

DISCUSSION PAPER SERIES

IZA DP No. 12175

**The Political Economy of Higher Education
Finance: How Information and Design
Affect Public Preferences for Tuition**

Philipp Lergetporer
Ludger Woessmann

FEBRUARY 2019

DISCUSSION PAPER SERIES

IZA DP No. 12175

The Political Economy of Higher Education Finance: How Information and Design Affect Public Preferences for Tuition

Philipp Lergetporer

ifo Institute at the University of Munich and CESifo

Ludger Woessmann

University of Munich, ifo Institute, CESifo, IZA and CAGE

FEBRUARY 2019

Any opinions expressed in this paper are those of the author(s) and not those of IZA. Research published in this series may include views on policy, but IZA takes no institutional policy positions. The IZA research network is committed to the IZA Guiding Principles of Research Integrity.

The IZA Institute of Labor Economics is an independent economic research institute that conducts research in labor economics and offers evidence-based policy advice on labor market issues. Supported by the Deutsche Post Foundation, IZA runs the world's largest network of economists, whose research aims to provide answers to the global labor market challenges of our time. Our key objective is to build bridges between academic research, policymakers and society.

IZA Discussion Papers often represent preliminary work and are circulated to encourage discussion. Citation of such a paper should account for its provisional character. A revised version may be available directly from the author.

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Political Economy of Higher Education Finance: How Information and Design Affect Public Preferences for Tuition*

Public preferences for charging tuition are important for determining higher education finance. To test whether public support for tuition depends on information and design, we devise several survey experiments in representative samples of the German electorate (N>19,500). The electorate is divided, with a slight plurality opposing tuition. Providing information on the university earnings premium raises support for tuition by 7 percentage points, turning the plurality in favor. The opposition-reducing effect persists two weeks after treatment. Information on fiscal costs and unequal access does not affect public preferences. Designing tuition as deferred income-contingent payments raises support by 16 percentage points, creating a strong majority favoring tuition. The same effect emerges when framed as loan payments. Support decreases with higher tuition levels and increases when targeted at non-EU students.

JEL Classification: I22, H52, D72, D83

Keywords: tuition, higher education, political economy, survey experiments, information, earnings premium, income-contingent loans, voting

Corresponding author:

Ludger Woessmann
Ifo Institute for Economic Research at the University of Munich
Poschingerstr. 5
81679 Munich
Germany
E-mail: woessmann@ifo.de

* This paper has been replaced by two separate new and substantially expanded papers – one on earnings information, one on income contingency – which are available as IZA Discussion Papers 14386 and 14991, respectively. For valuable comments and discussion, we would like to thank Nick Barr, Eric Bettinger, Leonardo Bursztyn, Sue Dynarski, Achim Goerres, Rick Hanushek, Juanna Joensen, Wilbert van der Klaauw, Volker Meier, Richard Murphy, Ricardo Perez-Truglia, Randall Reback, Lisa Simon, Marty West, and seminar participants at Harvard, Stanford, Ohio, Prague, Munich, Konstanz, Freiburg, Karlsruhe, Nuremberg, Ifo Institute, the FRBNY/NYU education seminar, the CESifo education meeting, the IZA workshop on education, interventions and experiments, and the annual meetings of SOLE in Raleigh, EEA in Mannheim, the German Economic Association in Augsburg, and its economics of education group in Bamberg. We are grateful to Elisabeth Grewenig, Sarah Kersten, Franziska Kugler, Laura Oestreich, and Katharina Werner for their collaboration in designing and executing the surveys. This study is registered in the AEA RCT Registry as AEARCTR-0003873. Financial support by the Leibniz Competition (SAW-2014-ifo-2) and the German Science Foundation (CRC TRR 190) is gratefully acknowledged. This paper has been replaced by two separate new and substantially expanded papers – one on earnings information, one on income contingency – which are available as IZA Discussion Papers 14386 and 14991, respectively.

1. Introduction

The question whether higher education should be free or whether students should pay tuition creates strong political tensions. Politicians in countries with long traditions of tuition suggest making higher education free of charge, and countries without tuition see attempts to introduce it. For example, in the last U.S. presidential election campaign, Democratic candidate Bernie Sanders created substantial public attention by campaigning for tuition-free colleges. In traditionally tuition-free Germany, seven of the sixteen states introduced tuition in 2006/07. Clearly, whenever governments try to introduce or raise tuition, there is substantial public opposition. Plans for tuition raises in England in 2010 triggered a wave of student unrest, and proposed tuition hikes regularly spark campus protests in the United States.¹ In Germany, four of the seven state governments that had introduced tuition got voted out of power in the next election, and all abandoned tuition within a few years. In political debates, proponents argue that charging tuition provides universities with resources necessary to maintain quality and make future well-offs pay for part of the services they receive. By contrast, opponents argue that tuition undermines access and equity in tertiary education. The fiscal, human capital, and equity implications of charging tuition have been studied extensively in the economics literature (e.g., Lovenheim and Turner, 2017).

The public opinion battles over tuition indicate, however, that the feasibility of reforming higher education finance ultimately raises an important political economy question: What determines political majorities for or against charging tuition? In this paper, we study the extent to which two groups of factors affect the electorate's support for tuition: factual information about central aspects of higher education and the design of the tuition scheme.

Recent German history provides a prototype of the political economy conflicts that surround tuition. After the Constitutional Court had declared a federal ban on tuition unconstitutional in 2005, nearly half the states introduced tuition of mostly 500 Euro per semester (see section 2.2 for details). By the end of 2014, however, all of them have abolished tuition again in response to an apparently overwhelming public opposition. In fact, though, little is known about the extent to which the public at large – as opposed to special interest groups – favors or opposes tuition and how these public preferences are shaped.

¹ For example, one protest against plans to raise tuition in England drew 50,000 students in November 2010 (BBC, 21 November 2012, <http://www.bbc.com/news/education-20412792> [accessed 9 October 2018]). Over time, there have been many campus protests across colleges in the United States against proposed tuition hikes (e.g., Washington Post, 20 November 2014, <http://www.washingtonpost.com/news/post-nation/wp/2014/11/20/university-of-california-students-protest-tuition-hikes-a-look-at-some-campus-protests-over-the-years/> [accessed 9 October 2018]), and observers increasingly raise concerns over soaring student debt (e.g., Oreopoulos and Petronijevic, 2013).

To elicit the electorate's preferences for tuition, we conducted five annual representative surveys (2014-2018) of the German voting-age population with over 19,500 observations in total. To study determinants of the public's preferences for tuition, we implemented a series of randomized experiments in the surveys.

The first set of experiments tests the effects of correcting potentially biased beliefs about different economic aspects of the higher education system on public support for tuition.² Our main focus is on studying the effects of randomized information provision about the earnings differential between persons with and without a university degree. We subject this experimental treatment to several tests of robustness and replicability and study persistence in a follow-up survey about two weeks after the information treatment. Additional experimental treatments provide information on the overall public cost per university student and on unequal access to university education by family background.

The second set of experiments investigates the effects of the design of the tuition payment scheme on public preferences. Our main focus is on whether public support for tuition depends on whether payments are due while attending university or deferred until students earn sufficient income. Our experimental treatment allows us to compare preferences for regular upfront tuition to preferences for income-contingent tuition that is due after graduation and has to be paid only if income exceeds a certain threshold.³ We again test for replicability of results, as well as for robustness to framing income-contingent tuition as loan payments. We also complement our results for the voting-age population with a separate survey of adolescents, the highly relevant interest group of potential future university students. Public opposition may not only focus on the basic principle of charging tuition, but also on its level. Another design experiment thus varies tuition levels across experimental groups to study the public's responsiveness towards the level of tuition. A final experiment compares preferences for universal tuition versus tuition for non-EU citizens to test the extent to which public support is affected by which group of students is targeted.

² Previous evidence suggests that people are ill-informed about relevant aspects regarding university tuition (e.g., Bettinger et al., 2012; Hoxby and Turner, 2015; McGuigan, McNally, and Wyness, 2016).

³ Economic research generally indicates positive equity and efficiency aspects of income-contingent tuition (e.g., Chapman, Higgins, and Stiglitz, 2014; Findeisen and Sachs, 2016; Stantcheva, 2017; Diris and Ooghe, 2018; Barr et al., 2019), which is currently in place in Australia, England, and New Zealand. A related concept – income share agreements where tuition is paid as a percentage of later income – is gaining ground also in the U.S. It has been adopted, for example, by Purdue University and by an online learning start-up that recently raised major funding by leading venture capitalists (see New York Times, 8 January 2019, <https://www.nytimes.com/2019/01/08/business/dealbook/education-student-loans-lambda-schools.html> [accessed 16 February 2019]). However, the topic never took center stage in the German tuition debate.

Our results indicate that the German public is strongly divided over whether or not to charge tuition. In the control groups of the five survey waves, 45-46 percent of the population oppose tuition, whereas 40-47 percent favor it (the remainder being indifferent). In all but one wave, a slight public plurality is against charging tuition.⁴

The key result of the information experiments is that information about the university earnings premium turns the plurality opposing tuition into a strong plurality in favor. When respondents are informed about the university earnings premium, support for tuition increases by 6.9 percentage points and opposition decreases by 8.2 percentage points, rendering 49.6 percent of the electorate in favor and 37.8 percent opposed. Public preferences for tuition are thus partly based on imperfect relevant information. We replicate the effect with the same magnitude in three different representative samples and show that it is insensitive to whether the earnings information is presented in relative or absolute terms. The treatment effect persists in a follow-up survey about two weeks after the information was provided. Our analysis of baseline beliefs (elicited prior to the experiment) suggests that the treatment effect operates through correcting downward-biased beliefs about the earnings differential.

The additional information experiments do not significantly affect public preferences for tuition. Information about the public costs of university does not change support for or opposition to tuition. The same is true for information about unequal access to university.

The key result of the design experiments is that designing tuition as deferred income-contingent payments creates a strong majority favoring tuition. Replacing the regular upfront design by a deferred income-contingent design raises support for tuition by 15.7 percentage points and reduces opposition by 17.7 percentage points. 60.3 percent of the electorate favor income-contingent tuition and only 28.2 percent are opposed. This finding again replicates in three different representative samples. In addition, it is robust to framing this form of tuition as income-contingent student loans, indicating that results are not affected by loan aversion.

Additional results of the design experiments reveal sensitivity of public preferences to the level and target group of tuition. Specifying a tuition level of 500 Euro per semester – the amount charged in Germany before tuition was abolished – does not significantly change preferences compared to the control group for whom the level was not specified. But specifying a tuition level of 1,500 Euro per semester reduces support by 11.7 percentage points, creating a majority opposed to tuition. Finally, in contrast to general tuition, a strong

⁴ The only exception is the final wave (2018), where 47 percent favor tuition and 45 percent oppose it. While support is roughly constant in 2015-2017, it is slightly (but not significantly) lower in 2014 and slightly (but not significantly) higher in 2018. Opposition against tuition is very flat, and the undecided group decreases slightly.

majority supports tuition for non-EU citizens. Overall, the findings highlight the critical role of specific design features of tuition for shaping its acceptability in the public.

The generality of our results is corroborated by analyses of effect heterogeneity which show that treatment effects vary little across sociodemographic subgroups. From a political economy perspective, it is noteworthy that treatment effects tend to be stronger for politically active respondents such as frequent voters. While interest groups with pecuniary self-interest – i.e., (potential) students and parents – are generally less supportive of tuition, they exhibit similar reactions to most treatments as the general population. Among adolescents, a majority opposes regular tuition, but not deferred income-contingent tuition. Taken together, our results imply that policy makers have some leeway to reform higher education finance if (i) the public is being informed about relevant underlying facts and (ii) the tuition scheme is designed adequately in terms of income contingency, tuition levels, and target group.

Our paper contributes to several strands of economics literature. It is closely related to the normative⁵ and positive political economy literature on education finance (e.g., Fernandez and Rogerson, 1995; Epple and Romano, 1996; Soares, 2003; Glomm, Ravikumar, and Schiopu, 2011) and, in particular, to papers that focus on higher education (e.g., De Fraja, 2001; Borck and Wimbersky, 2014).⁶ The latter studies investigate voter majorities for alternative systems of higher education finance, such as traditional subsidy schemes or income-contingent loans, and how they are affected by changes in household characteristics such as income, ability, and risk preferences. In contrast to the large body of theoretical literature on the political economy of higher education finance, empirical evidence is scarce. While some empirical papers relate political processes to university funding (e.g., Lowry, 2001), we are unaware of existing experimental evidence on what determines public support for tuition. We also extend the political economy literature by emphasizing the role that information plays in shaping the electorates' preferences on higher education finance.

While this paper is on the political economy of university tuition, it is also linked to the large literature that investigates the effects of tertiary education costs on student outcomes. For example, Dynarski (2003), Garibaldi et al. (2012), and Murphy, Scott-Clayton, and Wyness (2019) provide evidence from the United States, Italy, and England, respectively.

⁵ From a normative perspective, most economists seem to favor some degree of user financing in higher education (e.g., Wigger and Weizsäcker, 2001; Barr, 2004; Johnstone, 2004; Jacobs and van der Ploeg, 2006; Diris and Ooghe, 2018), but we do not take a normative stance here. Our research interest is purely positive, concerning the determinants of electoral majorities towards tuition. Still, our results show that income-contingent tuition, often advocated in the normative literature, is supported by a solid majority of the German population.

⁶ A further theoretical literature studies the efficiency and distributional properties of different higher education finance schemes (e.g., Bovenberg and Jacobs, 2005; Findeisen and Sachs, 2016; Stantcheva, 2017).

Helbig, Baier, and Kroth (2012), Hübner (2012), Dwenger, Storck, and Wrohlich (2012), and Thomsen and von Haaren-Giebel (2016) investigate the effects of the above-mentioned reforms in Germany. In contrast to public concerns, these studies suggest that the effects of tuition on average student outcomes or equity were rather limited in Germany.

Finally, this study relates to the growing economics literature that uses survey experiments to study determinants of educational choices (e.g., Wiswall and Zafar, 2015; Bleemer and Zafar, 2018; Abraham et al., 2018) and of public policy preferences in different areas (e.g., Cruces, Perez-Truglia, and Tetaz, 2013; Kuziemko et al., 2015; Bursztyn, 2016; Haaland and Roth, 2017; Alesina, Stantcheva, and Teso, 2018; Lergetporer et al., 2018; Roth and Wohlfart, 2018). We extend this literature by applying the methodology of survey experiments to the political economy of higher education finance.

The paper is structured as follows. Section 2 provides conceptual background, as well as institutional background on higher education finance in Germany. Section 3 describes our dataset and the experimental design. Section 4 presents our results on information provision, with a particular focus on earnings information. Section 5 presents our results on design features, with a particular focus on income-contingent tuition. Section 6 provides subgroup analyses, with a particular focus on special interest groups and politically active respondents, as well as additional results of an adolescent survey. Section 7 concludes.

2. Background

We start by providing conceptual background on the political economy of financing higher education and institutional background on tuition in Germany, arguing that Germany provides an interesting case to study public opinion on whether higher education should be free or not.

2.1 The Role of Information and Design in Public Support for Tuition

The political economy of higher education finance. Higher education can be financed either publicly by governments or privately by students themselves (or by any combination of the two). In practice, the extent to which students have to pay for attending university differs immensely around the world. While public universities in some countries such as Japan, the United Kingdom, and the United States charge annual tuition of 5,000 dollars and more on average, attending university is totally free of charge in the public university systems of all Nordic countries and several other continental European countries (OECD, 2018).⁷

⁷ The average annual tuition charged by public or government-dependent institutions for bachelor programs in 2015/16 is 5,218 US dollars in Japan, 11,062 dollars in the United States, and 11,797 dollars in the United

Whether to charge tuition for attending higher education or not is determined in the political process. In fact, in many countries there are fundamental debates about whether college should be free of charge or not (see Murphy, Scott-Clayton, and Wyness, 2019, for recent discussion). Obviously, there has been substantial public opposition against introducing or raising tuition in any country that has tried to do so. So the form of higher education finance is ultimately a question of political economy. At the most basic level, it comes down to the question whether voters favor or oppose that students pay (part of) the costs of university attendance themselves. Apart from public opinion, other factors like the power of special interest groups of course also play a role in the political process. While not ignoring the latter aspects, our analysis is mainly concerned with understanding the preferences of the electorate and its determinants.

We focus on two groups of factors that we deem potentially relevant for the public's opinion on tuition: the amount of information that the electorate has on economic aspects of the higher education system and features of the design of tuition payments. In each group, we focus on aspects that are central to the political and scientific discourse on how universities should be financed.⁸

Information. The beliefs of the electorate are often biased (e.g., Gilens, 2001; Norton and Ariely, 2011), and its (mis)information can play an important role in elections (e.g., Allcott, and Gentzkow, 2017). A growing literature shows that imperfect information and ensuing behavioral biases can have important repercussions for individual decisions (see Lavecchia, Liu, and Oreopoulos, 2016, for a review focused on issues in the economics of education). We apply the analysis of imperfect information to the topic of the public's preferences for charging university tuition.

Among different aspects of potentially relevant information, our main focus is on information about the earnings premium related to higher education.⁹ Beliefs about what graduates stand to gain may affect the electorate's opinion about how much university attendants should contribute to the costs of attending university. A large recent literature shows that many people are unaware of the size of the returns to higher education, so that providing information on the relative earnings of university graduates can affect college-going

Kingdom (OECD, 2018). Tuition in independent private institutions is even higher, reaching 21,189 dollars on average in the United States. Countries where public universities do not charge any tuition include Denmark, Estonia, Finland, Norway, Poland, the Slovak Republic, Slovenia, Sweden, and Turkey (OECD, 2018).

⁸ Of course, other factors, as well as other dimensions of information and design, might also be relevant for determining public support for tuition. We consider their investigation an interesting avenue for future research.

⁹ In line with other studies (e.g., Bleemer and Zafar, 2018), we do not mean to imply that these differentials reflect causal returns to university education.

decisions (e.g., Jensen, 2010; Wiswall and Zafar, 2015; Bleemer and Zafar, 2018; Delavande and Zafar, 2019). Similarly, providing information on relative earnings may affect political support for tuition. In particular, if voters underestimate the returns to university education, informing them about the actual returns may increase the extent to which they support user fees in higher education – and vice versa if they overestimate them.

We also study two additional aspects of information: information about public costs of and unequal access to university. If voters are unaware of the current cost of university to society, providing them with information about how much the government spends for students' university education may trigger budget considerations about the right public-private balance of university funding. In addition, if voters are motivated by distributional considerations and are misinformed about the extent of intergenerational inequality (e.g., Alesina, Stantcheva, and Teso, 2018), providing information about the differing university access of children from different family backgrounds may affect voters' views on charging tuition.

Design. Our main focus in the area of design features is on tuition payment options. In particular, we study whether public support for tuition depends on whether it has to be paid upfront while attending university or whether payment is deferred until the former students earn sufficiently high income in the labor market. There is an extensive literature on the effects of income-contingent payment on efficiency and equity aspects of tuition (e.g., Barr, 2004; Chapman, 2006; Jacobs and van der Ploeg, 2006; Chapman, Higgins, and Stiglitz, 2014; Dynarski, 2014; Findeisen and Sachs, 2016; Stantcheva, 2017; Diris and Ooghe, 2018; Barr et al., 2019). Our focus, by contrast, is on whether and how this design feature affects the electorate's support for or opposition to tuition. Given that debt aversion has been shown to play a role in many economic contexts including higher-education decisions (e.g., Loewenstein and Thaler, 1989; Oosterbeek and van den Broek, 2009; Field, 2009; Abraham et al., 2018), one aspect of the analysis is whether support for income-contingent tuition depends on whether it is framed as deferred tuition payments or as upfront tuition payments combined with income-contingent loans that involve the initial accumulation of debt.

