ESSAYS ON THE ECONOMICS OF SOCIAL CHANGE

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For Mareike

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CONTENTS

INTRODUCTION

Societies are subject to complex, swift, and often unanticipated changes (Nowak and Vallacher 2019). Events like the collapse of the Soviet Union suggest that norms, beliefs, and at times, even entire political systems that seemed irrevocable for generations can change fundamentally in short periods of time (Kuran 1991; Nowak and Vallacher 2019). Yet, even in the absence of such macro-shocks, we have observed several instances of rapid social change in recent history: the share of the world population living in democracies has tripled since World War II (Boese 2021); women's labor force participation in Western countries has roughly doubled in the twentieth century (Ortez-Ospina et al. 2018); and attitudes towards immigration and minorities have become considerably more liberal since the 1990s (Ademmer and Stöhr 2018; J. M. Jones 2021; Livingston and Brown 2017; McCarthy 2021; Wissenschaftliche Dienste des Deutschen Bundestages 2016).

The dynamics of social change have been major objects of study in various disciplines ranging from evolutionary biology to the social sciences.[1] Applying the economist's toolkit to the question of what determines social change, Acemoglu and Robinson (2001, 2006, 2012) show that changes in control over economic resources can spur shifts in political power that ultimately can give rise to fundamental social changes such as the extension of political rights. Since then, the economics and political science literature has documented several determinants of social change including economic crises (Caprettini and Voth 2021; Doerr et al. 2021), migration (Fouka et al. 2021), and (media) technology (García-Jimeno et al. 2022; Melander 2020; Shirky 2011; Zhuravskaya et al. 2020).

A related strand of the literature combines insights from social psychology and economics to analyze the dynamics of social change: the concept of "pluralistic ignorance" introduced by D. Katz and Allport (1931) posits that individuals incorrectly believe that they are alone in their views and act accordingly, when in reality many of their peers share their views.[2] Extending this concept to the question of political transitions, Kuran (1991) argues that many individuals opposed the communist regime

[1] A prominent contribution from evolutionary biology studying social change is Diamond's (1997) account of the "Rise of the West". Anthropologists and archaeologists Weiss and Bradley (2001), for example, study the determinants of societal collapse. Kuran (1991) studies the political transitions in Eastern Europe culminating in the collapse of the Soviet Union from the perspective of economics.

[2] See Bursztyn, González, et al. (2020) and Tufekci (2018) for more detailed accounts of the concept of "pluralistic ignorance".

in the Soviet Union, but did not voice their opposition since they believed that others supported it. Bursztyn, Egorov, and Fiorin (2020) and Bursztyn, González, et al. (2020) apply this concept to attitudes towards immigration and women's participation in the labor market. They show that public opinion can change rapidly if people learn that their own preferences are shared by a greater fraction of their peers than they anticipated. This implies that correcting individuals' misperceptions about what others think, e.g., via social media can add decisive momentum to social change.[3]

Building on these insights, this thesis aims to expand our understanding of the economics of social change by studying a *catalyst*, a *mechanism*, and a *response* to social change. To this end, this thesis discusses novel empirical evidence drawn from a natural experiment in German history and two recent field experiments: in Chapter 1, which is joint work with Mathias Bühler and Leonhard Vollmer, we study the role of education in promoting social change in the context of the German women's rights movement. The movement entered the public stage in 1848 and subsequently fundamentally changed women's role in society, manifesting in obtaining suffrage in 1919 and achieving full legal equality in the civil code by 1958 (Bundeszentrale für politische Bildung 2018). Complementary to recent work studying the success factors of *existing* social movements (e.g. Dippel and Heblich 2021; Enikolopov et al. 2020; García-Jimeno et al. 2022), we take a long-run perspective to trace the effect of education on the *emergence* of the women's rights movement through history.

