning a repeat referendum. Columns three to four show their assessment of the consequences of Brexit for the UK economy, them personally as well as society coded on a 7-point Likert scale ranging from "Very positive" (1) to "Very negative" (7). Column six shows how uncertain they are about their stance, coded on a 5-point Likert scale ranging from "Very Uncertain" (1) to "Very Certain" (5). All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.6: Beliefs about Brexit, Remainers

	Share Pro Remain	Prob Remain Win	Personal Cons	Economic Cons	Societal Cons	Soc Cons, Uncert
High Level Remain	1.790***	1.929***	0.082	0.033	0.099**	-0.064
	(0.548)	(0.677)	(0.051)	(0.050)	(0.050)	(0.053)
High Uncert Remain	-0.713	-0.770	-0.068	-0.094*	-0.112**	0.006
	(0.553)	(0.671)	(0.051)	(0.050)	(0.050)	(0.053)
High Level Leave	0.048	-0.270	0.011	-0.036	0.020	0.060
	(0.545)	(0.673)	(0.051)	(0.050)	(0.051)	(0.054)
High Uncert Leave	0.641	0.976	-0.149***	-0.029	-0.079	0.079
	(0.545)	(0.673)	(0.051)	(0.050)	(0.050)	(0.053)
Observations	2240	2228	2226	2226	2226	2226
Control Mean	54.6433	58.42684	5.404313	5.823899	5.689128	2.757862

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Remain supporters' beliefs about another referendum as well as Brexit. Columns one and two show the impact on participants' beliefs about the share of the population currently supporting Remain and likelihood of Remain winning a repeat referendum. Columns three to four show their assessment of the consequences of Brexit for the UK economy, them personally as well as society coded on a 7-point Likert scale ranging from "Very positive" (1) to "Very negative" (7). Column six shows how uncertain they are about their stance, coded on a 5-point Likert scale ranging from "Very Uncertain" (1) to "Very Certain" (5). All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.7: Plans for a Potential Repeat Referendum, Leavers

	Vote Remain	Vote Leave	Abstain	Accept Result
High Level Remain	-0.017*	0.019	-0.002	0.001
	(0.010)	(0.016)	(0.013)	(0.023)
High Uncert Remain	-0.023**	0.045***	-0.023*	-0.005
	(0.010)	(0.016)	(0.013)	(0.023)
High Level Leave	0.011	-0.016	0.004	0.024
	(0.010)	(0.016)	(0.013)	(0.023)
High Uncert Leave	0.012	-0.007	-0.006	-0.017
	(0.010)	(0.016)	(0.013)	(0.023)
Observations	1811	1811	1811	1811
Control Mean	.0546659	.8514633	.0938708	.5146328

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Leave supporters' intentions for a potential repeat referendum. Columns one and two show the impact on intention to vote Remain or Leave in such a referendum respectively. Column three shows intention to abstain, and column four shows likelihood to acccept the result. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.8: Plans for a Potential Repeat Referendum, Remainers

	Vote Remain	Vote Leave	Abstain	Accept Result
High Level Remain	0.001	-0.002	0.001	-0.013
	(0.011)	(0.006)	(0.009)	(0.014)
High Uncert Remain	-0.003	0.002	0.001	-0.019
	(0.011)	(0.006)	(0.009)	(0.014)
High Level Leave	0.002	-0.017***	0.014	-0.019
	(0.010)	(0.006)	(0.009)	(0.014)
High Uncert Leave	-0.000	0.002	-0.002	-0.014
	(0.011)	(0.006)	(0.009)	(0.013)
Observations	2228	2228	2228	2228
Control Mean	.9232496	.0233393	.0534111	.8819569

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Remain supporters' intentions for a potential repeat referendum. Columns one and two show the impact on intention to vote Remain or Leave in such a referendum respectively. Column three shows intention to abstain, and column four shows likelihood to acccept the result. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.9: Beliefs about Perception of Supporters & Opponents of a Referendum, Leavers

	Public Support				Public Opposition			
	Population	Ref Group	Remainers	Leavers	Population	Ref Group	Remainers	Leavers
High Level Remain	0.024	0.021	0.200***	-0.068	-0.079	-0.082*	-0.185***	0.084
	(0.047)	(0.047)	(0.054)	(0.051)	(0.048)	(0.050)	(0.056)	(0.058)
High Uncert Remain	0.090*	0.098**	-0.049	0.046	0.024	0.006	0.028	0.064
	(0.046)	(0.047)	(0.054)	(0.051)	(0.049)	(0.050)	(0.056)	(0.058)
High Level Leave	0.090*	0.119**	0.092*	0.129**	-0.010	-0.013	0.062	-0.110*
	(0.047)	(0.047)	(0.054)	(0.051)	(0.049)	(0.050)	(0.056)	(0.058)
High Uncert Leave	0.032	0.012	-0.056	0.037	-0.036	-0.035	0.026	-0.166***
	(0.047)	(0.047)	(0.054)	(0.051)	(0.048)	(0.050)	(0.057)	(0.058)
Observations	1818	1818	1818	1818	1818	1818	1818	1818
Control Mean	2.630363	2.473597	3.552805	2.193069	3.29868	3.552805	2.60396	3.59516

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Leave supporters' beliefs about the perception of public support for/opposition to it. Columns one to four show the impact on participants' beliefs on how the general population, their reference group, Remainers and Leavers view supporters of another referendum. Columns five to eight do the same for opponents of another referendum. The outcome is a categorical variable indicating whether participants think a specific group views opposition "Negatively" (1) or "Positively" (5) on a 5-point Likert scale. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.10: Beliefs about Perception of Supporters & Opponents of a Referendum, Remainers

	Public Support				Public Opposition			
	Population	Ref Group	Remainers	Leavers	Population	Ref Group	Remainers	Leavers
High Level Remain	0.090**	0.079**	0.174***	-0.073	0.005	-0.103**	-0.115**	0.163***
	(0.041)	(0.038)	(0.039)	(0.046)	(0.039)	(0.041)	(0.045)	(0.049)
High Uncert Remain	-0.002	-0.009	-0.094**	0.022	0.042	0.029	0.090**	-0.044
	(0.042)	(0.038)	(0.039)	(0.046)	(0.039)	(0.041)	(0.044)	(0.049)
High Level Leave	-0.000	-0.040	-0.060	0.075	0.034	0.031	0.007	-0.006
	(0.042)	(0.038)	(0.039)	(0.046)	(0.039)	(0.041)	(0.045)	(0.049)
High Uncert Leave	0.042	-0.043	-0.037	0.098**	-0.039	-0.003	0.040	-0.095*
	(0.042)	(0.038)	(0.039)	(0.046)	(0.039)	(0.041)	(0.044)	(0.049)
Observations	2229	2229	2229	2229	2229	2229	2229	2229
Control Mean	3.243607	3.677434	4.002692	2.315388	2.965904	2.696725	2.46882	3.513683

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Leave supporters' beliefs about the perception of public support for/opposition to it. Columns one to four show the impact on participants' beliefs on how the general population, their reference group, Remainers and Leavers view supporters of another referendum. Columns five to eight do the same for opponents of another referendum. The outcome is a categorical variable indicating whether participants think a specific group views opposition "Negatively" (1) or "Positively" (5) on a 5-point Likert scale. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.11: Beliefs about Effectiveness, Leavers

	Public Support Effective Tool	Public Opposition Effective Tool
High Level Remain	-0.012	-0.000
	(0.053)	(0.052)
High Uncert Remain	-0.111**	-0.043
	(0.053)	(0.052)
High Level Leave	-0.161***	-0.012
	(0.053)	(0.052)
High Uncert Leave	-0.154***	-0.123**
	(0.054)	(0.052)
Observations	1813	1813
Control Mean	3.381688	2.972421

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Leave supporters' beliefs about the effectiveness of public campaigning. Columns one shows the impact on participants' beliefs about public support and column two about public opposition. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.B.12: Beliefs about Effectiveness, Remainers

	Public Support Effective Tool	Public Opposition Effective Tool
High Level Remain	-0.033	-0.015
	(0.044)	(0.042)
High Uncert Remain	-0.061	-0.109***
	(0.044)	(0.042)
High Level Leave	0.016	-0.001
	(0.044)	(0.042)
High Uncert Leave	-0.064	-0.070*
	(0.044)	(0.041)
Observations	2228	2228
Control Mean	2.694345	3.023339

Notes: This table shows the effects of learning that a high fraction of Leavers/Remainers agrees with the appropriateness of a repeat referendum and is uncertain about this stance on Remain supporters' beliefs about the effectiveness of public campaigning. Columns one shows the impact on participants' beliefs about public support and column two about public opposition. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by OLS. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

3.C LATE Tables

TABLE 3.C.1: Campaigning: Pooled

	Donate Pro Ref	Donate Anti Ref	Tweet Pro Ref	Tweet Anti Ref
Higher Level	0.057	0.038	0.006	-0.027
	(0.063)	(0.060)	(0.048)	(0.037)
Higher Uncert	0.110*	0.110*	-0.012	0.009
	(0.062)	(0.059)	(0.049)	(0.041)
Observations	4116	4118	1958	1958
Control Mean	.3554422	.3016027	.0490642	.0343955

Notes: This table shows the effects of learning that a higher fraction of both Leavers and Remainers than previously thought agrees with the appropriateness of a repeat referendum and is uncertain about this stance on willingness to campaign. Columns one and two show the impact on participants' willingness to donate to the People's Vote and Better Off Out campaigns. Columns three and four shows the impact on their willingness to tweet their support for or opposition to a repeat referendum. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by 2SLS using treatment assignment to high numbers as instruments. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.C.2: Campaigning: Pooled, Own/Other

	Donate Pro Ref	Donate Anti Ref	Tweet Pro Ref	Tweet Anti Ref
Higher Level Own	0.008	0.017	0.004	-0.011
	(0.027)	(0.025)	(0.018)	(0.015)
Higher Uncert Own	0.067*	0.067*	0.047*	-0.000
	(0.039)	(0.036)	(0.028)	(0.023)
Higher Level Other	0.018	-0.006	-0.002	0.020
	(0.034)	(0.031)	(0.025)	(0.020)
Higher Uncert Other	0.061	0.025	-0.034	0.011
	(0.041)	(0.038)	(0.029)	(0.024)
Observations	4116	4118	1977	1977
Control Mean	.3554422	.3016027	.0490642	.0343955

Notes: This table shows the effects of learning that a higher fraction of people in one's own and the opposed political camp than previously thought agrees with the appropriateness of a repeat referendum and is uncertain about this stance on willingness to campaign. Columns one and two show the impact on participants' willingness to donate to the People's Vote and Better Off Out campaigns. Columns three and four shows the impact on their willingness to tweet their support for or opposition to a repeat referendum. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by 2SLS using treatment assignment to high numbers as instruments. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.C.3: Campaigning: Leavers

	Donate Pro Ref	Donate Anti Ref	Tweet Pro Ref	Tweet Anti Ref
Higher Level Remain	0.002	-0.034	0.022	0.038
	(0.031)	(0.041)	(0.017)	(0.036)
Higher Uncert Remain	0.036	-0.053	0.018	0.037
	(0.050)	(0.067)	(0.029)	(0.058)
Higher Level Leave	0.064	0.045	0.025	-0.068
	(0.061)	(0.080)	(0.037)	(0.071)
Higher Uncert Leave	0.115**	0.075	0.082**	-0.005
	(0.057)	(0.076)	(0.037)	(0.071)
Observations	1857	1860	752	752
Control Mean	.1513193	.5204301	.0171504	.0870712

Notes: This table shows the effects of learning that a higher fraction of Leavers/Remainers than previously thought agrees with the appropriateness of a repeat referendum and is uncertain about this stance on willingness to campaign of Leave supporters. Columns one and two show the impact on participants' willingness to donate to the People's Vote and Better Off Out campaigns. Columns three and four shows the impact on their willingness to tweet their support for or opposition to a repeat referendum. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by 2SLS using treatment assignment to high numbers as instruments. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** $p<0.01$,** $p<0.05$,* $p<0.1$.

TABLE 3.C.4: Campaigning: Remainers

	Donate Pro Ref	Donate Anti Ref	Tweet Pro Ref	Tweet Anti Ref
Higher Level Remain	-0.010	0.004	0.000	-0.004
	(0.029)	(0.020)	(0.022)	(0.003)
Higher Uncert Remain	0.026	0.076**	0.023	0.000
	(0.052)	(0.035)	(0.038)	(0.005)
Higher Level Leave	0.034	0.038	-0.055	0.001
	(0.071)	(0.048)	(0.053)	(0.010)
Higher Uncert Leave	0.087	0.087**	-0.065	-0.001
	(0.062)	(0.042)	(0.045)	(0.007)
Observations	2259	2258	1206	1206
Control Mean	.5232404	.1213463	.0689089	.0016407

Notes: This table shows the effects of learning that a higher fraction of Leavers/Remainers than previously thought agrees with the appropriateness of a repeat referendum and is uncertain about this stance on willingness to campaign of Remain supporters. Columns one and two show the impact on participants' willingness to donate to the People's Vote and Better Off Out campaigns. Columns three and four shows the impact on their willingness to tweet their support for or opposition to a repeat referendum. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by 2SLS using treatment assignment to high numbers as instruments. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.C.5: Plans to Vote, Pooled

	Conserv	Lib Dem	Labour	Brexit	Abstain
Higher Level	-0.038	-0.054	0.011	-0.015	0.060*
	(0.055)	(0.043)	(0.059)	(0.032)	(0.035)
Higher Uncert	-0.013	-0.062	-0.006	-0.067**	0.049
	(0.053)	(0.042)	(0.059)	(0.030)	(0.033)
Observations	4172	4172	4172	4172	4172
Control Mean	.2610012	.1180442	.3201397	.0551804	.0572759

Notes: This table shows the effects of learning that a higher fraction of both Leavers and Remainers than previously thought agrees with the appropriateness of a repeat referendum and is uncertain about this stance on voting intentions. Columns one to four show the impact on participants' intent to vote for the Conservatives, Liberal Democrats, Labour and the Brexit Party in the 2019 election respectively. Column five shows the impact on their intent to abstain from the election. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by 2SLS using treatment assignment to high numbers as instruments. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.C.6: Plans to Vote, Own Other

	Conserv	Lib Dem	Labour	Brexit	Abstain
Higher Level Own	-0.001	0.018	-0.029	-0.020	0.020
	(0.023)	(0.019)	(0.026)	(0.014)	(0.014)
Higher Uncert Own	-0.006	0.001	-0.024	-0.020	0.019
	(0.033)	(0.027)	(0.037)	(0.020)	(0.020)
Higher Level Other	0.006	-0.063***	0.005	0.018	0.024
	(0.029)	(0.024)	(0.033)	(0.017)	(0.018)
Higher Uncert Other	0.005	-0.045	0.022	-0.054***	0.030
	(0.035)	(0.029)	(0.039)	(0.021)	(0.021)
Observations	4255	4255	4255	4255	4255
Control Mean	.2610012	.1180442	.3201397	.0551804	.0572759

Notes: This table shows the effects of learning that a higher fraction of people in one's own and the opposed political camp than previously thought agrees with the appropriateness of a repeat referendum and is uncertain about this stance on voting intentions. Columns one to four show the impact on participants' intent to vote for the Conservatives, Liberal Democrats, Labour and the Brexit Party in the 2019 election respectively. Column five shows the impact on their intent to abstain from the election. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by 2SLS using treatment assignment to high numbers as instruments. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

TABLE 3.C.7: Plans to Vote, Leave

	Conserv	Lib Dem	Labour	Brexit	Abstain
Higher Level Remain	0.002	-0.013	-0.016	0.032	0.002
	(0.041)	(0.013)	(0.029)	(0.027)	(0.021)
Higher Uncert Remain	-0.011	0.001	0.064	-0.117***	0.029
	(0.066)	(0.021)	(0.047)	(0.045)	(0.034)
Higher Level Leave	-0.068	0.024	0.035	-0.046	0.052
	(0.080)	(0.025)	(0.056)	(0.054)	(0.041)
Higher Uncert Leave	-0.069	-0.023	0.047	-0.057	0.031
	(0.074)	(0.023)	(0.052)	(0.050)	(0.038)
Observations	1885	1885	1885	1885	1885
Control Mean	.5015609	.0234131	.1316337	.116025	.0686785

Notes: This table shows the effects of learning that a higher fraction of Leavers/Remainers than previously thought agrees with the appropriateness of a repeat referendum and is uncertain about this stance on the voting intentions of Leave supporters. Columns one to four show the impact on participants' intent to vote for the Conservatives, Liberal Democrats, Labour and the Brexit Party in the 2019 election respectively. Column five shows the impact on their intent to abstain from the election. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by 2SLS using treatment assignment to high numbers as instruments. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** $p<0.01$,** $p<0.05$,* $p<0.1$.