We also study two additional aspects of design: the level of tuition and its target group. The electorate's view on tuition may not only relate to the basic principle of charging tuition, but also to the level of tuition. In fact, tuition levels and their rapid increase compared to what the electorate is used to are at the forefront of the public debate about tuition in many contexts (e.g., the English experience). In addition, tuition may be designed as universal or as targeted at specific groups of students. In particular, in some settings students whose families do not live (and thus pay taxes) in the respective constituency whose public budget funds the

university have to pay particular tuition, such as non-EU students in several EU countries or out-of-state students in the United States (e.g., Lange, 2013; Jaquette and Curs, 2015).¹⁰ We study whether the public's preferences for tuition differ between universal tuition and this form of targeted tuition, thereby testing a political-economy underpinning for the common practice in many countries to charge higher tuition for non-residents.

Special interest groups. One reason why initiatives that have majority support among voters may not be enacted in actual policy is that special interest groups may exert stronger impact on the political process than other groups. In particular, special interest groups may invest resources to influence voting outcomes in their favor (e.g., Grossman and Helpman, 2001). In further analysis, we therefore study whether support for tuition, as well as the different determining factors discussed above, differ between groups that do and do not have a particular pecuniary self-interest in whether higher education is financed by tuition or not.

2.2 Institutional Background on Higher Education Finance in Germany

To study the determinants of voter support for charging tuition or providing higher education free of charge, we turn to the setting of the German electorate. In Germany, the legislative and executive power over public education, including tertiary education, is vested in the 16 states (*Länder*). The federal government is merely granted power over framework legislation concerning the general principles of tertiary education.¹¹ Tuition was banned from public universities in 1970. This ban was enacted in federal legislation (*Hochschulrahmengesetz*) in 2002. Shortly thereafter, six of the 16 states filed a lawsuit against the nationwide ban because it interfered with their constitutionally guaranteed autonomy of education policy. In 2005, the Federal Constitutional Court ruled in favor of the suing states and allowed each state to decide on the implementation of university tuition autonomously. Subsequently, seven states – all with the conservative CDU/CSU party in government – introduced tuition in 2006 or 2007 (Baden-Württemberg, Bavaria, Hamburg, Hesse, Lower Saxony, North Rhine-Westphalia, and Saarland). No state governed by social democrats (SPD) introduced tuition. Most tuition states charged a fixed amount of 500 Euro per semester.¹²

¹⁰ In the United States, average tuition in public four-year institutions for out-of-state students is 2.8 times as high as for in-state students (24,854 vs. 8,804 dollars in 2016-17; U.S. Department of Education, 2019).

¹¹ The permanent funding for universities comes from states' general budgets. The allocation formulas for each university vary across states. Until 2014, the federal government was not allowed to engage in permanent funding of the education system. In 2014, this regulation was relaxed for higher education. Note also that the vast majority of 91 percent of students in Germany attend public universities (Statistisches Bundesamt, 2019).

¹² Bavaria and North Rhine-Westphalia are exceptions: In Bavaria (North Rhine-Westphalia), universities were allowed to charge tuition between 300 Euro (0 Euro) and 500 Euro per semester. Still, most universities in these states also charged 500 Euro per semester.

The introduction of tuition was accompanied by nationwide student protests and controversial legal and political debates that were highly visible in the media. Ultimately, the opposition against tuition in the German public led to the fact that tuition was abolished again in every single state between 2010 and 2014.¹³ A case in point is the state of Bavaria, where a public petition against tuition was signed by more than 1.3 million eligible voters (14 percent of the electorate) in January 2013. To stop the pending referendum, the conservative Bavarian government, which had introduced tuition in the first place, abolished tuition again by fall term 2013/14. The fact that only 216,000 students were enrolled at Bavarian universities at the time suggests that the abolition was backed by large parts of the general population.

The topic of university tuition also played a prominent role in state election campaigns in all other six tuition states. In four states, the CDU got voted out of power in the next state elections, often with the tuition topic allegedly playing a major role, and the newly elected social-democratic governments abolished tuition shortly after assuming office (Baden-Württemberg, Hamburg, Lower Saxony, and North Rhine-Westphalia). In Hesse, the social-democratic and green majority in the parliament abolished tuition while the CDU prime minister was still in office. In the Saarland, tuition was abolished by the coalitional government at the insistence of the Greens (see Kauder and Potrafke, 2013).

While the political debate about tuition has basically stopped with its abolition, the latest reform of Germany's higher education finance concerns the introduction of tuition targeted at non-EU citizens.¹⁴ Accompanied by new student protests, Baden-Württemberg's coalition government of CDU and the Greens introduced tuition of 1,500 Euro per semester for non-EU students. This form of tuition is also part of the recent coalition agreement between the CDU and the liberal free democratic party (FDP) in North Rhine-Westphalia.

These background features make Germany a particularly interesting case to study the determinants of public support for tuition. The fact that some German states had introduced tuition but failed to keep them up in the political process means that voters are aware that the question of tuition is a politically relevant topic. At the same time, it is a politically open topic where the electorate's preferences may have implications for actual policy reform initiatives.

¹³ Several nationwide newspapers and magazines continuously published dossiers on the political debate over tuition (see Dwenger, Storck, and Wrohlich, 2012, for a short account). For a summary of the political controversies surrounding tuition and the importance of the public's opinion for their abolishment in Germany, see, for instance, Times Higher Education, 23 May 2013, <https://www.timeshighereducation.com/news/unbridled-success-germanys-fee-foes-claim-victory/2003928.article> [accessed 8 October 2018] or Economy Watch, 8 October 2014, <http://www.economywatch.com/features/Germany-scraps-tuition-fees-after-mass-student-protests-cause-shift-in-public-opinion.10-08-14.html> [accessed 8 October 2018].

¹⁴ While differential treatment of students from Germany and other EU countries is not feasible by law, differential treatment of students from non-EU countries is feasible in principle.

Given this setting, our results may be most relevant for countries that, like Germany, struggle to introduce or maintain tuition. Still, voter responsiveness to different features of information and design may also carry broader relevance for countries with longer traditions of charging tuition, in particular when the desirability of free higher education is publicly debated.

3. Data, Experimental Setup, and Empirical Strategy

This section describes our opinion survey, the design of the survey experiments, and the empirical model.

3.1 The Opinion Survey

The empirical research presented in this paper is based on five waves of the *ifo Education Survey* (2014 to 2018), an annual opinion survey on education policy that we conducted in Germany. Each wave was carried out between April and July of the respective year and covered a nationally representative sample of the German voting-age population (18 years and older). Each year, respondents were asked to answer some 30 questions on different education topics and provided their sociodemographic information at the end of the survey.

The surveys were implemented by Kantar Public (previously known as TNS Infratest), a German polling firm that has access to a nationally representative panel of adults. In the first four waves, stratified sampling was administered in two steps to account for the fact that a sizeable share of citizens in Germany (decreasing from 22 percent in 2014 to 17 percent in 2017) does not use the internet. Persons who use the internet were selected from an online panel and were polled through an online survey. Persons who do not use the internet were polled at their homes, where trained interviewers provided them with tablet computers for completing the survey autonomously. All respondents answered all questions on a computer. This mixed-mode approach ensures that our sample is representative of the German electorate, an aspect crucial for deriving general statements for the political economy of higher education finance (e.g., in the framework of median voter models). In the 2018 wave, the survey mode switched to a full online survey after an extensive analysis revealed that representativeness for the entire population could be achieved through re-weighting the online sample accordingly (Grewenig et al., 2018).

We employ survey weights that are calibrated to match administrative statistics with respect to age, gender, parental status, school degree, federal state, and municipality size. Inclusion of these weights does not substantially change the results of this paper, speaking towards the validity of our sampling strategy (results available upon request).

Each wave included an item asking respondents about their preferences for university tuition. To study the effects of information and design elements on participants' responses experimentally, respondents were randomly split into one control group and up to three treatment groups. To ensure representativeness in each experimental group and sufficient power to detect treatment effects, we generally aimed for 1,000 observations per experimental group. Each wave contains slightly over 4,000 observations (except for wave 2016, whose oversample of 713 school teachers is not considered here). Table 1 shows the data structure across waves and treatment groups. In total, there are 19,702 observations of people who were asked the tuition question over the five waves. As item non-response is very low at around 1 percent on average,¹⁵ the overall sample size is 19,577 responses to the tuition questions.¹⁶

In two survey waves, we collected additional data that complement the main adult survey. In wave 2017, we conducted a follow-up survey about two weeks after the main survey to test whether information effects persist beyond the setting of the main survey. In wave 2018, we complemented the adult survey with an adolescent survey whose 1,085 respondents are representative of the population aged 14-17 years.

3.2 The Survey Experiments

In all waves of the *ifo Education Survey*, we elicit respondents' preferences for tuition and conduct survey experiments that use random assignment to treatment and control groups to test whether aspects of information and design affect elicited preferences. Appendix Figure A1 provides an overview of the analyses implemented in the different survey waves. In each of the five waves, we elicit preferences for tuition in the control group by asking: "Do you favor or oppose that students at German universities cover a part of the costs of their studies themselves by tuition?"¹⁷ Respondents could pick one of five answer categories: strongly favor, somewhat favor, neither favor nor oppose, somewhat oppose, and strongly oppose.¹⁸

¹⁵ Whenever a question was left blank during the survey, a pop-up window with an encouragement to answer the respective question appeared. Respondents did not have the option to go back in the survey. Treatment status does not predict item non-response in our experiments (see Appendix Tables A2 and A3).

¹⁶ The 19,577 observations come from 15,013 individuals, as some respondents participated in more than one wave. In regressions that pool different waves, we cluster standard errors at the individual level. Excluding respondents who participated again does not alter our results (available upon request).

¹⁷ Like other recent survey-based research, our outcomes of interest are self-reported preferences (e.g., Cruces, Perez-Truglia, and Tetaz, 2013; Kuziemko et al., 2015; Falk et al., 2018). Recent evidence corroborates the relevance of survey-based outcome measures since they closely correspond to actual political behavior, such as signing petitions or donating to charity (e.g., Haaland and Roth, 2017; Alesina, Miano, and Stantcheva, 2018).

¹⁸ Appendix Table A1 shows the question wording of all experiments. A summary description of the entire survey is available at www.cesifo-group.de/ifo-bildungsbarometer. To prompt respondents to give a considered answer and to minimize the error of central tendency, the category "neither favor nor oppose" was placed below the other four answer categories. In methodological experiments on questions unrelated to tuition, we found that

Wave 2014: Information on Relative Earnings, Public Costs, and Unequal Access

In the first wave, we conducted an experiment to test how factual information on (i) the relative earnings differential between persons with and without a university degree, (ii) public costs per university student, and (iii) unequal access to university by parental background affects public preferences for tuition.

In the experiment, we randomly assigned respondents to one of four experimental groups. In the control group, we elicited preferences for tuition as indicated above. The three treatment groups were asked the exact same question, but they were additionally provided with different pieces of information directly above the question on the same survey screen.

In the first treatment group (*Earnings information*), respondents were informed that university graduates earn about 40 percent more each year than persons with a vocational education (apprenticeship) – the main alternative in the German education system. We computed this number using the German sample of the Programme for the International Assessment of Adult Competencies (PIAAC) 2012.

The second treatment group (*Cost information*) was informed that public expenditures for universities amount to 8,600 Euro per student each year (Statistisches Bundesamt, 2014).

In the third treatment group (*Access information*), respondents were informed that 75 percent of individuals whose parents hold a university degree, but only 25 percent of those without university-educated parents, go to university themselves, and that this difference already existed before tuition was introduced in Germany (Middendorff et al., 2013).

To gauge respondents' baseline information on the aspects covered in the treatments, we elicited their beliefs about relative earnings, public costs, and unequal access to university earlier on in the survey. To make the potential correction of false beliefs less immediate and thus to reduce the possibility of backfire effects (Nyhan and Reifler, 2010), these guessing questions were posed much earlier in the survey, well before the survey experiment.

Wave 2015: Income Contingency and Information on Absolute Earnings

In the second wave, we implemented a couple of experimental treatments, one as a robustness test of the earnings information treatment and one about income contingency of tuition payments. In the control group, we elicited public preferences for regular (upfront) tuition with the exact same question as in wave 2014.

this reduces the error of central tendency and leaves the relative frequency of policy support and opposition unaffected. Other survey design details, such as the ordering and number of answer categories or their horizontal versus vertical presentation, also did not affect answering behavior (detailed results available upon request).

In the first experimental treatment, we aim to test whether treatment effects of providing earnings information depend on providing it in relative as opposed to absolute terms. Rather than providing information that university graduates earn a certain percentage more than others as in the first wave, in the second wave we provide information on absolute earnings for individuals with different education degrees. Specifically, in the first treatment group (*Earnings information*) respondents were informed that on average, university graduates earn about 2,450 Euro net per month, persons with a vocational education (apprenticeship) earn about 1,850 Euro, and those without a degree earn about 1,400 Euro.¹⁹ Acknowledging the importance of replication in economic research (e.g., Maniadis et al., 2014), this treatment assesses the robustness and replicability of the effect of providing earnings information in the first wave.

To uncover the mechanisms through which the earnings information treatment impacts preferences for tuition, we elicited beliefs about net monthly earnings from all respondents earlier in the survey. This allows us to estimate heterogeneous treatment effects by baseline beliefs. We also asked how sure respondents were that their answer was close to correct on a scale from 1 (“very unsure”) to 7 (“very sure”) to differentiate between confidently held false beliefs (“misinformation”) and random guesses (“uninformedness”) (Kuklinski et al., 2000).

In the second treatment group (*Income contingency*), respondents were asked whether they favor or oppose downstream, income-contingent tuition. Specifically, the question reads: “In other countries, there is tuition that is due only after graduation, when the former students earn income. Tuition has to be paid only if their annual income exceeds a certain threshold. Do you favor or oppose that students at German universities cover a part of the costs of their studies themselves by this alternative form of tuition?” The experimental setting allows us to investigate whether the public’s preferences for tuition depend on whether tuition is designed as regular upfront payments or as deferred income-contingent payments. In contrast to regular tuition, income-contingent tuition was never introduced in Germany, and there were no major public debates about it. To convey that income-contingent tuition is a realistic policy option, we referred respondents to the fact that this form of tuition is in place in other countries.²⁰

¹⁹ These numbers represent median earnings in the German Microcensus 2012 (Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census year 2012). As income taxes are directly deduced from the pay slip, this presentation of net earnings data is arguably most accessible to Germans. The relative earnings differential in wave 2014 is based on gross income data from PIAAC.

²⁰ Since we did not refer to other countries in the control group, a potential concern is that the effect of the income-contingency treatment might be driven by the reference to other countries. We consider this possibility unlikely for two reasons. First, the question did not name any specific country but simply indicated that “other countries” have this form of tuition. Second, this concern relies on the unrealistic assumption that respondents are not aware of the fact that regular tuition is charged in other countries.

Wave 2016: Tuition Levels and Income Contingency

The experiment in the third wave included three experimental treatments in addition to the standard control group. In the first treatment group, we repeated treatment *Income contingency* from the previous wave in order to assess its replicability.

The other two experimental treatments investigate how the level of tuition affects their public acceptance. As in the previous waves, the question posed to the control group did not specify the level of tuition. This was to keep the question as short and simple as possible and to keep it general rather than making results dependent on any specific choice of levels. To test whether public preferences for tuition depend on their level, as well as which level of tuition respondents might have in mind when answering the control-group question, we implemented two treatments that specify two different levels of tuition.

Specifically, in the second treatment group (*Level 500*) respondents were asked whether they favor or oppose tuition of 500 Euro per semester. This level reflects the amount charged when tuition had been in place in Germany (see section 2.2).

In the third treatment group (*Level 1500*), respondents were asked about their opinion on tuition of 1,500 Euro per semester. Apart from indicating the specific tuition levels, the questions were phrased exactly as in the control group.

Wave 2017: Persistence of Treatment Effects of Earnings Information

In the fourth wave, we aimed to investigate whether the effect of providing earnings information persists beyond the short-run setting of the main survey. We thus conducted a follow-up survey about two weeks after the main survey. To yield enough power for the follow-up survey, respondents in the main survey were split into just two experimental groups: a control group and a treatment group (*Earnings information*) that received the earnings information. Specifically, similar to wave 2015, respondents in the treatment group were informed about net monthly earnings of persons with university degree (2,750 Euro), vocational degree (1,850 Euro), and no degree (1,400 Euro) before answering the same tuition question as the uninformed control group.²¹ The treatment was supplemented with a graphical depiction of the earnings information.

²¹ To provide respondents with the most recent information, we used the latest available version (2013) of the German Microcensus to compute earnings (Research Data Centres of the Federal Statistical Office and the statistical offices of the Länder, Microcensus, census year 2013). The slight change in numbers between 2012 and 2013 reflects that the Microcensus records earnings in 24 bins, with median earnings of university graduates increasing from the 2,450 Euro bin to the neighboring 2,750 Euro bin. Since treatment effects are statistically indistinguishable between waves 2015 and 2017, this feature does not seem to affect our results substantively.

To test whether the impact of providing earnings information persists beyond the immediate survey horizon, we invited respondents of the online sample of the main survey to participate in a follow-up survey at a later point. The follow-up survey re-elicited preferences for tuition but did not comprise any information treatment. A total of 2,363 respondents (64 percent of the online sample) participated in the follow-up survey. The median time elapsed between the main and the follow-up survey was 12 days (ranging from 5 to 41 days).

To compare preferences for regular and income-contingent tuition within individuals, we also elicited preferences for income-contingent tuition in a separate question in waves 2014 and 2017. The question was placed on a separate screen directly after eliciting preferences for regular tuition and worded identically to the question used in the experimental treatments of the other two waves. While this setup facilitates within-respondent comparisons of preferences for regular versus income-contingent tuition (see section 5.1), the consecutive and non-experimental elicitation could in principle bias responses on the second question, for example, if there is preference for giving consistent answers (e.g., Falk and Zimmermann, 2013). Our main analysis of preferences for income-contingent tuition is therefore restricted to the elicitations in waves 2015, 2016, and 2018.

Wave 2018: Loan Framing, Tuition for Non-EU Students, and Adolescent Survey

In the fifth wave, we test whether preferences for income-contingent tuition depend on loan aversion. Respondents in the first treatment group (*Income contingency*) were asked about deferred income-contingent tuition using the same wording as in the previous waves, which did not refer to loans but indicated a deferred payment of tuition.

In the second treatment group (*Student loans*), respondents were asked effectively the same question in a version that framed this form of tuition payoff-equivalently as tuition that is being paid in the form of income-contingent student loans. By explicitly referring to “loans” three times, this alternative treatment aims to test whether support for income-contingent tuition is affected by loan aversion.

A final experimental treatment tests the relevance of another design feature of tuition for shaping public preferences, namely its target group. In the third treatment group (*Non-EU students*), respondents were asked whether they favor or oppose tuition for non-EU citizens who study at German universities. This treatment allows us to speak to the current debate on the introduction of tuition for non-EU students in some German states (see section 2.2) and, more broadly, to test whether the public’s opinion on tuition differs depending on whether all students or just non-residents have to pay it.

To test whether our results on preferences for regular and income-contingent tuition of the voting-age population carry over to adolescents, we complemented the adult sample in the 2018 wave of the *ifo Education Survey* with an adolescent sample. We consider this sample particularly interesting not only because adolescents play an active role in shaping education policy as stakeholders in the education system and as future voters, but also because their higher-education decisions may be directly affected by reforms of higher education finance. The adolescent sample comprises 1,085 respondents who are representative of the German population aged 14 to 17 years.

To sustain statistical power, there were two experimental groups in the adolescent survey. The control group received the same control-group question on regular tuition as in the adult sample. The treatment group (*Income contingency*) received the question on income-contingent tuition with a slightly simplified wording to ease comprehension, describing income-contingent tuition as follows: “In other countries, there is tuition that has to be paid only after graduation – and only if the former students earn enough money.”