To this end, we study the arrival of finishing schools, the first institutions offering secondary education and teacher training to a considerable number of women in Germany. Leveraging newly collected historical microdata, we demonstrate the importance of finishing schools at three points in the history of the women's rights movement: first, following the arrival of finishing schools, women started to represent a larger share of the political, intellectual, and economic elite, from which the activist nucleus of the movement emerged. Second, later in the nineteenth century, cities with finishing schools were more likely to reveal support for the women's cause, which we show using letters sent to one of the first feminist newspapers circulated in Germany, *Frauen-Zeitung*. Third, cities with a history of finishing schools hosted more and larger women's rights organizations, key forces in the advancement of women's empowerment, in the early twentieth century. Our results are robust to a variety of empirical specifications and suggest that a world without finishing schools would not have seen a comparable level or pace of social change toward greater gender equality. Our findings thus indicate that educational institutions, which foster the exchange of critical ideas and provide the space to form networks, can function as important *catalysts* for social change.

[3] Tufekci (2018), for instance, speculates that social media contributed to the protests in the Arab world by clearing pluralistic ignorance about others' opposition to the old regimes.

In Chapter 2, which is joint work with Emilio Esguerra and Leonhard Vollmer, we study one potential *mechanism* underlying social change: anticipated peer effects. Recent field experiments established that people are more likely to act prosocially if their behavior is observable to others due to social image effects – that is, people care about how their actions are perceived by others and act accordingly (e.g. Bursztyn and Jensen 2017; DellaVigna, List, and Malmendier 2012; Perez-Truglia and Cruces 2017). The concept of anticipated peer effects, on the other hand, posits that people may also be more inclined to act prosocially when they are observed by others because they anticipate that their own prosocial behavior may motivate others to follow their good example. Anticipated peer effects might be particularly relevant in contexts where our actions generate positive externalities if those around us follow suit: examples range from everyday settings, e.g., ordering vegetarian food during a group meal to signal the importance of eco-friendly behavior, to high stakes decisions, such as whether to participate in a protest demanding civil liberties in an autocracy.

We study the role of anticipated peer effects using a survey-based online field experiment in the context of the COVID-19 vaccination campaign in Germany in 2021. The main outcome in our experiment is individuals' willingness to register for a COVID-19 vaccination, which constituted the only pathway to obtaining the vaccine at the time of the experiment. We find that individuals' willingness to sign up for a vaccination almost doubles when they learn that they can influence a peer's decision. We show that this finding is robust to various alternative explanations including social image effects and experimenter demand. Our findings further highlight that individuals are willing to incur considerable costs to send an encouraging signal to observing peers to follow their lead if they are convinced that an action can generate positive externalities. Our results thus help to rationalize why individuals are willing to bear the private costs of participating in a protest and related activities aimed at achieving social change despite the low chances of being pivotal. In these situations, the anticipation of other individuals following suit may push the benefits of these actions above their costs and as such, anticipated peer effects can function as one potential *mechanism* through which social change propagates in society.

Finally, Chapter 3 is placed in the context of the recent rise of right-wing populism in Western countries, which several scholars interpret "as a reactionary *response*" (Cantoni, Hagemeister, et al. 2020, p. 5, own emphasis) to progressive social change (Cantoni, Hagemeister, et al. 2020; Inglehart and Norris 2016; Margalit 2019). Yet, at the same time, we observe a growing number of grassroots efforts contending against right-wing populism, both in the streets and online (Mayer 2017; Mudde 2019; Tsakiridis 2021). In Germany, for instance, a broad network of grassroots organizations has emerged, which aims to reduce electoral support for the *Alternative for Germany* ("Alternative für Deutschland", AfD), Germany's most successful right-wing populist party since World War II. Given the multitude of online grassroots efforts to combat right-wing populism the question of whether grassroots organizations can reduce support for right-wing populism via social media emerges.

We study this question using a tightly controlled field experiment embedded in the Facebook campaign of one such grassroots organization, *Kleiner Fünf* (K5). Exploiting experimental variation as to where K5 disseminated its Facebook ads during the run up to a series of recent elections in Germany, we find that K5's campaign did not significantly affect the AfD's vote share and turnout: treatment effects are small in magnitude, precisely estimated, and robust to various different specifications. In combination with the high statistical power of our experiment, our estimates are thus more likely to reflect the "true" absence of any meaningful treatment effects than insufficient statistical power. Drawing on data from a complementary online experiment, we show that insufficient outreach on Facebook together with the absence of individual-level responses of attitudes and behavior explains why K5's Facebook campaign did not meaningfully shape aggregate election outcomes. Given this finding, future research may shed further light on the question of whether grassroots efforts aimed at combating right-wing populism using social media can be successful.