TABLE 3.C.8: Plans to Vote, Remainers

	Conserv	Lib Dem	Labour	Brexit	Abstain
Higher Level Remain	0.026*	0.022	-0.036	-0.009**	0.011
	(0.015)	(0.024)	(0.029)	(0.005)	(0.012)
Higher Uncert Remain	0.040	0.008	-0.063	0.005	0.002
	(0.027)	(0.042)	(0.051)	(0.008)	(0.022)
Higher Level Leave	0.014	-0.144**	0.055	0.006	0.054*
	(0.037)	(0.059)	(0.070)	(0.011)	(0.030)
Higher Uncert Leave	0.002	-0.089*	-0.012	-0.001	0.030
	(0.031)	(0.050)	(0.060)	(0.010)	(0.026)
Observations	2287	2287	2287	2287	2287
Control Mean	.0672953	.1980283	.4809258	.0060009	.0488641

Notes: This table shows the effects of learning that a higher fraction of Leavers/Remainers than previously thought agrees with the appropriateness of a repeat referendum and is uncertain about this stance on the voting intentions of Remain supporters. Columns one to four show the impact on participants' intent to vote for the Conservatives, Liberal Democrats, Labour and the Brexit Party in the 2019 election respectively. Column five shows the impact on their intent to abstain from the election. All regressions include controls for age, gender, employment, education, region, vote in the 2016 referendum, current opinion on the UK leaving the EU and certainty thereof, share of the reference group supporting Leave, and frequency of consuming as well as discussing news. They are estimated by 2SLS using treatment assignment to high numbers as instruments. Heteroskedasticity robust standard errors are given in parentheses. Stars indicate significance at standard levels: *** p<0.01,** p<0.05,* p<0.1.

Chapter 4

Acceptability of App-Based Contact Tracing for COVID-19

The COVID-19 pandemic is the greatest public health crisis of the last 100 years. Countries have responded with various levels of lockdown to save lives and stop health systems from being overwhelmed. At the same time, lockdowns entail large socioeconomic costs. One exit strategy is a mobile phone app that traces close contacts of those infected with COVID-19. Recent research has demonstrated the theoretical effectiveness of this solution in different disease settings. However, concerns have been raised about such apps because of the potential privacy implications. This could limit the acceptability of app-based contact tracing among the general population. This study investigates user acceptability of digital contact tracing of COVID-19 infections in a large scale survey spanning five countries hit by the pandemic. We measure intentions to use a contact-tracing app across different installation regimes and covariates. We find strong support for the app, when installation is voluntary as well as automatic, but that intention to use is 8pp higher under the first regime. Support also transcends countries, sub-groups of the population, and regional-level COVID-19 mortality rates. We further find that concerns about cyber security and privacy, together with lack of trust in government, are the main barriers to adoption. The available evidence suggests that app-based contact tracing may be a viable approach to control the diffusion of COVID-19, but that roll out matters for adoption.

4.1 Introduction

The COVID-19 pandemic is the greatest public health threat of the last 100 years. In the absence of effective treatment or vaccination (as of June 2020), the public health response has so far relied on non-pharmaceutical measures to limit the spread of the epidemic, such as physical distancing, case isolation, and manual contact tracingFerguson et al. [2020]. These measures have not been sufficient to stop the epidemic. Many countries have therefore resorted to partial or full “lockdown" measures to control the epidemic, severely limiting social and economic interactions among their citizens. Although lockdowns may help countries to keep the number of infections under controlCowling et al. [2020], they come at a great social and economic costINSEE [2020], Barrot et al. [2020], Bayham and Fenichel [2020], Adams-Prassl et al. [2020].

COVID-19 is difficult to trace by traditional methods as COVID-19 cases are infectious 1-2 days before experiencing symptoms and contacts on average become infectious 3-4 days after exposure. The window to achieve containment by manual contact tracing is thus extremely short. Ferretti, Wymant and colleaguesFerretti et al. [2020] have proposed *digital (app-based) contact tracing* as an alternative measure to contain the epidemic without the large economic costs of lockdowns. The idea is to use low-energy Bluetooth connections between phones to record the interactions users have with others, particularly those interactions that may pose a higher risk of infection (e.g., spending more than 15 minutes within two metres of another person). If a user is diagnosed with COVID-19, they can use the app to declare the diagnosis, which leads to a notification of all other users who have come in close contact with the infected person, asking them to isolate at home for 14 days or until they have been tested by the public health authority. The main advantage over traditional (manual) forms of contact tracing is that the app allows *instantaneous* notification of contacts, which is a key determinant of the effectiveness of case isolation and contact tracing strategies for COVID-19Ferretti et al. [2020]. Other advantages are that the automatic recording of contacts scales up easily, and avoids the loss of information due to patients’ recall bias and/or imperfect knowledge of the people they have been in contact with.

More and more countries are currently developing various types of contact-tracing apps and several countries have already launched one, e.g., Singapore GoS, Germany RKI and France GoF. The success of app-based contact tracing, however, critically depends on people's willingness to use the app. Hinch and colleagues Hinch et al. [2020] simulate the epidemic in the UK and show that the app reduces infections at all levels of take-up but that it is only sufficient to stop the epidemic if approximately 60% of the population use it. It is therefore important to gauge the strength of public support for this approach and to understand the factors that may hinder or facilitate take-up. For instance, since the app would need to trace individuals' interactions with others, privacy concerns may undermine support and adoption.Abeler et al. [2020] It is also possible that such a technological solution may not work as well for the less digitally literate share of the population, further increasing the unequal impact of the COVID-19 pandemic within and across countries Hargittai [2002]. In this sense, an "opt-out" installation policy, where mobile phone providers or Apple and Google Google [2020] would automatically install the app on phones, could maximize take-up. It is unclear, however, whether the public would be willing to support this more intrusive solution.

In light of the many open questions surrounding the viability of app-based contact tracing, we designed a survey to measure public support for this approach in five countries that are currently hit by the COVID-19 pandemic: France, Germany, Italy, the UK and the US. The specific objectives of our study are to (i) assess the overall acceptability among the public of app-based contact tracing under different installation policies (e.g., voluntary installation or automatic installation by the government); (ii) uncover country-level and individual-level variation in support for the app; and (iii) understand the main mechanisms that may facilitate or impede app usage across various subgroups of countries and individuals.

The rest of the paper is structured as follows: Section 4.2 describes how we designed the survey, our sample and the empirical strategy. Section 4.3 describes our findings. Section 4.4 discusses the implications of our findings for policy makers trying to curb the spread of COVID-19 infections and Section 4.5 concludes.

4.2 Methods

4.2.1 Survey Design

We conducted large online surveys in five countries (France, Germany, Italy, UK, and the US) to measure acceptability of app-based contact tracing for COVID-19 before apps were introduced in any of the five countries. A complete description of the survey can be found in the Appendix; here we provide an overview. At the beginning, after collecting respondents' informed consent, we described the app, explaining how a general version would function as well as the purpose it would serve (see Textbox 1). We abstracted from any details about centralised versus decentralised data storage procedures. Respondents had to pass a comprehension check to proceed further. We then asked respondents how likely they would be to install the app on their phone, if it became available to download voluntarily ("opt-in" installation policy). Respondents were then asked about their main reasons for and against installing the app as well as their compliance with self-isolation requests. Next, we assessed to what degree respondents would be open to an "opt-out" policy, where mobile phone providers would automatically install the app on all phones, but users would be able to uninstall the app at any time. We then collected demographic information and concluded the survey with questions about respondents' attitudes towards the government under different installation regimes.

We kept the survey design as similar as possible across all five countries, with a few exceptions to accommodate country differences with regard to lockdown measures in place at the time of taking the survey. The US survey (deployed last) contained a few additional questions, including robustness checks. See Section 4.A in the Appendix for more details.

Ethics approval was obtained from the University of Oxford (reference number ECONCIA20-21-06). No personal information was collected as part of the study.

Textbox 1. App Description in the UK Survey.

"Imagine there was an app that you could install on your mobile phone. This app would automatically alert you if you had been in close contact for at least 15 minutes with someone who was infected with the coronavirus. Such an app does not exist yet in the UK. But we, researchers from the University of Oxford, are interested in understanding what you would think about such an app. [...]

The app would be developed by the NHS. You would need to install the app by simply clicking a link. Once installed, the app would register which other users are close to you. The app would do this by using Bluetooth and your location.

The app would NOT access your contacts, photos, or other data held on your phone. Only the NHS would have access to the data collected. [...]

If the NHS diagnoses the coronavirus in somebody you have been in close contact with, the app would notify you automatically. The app would give you targeted advice on what to do. It will ask you to self-isolate at home for 14 days or until you have been tested for the virus.

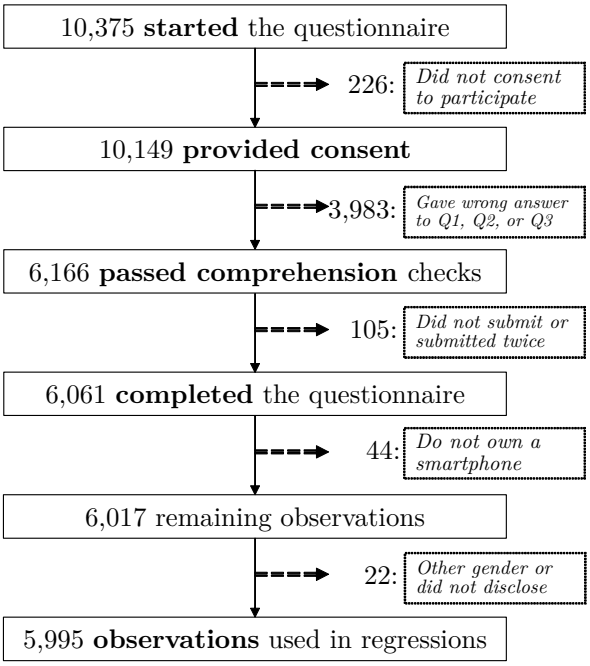
This would be useful since people can infect others even before they have a fever or a cough. Self-isolating would thus protect your family, friends and colleagues from being infected by you. At the same time, only people who were in contact with an infected person would need to self-isolate.

If you had not been in close contact with a confirmed case, then the app would show you an "all clear" message. [...]

If you are diagnosed with coronavirus, the app would notify all people you have been in close contact with, without identifying you to them, and advise them to self-isolate. This would increase the chance of finding all the people you might have infected and help make sure they can keep their loved ones safe as well. If enough people use the app, it will slow down the epidemic and might even stop it entirely."

The versions for the other countries differed with regard to the technology used (GPS and Bluetooth vs only Bluetooth) and lockdown restrictions in place generally and tied to app usage specifically. For details, see the Appendix.

FIGURE 4.2.1: Attrition and Final Sample



4.2.2 Data

The surveys were administered between 20$^{\text{th}}$ March and 10$^{\text{th}}$ April 2020. We recruited respondents through Luc.id, an online panel provider (now part of Cint). We targeted a sample size of 1000 respondents in each of the four European countries, and 2000 in the US – with quotas set for the samples to be representative of the overall population in terms of gender, age and region of residence. A total of 10375 individuals started the survey and 10308 consented to participate (a participation rate of 99%). Out of the people who consented to participate, 6166 passed the comprehension check and started the main questionnaire. After removing incomplete responses and duplicates, we have a sample of 6061 complete and unique responses (a completion rate of 59%). Finally, we removed 66 respondents who either did not own a mobile phone or did not disclose their gender, leaving us with a final sample of 5995 respondents. Figure 4.2.1 shows how many individuals opened the survey by clicking on the link as well as the subsequent attrition at different stages that left us with our final sample of 5995 complete responses.

To control for the potential effect of our recruitment method, we repeated the German survey with a probability-based sample in an offline recruited online panel.

See Section 4.B in the Appendix for further details on recruitment, filtering and attrition, and the final sample.

4.2.3 Empirical Strategy

Our main outcome variables measure respondents' intention to have the app installed on their phone under the two installation regimes (opt-in vs. opt-out). The outcomes were measured on a 5-point ordinal scale (opt-in: from *Definitely install* to *Definitely won't install*; opt-out: from *Definitely keep* to *Definitely uninstall*). In our regression analysis, we dichotomise these outcome measures (= 1 if definitely or probably install / keep the app, and = 0 otherwise).