3.3 Empirical Model

Our analysis of treatment effects is based on the following regression model:

$$y_i = \alpha + \beta_k T_{ki} + \lambda' Z_i + \varepsilon_i \quad (1)$$

where y_i is individual i 's preferences for tuition, T_{ki} is an indicator of whether individual i received treatment k , Z_i is a vector of control variables, and ε_i is an error term that is uncorrelated with all right-hand-side variables.²² The parameter of interest is β_k , which represents the average treatment effect of treatment k on preferences for tuition. Additional control variables are not required to identify the causal treatment effects due to random assignment to the experimental groups. Nevertheless, the inclusion of covariates may increase the precision of estimates.²³

To analyze effect heterogeneity of a given treatment across subgroups (defined over prior beliefs or sociodemographic characteristics), we extend our basic regression model to:

$$y_i = \alpha + \beta_1 T_i + \beta_2 S_i + \beta_3 T_i S_i + \lambda' Z_i + \varepsilon_i \quad (2)$$

²² In wave 2014, $k \in \{\text{Earnings information, Cost information, Access information}\}$; in 2015, $k \in \{\text{Earnings information, Income contingency}\}$; in 2016, $k \in \{\text{Income contingency, Level 500, Level 1500}\}$, in 2017, $k = \text{Earnings information}$; in 2018, $k \in \{\text{Income contingency, Student loans, Non-EU students}\}$.

²³ See table notes for lists of included covariates. The share of missing covariate data is very low in our samples (below 1 percent, on average). Throughout the paper, we impute missing covariates by a constant and include dummies indicating imputed values for each covariate. All qualitative results hold when instead observations with missing covariates are dropped (results available upon request).

where S_i equals one if respondent i is member of the respective subgroup, 0 otherwise. The treatment effect for non-members of the subgroup is given by β_1 , and β_3 measures the additional effect on the subgroup.

3.4 Tests of Randomization

To test whether randomization in our experiments successfully balanced respondents' characteristics across treatment and control groups, we investigate whether our rich set of covariates differs by treatment status. The initial columns of Appendix Tables A2 and A3 present means of observable characteristics for the control groups of the different waves. The remaining columns report coefficients and p -values of regressions of the form

$$C_i = \alpha + \gamma_k T_{ki} + \varepsilon_i \quad (3)$$

for each covariate C_i and each experiment separately.

It is reassuring that only 21 out of 398 regressions (5.3 percent) yield a coefficient γ_k that is significant at the 5 percent level, as would be expected by pure chance. Thus, the balancing tests suggest that random assignment worked as intended.

4. Experimental Results on Information Provision

We present our experimental results in three steps. In this section, we analyze how information provision affects public support for tuition. In the next section, we provide evidence on how the design of the tuition payment scheme affects preferences. Finally, we assess the generality of our findings by investigating treatment effect heterogeneity across sociodemographic subgroups and analyze special interest groups.

4.1 Baseline Results of Information Treatments on Earnings, Costs, and Access

In the first experiment (wave 2014), we focus on whether the public's preferences for whether or not students should cover part of the university costs themselves are affected by information on returns, costs, and access to higher education. Table 2 reports the effects of information provision on preferences for tuition based on equation (1). To depict political majorities for or against tuition, the first two columns focus on whether respondents are in favor of tuition (combining the answer categories "strongly favor" and "somewhat favor"), whereas the latter two columns focus on opposition against tuition (combining "somewhat oppose" and "strongly oppose"). The residual answer category on the five-point scale is "neither favor nor oppose" (see Appendix Table A4 for results for each answer category

separately).²⁴ Estimates in columns 1 and 3 do not include covariates, whereas columns 2 and 4 include standard covariates as indicated in the table notes.

Results indicate that at baseline, the German population is divided about charging tuition, with a plurality against it. 40.0 percent of respondents in the uninformed control group favor tuition and 45.7 percent oppose tuition (see control mean in Table 2); the remaining 14.3 percent are indifferent. Given the indication that public opposition against tuition played an important role in their abolishment (see section 2.2), it is remarkable that only a plurality, but not a solid majority of the electorate is against tuition. This suggests that special interest groups (see section 6 below) may have been particularly relevant in the abolition of tuition.

Informing participants about the earnings premium of university graduates substantially increases support for tuition, shifting the plurality of the public in favor of tuition. Treatment *Earnings information* increases the share of respondents favoring tuition significantly by 8.1 percentage points to 48.1 percent (column 1). Likewise, once informed about the university earnings premium, the share opposing tuition significantly decreases by 8.9 percentage points to 36.8 percent (column 3). Respondents' initial beliefs about the university earnings premium elicited earlier in the survey indicate that a larger share of the public underestimates rather than overestimates the earnings gap.²⁵ Thus, alleviating biased beliefs about the earnings differential through information provision has profound effects on public support for tuition, shifting the plurality of the public towards supporting tuition. As expected, regression coefficients hardly budge when covariates are included (columns 2 and 4).

Inspection of results by individual answer categories indicates that public opinion is shifted throughout the spectrum (Appendix Table A4, Panel A). Based on the preference elicitation on a five-point scale, treatment *Earnings information* thus impacts strongly as well as weakly held preferences.

In contrast to earnings information, the other two information treatments do not significantly change preferences for tuition (Table 2). Specifically, support for and opposition to tuition are unaffected by whether respondents in treatment *Cost information* are informed about how much the federal government and the federal states currently spend on tertiary education for each student. The null effect of the cost information treatment is particularly

²⁴ All regression results presented in this paper are estimated using OLS, but nonlinear (probit or multinomial logit) models yield qualitatively identical results (available upon request).

²⁵ Early in the 2014 survey, we elicited respondents' baseline beliefs on how much university graduates earn compared to persons with a vocational degree using a six-point scale (answer categories: less; about the same; up to 25 percent more; 25-50 percent more; 50-100 percent more; more than 100 percent more). On this question, 43 percent of respondents tick the correct answer category (25-50 percent more), 39 percent underestimate the earnings differential, and 18 percent overestimate it. See section 4.3 for further analysis of baseline beliefs.

remarkable as respondents' baseline beliefs about annual public costs per university student (which we elicited early on in the survey) are far below the actual value (median belief: 2,000 Euro; actual costs: 8,600 Euro). In line with previous studies (e.g., Kuklinski et al., 2000; Kuziemko et al., 2015), correcting false beliefs through information provision thus proves to be a necessary, but not sufficient condition for shifting policy preferences.

Similarly, public preferences for tuition are unaffected by being informed of unequal access to university by parental background. Treatment *Access information* informs participants that about 75 percent of children of parents with a university degree and 25 percent of children of parents without a university degree take up university studies. In this case, the reason for the null effect of the information treatment might be that the public is already relatively well informed about the extent of unequal access to university education: 82 percent of respondents correctly anticipated that the university participation rate of individuals whose parents hold a university degree is 50 percent or more (after being informed that the respective share for those without university-educated parents is 25 percent).²⁶

In sum, our results show that information about the earnings differential between persons with and without a university degree affects the public's preferences regarding higher education finance, while information on fiscal costs or unequal access to university education does not. In the remainder of this section, we therefore focus on the *Earning information* effect and investigate its replicability, underlying belief-updating process, and persistence.

4.2 Tests for Sensitivity and Replicability of the Earnings Information Effect

To further scrutinize the effect of earnings information on preferences for tuition, we subject the *Earnings information* treatment to sensitivity and replication tests in waves 2015 and 2017. Wave 2014 presented the earnings information in relative terms, indicating the earnings advantage of university graduates in percent. To test whether the treatment effect is sensitive to the provision of relative earnings information, waves 2015 and 2017 presented the earnings information in absolute terms, indicating the respective net monthly earnings levels of individuals with different degrees (see section 3.2 for details).

Intriguingly, the 2014 results are perfectly robust in the alternative presentation of wave 2015, presented to a new representative sample of the German electorate. As columns 1 and 5

²⁶ There are, of course, several possible explanations for why information treatments fail to affect policy preferences. For treatment *Cost information*, it might be that (i) public costs are simply irrelevant for respondents when forming preferences for tuition or that (ii) government spending levels are less tangible and informative than, say, earnings differentials. For treatment *Access information*, it might be that (i) respondents fail to connect the information provided with tuition policies as the connection between the two is arguably relatively indirect (e.g., Kuziemko et al., 2015) or that (ii) the information provided is too complicated for respondents to digest.

of Table 3 indicate, 43.8 percent of control-group participants favor charging tuition and 46.4 percent oppose it. In the treatment group that is informed about the respective absolute earnings of different graduates, support for tuition is again significantly increased (by 5.2 percentage points) and opposition reduced (by 7.1 percentage points), turning the plurality around to be in favor of tuition.

We replicate the wave 2015 experiment in wave 2017 in yet another representative sample of the German electorate. Again, results are almost identical (Panel A of Table 4): In the control group, 43.3 percent favor tuition and 45.8 percent oppose it. Treatment *Earnings information* raises support by 7.1 percentage points and reduces opposition by 8.0 percentage points. In fact, in the case of wave 2017, a slight majority of 50.4 percent of respondents in the informed treatment group is in favor of students paying tuition.²⁷

Figure 1 shows pooled results of the *Earnings information* experiment across the three waves. On average, providing respondents with information on the university earnings premium increases support for tuition by 6.9 percentage points from 42.7 to 49.6 percent and reduces opposition against tuition by 8.2 percentage points from 46.0 to 37.8 percent. Pooled regressions indicate that the *Earnings information* treatment effects are in fact statistically indistinguishable across the three experiments, as are baseline preferences in the control groups over the three waves (see Table 12 below).

In sum, the effect of *Earnings information* replicates in three different representative samples of the German voting-age population and does not hinge on whether the earnings differential is presented in relative or absolute terms. This replication exercise corroborates that the reported treatment effect does not reflect a false-positive result but rather the true causal effect of the treatment on public preferences (see Maniadis et al., 2014).

4.3 Effect Heterogeneity of Earnings Information Treatment by Baseline Beliefs

To study whether treatment effects of *Earnings information* differ by baseline beliefs about the earnings differential, in wave 2015 we elicited respondents' beliefs about the earnings differential early in the survey. Specifically, we provided information on the earnings of persons with a vocational degree (1,850 Euro) as an anchor and asked respondents to guess the average net monthly earnings of university graduates. The median guess of 2,500

²⁷ Wave 2017 also included two additional randomly provided pieces of information on higher education finance: the current absence of tuition in Germany and the availability of comprehensive public student aid (so-called *BAföG*). This information was provided on a previous screen in relation to a question on participants' hypothetical educational aspirations (see Lergetporer, Werner, and Woessmann, 2018). These information treatments did not significantly affect opinions on tuition (detailed results available upon request).

Euro is close to the correct value 2,450 Euro, and guesses range from 1,800 Euro at the 10th percentile to 3,500 Euro at the 90th percentile. To ensure that our results are not driven by extreme outliers, we winsorized observations at the 2nd and 98th percentile of the guess distribution for the analysis.

Results of an interacted model indicate that the effect of the information treatment decreases with prior beliefs about the university earnings premium. Following equation (2), the interacted specification includes an interaction term between treatment *Earnings information* and initial beliefs. Relative beliefs are expressed as the difference between the estimated earnings of university graduates and actual earnings of those without a degree, divided by the actual earnings difference between the two groups. Thus, the main effect on the treatment indicator in the interacted specification indicates the treatment effect for those who thought that university graduates earned the same as those without a degree. Results in column 2 of Table 3 indicate that the treatment effect on support for tuition is marginally significantly stronger the lower the initial guess of the university earnings premium. For individuals who guessed that there was no earnings premium, the treatment effect is estimated at 12.6 percentage points. Thus, the information treatment induced a larger information shock on the preferences of under-estimators than on those who hold more accurate beliefs. The treatment effect on opposition against tuition shows a similar pattern of heterogeneity, although insignificantly so.

To be able to differentiate inaccurately held beliefs from general ignorance, we also elicited how confident respondents are about their beliefs on a scale from 1 (“very unsure”) to 7 (“very sure”).²⁸ Splitting the sample by the confidence with which respondents hold their beliefs, the pattern of heterogeneous treatment effects by baseline beliefs is more pronounced for those who are relatively sure about their beliefs (columns 3 and 7) than for their counterparts (columns 4 and 8). Taken together, these results suggest that the information treatment mainly operates through correcting confidently held false beliefs.²⁹

4.4 Persistence of the Earnings Information Effect in a Follow-up Survey

The treatment effects presented thus far are measured directly after information provision within the same survey. To investigate whether the effect of the *Earnings information*

²⁸ 25.9 percent of respondents are relatively sure that their stated belief is close to correct (choosing a value between 5 and 7), 41.7 percent are relatively unsure (choosing a value between 1 and 3), and 32.4 percent choose the middle category. Interestingly, belief accuracy and confidence are not positively correlated. This finding is in line with earlier evidence on public misperceptions about welfare (see Kuklinski et al., 2000).

²⁹ This pattern is similar in wave 2017, although treatment effect heterogeneity does not reach statistical significance (results available upon request).

treatment persists beyond the immediate survey horizon, in wave 2017 we re-elicited beliefs about earnings and preferences for tuition (without any information treatment) in a follow-up survey about two weeks after the main survey. Of the 3,696 participants in the online version of the main survey, 2,363 respondents (64 percent) participated again in the follow-up survey.³⁰ While several observable characteristics predict participation in the follow-up, characteristics of follow-up survey participants are well balanced between treatment and control group (see Appendix Table A5). Furthermore, treatment status in the main survey does not significantly predict participation in the follow-up survey. Thus, treatment effects in the follow-up survey can be interpreted as the persistent causal effect of the *Earnings information* treatment among those who selected to participate in the follow-up survey.

Being informed about the earnings of university graduates in the main survey significantly improves participants' guesses of these earnings in the follow-up survey. Identifying guesses within an interval of 20 percent above or below the true value as correct guesses, the estimates in the first three columns of Panel B of Table 4 show that treated individuals are 12 percentage points more likely to guess graduate earnings correctly than individuals from the control group, reducing both underestimation and overestimation.³¹

Earnings information provision in the main survey also retains a persistent effect on preferences for tuition in the follow-up survey. The estimates in columns 4 and 6 of Panel B of Table 4 indicate that treatment *Earnings information* turns a plurality against tuition in the control group (44 percent in favor, 48 percent opposed) into a plurality favoring tuition (46 percent in favor, 42 percent opposed). While the opposition-reducing effect is highly significant, the support-increasing effect is shy of statistical significance. Again, the inclusion of control variables does not alter the qualitative results.

The results of the follow-up survey indicate that a significant share of participants remembers the information provided in the main survey and updates their beliefs accordingly. The fact that treatment *Earnings information* changes pluralities for tuition persistently makes us confident about the validity of our findings beyond the immediate survey context. In particular, this persistence makes it highly unlikely that treatment effects (measured immediately after information provision) are contaminated by experimenter demand effects or priming effects. While using survey responses as dependent variable can bias treatment effect

³⁰ This take-up rate is relatively high compared to other recent follow-up surveys, such as 14 percent in Kuziemko et al. (2015) and 24 percent in Alesina, Stantcheva, and Teso (2018).

³¹ The same qualitative results are obtained for alternative intervals such as 15 or 25 percent (results available upon request).

estimates if the treatment is correlated with such reporting biases (Bertrand and Mullainathan, 2001), the provided evidence suggests that this is not a major issue in our setting.³²

In sum, information about the university earnings premium has a strong and significant effect on public preferences for charging tuition, turning a plurality against tuition into a plurality in favor. By contrast, public preferences for tuition are unaffected by information on public costs or unequal access to university. The treatment effect of earnings information is replicated in three separate representative surveys, insensitive to relative or absolute earnings provision, stronger the lower respondents' initial guesses of graduate earnings, and persistent beyond the short-run survey setting. As a next step, we turn to whether public preferences for tuition also depend on different design elements of the tuition payments.

5. Experimental Results on Design Features

We present four sets of results on the effects of the design of tuition on their public acceptance. First, we analyze how public support for income-contingent tuition compares to regular upfront tuition. Second, we study how support for income-contingent tuition depends on the absence or use of a loan framing. Third, we investigate how the level of tuition affects public preferences. Fourth, we test whether targeting tuition towards non-EU citizens, a common practice in many countries, affects public support for tuition.

5.1 Income Contingency: Baseline Results, Replication, and Within-Subject Analysis

All results reported so far refer to public preferences for regular tuition that has to be paid upfront while the student attends university. This is the only form of tuition that had been in place in Germany. However, other countries – Australia, England, and New Zealand – have implemented a tuition system where payments are deferred until the former students later earn sufficient income on the labor market. To test whether public opinion on tuition depends on the regular vs. deferred income-contingent design, in the second wave (2015) we implemented experimental treatment *Income contingency* that asks the treatment group about their preferences for income-contingent tuition. We compare these preferences to a control group who was asked about their preferences for regular tuition.

³² Experimenter demand effects occur if respondents infer the experimenters' intentions from the treatment and adapt their answers accordingly (see Zizzo, 2010). Information treatments may also trigger an immediate and temporary activation of certain concepts in respondents' memory, influencing answering behavior unconsciously (priming effects). The persistence of treatment effects in the follow-up survey speaks against such unintended effects (see also Haaland and Roth, 2017; Cavallo et al., 2017). Consistently, Mummolo and Peterson (2019) show that experimenter demand effects are likely absent in survey experiments, and de Quidt et al. (2018) also show that they hardly affect results of experiments on economic preferences.

Results show that designing tuition as income-contingent payments substantially increases public support, creating a strong majority of the electorate in favor of tuition. As indicated in Table 5, treatment *Income contingency* raises support for tuition from 43.8 percent for regular tuition in the 2015 control group by 15.3 percentage points. Concurrently, opposition against tuition is reduced by 15.8 percentage points from a baseline of 46.4 percent.

The results replicate very closely in two other representative samples of the German electorate in waves 2016 and 2018. By stacking the control groups and *Income contingency* treatment groups of the three waves, Table 5 allows for a direct comparison of results across waves. Support for regular tuition in the control group does not differ significantly across the waves, and the *Income contingency* treatment effect is almost identical. The same is true for opposition against tuition, with the treatment effect in the final wave if anything slightly larger. The replicability indicates that the highly significant effect of treatment *Income contingency* on preferences for tuition is unlikely a chance result.

Figure 2 shows the pooled results of the *Income contingency* experiment across the three waves. On average, the income-contingent design increases support for tuition by 15.7 percentage points and reduces opposition against tuition by 17.7 percentage points. The treatment turns a plurality opposing tuition in the regular design into a strong majority favoring tuition when designed as income-contingent payments. On average, 60.3 percent of participants are in favor of having tuition that is due only after graduation when the former students earn income and that has to be paid only if their annual income exceeds a certain threshold. Only 28.2 percent of the population oppose this form of tuition. Inspection of treatment effect estimates on each of the five underlying answer categories indicates that treatment *Income contingency* shifts strongly held preferences for and against tuition as well as less strongly held preferences (see Panel B of Appendix Table A4).

While experimental treatment in waves 2015, 2016, and 2018 allows us to estimate treatment effects of regular vs. income-contingent tuition design, in waves 2014 and 2017 we asked all participants first about their preferences for regular tuition (separated into control and information treatment groups) and as a subsequent separate question about their preferences for income-contingent tuition. The latter setup allows us to perform within-subject comparisons of preferences for regular and income-contingent tuition.