The three essays forming this thesis are self-contained, and as such, they can be explored independently. Each chapter is followed by appendices with additional material. A consolidated bibliography is presented at the end of the thesis.

Chapter 1

Education and the Women's Rights Movement

1.1 Introduction

What determines the emergence and success of social movements? Historically successful movements often passed three key milestones in their development (Della Porta and Mattoni 2016; Markoff 2015; Tilly et al. 2020): (i) a small number of dedicated activists develop critical ideas that challenge the status quo and begin forming networks; (ii) these activists then spread these ideas using available mass media; and (iii) institutionalize their movement. From Dr. Martin Luther King Jr. to Susan B. Anthony, from Nelson Mandela to V. I. Lenin, such leaders are often considerably more educated than their peers. While their education is arguably crucial in a movement's emergence, the arrival of educational opportunities often coevolves with economic development and culture (Duflo 2012; Goldin 2006; Morris and Staggenborg 2004). Thus, it remains unclear whether increasing educational attainment can bring about societal change by facilitating the emergence and success of social activists and movements.

In this paper, we isolate the role of education in the emergence of social movements by studying the women's rights movement in Germany, and its relation to the expansion of educational opportunities for women. By 1919, German women achieved suffrage largely due to the growing influence of women's rights associations (Schraut 2019). By 1909, these associations were present in more than 320 cities, with the women teachers' association alone organizing more than 23,000 female teachers. Much like women's rights movements in other countries at the time, early members utilized female-led newspapers (e.g. *Frauen-Zeitung*, 1849–1852) to expand public support for their cause beyond their own demographic of educated teachers, writers, and artists.

In many cases, these early leaders obtained their education at Germany's first institutions providing secondary education and teacher training to women: so-called finishing schools (*Höhere Töchterschulen*). Finishing schools only admitted women and were present in more than 170 cities by 1850. The first finishing schools in Germany were opened by foreign Catholic orders dedicated to female education: Ursuline nuns (Aachen, 1626) and the Congregation of Jesus (Munich, 1627). Despite focusing on religious teachings and manners, these nuns also critically engaged with the ecclesiastical and social discrimination against women and supported the educational and sociopolitical principles of the Enlightenment in the early 1800s (Conrad 1996). Religious finishing schools complemented their curriculum with instructions in foreign languages and arithmetic. In short, they represented the only possibility for women to obtain secondary education or the necessary qualifications to work and live independently as teachers.[1]

Against this background, we leverage the timing of finishing school establishment as a positive shock to the availability of education for women. Using finishing schools, we highlight the role of education at three stages in the history of the women's rights movement. First, in a panel of cities and notable individuals, women started to represent a larger share of the political, intellectual, and economic elite ("human capital elite") after cities established finishing schools. Second, women from these cities also sent a disproportionate share of editorial letters to the first feminist newspaper in the mid-nineteenth century. Third, cities with historical finishing schools had more, and larger, women's rights organizations by the beginning of the twentieth century. We argue that finishing schools facilitated the exchange of critical ideas about women's role in society and the formation of networks; thus contributing to the rise of a female human capital elite from which the nucleus of the women's rights movement emerged. Crucially, these pioneering women disseminated critical ideas among a wider public and founded local chapters to convert their movement into a successful societal force.

We combine three sets of novel historical microdata, each representing one milestone of the women's rights movement in Germany, with data on the availability of education for women across cities and time. Variation in the availability of education comes from the opening date and city of 225 finishing schools constructed between 1626 and 1850 (Zymek et al. 2005). Our measure of human capital in every city and period is derived from the *Neue Deutsche Biographie*. This biographical collection reports the places of birth and occupation for more than 150,000 individuals born between 800 AD and today. Its editors ignored local and time-bound personalities and only included individuals in a high position of responsibility who impacted the general societal course. Thus, these data provide the most comprehensive historical account of Germany's political, intellectual, and economic elite. We measure the dissemination of critical ideas by digitizing all letters to the editor in one of the first, and quickly banned, fem-