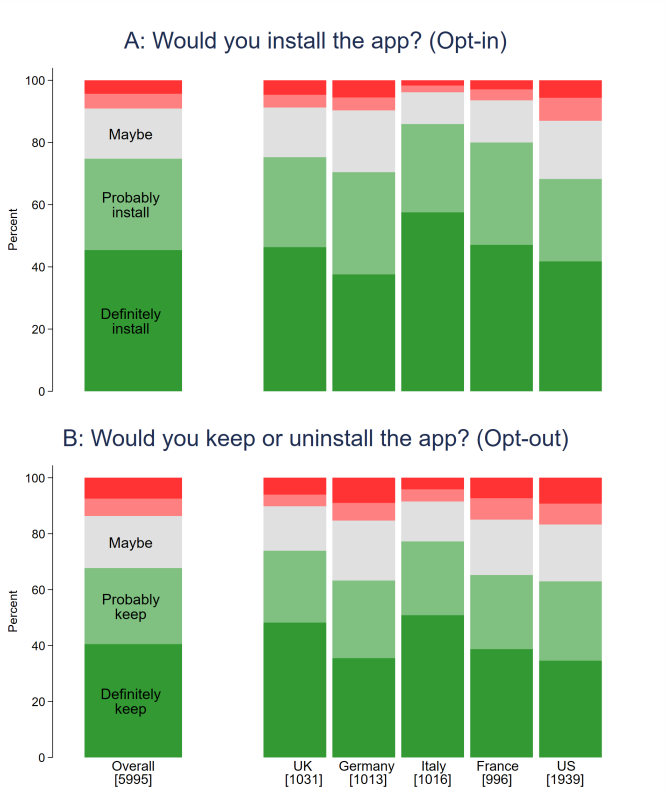
We use multivariate regression analysis (linear probability models; probit and ordered logit in additional analyses presented in the Appendix) to examine the relationship between intention to install and a number of covariates: age, gender, country, presence of comorbidities (diabetes, high blood pressure, heart or breathing problems), usage of mobile phone outside the house, frequency of social interactions, ability to work from home during the lockdown, ability to obtain sick pay while working from home, trust in national government, and incidence of COVID-19 deaths in a respondent's region of residence (see Section 4.C.3 in the Appendix for more details). Table 4.B.2 in the Appendix presents a summary of these covariates.

4.3 Results

We find broad support for app-based contact tracing. Support is high in all countries, across all subgroups of the population, and under both installation regimes (opt-in and opt-out). Panel A of Figure 4.3.1 shows that, under the voluntary (opt-in) installation regime, 4484 out of 5995 respondents (74.8%) across all countries say they would probably or definitely download the contact-tracing app, if it was available. Panel B shows that 4059 out of 5995 respondents (67.7%) also say they would probably or definitely keep the app installed on their phone under the automatic (opt-out) installation regime. In both regimes, the share of respondents who say they would not have the app installed on their phone is very small (red portion of the bars in Figure 4.3.1).

Support is high in all five countries where we implemented the survey: in each country, at least 68% of respondents say that they would install or keep the app. Moreover, Figures 4.C.3, 4.C.4, and 4.C.5 in the Appendix show that support for the app is generally high across various subgroups of the population (e.g., across men and women, across different age groups, etc.), suggesting widespread acceptability of the app-based contact tracing solution to the COVID-19 pandemic.

FIGURE 4.3.1: Likelihood of Having the App Installed, under Opt-in and Opt-out Regimes and by Country

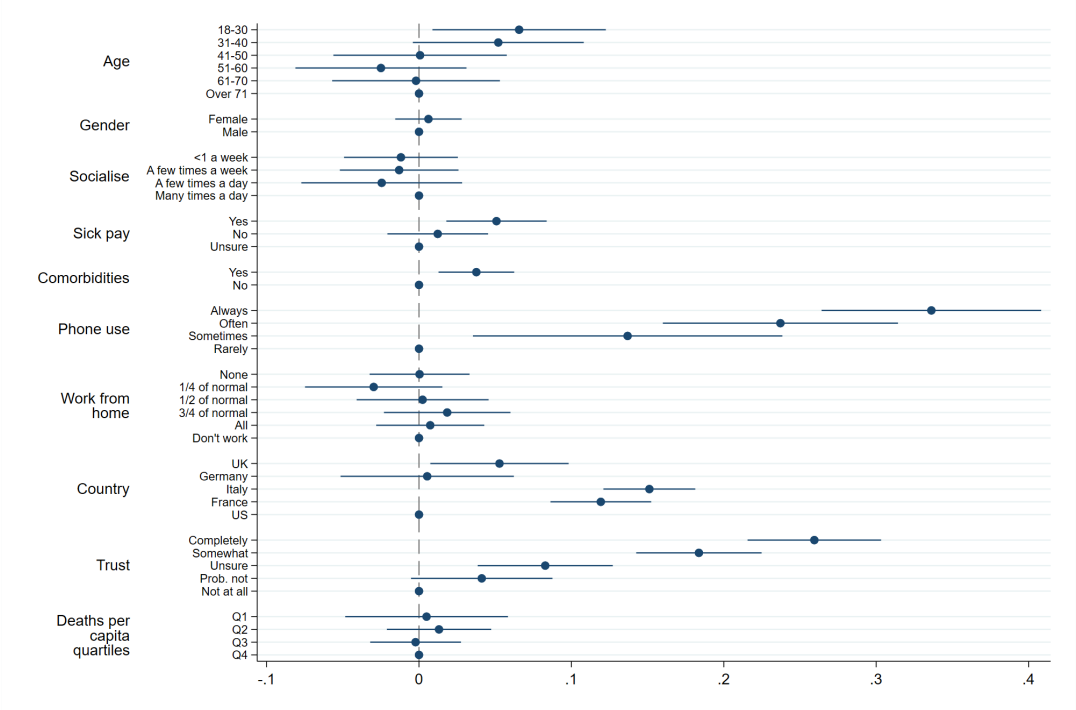


Note: Light/dark red bars correspond to Probably/Definitely won't install in Panel A and Probably/Definitely uninstall in Panel B.

Despite the broad and widespread acceptability of the app, we find that support varies systematically across countries and individuals. For instance, Figure 4.3.1 shows that Germany and the US are relatively less supportive of the app compared to the other countries. This is the case both under the opt-in and opt-out regimes. Among individual characteristics, we find that those who have lower trust in their national government are more hesitant to have the app installed on their phones (Figure 4.C.5 in the Appendix).

We further explore this heterogeneity using multivariate regression analysis, where we examine the relationship between support for the app and a variety of individual- and country-level covariates. Figure 4.3.2 shows the impact that these covariates have on the probability of definitely or probably installing the app under the opt-in regime, using a linear probability model (see Section 4.C.1 in the Appendix for a similar analysis of the opt-out regime).

FIGURE 4.3.2: Determinants of Stating Definitely Install or Probably Install



Note: The dependent variable is an indicator variable taking the value 1 if a respondent chose *definitely install* or *probably install* when asked whether they would install the app or not, and 0 otherwise. We use a Linear Probability Model. Lines represent 95% confidence intervals calculated with heteroskedasticity-robust standard errors. All coefficients are the result of a single regression and thus display marginal effects. A coefficient of 0.1 implies a respondent who chose this option is 10 percentage points more likely to state they would definitely or probably install the app relative to the base category.

The analysis confirms that Germany and the US are significantly less supportive of the app, especially compared to France and Italy. Taking the two most extreme cases, respondents in Italy are 15.1 percentage points (95% CI 12.1 - 18.1) more likely to support the app than respondents in the US. Surprisingly, Figure 4.3.2 shows very little correlation between regional-level COVID-19 mortality rates and support for

the app.

Among individual-level characteristics, we find that people who carry their phone with them more often are more likely to install the app. Those who always carry their phone with them are 33.6 percentage points (95% CI 26.4 - 40.8) more likely to support the app than those who carry their phone only rarely. App support is also 3.7 percentage points (95% CI 1.3 - 6.2) larger among respondents with one or more comorbidities. Moreover, the probability of installing the app increases with trust in the government. People who completely trust the government are 25.9 percentage points (95% CI 21.6 - 30.3) more likely to install the app than those who do not have any trust in the government.

We find similar results using an ordered logit model, a linear probability model dichotomising on just *Definitely install*, and when using a probit model (see Appendix). Finally, results are also qualitatively similar when considering installation intentions under opt-out rather than opt-in (Figure 4.C.2 in the Appendix). Interestingly, under the opt-out regime, trust in government displays an even stronger correlation with the intention to keep the app installed on one's phone.

We can use the data on respondents' reasons for or against installing the app to better understand the nature of the observed variation in app support across countries and individuals. A first set of reasons against the app revolved around concerns about government surveillance at the end of the epidemic (mentioned by 2518 out of 5995 respondents, 42%) and cyber security (fears that the app could make the phone vulnerable to hackers; 2098 out of 5995 respondents, 35%). Respondents also reported that usage of the app may increase feelings of anxiety (out of 5995 respondents, 26%), possibly reflecting aversion to feedback about a possible infection. The most frequent reasons in favour of the app were willingness to protect family and friends (15589 out of 5995 respondents, 68%), a sense of responsibility towards the community (3177 out of 5995 respondents, 53%), and a hope that the app may stop the epidemic (3297 out of 5995 respondents, 55%). Figures 4.C.10 and 4.C.11 in the Appendix show the relationship between the probability of selecting a particular reason and country- and individual-level characteristics.

Several patterns are of interest. First, we find that, compared to other countries, respondents in Germany and the US are more likely to mention concerns about

government surveillance as one of the reasons against installing the app. In these countries we also see a larger share of respondents expressing concerns about security of the app, especially compared to Italy and the UK. Thus, concerns about privacy and security seem to be an important impediment to the adoption of the app, particularly in Germany and the US.

Among individual-level characteristics, we find that respondents who have less trust in their national government are also more likely to express concerns about government surveillance. This suggests that privacy concerns play a role in the negative relationship between trust in government and probability of installing the app found in Figure 4.3.2. In contrast, we find that frequent usage of mobile phones is related to a stronger perception of the potential benefits of the app: respondents who more often carry their phone with them are more likely to believe that the app would benefit them, by helping them stay healthy and keeping them informed about the risks of infection.

4.4 Discussion

4.4.1 Implication of Principal Findings

In our study, we find high support for app-based contact tracing – irrespective of age, gender, region or even country of residence. Since the effectiveness of app-based contact tracing crucially depends on a sufficient level of take-up, our findings are encouraging for the prospects of this approach. Although support is high in all countries and subgroups of the population, the data reveal that concerns about cyber security and privacy, coupled with trust in government, are important determinants of support. Countries with stronger privacy and security concerns (Germany and the US) are relatively less supportive of app-based contact tracing. Individuals who have less trust in their national government are also less supportive.

The lack of trust in government can have far-reaching implications. Our analysis shows that this factor has a negative effect on people's intention to install a contact-tracing app on their phones. Furthermore, supplementary analysis (see Section 4.C.6 in the Appendix) also shows that people with lower trust in government are more in favour of an opt-in installation policy than an opt-out regime where the government

asks mobile phone providers to automatically install the app on all phones. An opt-out regime is likely to translate into higher effective installation rates, for instance by reducing the negative effects of procrastination or unawareness Johnson and Goldstein [2003]. However, our data suggests that only governments that enjoy a relatively high level of trust from their citizens may be able to resort to more paternalistic approaches. A policy implication of these findings is that governments should consider delegating the organisation of app-based contact tracing to a highly-reputable and transparent public health authority at arm's length from the government. If the mobile phone's operating system (e.g., iOS or Android) does the contact-tracing directly, trust in, e.g., Apple or Google, would become more important.

Our results also point towards the need to address privacy and cyber security concerns with an app design that respects user personal data as much as possible. Research on the privacy implications of app-based contact tracing, and the potential solutions to these concerns, is currently underway Abeler et al. [2020], Cho et al. [2020], Günther et al. [2020]. Interestingly, however, when we ask our respondents how the data collected by the app should be treated, we find that nearly 60% would consent to making the de-identified data available to research.

4.4.2 Limitations

Our study has some limitations that we tried to address in different ways. First, respondents recruited online may not be representative of the entire population. In particular, digital literacy and willingness to share data could be higher among such respondents. To ensure that our results do not hinge on our specific sample, we replicated an abridged version of the German survey with a different panel provider that randomly recruits its participants offline. Our results remain almost completely unchanged (see Section 4.B.3 in the Appendix).

Second, our survey asked hypothetical questions about future behaviour. However, high levels of *intended* installations may not directly translate into *actual* installations. While studies often find good correlation between what people declare they would do in surveys and actual behaviour Hainmueller et al. [2015], Chandon et al. [2005], Waterfield et al. [2020], Jordan et al. [2020], Zhang et al. [2020], even in relation to app installations Keusch et al. [2019], Kreuter et al. [2018], Wenz et al.

[2019], Jäckle et al. [2019], many things will have changed between the time of the surveys and when countries eventually introduce the app. For example, at the time of the survey, Italy's epidemic was close to its peak and the urgency of the situation was very salient. As the epidemic recedes, the perceived need to do something about the epidemic will also recede (see, e.g., Zhang et al. [2020], Betsch et al. [2020]). Moreover, we made participants aware of the app and explained the app and its potential effects. In reality, many potential users will not be aware of the app, might not engage with the concept, or will not be willing or able to spend the time to find and install the app. More generally, a reported willingness to install is only a necessary first stage to adoption, and our findings about heterogeneity in support point towards specific subgroups of the population that may need stronger encouragements to adoption. We show in Section 4.C.4 of the Appendix that respondents who would install the app mention far more reasons *for* its adoption than those who would not install it (but a similar number of reasons *against*). We show in Section 4.C.9 that respondents in our replication study who did not answer the comprehension questions correctly were less willing to install. Stressing the various benefits of the app, to oneself and others, and explaining the working and purpose of the app may be a particularly effective strategy to foster adoption. Further research will be needed to understand how to translate a person's willingness to install into the person actually installing the app.

Third, in our survey, we measured support for the general concept of app-based contact tracing, leaving out specific details regarding the implementation, which were not available to us at the time respondents took the survey. One downside of only surveying about the general idea is that it might be harder for respondents to visualise how such a system could work, which may increase hypothetical bias. However, we find that the details we gave about implementation (e.g., whether the app uses Bluetooth or GPS) seem to have very little impact on support. This suggests that our general measure of support for app-based contact tracing may be portable across different implementation settings.

Fourthly, our results analysing heterogeneity by age rely on coarse age binning. Such banding is subject to flaws if the sample is not distributed well across bands, which is not possible to verify in our study. Thus results by age should be read with this limitation in mind.

Finally, our survey respondents were recruited from a specific subset of industrialised Western democracies. Attitudes towards app-based contact tracing may vary across countries with different levels of development and political regimes. It is nevertheless encouraging, in terms of external validity, that we observe a strong similarity in responses across the five countries we sampled, and that analogous findings have been reported in ongoing surveys conducted in Australia and TaiwanDennis et al.. In developing countries and among disadvantaged populations, the more limited access to smartphones raises both efficacy and equity issues; the development of low-cost Bluetooth devices with similar functionalities could improve access to digital contact tracing.