Table 6 shows the distribution of individuals' preference profiles for the two questions (control groups only). Overall, a majority of 64.4 percent of respondents favor income-contingent tuition in this setting, compared to 41.3 percent favoring regular tuition. The non-experimental elicitation of preferences for income-contingent tuition after eliciting

preferences for regular tuition thus provides similarly large support for income-contingent tuition as in the experimental elicitation in the other three waves.³³

The numbers on the main diagonal of Table 6 reveal that the majority of respondents (55.2 percent) has the same preferences for both types of tuition. As is evident from the areas above and below the main diagonal, only 9.0 percent are more supportive of regular than income-contingent tuition, whereas 35.8 percent are more supportive towards the latter. The biggest difference comes from 25.2 percent of respondents who oppose regular tuition but are in favor of income-contingent tuition.

The consecutive elicitation in waves 2014 and 2017 also allows us to estimate the effect of the *Earnings information* treatment on preferences for income-contingent tuition. The *Earnings information* treatment is provided together with the elicitation of preferences for regular tuition, and preferences for income-contingent tuition are elicited on the next survey screen. The analysis requires the assumption that respondents memorize the information provided in the survey experiment when answering the subsequent question on income-contingent tuition, which seems reasonable given that effects of treatment *Earnings information* persist in a follow-up survey two weeks later (see section 4.4). Results shown in Appendix Table A6 do not suggest that the *Earnings information* treatment has a noteworthy effect on preferences for income-contingent tuition. Preferences for income-contingent tuition, which are already more supportive than for regular tuition, thus seem less malleable to providing information on relative earnings.

5.2 Income Contingency: Loan Framing

While the income-contingent version of tuition entails an intertemporal deferral of payments, the baseline formulation of treatment *Income contingency* does not include an explicit reference to loans. Income-contingent tuition is characterized as a deferred payment, rather than as an upfront payment obligation combined with taking up a loan with an equivalent repayment scheme (as effectively implemented in Australia, England, and New Zealand). While these two characterizations refer to equivalent payment schemes, public opinion may differ between the two characterizations if people are subject to loan aversion (e.g., Loewenstein and Thaler, 1989; Oosterbeek and van den Broek, 2009; Field, 2009;

³³ In fact, support in the non-experimental setting is slightly higher, suggesting that prior consideration of regular tuition might induce a few participants to look more positively at income-contingent tuition (although wave effects may also play a role).

Abraham et al., 2018). It is often suggested that Germans may have a particularly strong aversion to taking up loans.³⁴

To test whether public preferences for income-contingent tuition depend on the absence of a reference to loans, in wave 2018 we implemented a second treatment, *Student loans*, that makes explicit reference to loan repayment requirements in the description of the income-contingent tuition scheme. As indicated in Table 5, this treatment has virtually the identical effect on public support for tuition as the baseline *Income contingency* treatment. Even across the five underlying answer categories, there are no statistically significant differences between the two treatment groups (see Panel B of Appendix Table A4). Thus, the majority appeal of income-contingent tuition is unaffected by making its debt aspect salient through an explicit loan framing, indicating that respondents understand the underlying payment schemes and the intertemporal deferral of payment obligations well. This finding is particularly interesting in light of the relevance that debt aversion seems to have for students' preferences for income-contingent student loan repayment schemes (Abraham et al., 2018).

In sum, we provide robust evidence that designing tuition as income-contingent rather than regular payments has a profound effect on its public appeal. Tuition has strong majority support if it is charged after graduation and only if future income exceeds a certain threshold.

5.3 Level of Tuition

As a second design feature of higher education finance schemes, we investigate the level of tuition payments. People may object to the basic principle of making students pay, but opposition may also depend on a tuition level deemed too high or an increase deemed too fast. Thus, in wave 2016 we test the effects of two experimental treatments that mention specific levels of tuition, compared to a control group that does not include an explicit reference to specific tuition levels.

Results indicate that public support declines with higher tuition levels. Table 7 shows that treatment *Level 500* which indicates tuition of 500 Euro per semester – i.e., the level that had been charged in Germany before tuition was abolished – does not significantly affect public support for or opposition to tuition compared to the control group. By contrast, treatment *Level 1500* which indicates tuition of 1,500 Euro per semester has a strong negative impact on public preferences. It reduces support by 11.7 percentage points and increases opposition by

³⁴ For examples, see BBC, 13 February 2015, <https://www.bbc.com/news/business-31369185> [accessed 10 February 2019] or Handelsblatt, 26 January 2018, <https://www.handelsblatt.com/today/opinion/germans-and-money-germanys-investment-culture-has-the-makings-of-a-social-and-financial-crisis/23580894.html> [accessed 10 February 2019].

10.0 percentage points, creating a majority of 56.1 percent against tuition. The treatment particularly increases strongly held opposition preferences (Panel C of Appendix Table A4).

The results on tuition levels have two key implications. First, the insignificant treatment effect of *Level 500* suggests that respondents seem to have the previously experienced tuition level in mind when stating preferences for tuition in general (i.e., without specifying a level). This is important for the interpretation of our above results on information provision and income contingency. Second, attempts to introduce tuition three times as high as the previously experienced level might be infeasible because a public majority opposes it. The downward slope in the political demand for tuition limits policy makers' scope for shifting the costs of higher education finance from taxpayers to users.

5.4 Tuition Targeted at Non-EU Students

The third design element that we study in this paper is whether tuition is universal or has to be paid only by a specific student group. Specifically, in wave 2018 we implement the experimental treatment *Non-EU students* to test whether public preferences for tuition change when it is targeted only towards non-EU citizens.

Results reveal that there is strong majority appeal for tuition targeted at non-EU citizens. Table 8 shows that restricting tuition to non-EU students increases support by 24.6 percentage points compared to universal tuition and reduces opposition by 27.8 percentage points. That is, tuition for non-EU citizens is extremely popular with 71.8 percent support and only 17.5 percent opposition in the public. The large support-inducing effect mostly reflects a shift in strongly held preferences in favor of tuition, increasing the share who “strongly favor” tuition for non-EU students by 18.9 percentage points (see Panel D of Appendix Table A4).

These results suggest that tuition for non-EU citizens is much more likely to be politically feasible than general tuition. This might explain why, despite some pushback from student organizations, this form of tuition has been introduced in the state of Baden-Württemberg lately, and other states such as North Rhine-Westphalia have signaled willingness to do so as well. On a broader note, charging tuition from students whose families are not subject to taxation in the respective constituency and thus do not contribute to the public funding of universities may have much greater public appeal than universal tuition. One reason for the common practice to charge higher tuition for non-residents in various countries might thus be that the public supports this policy.³⁵

³⁵ Another important reason for choosing this policy instrument might be that, at least in the U.S., public universities lack the unilateral authority to increase tuition for residents (see Jaquette and Curs, 2015).

6. Analysis of Subgroups and Adolescents

The analyses so far focused on average treatment effects among the electorate. In this section, we analyze effect heterogeneity across various subgroups to investigate the generality of our findings and to focus on special interest groups and the politically active population. Beforehand, we provide descriptive evidence on how preferences for tuition vary by sociodemographic characteristics. We close with an analysis of a survey of adolescents.

6.1 Preferences for Tuition among Different Population Subgroups

To describe how subgroups of the population differ in their preferences for tuition, Table 9 presents OLS regressions of support for regular and income-contingent tuition on respondents' characteristics using stacked data of all waves. Expectably, persons who hold a university entrance degree (i.e., potential future students), university graduates, current university students, and (marginally) parents are less supportive of regular tuition. Older respondents, males, full-time employees, and partisans of the conservative party that had introduced tuition (CDU) are more likely to favor regular tuition.

Current university students are also significantly less supportive of income-contingent tuition. Interestingly, persons who hold a university entrance degree, university graduates, and parents are not less likely to support this form of tuition than their counterparts. Support for income-contingent tuition is positively associated with age, employment, and CDU partisanship. Overall, the associations are in line with an interpretation that policy preferences partly reflect pecuniary self-interest.³⁶

6.2 Heterogeneous Treatment Effects: Generality, Interest Groups, and Active Voters

To investigate whether treatments affect different subgroups differently, Table 10 reports heterogeneous treatment effects for subgroups of respondents. The analysis focuses on the treatments that show significant average effects in the full samples and presents coefficients on the treatment-subgroup interaction terms from regressions based on equation (2). Depending on the respective characteristic, the sample is split into two or three subgroups.

Overall, there is relatively little evidence of heterogeneous treatment effects across subgroups. Only 9 of the 80 interaction terms in Table 10 are statistically significant at the 5

³⁶ From wave 2015 onwards, we also elicited a couple of indicators of political interest. Frequent voters are more likely to support income-contingent tuition, but do not differ from their counterparts on regular tuition. Respondents who state that education is important for their vote choice do not differ significantly from their counterparts on either form of tuition (results available upon request).

percent level. Thus, the effects reported in the previous sections are rather general and not due to extreme reactions of subgroups with certain sociodemographic characteristics.

Still, the table reveals some interesting systematic heterogeneity. Persons who hold a university entrance degree and current students (i.e., those who would have to pay tuition if it were introduced) exhibit significantly smaller reactions to treatment *Earnings information* than the rest of the population. For *Income contingency*, this is only true for current students. The fact that these treatments are less effective for these subgroups is relevant as these special interest groups might play a key role in determining higher education finance (Glomm, Ravikumar, and Schiopu, 2011). The negative interaction coefficient for university students is also large in absolute terms for *Level 1500*, though interactions for university students are imprecisely estimated due to their relatively small number in the sample. At the same time, parents – another important interest group – react significantly more positively to treatments *Income contingency* and *Non-EU students* than non-parents.

From a political-economy perspective, it is noteworthy that treatment effects are, if anything, larger for respondents who are politically active. Respondents who report that they vote frequently respond more positively to treatments *Income contingency* and *Non-EU students*. Respondents who consider education topics important for their voting decision respond more positively to treatments *Income contingency* and *Level 1500*. This indicates the political feasibility of implementing higher education finance policies that have majority appeal, namely income-contingent tuition and tuition for non-EU citizens.

6.3 Preferences for Tuition in a Survey of Adolescents

Moving beyond analyzing subsamples within the voting-age population, we finally present preferences for tuition of our separate representative survey of adolescents aged 14 to 17 years (N=1,085). We randomly assigned adolescents to the control group or to treatment *Income contingency*. Appendix Table A7 presents descriptive statistics and shows that observable characteristics are balanced across experimental groups. Among the adolescents, 57 percent expect to earn a university entrance degree and 52 percent expect to earn a university degree. These majorities would be affected by higher education finance reforms in the future, which makes adolescents a highly relevant focus group.

A clear majority of adolescents opposes regular tuition. The control mean reported in Table 11 shows that 62.9 percent of adolescents are against regular tuition. Only 26.6 percent of adolescents favor regular tuition. This finding resembles our above finding that (potential) students are generally less supportive of tuition.

Intriguingly, changing to an income-contingent payment design has almost the same effect among adolescents as it has among adults. Table 11 reports treatment effect estimates based on equation (1). Treatment *Income contingency* increases support for tuition among adolescents by 14.0 percentage points. This highly significant increase reduces the majority opposing regular tuition into a plurality of 46.3 percent opposing income-contingent tuition (40.6 percent in favor; 13.1 percent indifferent). The treatment effect operates through shifting both strongly and weakly held preferences (see Panel E of Appendix Table A4).

Subgroup analysis of the adolescent sample reveals that pecuniary self-interest, clearly visible in support for regular tuition, does not carry over to support for income-contingent tuition. Unsurprisingly, adolescents who expect to obtain a university degree are much less likely to support tuition (see Appendix Table A8, column 1). Interestingly, though, support for income-contingent tuition does not differ significantly across adolescent subgroups (column 2). By implication, estimates of treatment effect heterogeneity based on equation (2) indicate that treatment *Income contingency* is substantially larger for those adolescents who expect to go to university (column 3). The only other significant treatment heterogeneity is a smaller treatment effect for adolescents whose mother is employed (who, on average, are already more favorable to regular tuition). The treatment effect is also marginally larger for adolescents who state that they are strongly interested in politics.

In sum, the overall pattern of preferences for regular versus income-contingent tuition in the adult population is also prevalent among adolescents. While similar to university students in the adult sample, adolescents are much more opposed to regular tuition, they are split over income-contingent tuition. This shows that the design of tuition is a critical determinant for preferences for tuition also among the relevant focus group of adolescents.

7. Conclusions

While efficiency and equity considerations of different forms of higher education finance have been extensively studied, their political feasibility ultimately depends on whether they find political majorities, which is less clear a priori. We provide causal and representative evidence on the determinants of the electorate's preferences for university tuition. We investigate how information provision and the design of payment schemes shape the German electorate's preferences for tuition in experiments implemented in five representative survey waves with more than 19,500 responses.

We summarize our main findings in Table 12, which uses the stacked data from all experiments and all five waves. Overall, public opinion on tuition is divided, with a slight

plurality opposed to charging tuition. There is no significant trend in preferences for tuition, although support increases slightly,³⁷ whereas opposition is flat over the five survey years. Reassuringly, treatment effects of the two experiments that were repeated in several waves are statistically indistinguishable across waves, showing that our main results replicate in several representative samples of the German electorate.

Our experimental treatments show that information on the university earnings premium turns a public plurality against tuition into a plurality favoring tuition. By contrast, information on public costs of and unequal access to universities does not affect public opinion. Designing tuition as deferred payments that depend on the level of future income shifts the electorate towards a strong majority favoring tuition. The strong support for income-contingent tuition does not depend on whether payment is framed in terms of loans. Public support also depends on the level of tuition: The public is divided over regular tuition of 500 Euro per semester, but a majority opposes tuition of 1,500 Euro. A large majority supports tuition for non-EU citizens, indicating that targeting also affects political feasibility.

At the most basic level, we provide proof of concept that information provision can affect public preferences for tuition. This calls for incorporating citizens' imperfect information into theoretical political economy models of higher education finance (e.g., De Fraja, 2001; Borck and Wimbersky, 2014). From a policy perspective, our results suggest that information campaigns might help to gather public support for reforming higher education finance. Since information imperfections among the public can yield suboptimal policies in democratic decision-making processes (Romer, 2003), such information provision might enhance welfare. While the electorate responds strongly to relative earnings information, budgetary and redistributive considerations appear of secondary relevance for opinion on tuition.

The expansion of income-contingent financing schemes has been identified as a key step towards improving higher education finance (e.g., Jacobs and van der Ploeg, 2006; Diris and Ooghe, 2018) because they have attractive efficiency and equity properties (e.g., Barr, 2004; Chapman, Higgins, and Stiglitz, 2014; Findeisen and Sachs, 2016; Stantcheva, 2017). While income-contingent tuition barely entered the German public debate, the fact that it has broad majority appeal indicates leeway for implementation. The majority appeal for income-contingent tuition also does not appear to be subject to loan aversion in our setting. At the same time, policy makers' scope for re-organizing higher education finance is limited by the public's aversion to tuition levels that exceed those previously charged in Germany.

³⁷ Support in wave 2018 is in fact significantly higher than in wave 2014, the only statistically significant difference in support or opposition over any pair of the five waves.

Recently soaring fiscal deficits in most OECD countries and the ensuing necessity for fiscal consolidation bring about the need to reconsider public education finance. This paper identifies what determines public support for reforms in this policy area. Germany provides an interesting setting for studying the political economy of tuition that may be particularly relevant for countries that struggle to introduce or maintain tuition. The generalizability of our findings to other countries, which might depend on institutional and political circumstances, requires further research. While acceptance of the principle of tuition may be higher in countries with longer experience with tuition, tuition levels seen as too high or rising too fast may still set upper limits to higher education finance reforms. This paper covers how relevant pieces of information and design of tuition schemes affect public preferences, but it does not address other potentially important determinants, such as the usage of tuition revenues or public provision of student aid. We consider the exploration of alternative settings and further determinants of public support for higher education finance schemes an interesting direction for future research.

References

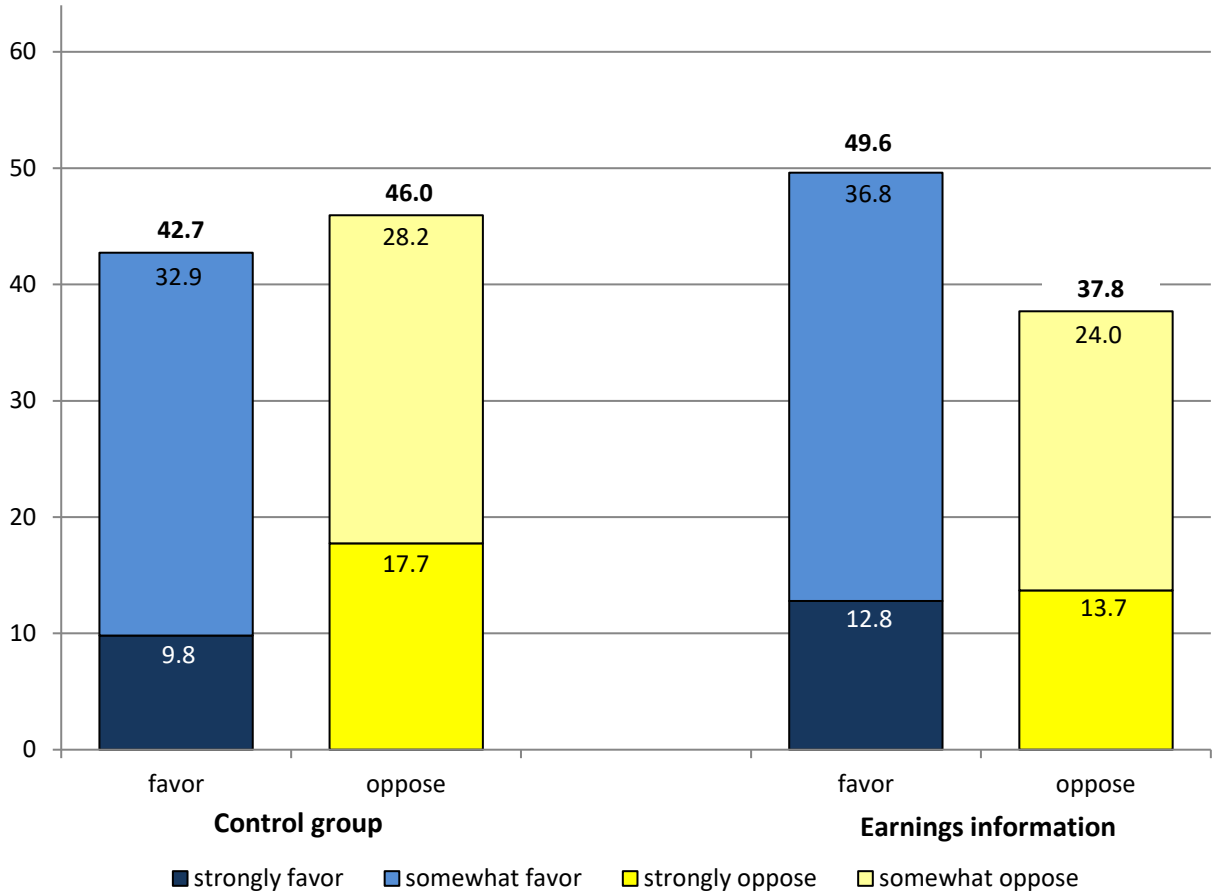
- Abraham, Katharine G., Emel Filiz-Ozbay, Erkut Y. Ozbay, Lesley J. Turner (2018). Framing effects, earnings expectations, and the design of student loan repayment schemes. NBER Working Paper 24484. Cambridge, MA: National Bureau of Economic Research.
- Alesina, Alberto, Armando Miano, Stefanie Stantcheva (2018). Immigration and redistribution. NBER Working Paper 24733. Cambridge, MA: National Bureau of Economic Research.
- Alesina, Alberto, Stefanie Stantcheva, Edoardo Teso (2018). Intergenerational mobility and support for redistribution. *American Economic Review* 108 (2): 521-554.
- Allcott, Hunt, Matthew Gentzkow (2017). Social media and fake news in the 2016 election. *Journal of Economic Perspectives* 31 (2): 211-36.
- Barr, Nicholas (2004). Higher education funding. *Oxford Review of Economic Policy* 20 (2): 264-283.
- Barr, Nicholas, Bruce Chapman, Lorraine Dearden, and Susan Dynarski (2019). The US college loans system: Lessons from Australia and England. *Economics of Education Review*, forthcoming.
- Bertrand, Marianne, Sendhil Mullainathan (2001). Do people mean what they say? Implications for subjective survey data. *American Economic Review: Papers & Proceedings* 91 (2): 67-72.
- Bettinger, Eric, Bridget Terry Long, Philip Oreopoulos, Lisa Sanbonmatsu (2012). The role of application assistance and information in college decisions: Results from the H&R block FAFSA experiment. *Quarterly Journal of Economics* 127 (3): 1205-1242.
- Bleemer, Zachary, Basit Zafar (2018). Intended college attendance: Evidence from an experiment on college returns and costs. *Journal of Public Economics* 157: 184-211.
- Borck, Rainald, Martin Wimbersky (2014). Political economics of higher education finance. *Oxford Economic Papers* 66 (1): 115-139.
- Bursztyjn, Leonardo (2016). Poverty and the political economy of public education spending: Evidence from Brazil. *Journal of the European Economic Association* 14 (5): 1101-1128.
- Cavallo, Alberto, Guillermo Cruces, Ricardo Perez-Truglia (2017). Inflation expectations, learning and supermarket prices: Evidence from field experiments. *American Economic Journal: Macroeconomics* 9 (3): 1-35.
- Chapman, Bruce (2006). Income contingent loans for higher education: International reforms. In *Handbook of the Economics of Education*, edited by Eric A. Hanushek, Finis Welch. Amsterdam: North Holland: 1435-1503.
- Chapman, Bruce, Timothy Higgins, Joseph E. Stiglitz, eds. (2014). *Income contingent loans: Theory, practice and prospects*. International Economic Association Conference Volume No. 153. Houndmills: Palgrave Macmillan.
- Cruces, Guillermo, Ricardo Perez-Truglia, Martin Tetaz (2013). Biased perceptions of income distribution and preferences for redistribution: Evidence from a survey experiment. *Journal of Public Economics* 98: 100-112.
- De Fraja, Gianni (2001). Education policies: Equity, efficiency and voting equilibrium. *Economic Journal* 111 (471): 104-119.
- De Quidt, Jonathan, Johannes Haushofer, Christopher Roth (2018). Measuring and Bounding Experimenteer Demand. *American Economic Review* 108 (11): 3266-3302.