[1] As Albisetti (1988, p. xiv) hypothesizes, "*the formal and informal curricula of these schools, when compared to those of the classical Gymnasien attended by boys from the same social groups, could stimulate in young girls an early awareness of, and a protest against, their 'second-class citizenship' rather than a submissive conformity to the 'German ideal of womanhood'.*"

inist newspapers in German history (*Frauen-Zeitung*, 1849–1852), which contain the sender's city of origin and first name. Finally, we obtain variation in the institutionalization of the women's rights movement in Germany from a comprehensive survey on more than 1,200 local chapters conducted by Germany's Imperial Statistical Office in 1909.

For the first milestone, an increased representation of women among the human capital elite, we merge the timing of finishing school openings and the birthplaces of notable individuals to a balanced panel of German cities. In an event-study design with city and period fixed effects, we find that the share of women among the human capital elite rose from 1.8% prior to the opening of schools, to 4% within 50 years. Notably, the share of unmarried women also increased from 2.2% to 3.6%, indicating that finishing schools improved women's opportunities to live independently and be recognized for their achievements.

Cities that establish finishing schools may differ in a wide range of characteristics. Such a selection process would be of concern to our interpretation if it correlates with women's status in society or a city's economic potential; then cities would exhibit different trends prior to school establishment. However, we find no evidence for differential pre-trends in women entering the human capital elite. Our findings are robust to including city and period fixed effects, linear time trends, and flexibly controlling for a rich set of predetermined educational, economic, and religious covariates separately in each period.

Differential population growth between cities might affect our interpretation if larger cities disproportionately attracted individuals from the human capital elite. We address this potential concern by dividing our main variable by the total number of notable individuals born, thus controlling for the size of the elite in every city and period.[2] In addition, we use women from the nobility, a demographic educated by private tutors, as a placebo to capture potentially different population growth rates. We find no evidence that population trends confound our estimates.

If cities establish finishing schools in response to changes in (local) attitudes towards women, we would wrongly attribute the effect of social change to the expansion of education. Thus, to distinguish the impact of education from other social changes, we test whether other important economic and cultural events predict a similar increase in the representation of women among the human capital elite. To this end, we employ a series of placebo exercises and test whether nonlinear changes in (i) economic activity, (ii) the returns to education, and (iii) gender-specific changes in culture predict a similar increase in the emergence of notable women. First, using construction

[2]Our estimated effect is then identified within a city's elite, net of population growth, if the share of elites relative to a city's population remains constant over time.

data from Cantoni, Dittmar, et al. (2018), we find that the establishment of finishing schools did not coincide with a surge in economic activity. Second, we document that the staggered introduction of male schools does not predict women entering the human capital elite; similarly, finishing schools have no impact on men entering the human capital elite. Third, to alleviate concerns about nonlinear gender-specific changes, we employ four markers of gender-specific cultural change as placebo treatments and find that none coincide with a rise in the female human capital elite. Finally, we show that our results are not driven by the Protestant Reformation arriving in cities.

In a final step, we take a different approach to deal with the potential endogeneous adoption of finishing schools. We first show point estimates from a classical difference-in-differences design, adopting recent advances in Baker et al. (2021), and second report estimates from an instrumental variables strategy. First, we define a set of cities based on whether they established a finishing school by 1850 (treatment group) or not (control group), and compare the shares of women entering the human capital elite after the opening of the first finishing school in 1626 (post period). Second, we instrument our treatment group using monasteries constructed before 1300 coupled with religious competition near the denominational divide. Throughout all specifications, we find no differential pre-trends, but a significant increase in women entering the human capital elite after the first finishing school was constructed. These findings carry over when analyzing every treatment period separately: even finishing schools established in the nineteenth century, when women were already more common among the human capital elite, significantly increase women's representation among the human capital elite.

We thus argue that finishing schools, and not cultural change, increased women's representation among the human capital elite. The increased representation is driven by the very demographic that represented the core of the women's rights movement: The share of female teachers and writers increased from 1.9 to 3.6% post finishing school opening, compared to men in the same category. Further, using their biographies to identify activists fighting for equal rights and women's suffrage, we show that the likelihood of an activist being born in a city increased from 1.6% to 6.9%.