4.5 Conclusions

In conclusion, our study shows strong public support for app-based contact tracing to tackle COVID-19. This is an important result since public support is a necessary condition for the viability of the approach. We find that stated willingness to use a contact tracing app is particularly high if installation is voluntary (although actual take-up may be lower in this case), if individuals' trust their government, and if they worry less about data usage and privacy. Further research is needed to gauge the extent to which public support for app-based contact tracing translates into actual app adoption and, more generally, to evaluate its potential for epidemic control.

Appendix

4.A Survey Details

4.A.1 Survey Design

Table 4.A.1 gives an overview of the structure of the survey that was administered to respondents. In the following, we describe the various parts in more details. After expressing their informed consent to take part in the study, respondents were given a description of the contact tracing app. We explained that the app would be developed by a national health organisation and that, once installed, it would register other users in close proximity via Bluetooth (and, in the case of the US and the UK, potentially location data). Users who were found to have been in close proximity to a confirmed case of COVID-19 for at least 15 minutes would be alerted by the app and asked to quarantine at home for 14 days or until they could be tested for the virus. All other users would see an "all clear" message. We further explained that an early quarantine would prevent individual users from passing on the virus to their loved ones in the early (presymptomatic) stages of their potential infection, and might slow down or even stop the epidemic. We also stressed that the identity of all users would be private throughout the entire process. In order to progress to the main part of the survey, respondents had to correctly answer three comprehension questions about the functioning of the app.

TABLE 4.A.1: Structure of the Survey

Part 1: **Introduction**	Informed consent Description of the app Comprehension checks
Part 2: **Voluntary installation (Opt-in)**	Willingness to install the app Reasons for and against installing the app Willingness to comply with self-isolation request Willingness to install at different stages of the epidemic
Part 3: **Automatic installation (Opt-out)**	Willingness to keep the automatically installed app Agreement with automatic installation policy Data policy preferences
Part 4: **Socio-demographics**	Among which: Age, Gender, Region, Encounters with non-household members, Health status, Phone use, Ability to work from home
Part 5: **Political support**	Party preference, Trust in government, Would opinion of government improve under automatic/voluntary installation policies

After this introductory part, the main questionnaire began, which was designed to address the following three objectives:

1. **Assess general support for adopting the app** (and complying with its requests);
2. **Identify the main drivers of adoption intentions** (reasons for and against);
3. **Evaluate support for different implementation policies** (opt-in vs. opt-out installation regimes).

First, respondents were asked to assess their likelihood of installing (or not) the app on their phone; responses were collected on a 5-point scale, from *Definitely install*

to *Definitely won't install*.[1] Respondents were then asked about their main reasons for and against installing the app; in both cases, respondents could select multiple reasons from a menu of options (the order of these options was randomised at the individual level). We subsequently asked respondents how likely they would be to comply with the request to self-isolate for 14 days if they had been in close contact with an infected person. Responses were collected on a 5-point scale from *Definitely comply* to *Definitely won't comply*. Those who did not select *Definitely comply* were then asked whether their chances of compliance would increase, decrease, or remain the same if health services committed to test them for the virus within two days from the start of their self-isolation (a negative test allowing them to stop self-isolating). Respondents who did not say they would *Definitely install* the app in response to the initial installation question were asked further questions about their willingness to install the app in three additional scenarios: (i) in case the epidemic had spread to someone in their community, (ii) to someone they knew personally, (iii) or in case an "all clear" message by the app would be associated with a relaxation in lockdown restrictions.

We next assessed whether respondents would be open to an "opt-out" policy: the government would require mobile phone providers to automatically install the app on all phones, but users would be able to immediately uninstall it. Respondents were asked about their willingness to keep (vs. uninstall) the app in this case (on a 5-point scale from *Definitely keep* to *Definitely uninstall*). As a follow-up question, they were asked to rate the extent to which they agreed with the following statement: "The government should ask mobile phone providers to automatically install the app on all phones" (on a 5-point scale from *Fully agree* to *Fully disagree*). Respondents who did not *Fully agree* with the statement were then asked whether their opinion would change if someone in their community, or someone they knew personally, had been infected with the virus. Finally, they were asked for their preference regarding the data policy that should be adopted once the epidemic is over: automatically delete all data, de-identify the data and make it available for research purposes, or some

[1] Respondents could also select the option *Don't know*, which in the analysis we merge with the mid-point of the scale, *May or may not install*. The options on the scale were always presented in descending order, except in the US where we randomised whether respondents saw the scale in descending or ascending order to test for possible order effects. We found no evidence of this. See Section **??** in the Appendix for more information.

other option of their choice.

The third block of the survey collected basic demographic information (age, gender, region of residence) as well as information about potential risk factors for contracting the virus (frequency of social interactions and health risks), use of smartphone, and ability to work from home and receive (some fraction) of payment.

The final block consisted of questions about political orientation and attitudes towards the government. We first asked respondents to state their political affiliation. We then asked them whether, in general, they "trusted the government to do what is right". Finally, we asked them whether their opinion of the government would improve in case of (i) an opt-in installation regime, and (ii) an opt-out regime.

We kept the survey design as similar as possible across all five countries, with a few exceptions. First, the UK survey was administered before the government issued a nation-wide lockdown. On the other hand, respondents in Germany, Italy and France, were surveyed *after* such a lockdown had been implemented. In the US, most states were under a "stay at home" order by the time of the survey, but not all of them. To reflect this difference in the environment respondents faced, we slightly adjusted the phrasing of some of our questions between areas in lockdown and those that were not.[2] Second, unlike in the other countries, we asked UK and US respondents about an app that might use GPS data in addition to Bluetooth. Third, in the US survey, which was run last, we included a question explicitly asking respondents which installation regime (opt-in or opt-out) they preferred. We did this in order to check the consistency between their actual preferences for installation regimes and their installation intentions under each regime elicited in Parts 2 and 3. We also added demographic questions about ethnicity, area of residence, health insurance and media use, as well as two versions of a question about willingness to install if a private company like Facebook endorsed it. The full texts of the different surveys can be found here: UK, France, Germany, Italy and US.

[2]For example, in the UK and areas in the US that were not under lockdown, we asked about willingness to install if the (local) government firstly introduced a lockdown and then tied a relaxation of restrictions to an "all clear" message on the app. In contrast, in Germany, Italy, France and areas in the US under a stay-at-home order, we simply asked about willingness to install if current restrictions were lifted in response to an "all clear" message.

4.A.2 Technical Details

The survey was administered between the 20th and 27th of March 2020 in the four European countries (France, Germany, Italy, UK), and between the 7th and 10th of April 2020 in the US. No personal data was collected at any point during the survey and we obtained informed consent as well as checked for bots before the survey began. Respondents who accepted the survey invitation were directed to our online questionnaire, programmed using the software Qualtrics. The survey was pre-tested before fielding by generating test observations (around 4000 iterations for each country) to check data quality and consistency. Moreover, we always had a soft launch before the full launch. That is, after collecting the first 100 responses, we paused recruitment and checked the data before launching the survey widely. No adjustments had to be made in any of the countries. Our survey was not password protected but distributed through an open link, so in principle respondents could take part multiple times. However, the panel provider Lucid assigns a unique ID to each respondent which we tracked within the survey, which allowed us to identify and exclude multiple entries from the same respondent. Only a very small number of respondents completed the survey more than once; see Section 4.B.2 for more details. Throughout the survey, respondents were allowed to navigate back and forth between screens except for when adaptive questioning was used. Furthermore, we enforced responses in all questions but those about reasons for and against installation (as we did not want to force people in favour of/opposed to installation to give reasons against/for, respectively). Consequently, every participant who submitted the survey also replied to every question. We did give people the option to choose "Don't know" in response to all the installation questions (as well as a question about sick payment in the demographics part); however, since generally not many people chose it, we merged this option with the midpoint of the scale during data analysis. The entire survey was stretched over 17-23 screens (21-28 screens in the US), depending on the adaptive questioning, with 1-5 items per screen. The average completion time was 11.0 minutes in France, 9.7 minutes in Germany, 10.3 minutes in Italy, 8.7 minutes in the UK, and 12.6 minutes in the US.

4.B Sample

4.B.1 Recruitment

In all five countries, we recruited respondents through Lucid, an online panel provider that works with a variety of sample suppliers to get a broad range of volunteers. Volunteers were recruited using a multitude of methods, from double opt-in panels (the vast majority), publishing networks, social media, and other types of online communities. In some cases, participants were furthermore recruited offline e.g., via TV and radio ads, or with mail campaigns. Participation was voluntary and, in the majority of cases, incentivised with most suppliers providing loyalty reward points or gift cards, and some providing cash payments. In each country, we set recruitment quotas so as to achieve a sample of respondents that was representative of the adult population of the respective country with regard to gender, age and region of residence (quota-based sampling). Furthermore, we did not invite individuals who did not own a mobile phone.[3]

4.B.2 Attrition and Final Sample

Across the five countries, 10375 individuals started the survey, out of which 10308 consented to participate. Before participants could begin the main part of the survey, we described the app, and asked three comprehension questions to ensure participants were paying adequate attention. Only if all three comprehension question were answered correctly were respondents allowed to continue. Out of the people who consented to participate, 3983 failed to answer all three comprehension questions correctly, leaving us with a sample of 6166 respondents. 105 of these respondents either exited the survey before submitting or took the survey more than once and were therefore excluded from the sample,[4] which left us with 6061 complete and unique responses. Finally, for practical reasons we dropped observations from 44 individuals who indicated in the comments section they did not own a smartphone, and 22 individuals who either did not identify as male or female, or who preferred not to disclose their gender. This gave us a final sample of 5995 participants.

[3] A few respondents still noted that they did not have a *smartphone* in the comments section (as a reason for not installing the app) - we excluded those responses from our final sample.

[4] For the few people who took the survey more than once, we kept their first submission.

TABLE 4.B.1: Demographics

		Respondents (%)					
		All	DE	FR	IT	UK	US
Age:	18-30	23.2	22.7	30.1	20.0	20.6	22.8
	31-40	21.0	25.5	23.1	16.9	20.5	20.1
	41-50	18.7	18.5	20.0	19.1	21.5	16.4
	51-60	17.3	20.8	13.7	20.2	18.5	15.2
	61-70	14.4	10.0	11.4	17.0	13.4	17.5
	Over 71	5.4	2.6	1.7	6.8	5.5	7.9
Gender:	Female	52.3	51.7	51.4	51.4	49.1	55.3
	Male	47.7	48.3	48.6	48.6	50.9	44.7
Health Problems:	Yes	27.8	28.7	20.7	23.0	28.7	32.9
	No	72.2	71.3	79.3	77.0	71.3	67.1
Sick Pay:	Yes	40.9	56.8	36.1	31.3	41.0	40.1
	No	43.6	22.7	54.3	52.9	30.6	51.0
	Unsure	15.5	20.5	9.5	15.8	28.4	9.0
Work from home:	None	26.9	34.9	33.6	21.7	26.7	22.1
	1/4 of normal	8.9	9.6	7.5	11.5	8.1	8.4
	1/2 of normal	8.9	8.6	7.3	9.4	8.5	9.7
	3/4 of normal	9.8	10.1	9.0	11.3	9.3	9.6
	All	16.8	13.7	13.9	16.4	17.7	19.6
	Don't work	28.7	23.1	28.6	29.7	29.7	30.6
Social:	<1 a week	54.3	46.8	67.9	69.2	33.2	54.7
	A few times a week	29.4	34.0	20.8	19.4	39.8	31.3
	A few times a day	6.8	9.5	3.7	4.4	10.7	6.2
	Many times a day	9.5	9.8	7.6	7.0	16.4	7.9
Phone use:	Always	80.3	74.3	81.4	87.8	77.8	80.4
	Often	14.0	18.2	13.7	9.8	14.5	13.9
	Sometimes	2.8	3.2	2.4	1.0	3.8	3.1
	Rarely	2.9	4.3	2.5	1.4	3.9	2.7
Trust:	Fully agree	13.1	12.7	8.8	16.2	15.4	12.6
	Somewhat agree	36.1	40.9	28.4	43.9	40.3	31.3
	Unsure	22.4	26.7	30.6	17.7	20.7	19.4
	Somewhat disagree	18.2	12.0	19.4	14.6	16.0	23.9
	Fully disagree	10.1	7.7	12.8	7.6	7.6	12.7
Cases per million:		1020.4	612.8	135.8	1433.0	175.5	1395.0
Deaths per million:		44.4	4.2	4.4	151.3	8.7	50.2
Respondents:		5995	1013	996	1016	1031	1939

Notes: Health Problems refers to diabetes, asthma, high blood pressure, heart or breathing problems. "Trust" indicates whether respondents agreed that, in general, they trust their government to do what is right. The number of cases and deaths per million people in each country refers to the number on the final day of surveying.

4.B.3 Sample Robustness

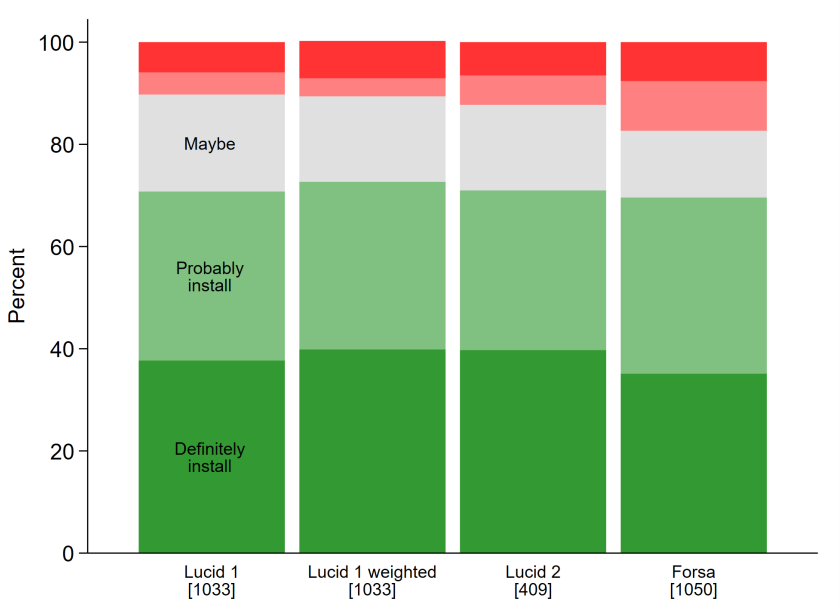
Lucid recruits survey participants online through a variety of sampling partners. From these partners, Lucid receives demographic information about the pool of potential participants and uses this information to target respondents in accordance with the quotas set for the survey. In our case, we targeted the gender and age composition as well as the regions of residence of the respondents to be representative of each country's overall population. After sending the survey out to an initial sample meeting these quotas, Lucid subsequently focuses on particular demographic groups in the re-sampling to meet the quotas in the final sample of participants (i.e., if initial take-up among people over the age of 60 was low, Lucid would disproportionately target them or even close data collection on younger cohorts completely after a while to ensure balance across age groups). However, while we tried to ensure that our sample was representative of each country's population in terms of gender, age and region of residence, the composition could still differ from the country averages in terms of other characteristics. Furthermore, respondents who are recruited online may be more tech savvy and willing to use a phone application than the average individual. This could potentially bias our estimates.