- Delavande, Adeline, Basit Zafar (2019). University choice: The role of expected earnings, non-pecuniary outcomes, and financial constraints. *Journal of Political Economy*, forthcoming.
- Diris, Ron, Erwin Ooghe (2018). The economics of financing higher education. *Economic Policy* 33 (94): 265-314.
- Dwenger, Nadja, Johanna Storck, Katharina Wrohlich (2012). Do tuition fees affect the mobility of university applicants? Evidence from a natural experiment. *Economics of Education Review* 31 (1): 155-167.
- Dynarski, Susan (2003). Does aid matter? Measuring the effect of student aid on college attendance and completion. *American Economic Review* 93 (1): 273-287.
- Dynarski, Susan (2014). An economist's perspective on student loans in the United States. Brookings Institution Economic Studies Working Paper Series. Washington, DC.
- Epple, Dennis, Richard E. Romano (1996). Ends against the middle: Determinating public service provision when there are private alternatives. *Journal of Public Economics* 62 (3): 297-325.
- Falk, Armin, Anke Becker, Thomas Dohmen, Benjamin Enke, David Huffman, Uwe Sunde (2018). Global evidence on economic preferences. *Quarterly Journal of Economics* 133 (4): 1645-1692.
- Falk, Armin, Florian Zimmermann (2013). A taste for consistency and survey response behavior. *CESifo Economic Studies* 59 (1): 181-193.
- Fernandez, Raquel, Richard Rogerson (1995). On the political economy of education subsidies. *Review of Economic Studies* 62 (2): 249-262.
- Field, Erica (2009). Educational debt burden and career choice: Evidence from a financial aid experiment at NYU Law School. *American Economic Journal: Applied Economics* 1 (1): 1-21.
- Findeisen, Sebastian, Dominik Sachs (2016). Education and optimal dynamic taxation: The role of income-contingent student loans. *Journal of Public Economics* 138: 1-21.
- Garibaldi, Pietro, Francesco Giavazzi, Andrea Ichino, Enrico Rettore (2012). College cost and time to complete a degree: Evidence from tuition discontinuities. *Review of Economics and Statistics* 94 (3): 699-711.
- Gilens, Martin (2001). Political ignorance and collective policy preferences. *American Political Science Review* 95 (2): 379-396.
- Grewenig, Elisabeth, Philipp Lergetporer, Lisa Simon, Katharina Werner, Ludger Woessmann (2018). Can online surveys represent the entire population? CESifo Working Paper 7222. Munich: CESifo.
- Glomm, Gerhard, B. Ravikumar, Ioana C. Schiopu (2011). The political economy of education funding. In *Handbook of the Economics of Education, Vol. 4*, edited by Eric A. Hanushek, Stephen Machin, Ludger Woessmann. Amsterdam: North Holland: 615-680.
- Grossman, Gene M., Elhanan Helpman (2001). *Special Interest Politics*. Cambridge, MA: MIT Press.
- Haaland, Ingar, Christopher Roth (2017). Labor market concerns and support for immigration. Working Paper.
- Helbig, Marcel, Tina Baier, Anja Kroth (2012). Die Auswirkung von Studiengebühren auf die Studierneigung in Deutschland: Evidenz aus einem natürlichen Experiment auf Basis der HIS-Studienberechtigtenbefragung. *Zeitschrift für Soziologie* 41 (3): 227-246.

- Hoxby, Caroline M., Sarah Turner (2015). What high-achieving low-income students know about college. *American Economic Review: Papers & Proceedings* 105 (5): 514-517.
- Hübner, Malte (2012). Do tuition fees affect enrollment behavior? Evidence from a 'natural experiment' in Germany. *Economics of Education Review* 31 (6): 949-960.
- Jensen, Robert (2010). The (perceived) returns to education and the demand for schooling. *Quarterly Journal of Economics* 125 (2): 515-548.
- Jacobs, Bas, Frederick van der Ploeg (2006). Guide to reform of higher education: A European perspective. *Economic Policy* 21 (47): 535-592.
- Jaquette, Ozan, Bradley R. Curs (2015). Creating the out-of-state university: Do public universities increase nonresident freshman enrollment in response to declining state appropriations? *Research in Higher Education* 56: 535-565.
- Johnstone, D. Bruce (2004). The economics and politics of cost sharing in higher education: Comparative perspectives. *Economics of Education Review* 23 (4): 403-410.
- Kauder, Björn, Niklas Potrafke (2013). Government ideology and tuition fee policy: Evidence from the German states. *CESifo Economic Studies* 59 (4): 628-649.
- Kuklinski, James H., Paul J. Quirk, Jennifer Jerit, David Schwieder, Robert F. Rich (2000). Misinformation and the currency of democratic citizenship. *Journal of Politics* 62 (3): 790-816.
- Kuziemko, Ilyana, Michael I. Norton, Emmanuel Saez, Stefanie Stantcheva (2015). How elastic are preferences for redistribution? Evidence from randomized survey experiments. *American Economic Review* 105 (4): 1478-1508.
- Lange, Thomas (2013). Return migration of foreign students and non-resident tuition fees. *Journal of Population Economics* 26: 703-718.
- Lavecchia, Adam M., Heidi Liu, Philip Oreopoulos (2016). Behavioral Economics of Education: Progress and Possibilities. In *Handbook of the Economics of Education, Vol. 5*, edited by Eric A. Hanushek, Stephen Machin, Ludger Woessmann. Amsterdam: North Holland: 1-74.
- Lergetporer, Philipp, Guido Schwerdt, Katharina Werner, Martin R. West, Ludger Woessmann (2018). How information affects support for education spending: Evidence from survey experiments in Germany and the United States. *Journal of Public Economics* 167: 138-157.
- Lergetporer, Philipp, Katharina Werner, Ludger Woessmann (2018). Does ignorance of economic returns and costs explain the educational aspiration gap? Evidence from representative survey experiments. CESifo Working Paper 7000. Munich: CESifo.
- Loewenstein, George, Richard Thaler (1989). Anomalies: Intertemporal choice. *Journal of Economic Perspectives* 3 (4): 181-193.
- Lovenheim, Michael, Sarah Turner (2017). *Economics of education*. New York: Worth Publ.
- Lowry, Robert C. (2001). The effects of state political interests and campus outputs on public university revenues. *Economics of Education Review* 20 (2): 105-119.
- Maniadis, Zacharias, Fabio Tufano, John A. List (2014). One swallow doesn't make a summer: New evidence on anchoring effects. *American Economic Review* 104 (1): 277-290.
- McGuigan, Martin, Sandra McNally, Gill Wyness (2016). Student awareness of costs and benefits of educational decisions: Effects of an information campaign. *Journal of Human Capital* 10 (4): 482-519.

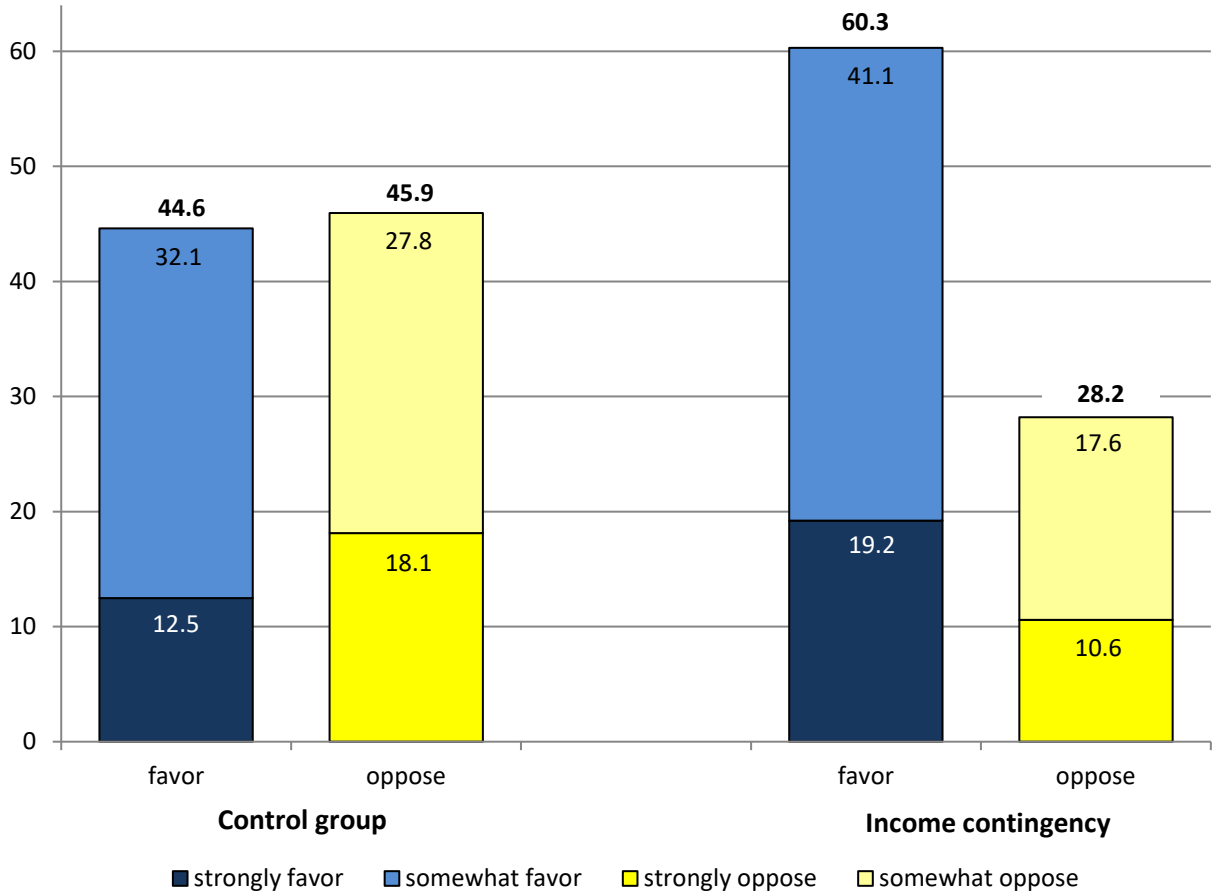
- Middendorff, Elke, Beate Apolinarski, Jonas Poskowsky, Maren Kandulla, Nicolai Netz (2013). *Die wirtschaftliche und soziale Lage der Studierenden in Deutschland 2012: 20. Sozialerhebung des Deutschen Studentenwerks durchgeführt durch das HIS-Institut für Hochschulforschung*. Berlin: Bundesministerium für Bildung und Forschung.
- Mummolo, Jonathan, Erik Peterson (2019). Demand effects in survey experiments: An empirical assessment. *American Political Science Review*, forthcoming.
- Murphy, Richard, Judith Scott-Clayton, Gill Wyness (2019). The end of free college in England: Implications for enrolments, equity, and quality. *Economics of Education Review*, forthcoming.
- Norton, Michael I., Dan Ariely (2011). Building a better America—One wealth quintile at a time. *Perspectives on Psychological Science* 6 (1): 9-12.
- Nyhan, Brendan, Jason Reifler (2010). When corrections fail: The persistence of political misperceptions. *Political Behavior* 32 (2): 303-330.
- OECD (2018). *Education at a glance 2018: OECD indicators*. Paris: Organisation for Economic Co-operation and Development.
- Oosterbeek, Hessel, Anja van den Broek (2009). An empirical analysis of borrowing behaviour of higher education students in the Netherlands. *Economics of Education Review* 28 (2): 170-177.
- Oreopoulos, Philip, Uros Petronijevic (2013). Making college worth it: A review of research on the returns to higher education. *The Future of Children* 23 (1): 41-65.
- Roth, Christopher, Johannes Wolfart (2018). Public debt and the demand for government spending and taxation. Working Paper.
- Romer, David (2003). Misconceptions and political outcomes. *Economic Journal* 113 (484): 1-20.
- Soares, Jorge (2003). Self-interest and public funding of education. *Journal of Public Economics* 87 (3-4): 703-727.
- Stantcheva, Stefanie (2017). Optimal taxation and human capital policies over the life cycle. *Journal of Political Economy* 125 (6): 1931-1990.
- Statistisches Bundesamt (2014). *Bildungsfinanzbericht 2013*. Wiesbaden: Stat. Bundesamt.
- Statistisches Bundesamt (2019). Studierende an Hochschulen. Fachserie 11 Reihe 4.1. Wiesbaden: Statistisches Bundesamt.
- Thomsen, Stephan, Friederike von Haaren-Giebel (2016). Did tuition fees in Germany constrain students' budgets? New evidence from a natural experiment. *IZA Journal of European Labor Studies* 5 (1): 6.
- U.S. Department of Education (2019). Digest of Education Statistics - <https://nces.ed.gov/programs/digest>. Washington, DC: National Center for Education Statistics. [accessed January 15, 2019]
- Wigger, Berthold U., Robert K. von Weizsäcker (2001). Risk, resources, and education: Public versus private financing of higher education. *IMF Staff Papers* 48 (3): 547-560.
- Wiswall, Matthew, Basit Zafar (2015). Determinants of college major choice: Identification using an information experiment. *Review of Economic Studies* 82 (2): 791-824.
- Zizzo, Daniel (2010). Experimenter demand effects in economic experiments. *Experimental Economics* 13 (1): 75-98.

Figure 1: The effect of providing earnings information on public preferences for tuition



Notes: Residual category: “neither favor nor oppose.” Data source: ifo Education Survey 2014, 2015, and 2017.

Figure 2: The effect of an income-contingent payment design on public preferences for tuition



Notes: Residual category: “neither favor nor oppose.” The slight difference in control-group support to Figure 1 is due to the fact that the experiments were fielded in different waves. Data source: ifo Education Survey 2015, 2016, and 2018.

Table 1: Data structure: Survey waves, experimental groups, and numbers of observations

| | Control group | Treatment groups | | | Total |
|-----------|---------------|--------------------------------------|------------------------------------|------------------------------------|--------|
| | (1) | (2) | (3) | (4) | (5) |
| Wave 2014 | 1,032 | <i>Earnings information</i> 1,030 | <i>Cost information</i> 1,056 | <i>Access information</i> 1,053 | 4,171 |
| Wave 2015 | 1,390 | <i>Earnings information</i> 1,355 | <i>Income contingency</i> 1,360 | | 4,105 |
| Wave 2016 | 781 | <i>Income contingency</i> 852 | <i>Level 500</i> 804 | <i>Level 1500</i> 865 | 3,302 |
| Wave 2017 | 2,075 | <i>Earnings information</i> 2,003 | | | 4,078 |
| Wave 2018 | 1,036 | <i>Income contingency</i> 1,005 | <i>Student loans</i> 970 | <i>Non-EU students</i> 1,035 | 4,046 |
| Total | | | | | 19,702 |

Notes: Numbers of observations per group. Numbers include participants with missing responses on the tuition question. The total number of responses on the tuition question is 19,577 observations. See Appendix Tables A2 and A3 for item non-response. Data source: ifo Education Survey 2014-2018.