To address these concerns and assess the external validity of our sample, we investigated its representativeness in two ways, using the German sample as an example. First, we re-created our key figures using sampling weights to harmonise the characteristics of our sample with the German population at large. Our results remained generally consistent - see for example Figure 4.B.1 which depicts the re-weighted response likelihoods to the question "How likely would you be to install the app on your phone". However, if unobserved factors like tech savviness are not strongly correlated with demographics, then using survey weights alone is not enough to un-bias the results. As a second step, we also tested whether our results are robust to alternate recruitment methods. One week after the initial survey, we repeated our online survey twice, once again with Lucid and also with Forsa. Although both surveys were conducted online, Forsa, unlike Lucid, recruits its online panel members from a probability based, randomly selected telephone sample.[5] This offline recruitment

[5]To recruit participants, Forsa randomly generates phone numbers, calls the respective households and invites them to participate in the survey. Due to the direct contact over the phone, the sample

process ensures that technical literacy and willingness to share data should play less of a role in the selection into the Forsa sample. We find almost exactly the same results in the replication survey, alleviating concerns about our original sample – see, for example, Figure 4.B.1 which depicts results for the question "How likely would you be to install the app on your phone". More details about the replication can be found in the German country report.

FIGURE 4.B.1: Would You Install the App? – Robustness in the German Sample



Note: *Lucid 1* refers to our initial, unweighted German sample collected with Lucid. *Lucid 1 weighted* refers to the same sample albeit now reweighted with population weights. *Lucid 2* refers to our follow-up German sample that we collected a week after the initial one with Lucid. Finally, *Forsa* refers to the sample we collected a week after the initial one with Forsa. Light/dark red bars correspond to Probably/Definitely won't install. For more information, please see the German country report.

4.B.4 Selective Attrition

Using data from only individuals who pass all three comprehension questions may introduce selection bias in our sample. For example, more tech savvy respondents may simultaneously be more likely to pass the comprehension checks and more inclined

should contain neither bots, nor respondents with a dubious identity (due to fake identities or multiple sign-ups).

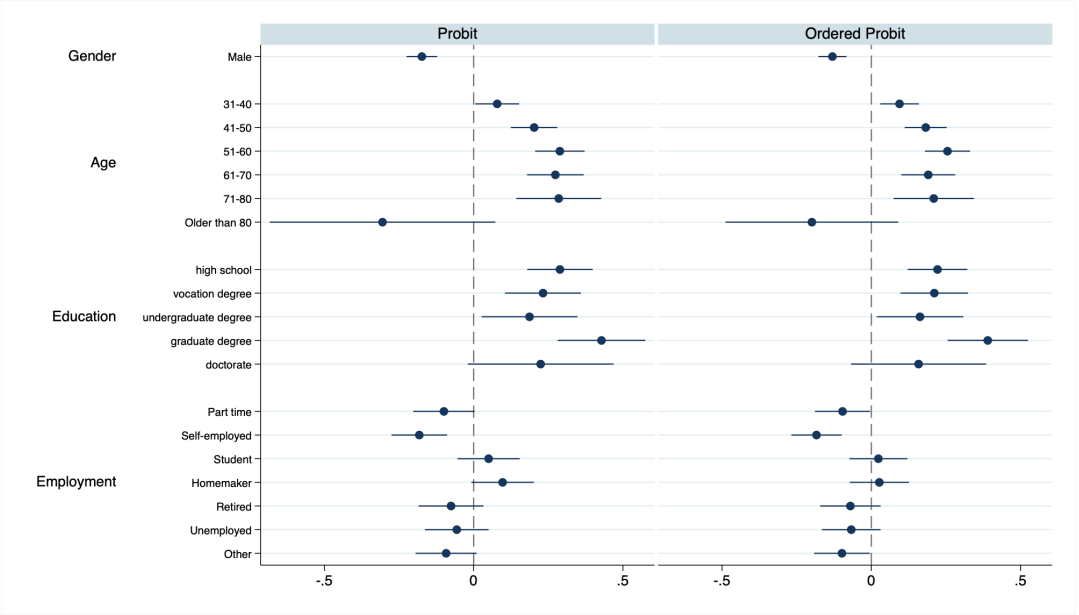
to install any app. To address this concern, we consider a selection model where the likelihood of a respondent consenting to the survey and correctly answering all comprehension questions is a function of the demographic information we obtained from Lucid for everyone who started the survey (age, gender, region, employment status, and, in the case of Italy, France and Germany, education). We estimate the following probit model:

$$P(i \text{ completes survey}|\mathbf{x}_i) = \Phi(\beta \mathbf{x}_i)$$

where $\Phi()$ gives the normal cumulative distribution function. We also estimate an ordered probit model, taking the stage of a respondent's drop out (consent, comprehension question 1, 2 or 3) as our ordinal outcome variable. The results of these empirical specifications are given below, in Figure 4.B.2. Respondents with less than high school education are significantly less likely to be included in our final sample. The same is true for male respondents, younger respondents, and the self-employed. Given our selection model, we re-weight our analyses with inverse probability weights reflecting the likelihood that each individual was sampled. Therefore, using our selection model, weights are constructed using the predicted probability of completing the survey, $\Phi(\hat{\beta}\mathbf{x}_i)$. The results from re-weighting the responses to the main questions on voluntary and automatic installation are given in Figure 4.B.3. Importantly, we see that the results are broadly consistent with the results presented in the main text.

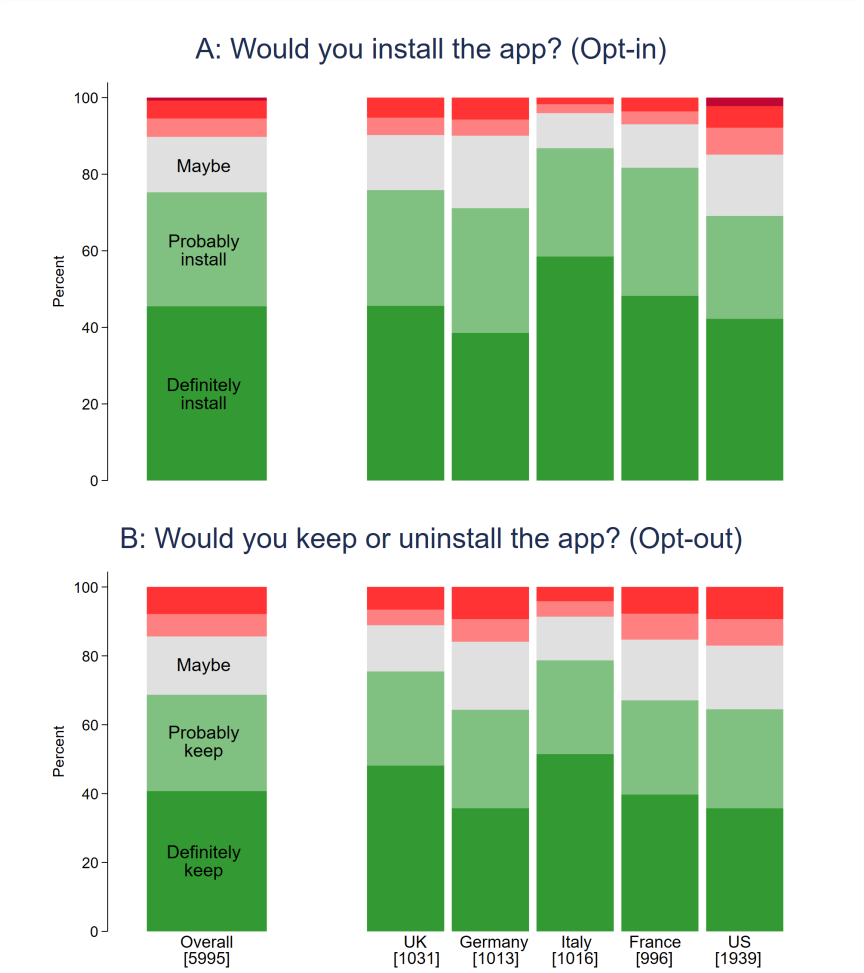
Finally, we also compared the demographic information we received from Lucid to the self-reported demographic information on gender and age. This allows us to check that respondents gave accurate information in our survey. 335 respondents (5.5%) gave at least one inconsistent answer. Removing these individuals has no effect on the results.

FIGURE 4.B.2: Selection Model



Note: The reference categories for the covariates are as follows: Female (Gender), 18-30 (Age), less than high school education (Education), Employed full-time (Employment). "Don't know", and empty answers are excluded. We also control for region fixed effects. Plotted points are the estimated model coefficients. Lines represent 95% confidence intervals calculated with heteroskedasticity-robust standard errors. Therefore the graph is predominantly helpful for assessing which demographic factors are statistically significant in predicting whether a respondent completes the survey.

FIGURE 4.B.3: Inverse Probability Weights of Staying in Sample



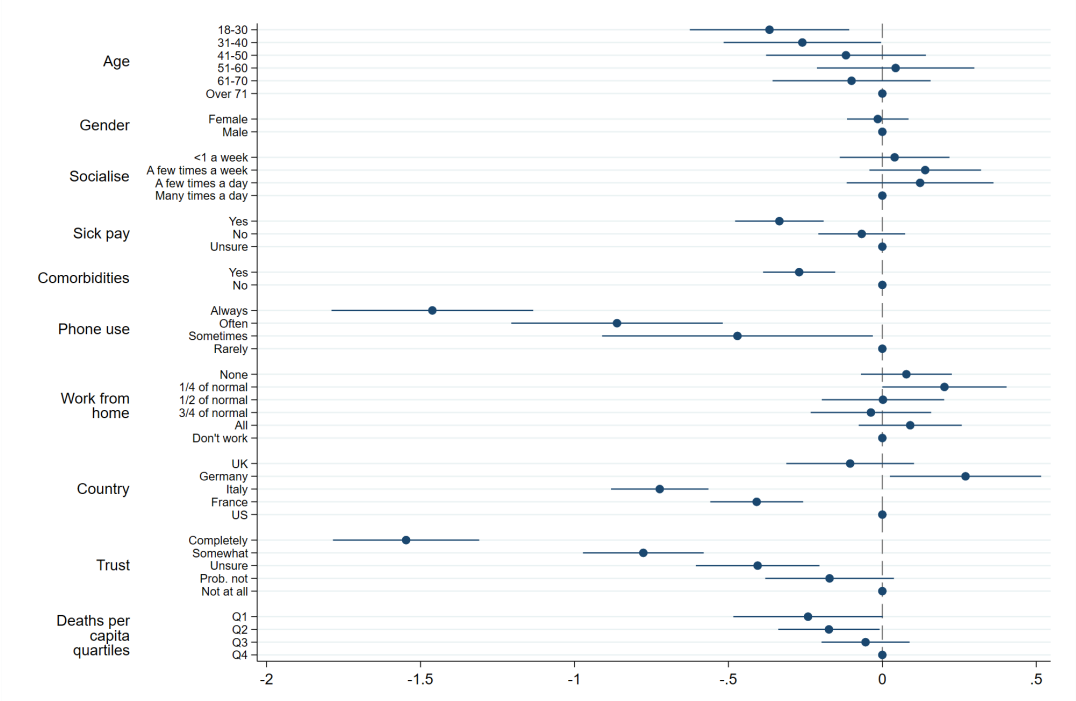
Note: In addition to the covariates shown in Figure 4.B.2, we also include region fixed effects, and country-specific education and employment coefficients. Light/dark red bars correspond to Probably/Definitely won't install (uninstall) in Panel A (B).

4.C Additional Results

4.C.1 Robustness to Modelling Assumptions

In the main part, we make a couple of modelling assumptions. Namely we use a Linear Probability Model and dichotomize the outcome measure by grouping everyone who said they would *probably or definitely install* the app into the "install" category (assigned value 1) while everyone else falls into the "non-install" category (with value 0). However, our results are robust to making different modelling assumptions as well. Figure 4.C.1 displays the results of the opt-in installation question if we model the entire choice space as an ordered logit where high numbers indicate a lower probability of installing the app, so that negative coefficients here have a similar interpretation to positive coefficients in the linear probability model in the main part. We see that the results look very similar to the ones obtained with the main specification (see Figure 4.3.2).

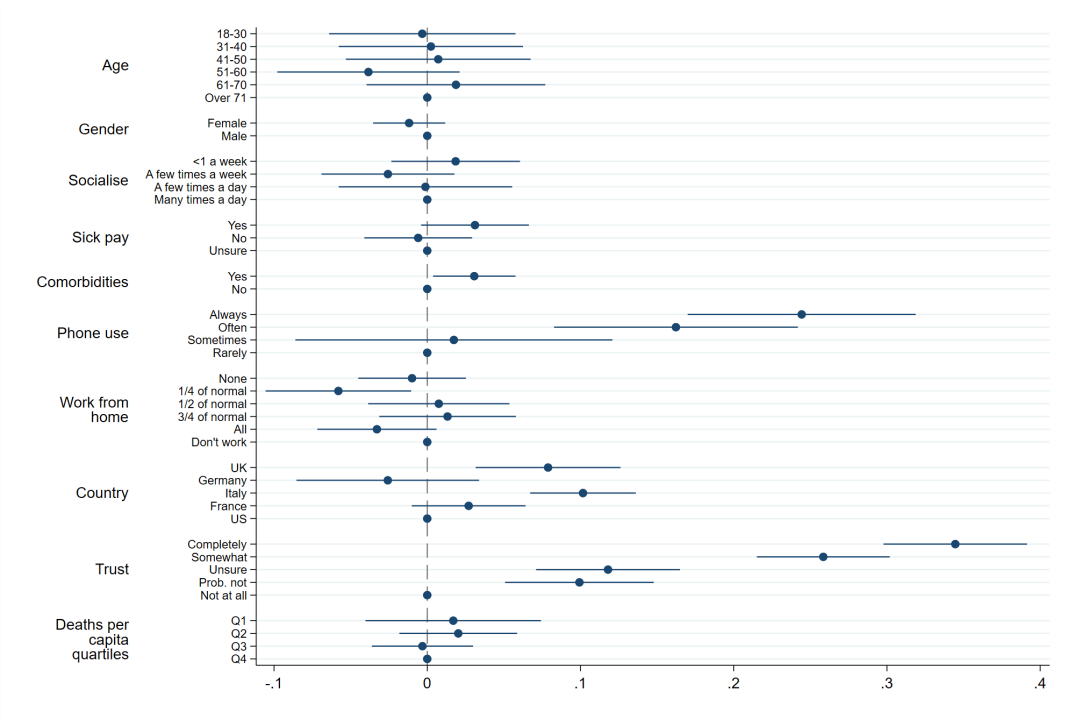
FIGURE 4.C.1: Ordered Logit Specification



Note: This figure replicates the analysis underlying Figure 4.3.2 using an Ordered Logit as opposed to a Linear Probability Model specification. Lines represent 95% confidence intervals calculated with heteroskedasticity-robust standard errors. All coefficients are the result of a single regression.

Furthermore, our results also hold when we sort only the people who say they would *definitely* (rather than definitely or probably) install the app into the install category – both using a linear probability and a probit specification. For brevity, we do not show these results here.

FIGURE 4.C.2: Determinants of Stating Definitely or Probably Keep



Note: This figure replicates the analysis of Figure 4.3.2 using the answers to the opt-out installation question as dependent variable. The dependent variable is an indicator variable taking the value 1 if a respondent chose *definitely keep* or *probably keep* when asked whether they would keep the app or not, and 0 otherwise. We use a Linear Probability Model. Lines represent 95% confidence intervals calculated with heteroskedasticity-robust standard errors. All coefficients are the result of a single regression and thus display marginal effects. A coefficient of 0.1 implies a respondent who chose this option is 10 percentage points more likely to state they would definitely or probably keep the app relative to the base category.

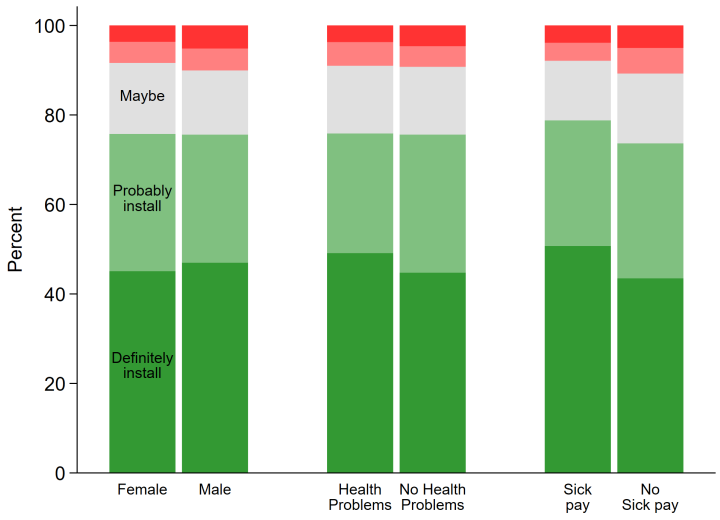
Just as we have looked at the covariates determining the choice to either install or not the app voluntarily (opt-in) in Figure 4.3.2, we can also look at the impact of different influencing factors on the decision to keep or immediately uninstall an automatically installed app (opt-out). Figure 4.C.2 displays the results if we once again dichotomise the outcome so that the dependent variable takes the value 1 if a respondent indicated that they would *probably or definitely keep* the app and 0 otherwise. We see that participants from the UK are relatively more likely to keep

the app in this scenario while French respondents are less likely in comparison. As with the opt-in installation outcomes, the results are robust to using an ordered logit specification as well as using a probit or linear probability specification when the "keep category" only consists of people saying they would *definitely keep* the app. For brevity, these robustness checks are omitted here.

4.C.2 Robustness across Demographics

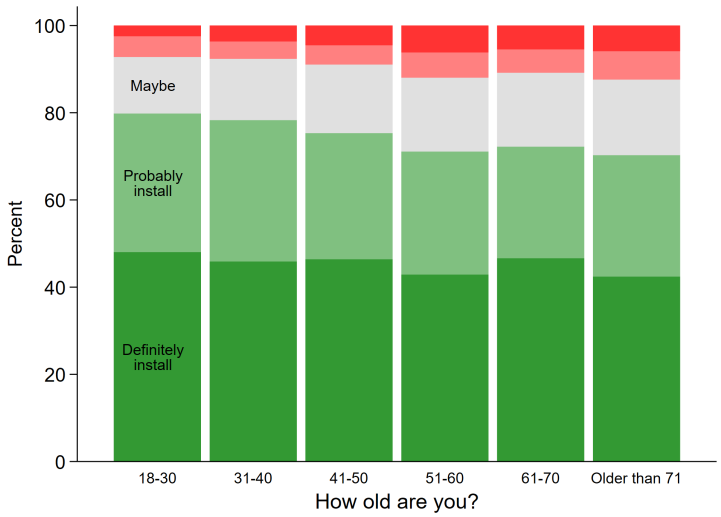
In the main text, we note that the results presented in Figure 4.3.1 hold across a variety of different demographic groups - this is illustrated in the following. Figure 4.C.3 shows that support for the app does not vary significantly when splitting outcomes by gender, the existence of comorbidities or the availability of sick pay. Figure 4.C.4 shows that there are only very small differences with regard to age. The only dimension where we see significant differences in support for the app is trust in government. Figure 4.C.5 shows that someone's propensity to install a contact-tracing app decreases the less they generally trust the government to "do what is right". The results of the opt-out scenario show the exact same pattern as the opt-in results. For brevity, we do not show them here.

FIGURE 4.C.3: Would You Install the App? (Opt-in): Comorbidities



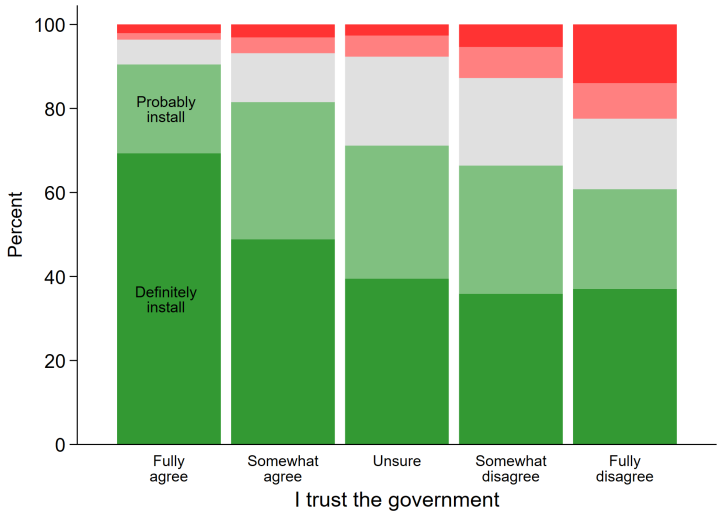
Note: The results here are based on the whole sample but split by the respondent's gender, reported comorbidities and access to sick pay. Light/dark red bars correspond to Probably/Definitley won't install.

FIGURE 4.C.4: Would You Install the App? (Opt-in): Age



Note: The results here are based on the whole sample but split by the age category the respondent reported. Light/dark red bars correspond to Probably/Definitley won't install.

FIGURE 4.C.5: Would You Install the App? (Opt-in): Trust in Govt



Note: The results here are based on the whole sample but split by the respondent's self reported trust in their national government. Light/dark red bars correspond to Probably/Definitley won't install.

4.C.3 Take Up and Disease Severity at the Geographical Level

We collect data on the number of infections as well as deaths due to COVID-19 at the *geographic area by day* level where the geographic area is at the same level of disaggregation as the respective survey asked for (e.g. state for the US). The data is publicly available on GitHub, and the sources are Santé Publique France (for France),

Protezione Civile (for Italy), the Robert Koch Institute (for Germany), PHE (for the UK), and the New York Times (for the US). We adapted the aggregation level of the French data from *département* to region.

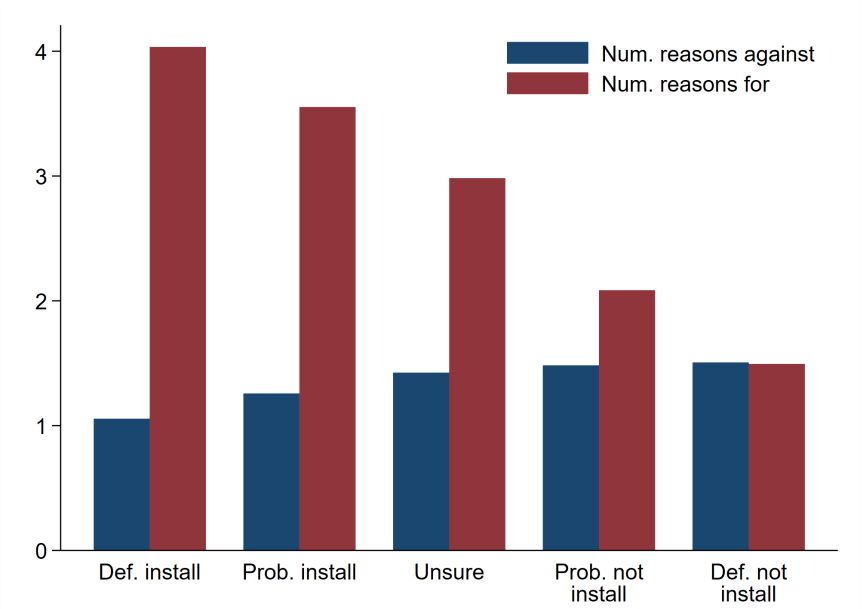
As can be seen in Figure 4.3.2 in the main part, we generally find only a very weak relationship between the severity of the outbreak (measured by deaths per capita as well as the absolute number of deaths) in someone's area of residence and the probability that they will want to download or keep the app. The exception to this rule are the very badly affected areas of New York and Northern Italy – here, respondents are about 10 percentage points more likely to state they would *definitely install* the app than the average participant. The fact that we do not find a strong effect generally could be due to a number of different factors. Firstly, death rates do not vary much across geographic areas once we remove New York and Northern Italy. Secondly, much of the rhetoric surrounding the severity of the disease is at the national, not sub-national level (again excluding New York and Northern Italy). Lastly, it is likely that the cartography of infections does not exactly follow the cartography of regions at our disposal.

4.C.4 Reasons for/against Installation and Take Up Decisions

Figure 4.C.6 shows that in general, far more reasons are given in favour of installing the app than against it – this is true even among those who state that they *probably or definitely won't install* the app. The number of reasons *for* installing the app decreases sharply as respondents become less likely to want to install it. This is not the case for reasons *against*, where most respondents select only one reason regardless of their intention to install. These findings suggest the need to better explain the various ways in which the app might benefit a user and his or her surroundings.

The reasons chosen most often by respondents across all countries in favour of and against installing the app are shown in Figures 4.C.7 and 4.C.8. While all reasons in favour of an installation appear to come somewhat evenly to people's minds, there is a strong clustering of reasons against the app around surveillance and security concerns as well as possible impacts on mental health. These reasons highlight the biggest concerns that would need to be addressed in the design and implementation of the app to make sure take-up would be sufficiently high. In addition to the common

FIGURE 4.C.6: Number of Reasons Given for/against the App, by Installation Preference



reasons displayed in Figures 4.C.7 and 4.C.8, we also gave a few answer options, which differed by country. In the UK, respondents were given "*Don't want the NHS to have access to my location data*" as an additional reason against, which was replaced by "*I don't want to activate Bluetooth*" in the other surveys. Germans in particular, were quite hesitant to activate Bluetooth, making it the third most popular reason against installation there. In the French, German, Italian and US surveys, respondents were given "*Would allow me to return to normal life faster*" as an additional reason in favour – however, it was not chosen by many people. Finally in the US survey the additional reason against "*No one else will use the app*" was the second most popular reason, given by 40% of respondents.

Figure 4.C.9 shows the impact each reason had on the probability with which a respondent stated they would probably or definitely install the app – controlling for all the covariates displayed in Figure 4.3.2. Unsurprisingly, the first seven reasons, which are in favour of the app, increase the probability of intending to install the app. Strikingly, out of the six reasons listed against installation (the latter half in the graph), only one had a large impact on installation decisions and that was "I would not benefit". This may indicate that while many people are concerned about government