Table 2: Effects of providing information about earnings, costs, and access on preferences for tuition

| | Support for tuition | | Opposition against tuition | |
|----------------------|---------------------|---------------------|----------------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| Earnings information | 0.081*** (0.024) | 0.083*** (0.023) | -0.089*** (0.024) | -0.091*** (0.023) |
| Cost information | 0.027 (0.024) | 0.033 (0.023) | -0.003 (0.024) | -0.011 (0.023) |
| Access information | -0.029 (0.023) | -0.012 (0.023) | 0.038 (0.024) | 0.021 (0.023) |
| Covariates | No | Yes | No | Yes |
| Control mean | 0.400 | 0.400 | 0.457 | 0.457 |
| Observations | 4,123 | 4,123 | 4,123 | 4,123 |
| R^2 | 0.007 | 0.058 | 0.009 | 0.079 |

Notes: OLS regressions. *Earnings information*, *Cost information*, and *Access information*: experimental treatments in the survey experiment. Sample: Wave 2014. Dependent variable: Col. (1)-(2): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; col. (3)-(4): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise. Residual category: “neither favor nor oppose.” Control mean: mean of the outcome variable in the control group. Covariates include age, income, patience, altruism, and dummies for gender, born in Germany, living in a tuition state, city size, living with partner in household, parents’ university degree, working in the education sector, highest educational attainment, professional degree, employment status, parent status, and political party preferences. Missing values of covariates are imputed; regressions with covariates include imputation dummies. Data source: ifo Education Survey 2014. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: Heterogeneous treatment effects of providing earnings information by initial beliefs

| | Support for tuition | | | | Opposition against tuition | | | |
|-------------------------------|---------------------|---------------------|--------------------|-----------------------|----------------------------|---------------------|--------------------|-----------------------|
| | All | | Sure about belief | Not sure about belief | All | | Sure about belief | Not sure about belief |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Earnings information | 0.052** (0.025) | 0.126*** (0.047) | 0.180** (0.084) | 0.101* (0.055) | -0.071*** (0.024) | -0.109** (0.046) | -0.139* (0.079) | -0.088 (0.054) |
| Belief (% of Actual) | | 0.042 (0.026) | 0.033 (0.046) | 0.045 (0.031) | | -0.013 (0.025) | 0.006 (0.045) | -0.023 (0.030) |
| Earnings information × Belief | | -0.066* (0.034) | -0.107* (0.058) | -0.049 (0.041) | | 0.038 (0.033) | 0.064 (0.055) | 0.022 (0.040) |
| Covariates | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Control mean | 0.438 | | | | 0.464 | | | |
| Observations | 2,730 | 2,682 | 750 | 1,932 | 2,730 | 2,682 | 750 | 1,932 |
| R ² | 0.058 | 0.056 | 0.130 | 0.053 | 0.078 | 0.072 | 0.139 | 0.072 |

Notes: OLS regressions. *Earnings information*: experimental treatment in the survey experiment. Sample: Control group and earnings information treatment group in wave 2015. Dependent variable: Col. (1)-(3): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; col. (4)-(6): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise. Residual category: “neither favor nor oppose.” Belief: continuous variable measuring the difference of the estimated earnings of university graduates from the actual earnings of those without a degree as a percentage of the actual earnings difference between the two groups. Control mean: mean of the outcome variable in the control group. “Sure about belief”: subgroup of respondents who were relatively sure that the stated belief is close to correct (choosing a value between 5 and 7 on a scale from 1 (“very unsure”) to 7 (“very sure”). “Not sure about belief”: respondents who chose a value between 1 and 4. Covariates include age, income, and dummies for gender, born in Germany, living in a tuition state, city size, living with partner in household, parents’ university degree, working in the education sector, school education, university degree, university student status, employment status, parent status, political partisanship, voting behavior and considering education important for the vote choice. Missing values of covariates are imputed; regressions include imputation dummies. Data source: ifo Education Survey 2015. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 4: Persistence of effects of earnings information on preferences for tuition in a follow-up survey

Panel A: Main survey

| | Support for tuition | | Opposition against tuition | |
|----------------------|---------------------|---------------------|----------------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| Earnings information | 0.071*** (0.018) | 0.073*** (0.018) | -0.080*** (0.018) | -0.077*** (0.018) |
| Covariates | No | Yes | No | Yes |
| Control mean | 0.433 | 0.433 | 0.458 | 0.458 |
| Observations | 4,059 | 4,059 | 4,059 | 4,059 |
| R ² | 0.005 | 0.068 | 0.007 | 0.078 |

Panel B: Follow-up survey

| | Beliefs | | | Support for tuition | | Opposition against tuition | |
|----------------------|----------------------|---------------------|----------------------|---------------------|------------------|----------------------------|---------------------|
| | Too low (1) | Correct (2) | Too high (3) | (4) | (5) | (6) | (7) |
| Earnings information | -0.052*** (0.016) | 0.122*** (0.020) | -0.070*** (0.016) | 0.027 (0.020) | 0.022 (0.020) | -0.054*** (0.020) | -0.046** (0.020) |
| Covariates | Yes | Yes | Yes | No | Yes | No | Yes |
| Control mean | 0.222 | 0.563 | 0.216 | 0.437 | 0.437 | 0.478 | 0.478 |
| Observations | 2,362 | 2,362 | 2,362 | 2,363 | 2,363 | 2,363 | 2,363 |
| R ² | 0.089 | 0.046 | 0.062 | 0.001 | 0.066 | 0.003 | 0.085 |

Notes: OLS regressions. *Earnings information*: experimental treatment in the main survey. Sample: Wave 2017. Dependent variable: Panel A, col. (1)-(2) and Panel B, col. (4)-(5): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; Panel A, col. (3)-(4) and Panel B, col. (6)-(7): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise; Panel B, col. (1)-(3): Dummy variables 1 = belief of graduate earnings is below, within, or above the correct range, respectively, where the correct range is defined by being within an interval of 20 percent below or above the true value, 0 otherwise. Control mean: mean of the outcome variable in the control group. Covariates include age, income, patience, risk tolerance, and dummies for gender, born in Germany, living in a tuition state, city size, living with partner in household, parents’ university degree, working in the education sector, highest educational attainment, professional degree, employment status, parent status, political party preferences, and voting behavior. Missing values of covariates are imputed; regressions with covariates include imputation dummies. Data source: ifo Education Survey 2017. Regressions in main survey weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 5: Effects of income-contingent payment design on preferences for tuition

| | Support for tuition | | Opposition against tuition | |
|-----------------------------------|---------------------|---------------------|----------------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| Income contingency | 0.153*** (0.026) | 0.151*** (0.025) | -0.158*** (0.026) | -0.154*** (0.025) |
| Income contingency × Wave 2016 | -0.000 (0.040) | 0.004 (0.038) | -0.007 (0.039) | -0.013 (0.038) |
| Income contingency × Wave 2018 | 0.001 (0.042) | 0.008 (0.041) | -0.033 (0.040) | -0.040 (0.040) |
| Income contingency × Loan framing | -0.003 (0.031) | -0.005 (0.031) | -0.006 (0.028) | -0.006 (0.028) |
| Wave 2016 | -0.014 (0.028) | -0.012 (0.028) | -0.003 (0.028) | 0.003 (0.028) |
| Wave 2018 | 0.033 (0.029) | 0.024 (0.030) | -0.011 (0.029) | 0.002 (0.029) |
| Covariates | No | Yes | No | Yes |
| Control mean | 0.438 | 0.438 | 0.464 | 0.464 |
| Observations | 7,360 | 7,360 | 7,360 | 7,360 |
| R^2 | 0.026 | 0.054 | 0.035 | 0.060 |

Notes: OLS regressions. *Income contingency*: experimental treatment in the survey experiment. Sample: Control groups and income contingency treatment groups in waves 2015, 2016, and 2018. Dependent variable: Col. (1)-(2): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; col. (3)-(4): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise. Residual category: “neither favor nor oppose.” Control mean: mean of the outcome variable in the control group. Covariates include age, income, and dummies for gender, born in Germany, living in a tuition state, city size, living with partner in household, parents’ university degree, working in the education sector, highest educational attainment, professional degree, employment status, parent status, political party preferences, and voting behavior. Missing values of covariates are imputed; regressions with covariates include imputation dummies. Data source: ifo Education Survey 2015, 2016, 2018. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 6: Within-subject comparison of preferences for regular and income-contingent tuition

| | Income-contingent tuition | | | Total (4) |
|-----------------|---------------------------|----------------|---------------|--------------|
| | Support (1) | Neutral (2) | Oppose (3) | |
| Regular tuition | | | | |
| Support | 0.334 | 0.023 | 0.056 | 0.413 |
| Neutral | 0.058 | 0.070 | 0.011 | 0.139 |
| Oppose | 0.252 | 0.048 | 0.148 | 0.448 |
| Total | 0.644 | 0.141 | 0.215 | 1.000 |

Notes: Sample: Control groups in waves 2014 and 2017. Shares of respondents who support, oppose, or are neutral towards tuition (weighted group means). Data source: ifo Education Survey 2014, 2017.

Table 7: Effects of tuition levels on preferences for tuition

| | Support for tuition | | Opposition against tuition | |
|--------------|----------------------|----------------------|----------------------------|---------------------|
| | (1) | (2) | (3) | (4) |
| Level 500 | -0.033 (0.029) | -0.029 (0.029) | 0.031 (0.030) | 0.029 (0.030) |
| Level 1500 | -0.117*** (0.028) | -0.111*** (0.027) | 0.100*** (0.029) | 0.091*** (0.029) |
| Covariates | No | Yes | No | Yes |
| Control mean | 0.424 | 0.424 | 0.460 | 0.460 |
| Observations | 2,426 | 2,426 | 2,426 | 2,426 |
| R^2 | 0.010 | 0.055 | 0.007 | 0.058 |

Notes: OLS regressions. *Level 500* and *Level 1500*: experimental treatment in the survey experiment. Sample: Control group and level treatment groups in wave 2016. Dependent variable: Col. (1)-(2): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; col. (3)-(4): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise. Residual category: “neither favor nor oppose.” Control mean: mean of the outcome variable in the control group. Covariates include age, income, patience, risk tolerance, and dummies for gender, born in Germany, living in a tuition state, city size, living with partner in household, parents’ university degree, working in the education sector, highest educational attainment, professional degree, employment status, parent status, political party preferences, and voting behavior. Missing values of covariates are imputed; regressions with covariates include imputation dummies. Data source: ifo Education Survey 2016. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8: Effects of targeting non-EU students on preferences for tuition

| | Support for tuition | | Opposition against tuition | |
|-----------------|---------------------|---------------------|----------------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| Non-EU students | 0.246*** (0.031) | 0.236*** (0.030) | -0.278*** (0.029) | -0.265*** (0.028) |
| Covariates | No | Yes | No | Yes |
| Control mean | 0.472 | 0.472 | 0.453 | 0.453 |
| Observations | 2,071 | 2,071 | 2,071 | 2,071 |
| R^2 | 0.063 | 0.106 | 0.089 | 0.151 |

Notes: OLS regressions. *Non-EU students*: experimental treatment in the survey experiment. Sample: Control group and non-EU student treatment group in wave 2018. Dependent variable: Col. (1)-(2): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; col. (3)-(4): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise. Residual category: “neither favor nor oppose.” Control mean: mean of the outcome variable in the control group. Covariates include age, income, patience, risk tolerance, and dummies for gender, born in Germany, living in a tuition state, city size, living with partner in household, parents’ university degree, working in the education sector, highest educational attainment, professional degree, employment status, parent status, political party preferences, and voting behavior. Missing values of covariates are imputed; regressions with covariates include imputation dummies. Data source: ifo Education Survey 2018. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 9: Who supports tuition?

| | Support for regular tuition | | Support for income-contingent tuition | |
|-----------------------------------|-----------------------------|---------|---------------------------------------|---------|
| | (1) | | (2) | |
| Age | 0.002*** | (0.001) | 0.002*** | (0.001) |
| Female | -0.060*** | (0.017) | -0.021 | (0.014) |
| Born in Germany | 0.042 | (0.038) | 0.008 | (0.032) |
| Tuition state | -0.007 | (0.018) | -0.001 | (0.015) |
| City size \geq 100,000 | -0.005 | (0.018) | -0.007 | (0.014) |
| Monthly household income (1000 €) | 0.006 | (0.007) | 0.005 | (0.006) |
| Partner in household | 0.006 | (0.019) | 0.010 | (0.016) |
| Parent(s) with university degree | 0.022 | (0.019) | -0.021 | (0.016) |
| Works in education sector | 0.060* | (0.032) | 0.001 | (0.027) |
| Middle school degree | 0.006 | (0.021) | 0.002 | (0.016) |
| University entrance degree | -0.058** | (0.028) | 0.014 | (0.022) |
| University degree | -0.065** | (0.029) | -0.033 | (0.025) |
| University student | -0.082* | (0.048) | -0.126*** | (0.042) |
| Full-time employed | 0.071*** | (0.022) | 0.049*** | (0.018) |
| Part-time employed | 0.039 | (0.026) | 0.041* | (0.023) |
| Self-employed | -0.059 | (0.042) | 0.076** | (0.033) |
| Unemployed | -0.005 | (0.038) | 0.004 | (0.032) |
| At least one child < 18 | -0.034 | (0.022) | 0.015 | (0.019) |
| All children > 18 | -0.043* | (0.025) | 0.034* | (0.020) |
| CDU partisan | 0.082*** | (0.020) | 0.052*** | (0.016) |
| Survey wave dummies | Yes | | Yes | |
| Constant | 0.259*** | (0.065) | 0.476*** | (0.053) |
| Observations | 6,283 | | 8,314 | |
| R^2 | 0.035 | | 0.031 | |

Notes: OLS regressions. Samples: Col. (1): control groups in waves 2014-2018; col. (2): Income contingency treatment groups in waves 2014-2018. Dependent variable: Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise. Missing values of covariates are imputed; regressions include imputation dummies. Data source: ifo Education Survey 2014-2018. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 10: Heterogeneous treatment effects

| Treatment: | Dependent variable: Support for tuition | | | | | | | |
|--|---|---------|--------------------|---------|------------|---------|-----------------|---------|
| | Earnings information | | Income contingency | | Level 1500 | | Non-EU students | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | |
| <i>Interaction of treatment with:</i> | | | | | | | | |
| Age under 45 (age 45-65) | 0.006 | (0.029) | -0.055 | (0.038) | 0.039 | (0.064) | -0.116* | (0.066) |
| Age over 65 (age 45-65) | 0.008 | (0.035) | 0.038 | (0.044) | -0.051 | (0.074) | 0.074 | (0.078) |
| Female (male) | 0.011 | (0.026) | 0.028 | (0.035) | 0.074 | (0.057) | 0.036 | (0.062) |
| Born in Germany (not born in Germany) | -0.019 | (0.056) | -0.007 | (0.086) | 0.042 | (0.142) | 0.081 | (0.152) |
| Tuition state (no tuition state) | 0.023 | (0.027) | -0.034 | (0.037) | -0.082 | (0.063) | -0.017 | (0.065) |
| City size \geq 100,000 (city size < 100,000) | -0.001 | (0.028) | 0.021 | (0.037) | 0.008 | (0.059) | -0.027 | (0.066) |
| Monthly household income above median (below median) | -0.041 | (0.026) | 0.011 | (0.035) | -0.032 | (0.058) | -0.014 | (0.062) |
| Partner in household (no partner in household) | 0.033 | (0.026) | 0.070* | (0.036) | 0.013 | (0.058) | 0.030 | (0.065) |
| Parent(s) with university degree (without university degree) | -0.047 | (0.029) | -0.035 | (0.039) | 0.001 | (0.066) | -0.148** | (0.066) |
| Works in education sector (not working in education sector) | 0.017 | (0.049) | -0.049 | (0.061) | 0.138 | (0.123) | -0.110 | (0.113) |
| Middle school degree (no degree) | -0.049 | (0.031) | -0.007 | (0.041) | 0.009 | (0.069) | 0.014 | (0.076) |
| University entrance degree (no degree) | -0.098** | (0.032) | -0.006 | (0.043) | 0.038 | (0.070) | 0.037 | (0.075) |
| University degree (no university degree) | -0.047 | (0.036) | 0.072 | (0.048) | 0.021 | (0.081) | 0.049 | (0.081) |
| University student (no university student) | -0.112* | (0.058) | -0.205** | (0.101) | -0.221 | (0.143) | 0.044 | (0.170) |
| Full-time employed (not full-time employed) | -0.020 | (0.027) | -0.025 | (0.036) | 0.068 | (0.060) | -0.038 | (0.061) |
| At least one child < 18 (no children) | 0.020 | (0.032) | 0.082* | (0.043) | -0.067 | (0.078) | 0.004 | (0.081) |
| All children > 18 (no children) | 0.006 | (0.031) | 0.101** | (0.042) | -0.017 | (0.065) | 0.205*** | (0.069) |
| CDU partisan (no CDU partisan) | -0.010 | (0.031) | -0.008 | (0.042) | -0.082 | (0.072) | 0.028 | (0.076) |
| Frequent voter (no frequent voter) | -0.027 | (0.038) | 0.089** | (0.044) | -0.010 | (0.067) | 0.204** | (0.085) |
| Education important for vote choice (not important) | -0.009 | (0.035) | 0.103** | (0.040) | 0.135** | (0.067) | 0.024 | (0.066) |

Notes: OLS regressions. Reported coefficients are interaction terms between the treatment indicated in the column headers and the respective variables indicated in the left column. Samples: Col. (1): Waves 2014, 2015, and 2017; col. (2): waves 2015, 2016, and 2018; col. (3): wave 2016; col. (4)-(5): wave 2018. Dependent variable: Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise. The table displays coefficients on the interaction term between treatment and subgroup indicators from estimates based on equation (2). The respective baseline category is indicated in parentheses. Data source: ifo Education Survey 2014, 2015, 2016, 2017, 2018. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 11: Effects of income-contingent payment design on preferences for tuition among adolescents

| | Support for tuition | | Opposition against tuition | |
|--------------------|---------------------|---------------------|----------------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| Income contingency | 0.140*** (0.030) | 0.138*** (0.030) | -0.166*** (0.032) | -0.167*** (0.031) |
| Covariates | No | Yes | No | Yes |
| Control mean | 0.266 | 0.266 | 0.629 | 0.629 |
| Observations | 1,085 | 1,085 | 1,085 | 1,085 |
| R^2 | 0.022 | 0.049 | 0.028 | 0.067 |

Notes: OLS regressions. *Income contingency*: experimental treatment in the survey experiment. Sample: Adolescent survey, 2018. Dependent variable: Col. (1)-(2): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; col. (3)-(4): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise. Residual category: “neither favor nor oppose.” Control mean: mean of the outcome variable in the control group. Covariates include age, patience, risk tolerance, and dummies for gender, born in Germany, living in a tuition state, city size, living with both parents in household, parents’ university degree, expected educational attainment, expected professional degree, parents’ employment status, having siblings (who study/studied), and political interest. Missing values of covariates are imputed; regressions with covariates include imputation dummies. Data source: ifo Education Survey 2018 (adolescent survey). Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table 12: Treatment effects on preferences for tuition: Pooled estimation

| | Support for tuition | | Opposition against tuition | |
|-----------------------------------|---------------------|---------------------|----------------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| <i>Information treatments</i> | | | | |
| Earnings information | 0.081*** (0.024) | 0.083*** (0.023) | -0.089*** (0.024) | -0.090*** (0.023) |
| Earnings information × Wave 2015 | -0.024 (0.036) | -0.032 (0.035) | 0.010 (0.035) | 0.016 (0.034) |
| Earnings information × Wave 2017 | -0.010 (0.030) | -0.015 (0.030) | 0.008 (0.030) | 0.012 (0.029) |
| Cost information | 0.027 (0.024) | 0.029 (0.023) | -0.003 (0.024) | -0.006 (0.023) |
| Access information | -0.029 (0.023) | -0.016 (0.023) | 0.038 (0.024) | 0.027 (0.024) |
| <i>Design of tuition scheme</i> | | | | |
| Income contingency | 0.153*** (0.026) | 0.147*** (0.026) | -0.158*** (0.026) | -0.151*** (0.025) |
| Income contingency × Wave 2016 | -0.000 (0.040) | 0.005 (0.039) | -0.007 (0.039) | -0.013 (0.038) |
| Income contingency × Wave 2018 | 0.001 (0.042) | 0.012 (0.041) | -0.033 (0.040) | -0.044 (0.040) |
| Income contingency × Loan framing | -0.003 (0.031) | -0.004 (0.031) | -0.006 (0.028) | -0.006 (0.028) |
| Level 500 | -0.033 (0.029) | -0.032 (0.029) | 0.031 (0.030) | 0.029 (0.030) |

(continued on next page)

Table 12 (continued)

| | Support for tuition | | Opposition against tuition | |
|---------------------|----------------------|----------------------|----------------------------|----------------------|
| | (1) | (2) | (3) | (4) |
| Level 1500 | -0.117*** (0.028) | -0.109*** (0.028) | 0.100*** (0.029) | 0.094*** (0.029) |
| Non-EU students | 0.246*** (0.031) | 0.246*** (0.031) | -0.278*** (0.029) | -0.275*** (0.029) |
| <i>Survey waves</i> | | | | |
| Wave 2014 | -0.024 (0.027) | -0.030 (0.026) | -0.004 (0.027) | 0.003 (0.027) |
| Wave 2015 | 0.014 (0.028) | 0.010 (0.028) | 0.003 (0.028) | 0.004 (0.028) |
| Wave 2017 | 0.009 (0.024) | 0.008 (0.024) | -0.002 (0.025) | -0.003 (0.025) |
| Wave 2018 | 0.047 (0.031) | 0.039 (0.031) | -0.007 (0.031) | -0.007 (0.031) |
| Covariates | No | Yes | No | Yes |
| Control mean | 0.400 | 0.400 | 0.457 | 0.457 |
| Observations | 19,577 | 19,577 | 19,577 | 19,577 |
| Clusters | 15,013 | 15,013 | 15,013 | 15,013 |
| R ² | 0.038 | 0.070 | 0.038 | 0.077 |