FIGURE 4.C.7: Percentage of Respondents Who Choose Each Reason for Installation

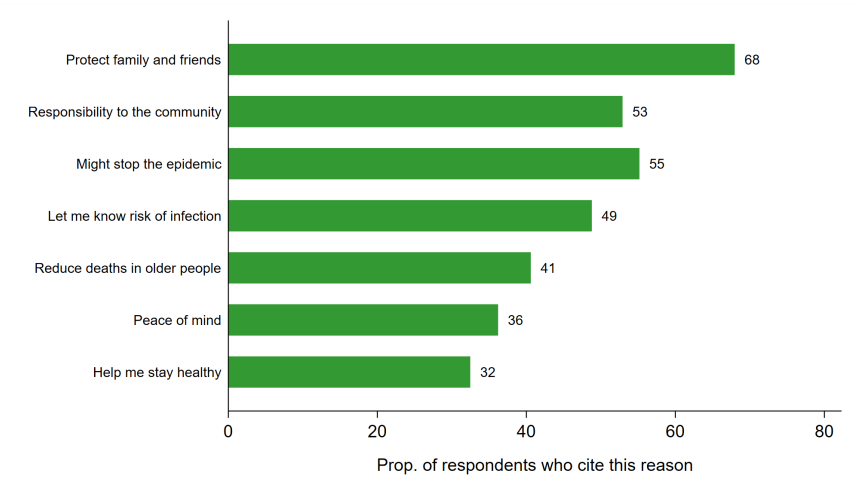
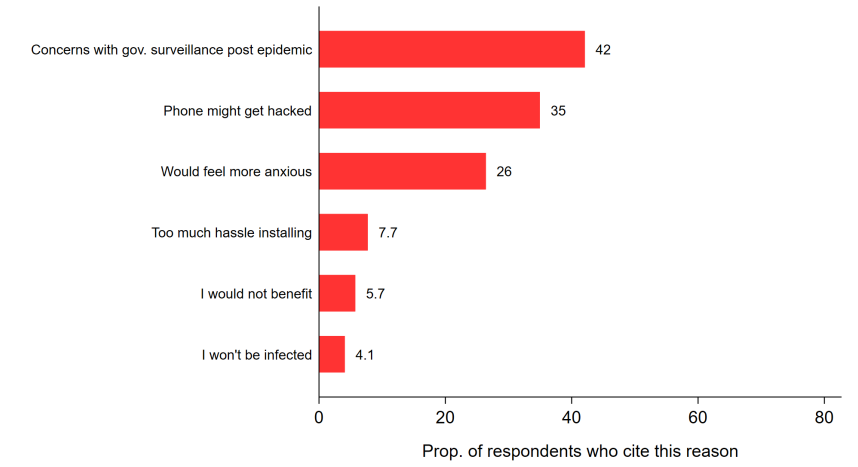
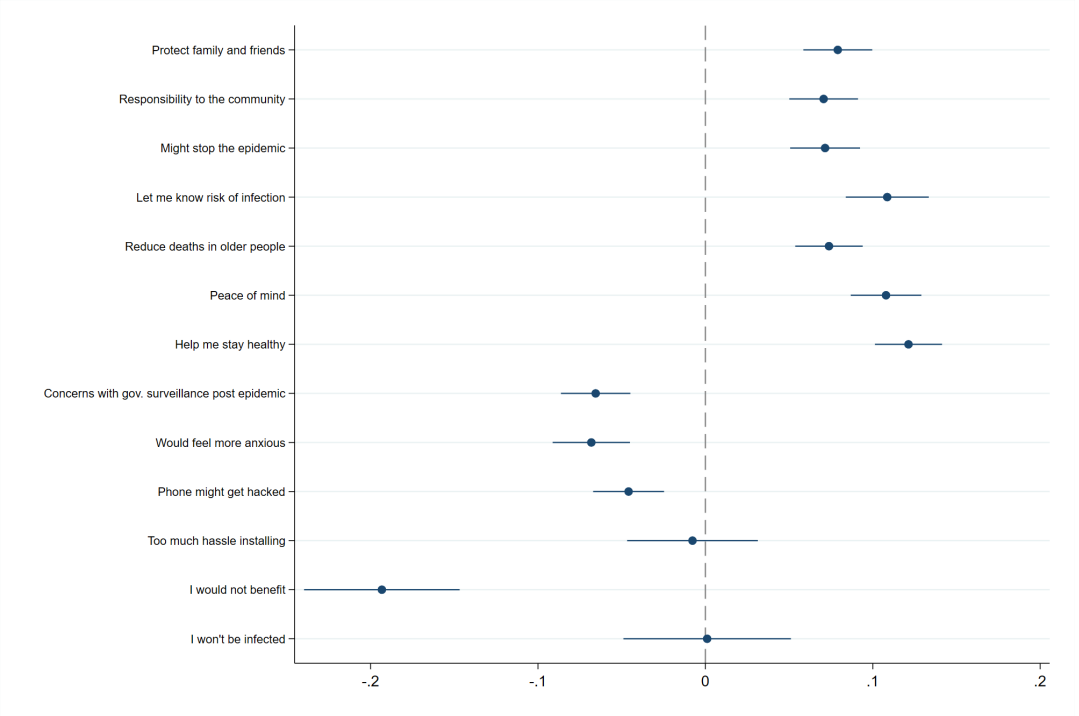


FIGURE 4.C.8: Percentage of Respondents Who Choose Each Reason against Installation



surveillance and cyber security, these reasons are not prohibitive of installing the app or only become so if they lead participants to conclude that the app would not be useful to them. We found similar results with an ordered logit model, a linear probability model dichotomising on just *definitely install*, when using a probit model and dropping the controls. Finally, the results are also qualitatively similar when we look at *opt-out* rather than opt-in decisions.

FIGURE 4.C.9: Impact of Reasons on Installation Probability

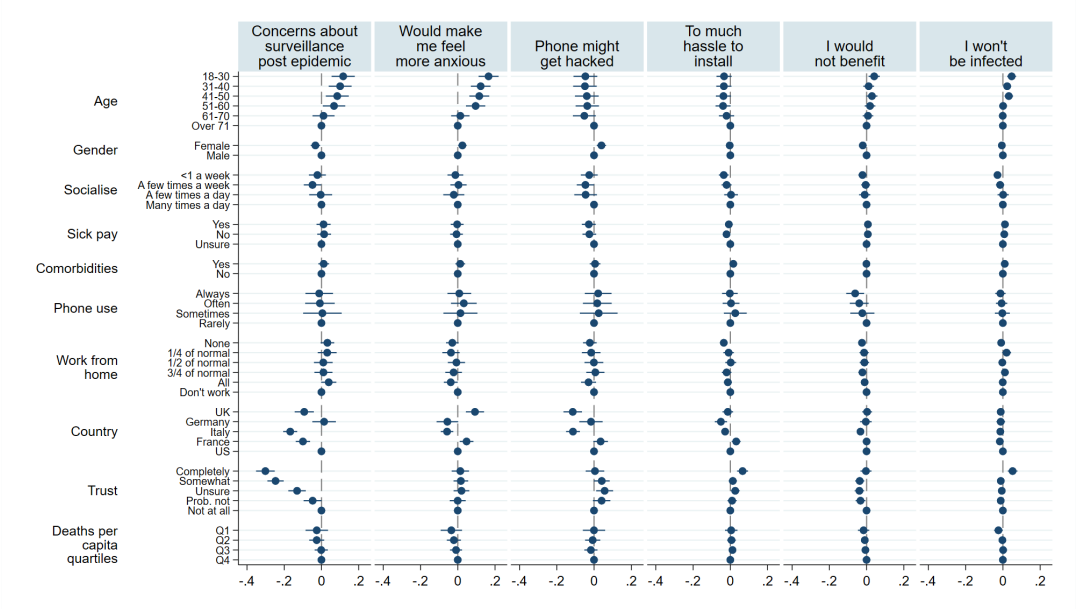


Note: The dependent variable is an indicator variable taking the value 1 if a respondent chose *definitely install* or *probably install* when asked whether they would install the app or not, and 0 otherwise. We use a Linear Probability Model. Lines represent 95% confidence intervals calculated with heteroskedasticity-robust standard errors. All coefficients are the result of a single regression and thus display marginal effects. A coefficient of 0.1 implies a respondent who chose this option is 10 percentage points more likely to state they would definitely or probably install the app.

4.C.5 Determinants of the Reasons Chosen

Figures 4.C.10 and 4.C.11 display the main determinants of reasons given for or against installing the app. Each column represents a separate linear probability model where the dependent variable takes the value 1 for individuals that chose that specific reason and the value 0 otherwise. The main takeaway from the graphs is that the reasons are largely not explained by the covariates. Only people who trust the government less as well as younger people are significantly more likely to name concerns surrounding post-epidemic surveillance as (one of) their main reasons against installation. Furthermore, younger people are less likely to cite *peace of mind* as a reason for installation. All results hold when using a probit model as well.

FIGURE 4.C.10: Determinants of Reasons Given Against Installation



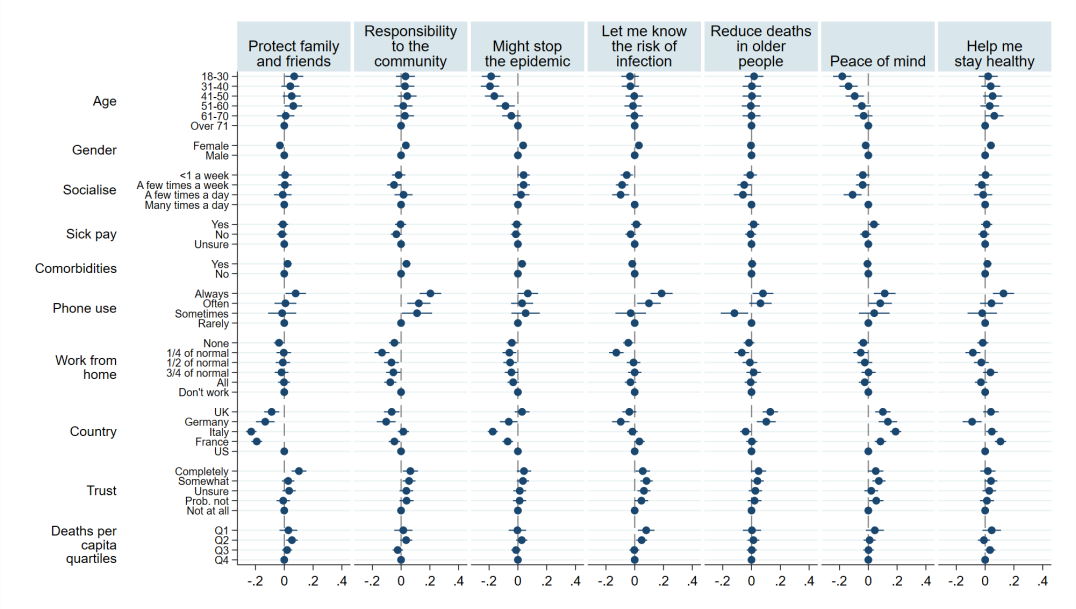
Note: Each column represents a separate Linear Probability Model with the dependent variable being an indicator variable taking the value one if a respondent gave the indicated reason. Lines represent 95% confidence intervals calculated with heteroskedasticity-robust standard errors. All coefficients within a column are the result of a single regression and thus display marginal effects. A coefficient of 0.1 implies a respondent who chose this option is 10 percentage points more likely to state the reason given.

4.C.6 Opt-in Versus Opt-out Preferences

Understanding whether an opt-in or an opt-out regime will yield a larger number of app users is crucial for the successful implementation of app-based contact tracing. Standard results from psychology suggest that many more people will keep the app in an opt-out regime than would install it voluntarily in an opt-in regime, as the former approach would reduce (mental) transaction costs and implicitly set having the app as the societal standard. However, Figure 4.3.1 shows that instead fewer people indicate they would keep the app rather than voluntarily download it. One reason for this may be that individuals, and in particular individuals who are concerned about potential government surveillance, may perceive automatic installation as an overreach by the government and thus choose to uninstall the app on principle.

To further understand respondents' preferences over an opt-in vs. opt-out regime, we directly asked US respondents which regime they would prefer. 60% would prefer voluntary to automatic installation. This fraction is constant across gender, region,

FIGURE 4.C.11: Determinants of Reasons Given for Installation



Note: Each column represents a separate Linear Probability Model with the dependent variable being an indicator variable taking the value one if a respondent gave the indicated reason. Lines represent 95% confidence intervals calculated with heteroskedasticity-robust standard errors. All coefficients within a column are the result of a single regression and thus display marginal effects. A coefficient of 0.1 implies a respondent who chose this option is 10 percentage points more likely to state the reason given.

political affiliation, lockdown status and other characteristics.

We can infer the preference between voluntary or automatic installation regime indirectly for the other four countries, where we did not ask directly for this preference. We use participants' intentions to install or keep the app in an op-in vs. opt-out regime to create an 'Intention measure'. We use participants' opinion of the national government in response to it introducing either regime, to create an 'Opinion measure'.

The opinion measure considers respondents' answers to the questions:

- How much do you agree with the following statement?

  "My opinion of the government would improve if they introduced the app, and allowed me to decide whether to install it or not"

- How much do you agree with the following statement?

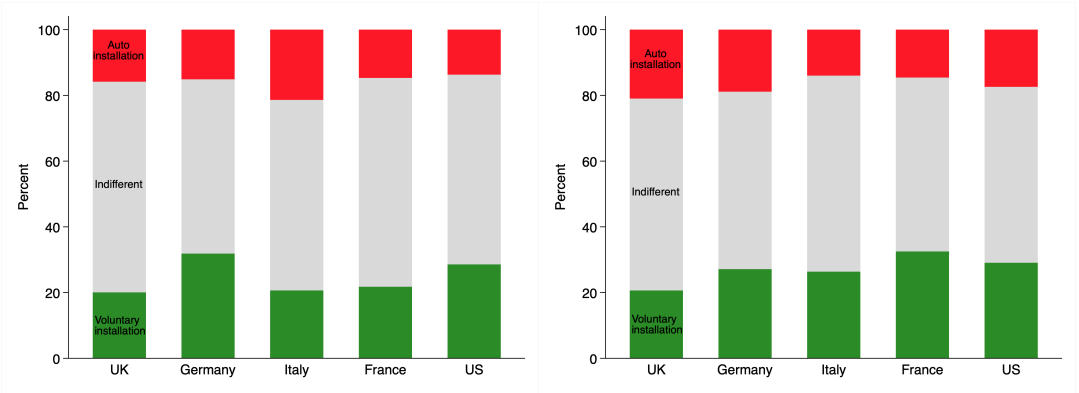
  "My opinion of the government would improve if they asked mobile phone providers to automatically install the app, and allowed me to decide whether to keep it or not"

The difference between respondents' answers to these two questions yields information about which regime they would prefer the government to introduce. Therefore, we create a variable that gives the difference between these two answers, such that when participants' opinion of the government would improve more (or worsen less) under the opt-in regime, this difference is negative. In this case, we would infer that they prefer an opt-in regime.

The Intention measure considers respondents' answers to the "Would you install this app?", and "Would you keep this app if it was automatically installed" questions. The difference between respondents' answers to these questions reflects their different intentions of having the app on their phone in either regime. Thus, this difference is again indicative of participants' preferences over the opt-in/opt-out regimes. As for the Opinion measure, when the Intention measure is negative we can infer that a respondent believes they would be more likely to have the app installed on their phone in an opt-in regime, which we take to mean that they prefer the opt-in regime.[6]

For both difference measures, we re-code the new variable into three categories: $d < 0$ (prefer opt-in), $d = 0$ (indifferent), and $d > 0$ (prefer opt-out). The differences in preferences, across countries, are displayed in Figure 4.C.12. The trends are broadly in-keeping with the other results.

FIGURE 4.C.12: Opt-in/Opt-out Preferences in Each Country



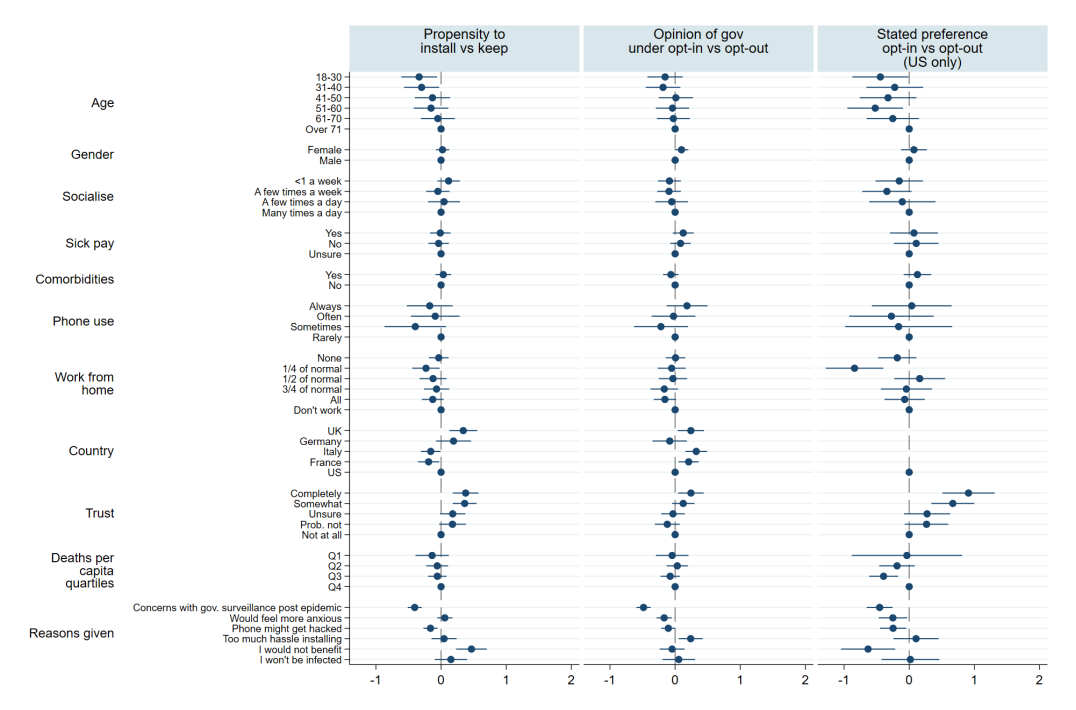
(A) Opinion Measure (B) Intention Measure

To investigate the factors that are associated with opt-in/opt-out preferences, we use a simple ordered logit model. The results from this analysis are presented in Figure 4.C.13. Respondents who express less trust in the government or who worry

[6]The validity of using these inferred preferences is considered in the Appendix subsection 4.C.6.

about government surveillance or their phone being hacked, are more likely to have a preference for the opt-in over the opt-out regime.

FIGURE 4.C.13: Determinants of Opt-in vs. Opt-out Preference



Note: Positive coefficient means prefer opt out. Ordered logit model. The points show estimated model coefficients, while the lines show 95% confidence intervals, calculated using heteroskedasticity-robust standard errors.

Validity of Difference Measures

In the US survey, respondents were asked directly whether they would prefer an opt-in or opt-out regime. The below cross tabulation demonstrates a strong relationship between respondents' stated choice (columns) and their inferred preference (rows), using the Opinion Measure. Only 10.4% of respondents' inferred preferences are inconsistent with their stated preferences - those who selected $d < 0$ and Automatic, or $d > 0$ and Voluntary. Of the respondents for which the Opinion measure is negative ($d < 0$), 80.4% stated that they prefer the Voluntary regime. Likewise, of the respondents for which the Opinion measure is positive ($d > 0$), 64.7% stated that they prefer the Automatic regime.

	US respondents	
	Voluntary	Automatic
$d < 0$	445	109
$d = 0$	564	555
$d > 0$	94	171

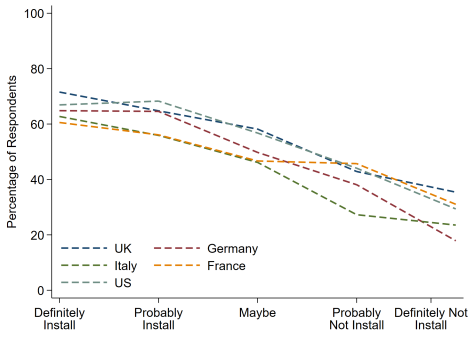
4.C.7 Data Usage

Respondents were asked for their preferences over what should happen to the data generated by the app. Respondents could state that they wanted the data immediately deleted, or that the (de-identified) data should be made available to researchers.

In the final sample, 59.9% of respondents answered that the de-identified data should be made available to researchers. Figure 4.C.14 demonstrates that respondents who were less likely to say they would download the app were also less likely to want their data to be made available to researchers. Surprisingly, Figure 4.C.14 demonstrates no relationship between trust in the government and data-sharing preferences. However, once we control for other covariates, this relationship does become statistically significant, as can be seen in Figure 4.C.15. This relationship is nevertheless non-monotonic and hence difficult to interpret. Figure 4.C.15 also demonstrates that older people, people who use their phones more regularly, and residents of the UK are more likely to consent to having their data shared.

FIGURE 4.C.14: Percentage of Respondents Wanting the Data to Be Made Available

(A) By intention to install



(B) By trust in government

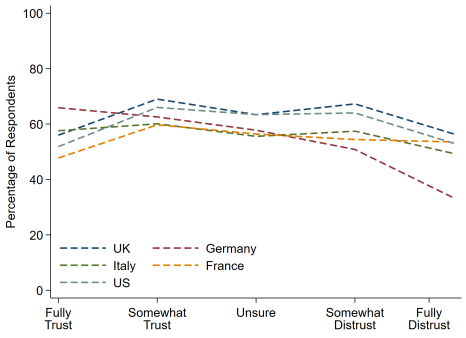
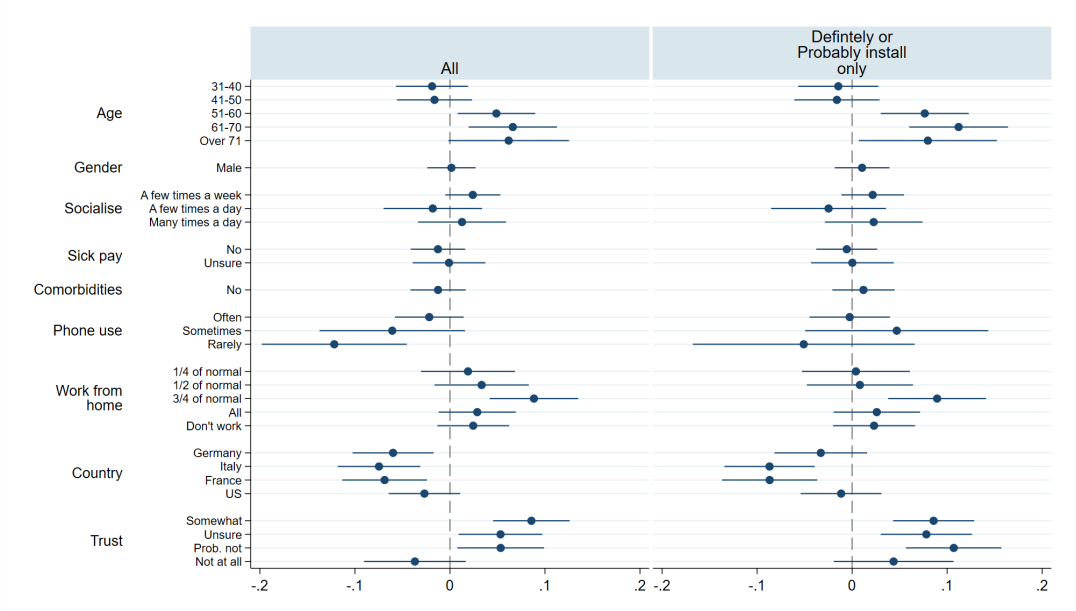


FIGURE 4.C.15: Determinants of Wanting the Data to Be Made Available



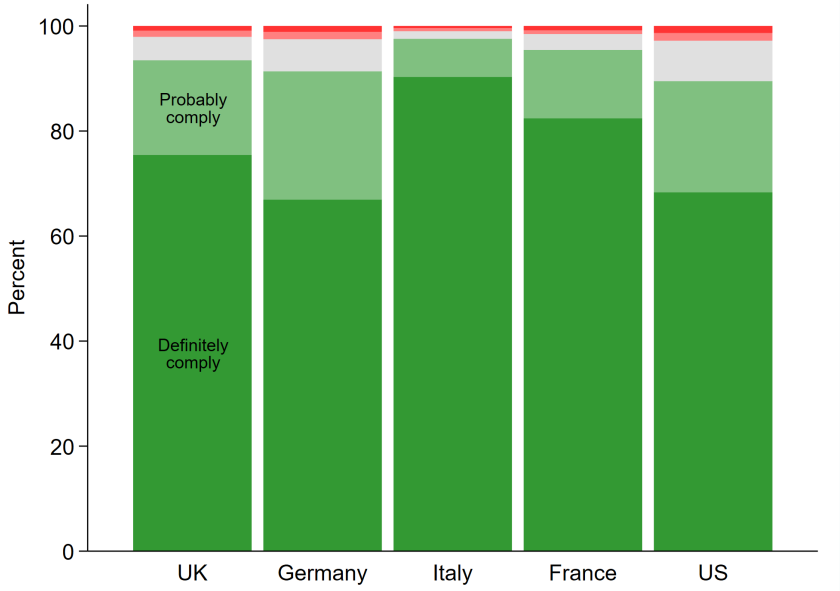
Note: Computed using a Linear Probability Model. The dependent variable is a dummy variable taking the value 1 if the respondent prefers the data be de-identified and made available, 0 otherwise. Lines represent 95% confidence intervals calculated with heteroskedasticity-robust standard errors. All coefficients within a column are the result of a single regression and thus display marginal effects. A coefficient of 0.1 implies a respondent who chose this option is 10 percentage points more likely to state they prefer the data be made available. The right hand column contains answers only from respondents who answered that they would definitely or probably install the app. The reference categories for the covariates are as follows: 18-30 (Age), Female (Gender), less than once a week (Socialise), receive sick pay (Sick Pay), none (work from home), UK (country), and completely (Trust)

4.C.8 Compliance with the App Self-Isolation Request

We asked respondents how likely they would be to comply with the request of self-isolating for 14 days if they had been in close contact with a person who was confirmed to be infected. Responses were collected on a 5-item scale ranging from *Definitely comply* to *Definitely won't comply*. As shown in Figure 4.C.16, the vast majority of respondents in all countries said they would comply with the self-isolation request. Support is again highest in Italy, where 96% of respondents declared they would

definitely or *probably comply*, and lowest in Germany, at a still very high 89%. We further asked respondents who did not say they would definitely comply, whether their chances of compliance would increase, decrease, or remain the same if the health services in their own country committed to test them quickly. In all countries, a commitment to quick testing would further increase compliance rates. This implies that the vast majority of people in all five countries are not only prepared to have the app installed on their phones, but also to use it as intended, even if this means sacrificing (more of) their personal freedom for a limited amount of time.[7]

FIGURE 4.C.16: Compliance with Isolation Request



4.C.9 Effect of Comprehension

In the German replication survey, conducted via Forsa (see Section 4.B.3), we collected all answers also for those respondents who failed to answer the comprehension questions correctly. For this survey, we can thus compare the willingness to install between respondents who did (N=1048) and who did not (N=149) answer all comprehension questions correctly. We find that 69.0% of the respondents who answered the comprehension questions correctly would definitely or probably install the app. In contrast, only 56.4% of the respondents who answered wrongly would do so. This

[7] As this survey asks hypothetical questions, we should caveat that *actual* compliance might be considerably lower.

might be because the respondents who failed the comprehension questions answered randomly. Or it could be that these respondents are actually less willing to install.

The latter interpretation is consistent with previous literatureKeusch et al. [2019] that showed a higher willingness to share data and information among the more technically informed users. A lack of understanding leading to a lower willingness to install would also be in line with the results of surveys that were conducted in Germany shortly after our survey. Some of these surveys did not explain the app in as much detail as our survey and these surveys often find lower willingness to install. For example, infratest Dimap asked respondents between 30 and 31 March 2020 about their willingness to install a contact-tracing app, and found that only 47% of respondents were willing to install the app.[8] The comparison between our surveys and these shorter surveys suggests that a detailed explanation of how the app works and how it could mitigate or stop the epidemic increases the willingness to install.

[8]The exact question (translated from German) was: "Assume that there is an app for your mobile phone that would allow you to track your COVID-19 symptoms as well as document your geolocation data. The app could thus show all users whether they had been close to a person infected with the coronavirus. Would you use such an app or not?"

Concluding Remarks

This thesis asks how uncertainty, and in particular uncertainty over future states of the world, shapes forward looking, dynamic choices. To this end, it theoretically and empirically explores four different contexts: the saving choices of migrants who face uncertainty over their future right to stay and thus location; investments into children's socio-emotional skills when their salience is low; political campaigning for democratic norms in situations when they might be ambiguous; and finally the adoption of health care tools when the associated costs and benefits are uncertain. In different settings, uncertainty over states of the world renders individuals more hesitant to make long term investments, often at the cost of current utility, or publicly commit to what might be a costly action. This cautiousness is rational a priori, but can render individuals worse off once the uncertainty has been resolved. Thus, it can exacerbates existing inequalities, for example, between migrants and natives or children from different socioeconomic backgrounds. It can also dissuade people from adopting a potentially advantageous behaviour in situations where there are strategic inter-dependencies, for example in the sphere of policy engagement and public health strategies. In both cases, dynamic effects exacerbate the initial impact, creating perpetuating cycles of inequality and non-adoption.

However, this thesis also provides evidence for the pronounced effect policies can have in offsetting the detrimental impact of uncertainty. Giving migrants access to citizenship fully closes the part of the saving rate gap between them and natives accruing to uncertainty over future right to stay. Giving kids formal feedback on their work ethic and social conduct in primary school narrows gaps in socio-emotional skills even ten years out. And targeted information campaigns appear to go a long way in both stimulating dialogue about democratic norms and uptake of health care tools. This suggests that policies which take the uncertainty many people have to factor

into their dynamic choices seriously can go a long way in an effort to reduce existing inequalities and improving outcomes for everyone.

Finally, this thesis opens up a variety of avenues for future research. While the impact of uncertainty over future resources is well studied and established, uncertainty over future states of the world has not received the same attention. This thesis documents how it can impact forward looking choices in the settings described above – yet, there are numerous other situations where accounting for uncertainty over future states of the world may help us find better answers to economic questions. Take for example employment uncertainty. An individual who is worried about losing their job in the city might be concerned about a drop in income and subsequently hold higher precautionary savings. However, if at the same time this person also worries about having to move back to their hometown if they were to become unemployed, then this second consideration might actually prompt them to save *less* in anticipation. This is because, if we assume that the individual moved to the city because they prefer its amenities, moving back home might mean deriving lower levels of marginal utility from consumption – decreasing their incentives to save. In a similar way, uncertainty over family status likely influences how singles and couples prepare for their future, as they might value consumption differently depending on whether or not they are on their own, in a relationship, have children, etc. Thus, accounting for uncertainty over future states might inform analyses on topics ranging from marriage markets to divorce risk and proceedings. Analogous arguments could be made for uncertainty over future health status, geographic amenities or the development of the climate, influencing a variety of dynamic economic choices. With this in mind, this thesis is intended not as an endpoint, but a beginning of an exploration of these questions.

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