Notes: OLS regressions. *Earnings information*, *Cost information*, *Access information*, *Income contingency*, *Level 500*, *Level 1500*, and *Non-EU students*: experimental treatments in the survey experiments. Sample: Waves 2014-2018. Dependent variable: Col. (1)-(2): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; col. (3)-(4): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise. Residual category: “neither favor nor oppose.” Control mean: mean of the outcome variable in the control group in wave 2014. Covariates include age, income, and dummies for gender, born in Germany, living in a tuition state, city size, living with partner in household, parents’ university degree, working in the education sector, school education, university degree, university student status, employment status, parent status, and political partisanship. Missing values of covariates are imputed; regressions with covariates include imputation dummies. Data source: ifo Education Survey 2014-2018. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Figure A1: Overview of the analyses across the five surveys

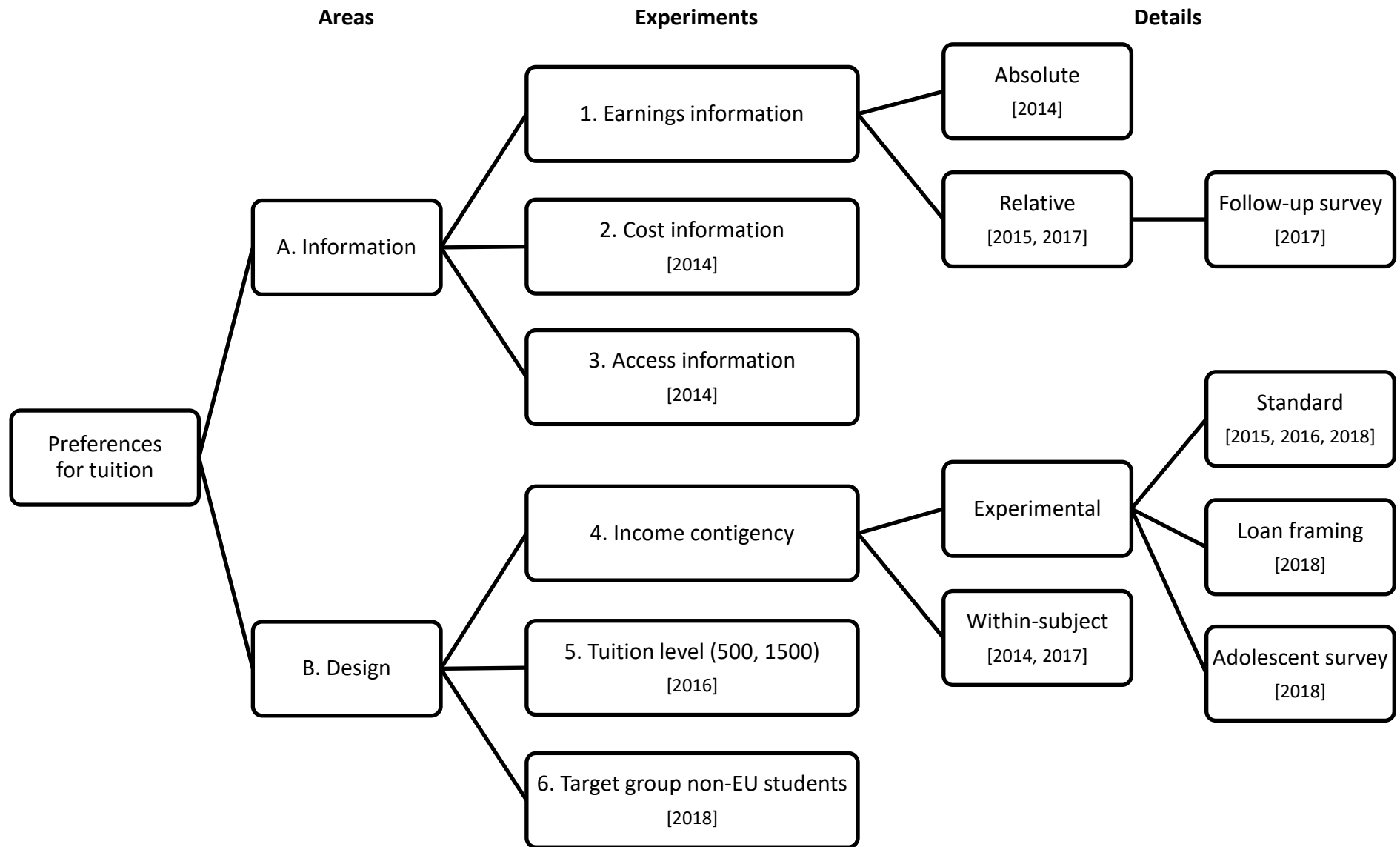


Table A1: Wording of survey questions

| No. | Exp. group | English translation | German original |
|------------------|----------------------|--|--|
| Wave 2014 | | | |
| 31 | Control group | Do you favor or oppose that students at German universities cover a part of the costs of their studies themselves by tuition? | Sind Sie dafür oder dagegen, dass Studierende, die an einer Universität oder Hochschule ^a in Deutschland studieren, einen Teil der Studienkosten durch Studiengebühren tragen? |
| | Earnings information | University graduates earn about 40 percent more each year than persons with a vocational education (apprenticeship). Do you favor or oppose... (see Control) | Personen mit einem abgeschlossenen Studium verdienen Jahr für Jahr durchschnittlich rund 40 Prozent mehr als Personen mit einer beruflichen Ausbildung (Lehre). Sind Sie... (see Control) |
| | Cost information | The federal government and the federal states currently spend about 8,600 Euro per student at a university each year. Do you favor or oppose... (see Control) | Bund und Länder geben derzeit im Durchschnitt jährlich rund 8.600 Euro für einen Studienplatz an einer Universität oder Hochschule aus. Sind Sie... (see Control) |
| | Access information | About 75 percent of the children of parents with a university degree take up university studies, and about 25 percent of the children of parents without a university degree do so. This difference already existed before tuition was introduced in Germany. Do you favor or oppose... (see Control) | Von Kindern aus Akademikerfamilien nehmen etwa 75 Prozent ein Studium auf, von Kindern aus Nicht-Akademikerfamilien sind es etwa 25 Prozent. Diesen Unterschied gab es auch schon vor der Einführung von Studiengebühren in Deutschland. Sind Sie... (see Control) |
| Wave 2015 | | | |
| 27 | Control group | See Control group in <i>Wave 2014</i> | See Control group in <i>Wave 2014</i> |
| | Earnings information | On average, university graduates earn about 2,450 Euro net per month, persons with a vocational education (apprenticeship) earn about 1,850 Euro and persons without any degree earn about 1,400 Euro. Do you favor or oppose... (see Control) | Personen mit einem abgeschlossenen Studium verdienen durchschnittlich etwa 2.450 Euro netto im Monat, Personen mit abgeschlossener beruflicher Ausbildung (Lehre) etwa 1.850 Euro und Personen ohne abgeschlossene Ausbildung etwa 1.400 Euro. Sind Sie... (see Control) |
| | Income contingency | In other countries, there is tuition that is due only after graduation, when the former students earn income. Tuition has to be paid only if their annual income exceeds a certain threshold. Do you favor or oppose that students at German universities cover a part of the costs of their studies themselves by this alternative form of tuition? | In anderen Ländern gibt es Studiengebühren, die erst nach dem Abschluss des Studiums erhoben werden, wenn die ehemaligen Studierenden Einkommen erzielen. Die Gebühren müssen nur dann zurückgezahlt werden, wenn ihr Jahreseinkommen über einem gewissen Schwellenwert liegt. Sind Sie dafür oder dagegen, dass Studierende, die an einer Universität oder Hochschule in Deutschland studieren, einen Teil der Studienkosten durch eine solche Form von Studiengebühren tragen? |
| Wave 2016 | | | |
| 21 | Control group | See Control group in <i>Wave 2014</i> | See Control group in <i>Wave 2014</i> |
| | Income contingency | See Income contingency in <i>Wave 2015</i> | See Income contingency in <i>Wave 2015</i> |

(continued on next page)

Table A1 (continued)

| No. | Exp. group | English translation | German original |
|------------------|----------------------|--|--|
| | Level 500 | Do you favor or oppose that students at German universities cover a part of the costs of their studies themselves by tuition of 500 Euro per semester (half year)? | Sind Sie dafür oder dagegen, dass Studierende, die an einer Universität oder Hochschule in Deutschland studieren, einen Teil der Studienkosten durch Studiengebühren in Höhe von 500 Euro pro Semester (Halbjahr) tragen? |
| | Level 1500 | Do you favor or oppose that students at German universities cover a part of the costs of their studies themselves by tuition of 1,500 Euro per semester (half year)? | Sind Sie dafür oder dagegen, dass Studierende, die an einer Universität oder Hochschule in Deutschland studieren, einen Teil der Studienkosten durch Studiengebühren in Höhe von 1500 Euro pro Semester (Halbjahr) tragen? |
| Wave 2017 | | | |
| 27 | Control group | See Control group in <i>Wave 2014</i> | See Control group in <i>Wave 2014</i> |
| | Earnings information | On average, university graduates earn about 2,750 Euro net per month, persons with a vocational education (apprenticeship) earn about 1,850 Euro and persons without any degree earn about 1,400 Euro. Do you favor or oppose... (see Control) | Personen mit einem abgeschlossenen Studium verdienen durchschnittlich etwa 2.750 Euro netto im Monat, Personen mit abgeschlossener beruflicher Ausbildung (Lehre) etwa 1.850 Euro und Personen ohne abgeschlossene Ausbildung etwa 1.400 Euro. Sind Sie... (see Control) |
| Wave 2018 | | | |
| 11 | Control group | See Control group in <i>Wave 2014</i> | See Control group in <i>Wave 2014</i> |
| | Income contingency | See Income contingency in <i>Wave 2015</i> | See Income contingency in <i>Wave 2015</i> |
| | Student loans | In other countries, there is tuition that is being paid in the form of interest-free student loans. Loan repayments are due only after graduation, when the former students earn income. The student loans only have to be paid if their annual income exceeds a certain threshold. Do you favor or oppose that students at German universities cover a part of the costs of their studies themselves by this alternative form of tuition? | In anderen Ländern gibt es Studiengebühren, die in Form von zinsfreien Studienkrediten bezahlt werden. Die Kreditrückzahlungen sind erst nach dem Abschluss des Studiums fällig, wenn die ehemaligen Studierenden Einkommen erzielen. Die Studienkredite müssen nur dann zurückgezahlt werden, wenn ihr Jahreseinkommen über einem gewissen Schwellenwert liegt. Sind Sie dafür oder dagegen, dass Studierende, die an einer Universität oder Hochschule in Deutschland studieren, einen Teil der Studienkosten durch diese alternative Form von Studiengebühren tragen? |
| | Non-EU students | Do you favor or oppose that non-EU students at German universities cover a part of the costs of their studies themselves by tuition? | Sind Sie dafür oder dagegen, dass ausländische Studierende, die nicht aus der EU kommen und an einer Universität oder Hochschule in Deutschland studieren, einen Teil der Studienkosten durch Studiengebühren tragen? |

Notes: No.: consecutive ordering of the question in the ifo Education Survey of the respective year. Exp. group: experimental group to which the respective question is posed. Answer categories in each case: strongly favor; somewhat favor; somewhat oppose; strongly oppose; neither favor nor oppose (Ich bin sehr dafür; eher dafür; eher dagegen; sehr dagegen; weder dafür noch dagegen). ^a Literal translation: "universities or universities of applied sciences."

Table A2: Summary statistics and balancing tests: Waves 2014 and 2015

| | Control groups | | Wave 2014 | | | Wave 2015 | |
|----------------------------------|------------------|------------------|-----------------------------|-------------------------|---------------------------|-----------------------------|---------------------------|
| | Mean | | Difference | | | Difference | |
| | Wave 2014 (1) | Wave 2015 (2) | Earnings information (3) | Cost information (4) | Access information (5) | Earnings information (6) | Income contingency (7) |
| Age | 51.00 | 49.40 | -1.42 | -0.55 | -1.30 | 1.27 | 1.68 |
| Female | 0.50 | 0.53 | 0.01 | 0.01 | 0.03 | -0.01 | -0.02 |
| Born in Germany | 0.94 | 0.95 | -0.00 | 0.01 | 0.01 | 0.00 | -0.01 |
| Tuition state | 0.69 | 0.73 | -0.02 | 0.02 | 0.05** | -0.03 | -0.06 |
| City size \geq 100,000 | 0.33 | 0.32 | 0.01 | -0.04* | -0.02 | -0.00 | 0.00 |
| Monthly household income (€) | 2,133 | 2,215 | 8.9 | -37.3 | -33.3 | 74.2 | 50.3 |
| Partner in household | 0.58 | 0.59 | 0.02 | -0.01 | -0.04 | 0.01 | 0.03 |
| Parent(s) with university degree | 0.24 | 0.27 | 0.01 | 0.02 | 0.02 | 0.00 | 0.00 |
| Works in education sector | 0.06 | 0.11 | 0.01 | 0.01 | 0.01 | -0.02 | 0.00 |
| Highest education attainment | | | | | | | |
| No degree/basic degree | 0.40 | 0.39 | 0.02 | -0.00 | -0.00 | -0.01 | 0.03 |
| Middle school degree | 0.32 | 0.31 | -0.03 | -0.01 | -0.01 | -0.00 | -0.01 |
| University entrance degree | 0.28 | 0.30 | 0.00 | 0.01 | 0.01 | 0.01 | -0.02 |
| Professional degree | | | | | | | |
| No degree | 0.10 | 0.13 | 0.01 | -0.01 | 0.02 | -0.04* | -0.03* |
| Vocational degree | 0.72 | 0.70 | 0.00 | -0.01 | -0.02 | 0.03 | 0.02 |
| University degree | 0.15 | 0.16 | -0.01 | 0.01 | 0.01 | -0.00 | 0.03 |
| In training | 0.07 | 0.06 | 0.01 | 0.02 | 0.01 | -0.01 | -0.02 |
| University student | 0.05 | 0.05 | 0.00 | -0.00 | 0.01 | -0.01 | -0.03** |
| Employment status | | | | | | | |
| Full-time employed | 0.37 | 0.33 | -0.04 | -0.01 | -0.03 | 0.04 | 0.05** |
| Part-time employed | 0.09 | 0.13 | 0.03** | 0.01 | 0.03** | -0.01 | 0.00 |

(continued on next page)

Table A2 (continued)

| | Control groups | | Wave 2014 | | | Wave 2015 | |
|----------------------------|----------------|-----------|----------------------|------------------|--------------------|----------------------|--------------------|
| | Mean | | Difference | | | Difference | |
| | Wave 2014 | Wave 2015 | Earnings information | Cost information | Access information | Earnings information | Income contingency |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Self-employed | 0.02 | 0.04 | 0.02** | 0.01 | 0.02* | -0.00 | -0.01 |
| Unemployed | 0.07 | 0.07 | -0.01 | -0.02 | -0.01 | -0.03** | -0.03** |
| Parent status | | | | | | | |
| No children | 0.37 | 0.32 | 0.02 | 0.01 | 0.01 | 0.00 | -0.00 |
| At least one child < 18 | 0.23 | 0.31 | 0.00 | -0.02 | -0.01 | -0.02 | -0.02 |
| All children > 18 | 0.40 | 0.37 | -0.02 | 0.01 | 0.00 | 0.01 | 0.02 |
| Political party preference | | | | | | | |
| CDU | 0.26 | 0.26 | 0.00 | 0.01 | -0.04** | 0.00 | -0.05** |
| SPD | 0.22 | 0.22 | -0.02 | -0.01 | -0.01 | -0.01 | 0.02 |
| Linke | 0.05 | 0.08 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |
| Grüne | 0.09 | 0.07 | 0.01 | 0.01 | 0.01 | 0.01 | 0.02 |
| Other | 0.06 | 0.05 | 0.02* | 0.00 | 0.01 | -0.01 | -0.00 |
| None | 0.33 | 0.33 | -0.02 | -0.02 | 0.02 | -0.00 | 0.01 |
| Patience (11-point scale) | 6.86 | | -0.29** | -0.14 | -0.02 | | |
| Altruism (11-point scale) | 7.15 | | 0.11 | 0.22* | 0.14 | | |
| Frequent voter | | 0.77 | | | | 0.04* | 0.02 |
| Education important vote | | 0.72 | | | | -0.01 | 0.02 |
| Non-response | 0.01 | 0.01 | 0.00 | 0.00 | 0.00 | 0.01 | 0.01 |
| Observations | 1,032 | 1,390 | 1,030 | 1,056 | 1,053 | 1,355 | 1,360 |

Notes: Weighted group means. “Difference” displays the difference in means between the control group of the respective year and the treatment groups. Significance levels of “Difference” stem from linear regressions of the respective background variable on treatment dummies. Data source: ifo Education Survey 2014, 2015. Regressions weighted by survey weights. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table A3: Summary statistics and balancing tests: Waves 2016, 2017, and 2018

| | Control groups | | | Wave 2016 | | | Wave 2017 | Wave 2018 | | |
|----------------------------------|---------------------|---------------------|---------------------|------------------------------|---------------------|----------------------|--------------------------------|------------------------------|-------------------------|----------------------------|
| | Mean | | | Difference | | | Difference | Difference | | |
| | Wave 2016 (1) | Wave 2017 (2) | Wave 2018 (3) | Income contingency (4) | Level 500 (5) | Level 1500 (6) | Earnings information (7) | Income contingency (8) | Student loans (9) | Non-EU students (10) |
| Age | 50.11 | 49.57 | 51.51 | 2.44* | -1.11 | 0.19 | 1.87** | -1.39 | -2.18 | -0.37 |
| Female | 0.51 | 0.51 | 0.51 | 0.02 | -0.01 | -0.01 | 0.01 | -0.00 | -0.02 | 0.01 |
| Born in Germany | 0.96 | 0.95 | 0.95 | -0.00 | -0.01 | -0.01 | -0.01 | 0.02* | 0.00 | 0.02 |
| Tuition state | 0.72 | 0.70 | 0.72 | -0.01 | -0.01 | -0.04 | 0.02 | -0.03 | 0.00 | -0.02 |
| City size \geq 100,000 | 0.31 | 0.32 | 0.32 | -0.01 | 0.02 | 0.05* | -0.01 | -0.01 | -0.00 | 0.00 |
| Monthly household income (€) | 2,131 | 2,252 | 2,554 | 41.9 | 71.5 | 2.3 | 30.8 | -189.8** | -140.2 | -62.9 |
| Partner in household | 0.54 | 0.55 | 0.60 | 0.01 | 0.04 | 0.04 | -0.01 | -0.03 | -0.03 | -0.00 |
| Parent(s) with university degree | 0.23 | 0.28 | 0.30 | 0.01 | 0.03 | 0.03 | -0.02 | -0.03 | -0.02 | -0.01 |
| Works in education sector | 0.08 | 0.08 | 0.10 | -0.00 | 0.01 | -0.01 | 0.00 | 0.00 | 0.00 | -0.02 |
| Highest educational attainment | | | | | | | | | | |
| No degree/basic degree | 0.38 | 0.37 | 0.37 | 0.02 | 0.01 | 0.01 | 0.03 | -0.00 | -0.00 | 0.02 |
| Middle school degree | 0.32 | 0.31 | 0.31 | -0.02 | -0.00 | -0.04 | -0.02 | 0.00 | -0.03 | -0.01 |
| University entrance degree | 0.30 | 0.32 | 0.32 | -0.01 | -0.01 | 0.02 | -0.01 | 0.00 | 0.03 | -0.02 |
| Professional degree | | | | | | | | | | |
| No degree | 0.14 | 0.09 | 0.06 | -0.01 | -0.04* | -0.01 | 0.02 | 0.02 | 0.02 | 0.02 |
| Vocational degree | 0.70 | 0.71 | 0.75 | -0.01 | 0.01 | 0.00 | -0.01 | -0.03 | -0.01 | -0.01 |
| University degree | 0.12 | 0.15 | 0.17 | 0.01 | 0.00 | -0.00 | -0.01 | -0.01 | 0.01 | -0.02 |
| In training | 0.06 | 0.09 | 0.06 | 0.01 | 0.03 | 0.02 | -0.01 | 0.02 | -0.02 | 0.00 |
| University student | 0.05 | 0.06 | 0.03 | 0.00 | 0.01 | 0.01 | -0.00 | 0.02* | 0.00 | 0.01 |
| Employment status | | | | | | | | | | |
| Full-time employed | 0.34 | 0.37 | 0.38 | -0.02 | 0.03 | 0.01 | -0.04** | 0.01 | 0.01 | -0.01 |
| Part-time employed | 0.15 | 0.12 | 0.13 | -0.00 | -0.02 | -0.05*** | -0.00 | 0.03 | 0.03 | 0.05** |

(continued on next page)

Table A3 (continued)

| | Control groups | | | Wave 2016 | | | Wave 2017 | Wave 2018 | | |
|----------------------------|---------------------|---------------------|---------------------|------------------------------|---------------------|----------------------|--------------------------------|------------------------------|-------------------------|----------------------------|
| | Mean | | | Difference | | | Difference | Difference | | |
| | Wave 2016 (1) | Wave 2017 (2) | Wave 2018 (3) | Income contingency (4) | Level 500 (5) | Level 1500 (6) | Earnings information (7) | Income contingency (8) | Student loans (9) | Non-EU students (10) |
| Self-employed | 0.03 | 0.04 | 0.05 | -0.00 | 0.01 | 0.02* | -0.00 | -0.01 | 0.01 | 0.01 |
| Unemployed | 0.06 | 0.05 | 0.03 | 0.00 | -0.00 | -0.03** | -0.00 | 0.00 | 0.01 | -0.00 |
| Parent status | | | | | | | | | | |
| No children | 0.43 | 0.43 | 0.40 | -0.02 | -0.04 | -0.03 | -0.01 | -0.00 | 0.00 | 0.03 |
| At least one child < 18 | 0.20 | 0.208 | 0.23 | 0.00 | 0.01 | 0.01 | -0.00 | -0.03 | -0.00 | -0.06** |
| All children > 18 | 0.37 | 0.37 | 0.37 | 0.02 | 0.03 | 0.01 | 0.02 | 0.04 | 0.00 | 0.02 |
| Political party preference | | | | | | | | | | |
| CDU | 0.21 | 0.25 | 0.21 | 0.00 | -0.01 | -0.01 | 0.01 | -0.01 | 0.02 | 0.03 |
| SPD | 0.17 | 0.20 | 0.20 | 0.02 | 0.01 | 0.02 | 0.01 | -0.02 | -0.02 | -0.06** |
| Linke | 0.08 | 0.08 | 0.11 | -0.01 | -0.01 | -0.02 | -0.00 | -0.03 | 0.00 | -0.01 |
| Grüne | 0.08 | 0.06 | 0.07 | -0.01 | 0.01 | -0.01 | -0.01 | -0.01 | 0.01 | 0.02 |
| Other | 0.15 | 0.14 | 0.18 | -0.01 | -0.00 | 0.00 | -0.01 | 0.02 | -0.01 | 0.01 |
| None | 0.32 | 0.29 | 0.23 | 0.01 | -0.00 | 0.01 | 0.00 | 0.04 | 0.01 | 0.01 |
| Patience (11-point scale) | 5.79 | 6.04 | 6.15 | 0.18 | 0.18 | 0.26 | -0.09 | 0.17 | 0.13 | 0.20 |
| Risk tolerance (11-point) | 4.24 | 4.27 | 4.32 | 0.00 | 0.07 | 0.10 | -0.16* | -0.12 | 0.23 | -0.13 |
| Frequent voter | 0.77 | 0.82 | 0.85 | 0.03 | -0.02 | -0.00 | -0.02 | -0.03 | -0.00 | -0.00 |
| Education important vote | 0.75 | 0.74 | 0.75 | 0.03 | 0.03 | 0.05** | -0.03* | -0.04 | -0.04 | -0.08*** |
| Non-response | 0.01 | 0.01 | 0.00 | 0.00 | -0.00 | 0.00 | 0.01 | 0.00 | 0.00 | 0.00 |
| Observations | 781 | 2,075 | 1,036 | 852 | 804 | 865 | 2,003 | 1,005 | 970 | 1,035 |

Notes: Weighted group means. “Difference” displays the difference in means between the control group of the respective year and the treatment groups. Significance levels of “Difference” stem from linear regressions of the respective background variable on treatment dummies. Data source: ifo Education Survey 2016, 2017, 2018. Regressions weighted by survey weights. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table A4: Detailed results by five answer categories

| | Strongly favor (1) | Somewhat favor (2) | Neither favor nor oppose (3) | Somewhat oppose (4) | Strongly oppose (5) |
|---|-----------------------|-----------------------|---------------------------------|------------------------|------------------------|
| <i>Panel A: Specification of Table 2</i> | | | | | |
| Earnings information | 0.027* (0.016) | 0.056** (0.022) | 0.008 (0.017) | -0.036* (0.021) | -0.054*** (0.017) |
| Cost information | 0.000 (0.014) | 0.033 (0.022) | -0.023 (0.016) | -0.007 (0.021) | -0.004 (0.019) |
| Access information | -0.011 (0.014) | -0.001 (0.022) | -0.009 (0.017) | 0.024 (0.022) | -0.003 (0.019) |
| Control mean | 0.101 | 0.299 | 0.143 | 0.267 | 0.190 |
| Observations | 4,123 | 4,123 | 4,123 | 4,123 | 4,123 |
| R^2 | 0.025 | 0.034 | 0.029 | 0.017 | 0.072 |
| <i>Panel B: Specification of Table 5</i> | | | | | |
| Income contingency | 0.070*** (0.019) | 0.081*** (0.025) | 0.003 (0.015) | -0.095*** (0.022) | -0.059*** (0.019) |
| Income contingency × Wave 2016 | 0.017 (0.027) | -0.013 (0.037) | 0.009 (0.025) | -0.009 (0.033) | -0.005 (0.029) |
| Income contingency × Wave 2018 | -0.022 (0.032) | 0.029 (0.040) | 0.032 (0.023) | 0.006 (0.036) | -0.046* (0.028) |
| Income contingency × Loan framing | -0.010 (0.027) | 0.005 (0.032) | 0.011 (0.020) | -0.031 (0.024) | 0.024 (0.017) |
| Wave 2016 | -0.004 (0.018) | -0.007 (0.026) | 0.009 (0.018) | 0.002 (0.025) | 0.000 (0.022) |
| Wave 2018 | 0.048** (0.022) | -0.024 (0.027) | -0.026* (0.015) | -0.001 (0.027) | 0.003 (0.022) |
| Control mean | 0.096 | 0.240 | 0.257 | 0.217 | 0.190 |
| Observations | 7,360 | 7,360 | 7,360 | 7,360 | 7,360 |
| R^2 | 0.023 | 0.024 | 0.031 | 0.024 | 0.046 |

(continued on next page)

Table A4 (continued)

| | Strongly favor (1) | Somewhat favor (2) | Neither favor nor oppose (3) | Somewhat oppose (4) | Strongly oppose (5) |
|--|-----------------------|-----------------------|---------------------------------|------------------------|------------------------|
| <i>Panel C: Specification of Table 7</i> | | | | | |
| Level 500 | -0.000 (0.017) | -0.029 (0.027) | -0.000 (0.019) | -0.016 (0.026) | 0.045* (0.024) |
| Level 1500 | -0.029* (0.016) | -0.083*** (0.025) | 0.021 (0.019) | 0.009 (0.027) | 0.082*** (0.024) |
| Control mean | 0.030 | 0.211 | 0.411 | 0.225 | 0.123 |
| Observations | 2,426 | 2,426 | 2,426 | 2,426 | 2,426 |
| R ² | 0.038 | 0.040 | 0.070 | 0.025 | 0.080 |
| <i>Panel D: Specification of Table 8</i> | | | | | |
| Non-EU students | 0.189*** (0.027) | 0.047 (0.030) | 0.029* (0.017) | -0.136*** (0.025) | -0.128*** (0.019) |
| Control mean | -0.060 | 0.433 | 0.341 | 0.130 | 0.156 |
| Observations | 2,071 | 2,071 | 2,071 | 2,071 | 2,071 |
| R ² | 0.109 | 0.034 | 0.059 | 0.081 | 0.099 |
| <i>Panel E: Specification of Table 11</i> | | | | | |
| Income contingency | 0.071*** (0.018) | 0.066** (0.027) | 0.029 (0.021) | -0.036 (0.029) | -0.130*** (0.028) |
| Control mean | 0.056 | 0.211 | 0.104 | 0.308 | 0.321 |
| Observations | 1,085 | 1,085 | 1,085 | 1,085 | 1,085 |
| R ² | 0.048 | 0.036 | 0.024 | 0.032 | 0.062 |

Notes: OLS regressions. Dependent variable: Dummy variables 1 = answer category given in respective table header, 0 otherwise. All specification include covariates. Control mean: mean of the outcome variable in the control group. See the respective tables for additional notes. Data source: ifo Education Survey 2014-2018. Regressions weighted by survey weights. Robust standard errors in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table A5: Summary statistics and balancing tests: Follow-up survey

| | Summary statistics by participation status | | Balancing in follow-up survey | |
|----------------------------------|--|-----------------------------------|-------------------------------|--------------------------------------|
| | Non-participants Mean (1) | Participants Difference (2) | Control group Mean (3) | Treatment group Difference (4) |
| Age | 40.13 | 6.70*** | 51.09 | 0.83 |
| Female | 0.58 | -0.01*** | 0.52 | -0.01 |
| Born in Germany | 0.94 | 0.01 | 0.96 | 0.01 |
| Tuition state | 0.66 | 0.01 | 0.72 | 0.00 |
| City size \geq 100,000 | 0.40 | -0.04** | 0.30 | 0.01 |
| Monthly household income (€) | 2,312 | 97.60* | 2,302 | 35.89 |
| Partner in household | 0.57 | 0.02 | 0.57 | 0.01 |
| Parent(s) with university degree | 0.34 | -0.03** | 0.26 | -0.03 |
| Works in education sector | 0.10 | -0.01 | 0.07 | 0.00 |
| Highest education attainment | | | | |
| No degree/basic degree | 0.19 | 0.03** | 0.39 | 0.00 |
| Middle school degree | 0.36 | 0.03** | 0.31 | 0.01 |
| University entrance degree | 0.45 | -0.07*** | 0.3 | -0.02 |
| Professional degree | | | | |
| No degree | 0.07 | -0.01 | 0.07 | 0.02* |
| Vocational degree | 0.63 | 0.09*** | 0.77 | -0.01 |
| University degree | 0.17 | 0.01 | 0.14 | 0.01 |
| In training | 0.18 | -0.10*** | 0.06 | -0.01 |
| University student | 0.14 | -0.07*** | 0.04 | 0.00 |
| Employment status | | | | |
| Full-time employed | 0.39 | 0.01 | 0.35 | -0.02 |
| Part-time employed | 0.13 | 0.00 | 0.12 | 0.00 |
| Self-employed | 0.04 | 0.00 | 0.04 | 0.00 |

(continued on next page)

Table A5 (continued)

| | Summary statistics by participation status | | Balancing in follow-up survey | |
|---------------------------------|--|-----------------------------------|-------------------------------|--------------------------------------|
| | Non-participants Mean (1) | Participants Difference (2) | Control group Mean (3) | Treatment group Difference (4) |
| Unemployed | 0.05 | 0.00 | 0.05 | -0.01 |
| Parent status | | | | |
| No children | 0.55 | -0.10*** | 0.40 | -0.02 |
| At least one child < 18 | 0.23 | -0.01 | 0.19 | 0.01 |
| All children > 18 | 0.21 | 0.11*** | 0.41 | 0.00 |
| Political party preference | | | | |
| CDU | 0.22 | 0.02 | 0.23 | 0.02 |
| SPD | 0.19 | 0.02 | 0.25 | -0.02 |
| Linke | 0.08 | 0.01 | 0.08 | -0.01 |
| Grüne | 0.07 | -0.01 | 0.05 | -0.01 |
| Other | 0.14 | 0.00 | 0.12 | 0.02 |
| None | 0.30 | -0.03** | 0.27 | -0.01 |
| Patience (11-point scale) | 6.24 | 0.03 | 6.29 | -0.06 |
| Risk tolerance (11-point scale) | 4.71 | -0.37*** | 4.18 | 0.08 |
| Frequent voter | 0.76 | 0.07*** | 0.85 | -0.02 |
| Education important vote | 0.78 | -0.04*** | 0.72 | -0.02 |
| Observations | 1,333 | 2,363 | 1,183 | 1,180 |

Notes: Unweighted group means. Col. (1) and (2) compare online respondents in wave 2017 by participation in the follow-up survey. Col. (3) and (4) compare respondents who participated in the follow-up survey by treatment status in wave 2017. "Difference" displays the difference in means between the respective groups. Significance levels of "Difference" stem from linear regressions of the respective background variable on group dummies. Data source: ifo Education Survey 2017. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table A6: Effects of earnings information on preferences for income-contingent tuition

| | Support for income-contingent tuition | | Opposition against income-contingent tuition | |
|----------------------------------|---------------------------------------|-------------------|--|-------------------|
| | (1) | (2) | (3) | (4) |
| Earnings information | 0.012 (0.023) | 0.011 (0.023) | 0.004 (0.020) | 0.004 (0.020) |
| Wave 2017 | 0.030 (0.030) | 0.025 (0.030) | -0.020 (0.026) | -0.016 (0.026) |
| Earnings information × Wave 2017 | -0.037 (0.044) | -0.034 (0.043) | 0.066* (0.039) | 0.059 (0.038) |
| Covariates | No | Yes | No | Yes |
| Control mean | 0.635 | 0.635 | 0.221 | 0.221 |
| Observations | 3,037 | 3,037 | 3,037 | 3,037 |
| R^2 | 0.000 | 0.036 | 0.002 | 0.039 |

Notes: OLS regressions. *Earnings information*: experimental treatment in the survey experiment. Sample: Waves 2014 and 2017. Dependent variable: Col. (1)-(2): Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise; col. (3)-(4): Dummy variables 1 = “strongly oppose” or “somewhat oppose” tuition, 0 otherwise. Residual category: “neither favor nor oppose.” Control mean: mean of the outcome variable in the control group in wave 2014. Covariates include age, income, and dummies for gender, born in Germany, living in a tuition state, city size, living with partner in household, parents’ university degree, working in the education sector, school education, university degree, university student status, employment status, parent status, and political partisanship. Missing values of covariates are imputed; regressions with covariates include imputation dummies. Data source: ifo Education Survey 2014, 2017. Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table A7: Summary statistics and balancing tests: Adolescent survey

| | Control group: Mean (1) | Income contingency: Difference (2) |
|--------------------------------------|----------------------------|---------------------------------------|
| Age | 15.98 | 0.04 |
| Female | 0.47 | 0.01 |
| Born in Germany | 0.98 | -0.01 |
| Tuition state | 0.73 | 0.02 |
| City size \geq 100,000 | 0.28 | 0.01 |
| Lives with both parents | 0.74 | -0.03 |
| Parent(s) with university degree | 0.38 | -0.04 |
| Expected educational attainment | | |
| No degree/basic degree | 0.08 | -0.01 |
| Middle school degree | 0.35 | -0.02 |
| University entrance degree | 0.57 | 0.04 |
| Expected professional degree | | |
| No degree | 0.01 | -0.01 |
| Vocational degree | 0.46 | -0.02 |
| University degree | 0.52 | 0.03 |
| Parents' employment status | | |
| Mother full-time employed | 0.38 | 0.01 |
| Mother part-time employed | 0.38 | -0.01 |
| Mother unemployed | 0.04 | 0.01 |
| Father full-time employed | 0.85 | -0.05* |
| Father part-time employed | 0.03 | 0.01 |
| Father unemployed | 0.02 | 0.01 |
| At least one sibling | 0.80 | -.043 |
| At least one sibling studies/studied | 0.18 | -.036 |
| Politically interested | 0.33 | .016 |
| Patience (11-point scale) | 6.16 | 0.25 |
| Risk tolerance (11-point scale) | 5.26 | -0.03 |
| Observations | 525 | 560 |

Notes: Sample: Adolescent survey, 2018. Weighted group means. "Difference" displays the difference in means between the control group and the treatment group. Significance levels of "Difference" stem from linear regressions of the respective background variable on treatment dummies. Data source: ifo Education Survey 2018 (adolescent survey). Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A8: Correlates of preferences for tuition and heterogeneous treatment effects in the adolescent survey

| | Support for regular tuition | | Support for income-contingent tuition | | Heterogeneous treatment effects | |
|---------------------------------------|-----------------------------|---------|---------------------------------------|---------|---------------------------------|---------|
| | (1) | | (2) | | (3) | |
| Age | 0.010 | (0.020) | 0.013 | (0.020) | 0.037 | (0.061) |
| Female | -0.005 | (0.041) | -0.006 | (0.045) | 0.002 | (0.061) |
| Born in Germany | 0.083 | (0.129) | 0.129 | (0.125) | 0.074 | (0.172) |
| Tuition state | -0.002 | (0.044) | 0.020 | (0.048) | 0.046 | (0.065) |
| City size \geq 100,000 | -0.013 | (0.043) | 0.032 | (0.047) | 0.013 | (0.063) |
| Lives with both parents | 0.077 | (0.047) | -0.036 | (0.053) | -0.053 | (0.067) |
| Parent(s) with university degree | 0.097** | (0.046) | -0.058 | (0.048) | -0.101 | (0.062) |
| Expects middle school degree | 0.047 | (0.086) | -0.029 | (0.101) | -0.077 | (0.131) |
| Expects university entrance degree | -0.021 | (0.089) | -0.030 | (0.104) | 0.055 | (0.124) |
| Expects university degree | -0.116** | (0.055) | 0.044 | (0.054) | 0.159*** | (0.061) |
| Full-time employed mother | 0.125** | (0.053) | -0.054 | (0.058) | -0.122** | (0.062) |
| Part-time employed mother | 0.122** | (0.050) | 0.057 | (0.059) | 0.046 | (0.043) |
| Full-time employed father | -0.006 | (0.062) | 0.069 | (0.067) | 0.041 | (0.078) |
| Part-time employed father | -0.063 | (0.112) | -0.081 | (0.115) | -0.126* | (0.073) |
| At least one sibling | 0.003 | (0.052) | -0.059 | (0.053) | -0.039 | (0.072) |
| At least on sibling studies (studied) | -0.077 | (0.054) | 0.006 | (0.067) | 0.072 | (0.082) |
| Politically interested | -0.008 | (0.046) | 0.064 | (0.049) | 0.106* | (0.063) |
| Constant | -0.071 | (0.355) | 0.065 | (0.367) | | |
| Observations | 525 | | 560 | | | |
| R^2 | 0.055 | | 0.036 | | | |

Notes: Sample: Adolescent survey, 2018. OLS regressions. Dependent variable: Dummy variables 1 = “strongly favor” or “somewhat favor” tuition, 0 otherwise. Samples: Col. (1): control group; col. (2): respondents in treatment *Income Contingency*; col. (3): full adolescent sample. Col. (3) displays coefficients on the interaction term between treatment *Income Contingency* and subgroup indicators from estimates based on equation (2). Missing values of covariates are imputed; regressions include imputation dummies. Data source: ifo Education Survey 2018 (adolescent survey). Regressions weighted by survey weights. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.