This activist nucleus started to form networks early on. We find that after the opening of finishing schools, the probability that a notable woman is mentioned in another woman's biography from the same city increased threefold.[3] To show that these networks, and not finishing schools per se, matter for increased human capital representation, we identify 500 women who migrated during their lifetime. While cities do

[3] These connections are only recorded if they were substantial: for example, if women collaborated on the foundation of a local chapter of a women's rights association. An example of such a connection is the connection between Helene Lange and Gertrud Bäumer who jointly published the feminist newspaper "*Die Frau*" from 1893 onwards.

not differentially attract women before the establishment of finishing schools, women start migrating to cities in which a native notable woman has already established a network.[4]

After leaders of successful social movements have developed critical ideas and formed an early network, they begin spreading their ideas using available mass media and institutionalize their movement. We document this second historical milestone of the German women's rights movement by linking the presence of finishing schools to letters to the editor of the first feminist newspaper (*Frauen-Zeitung*, 1849–1852) in a cross-sectional analysis. Compared to cities without finishing schools, cities with finishing schools are three times as likely to send a letter to *Frauen-Zeitung* in support of the women's cause, indicating a more successful propagation of critical ideas in their city of origin.

The third historical milestone of the women's rights movement we study is its institutionalization. Local chapters of the German women's rights movement sprung up from 1848, with the first organization specifically targeting female education being founded in the 1880s. Yet by 1909, only 37% of cities without finishing schools had established a women's rights organization, compared to 78% of cities with finishing schools in 1850. This difference is even more pronounced for educational organizations, at 5% and 29% respectively; these organizations have an order of magnitude more members when located in a city with finishing schools.

In these cross-sectional results, unobserved differences between cities, previously captured by fixed effects, might reemerge and bias our estimates. We thus always control for economic, religious, and educational covariates to mitigate the threat from differential attitudes towards women. In addition, bias-adjusted point estimates (Oster 2019), estimates from an instrumental variables strategy, using monasteries in 1300 coupled with religious competition as an instrument, as well as estimates from a propensity score matching show a robust and stable impact of finishing schools on all cross-sectional outcomes.

In sum, our findings indicate that educational institutions, which foster the exchange of critical ideas and provide the space to form networks, can function as important catalysts for the emergence of a group of leading activists. Using newspapers to disseminate critical ideas and founding local chapters to institutionalize their movement, these leading activists turned an initially upper-class movement into a broad societal force. Their legacy is still felt today, as cities with finishing schools in 1850 have

[4]These migrating women are a subset that - in our main results - are assigned to their cities of birth. We only assign them to their city of death to identify whether finishing schools were a pull factor in their migration decision. Our results are not the result of a violation of the stable unit treatment value assumption (SUTVA), and are robust to excluding these women, excluding neighboring cities and choosing a larger unit of observation (Appendix 1.D).

brought forth a higher number of female members of parliament in any democratically elected parliament since 1919.

Our paper expands upon a thriving literature in economics studying the increasing representation of women starting in the late nineteenth century (Bertocchi and Bozzano 2016; Fernández 2013; Goldin 1990, 2006; Nekoei and Sinn 2021). First, by disentangling the availability of secondary education from other cultural and societal changes, we show that education was a key driver behind the women's rights movement and the improving status of women in society. Second, and at a more general level, our results indicate that the positive effects of education are not limited to students themselves. In our case, women from various backgrounds benefited from extending education to an initially limited number of women. Thus, our paper also informs a large body of literature in development economics studying the effects of interventions targeted at reducing gender inequality in education (Beaman et al. 2009; Chattopadhyay and Duflo 2004). By providing evidence on the effects of secondary education for women from the historical case of Germany, our paper highlights the potential long-run benefits of such interventions for society at large.

This paper also complements a recent literature in economics, which has highlighted the importance of civic leadership (Dippel and Heblich 2021) and technology (Enikolopov et al. 2020; García-Jimeno et al. 2022; Melander 2020) in promoting the success of existing social movements. We extend this literature by studying how social movements, and their leaders, emerge in the first place. A prominent theory in sociology is that educational capital is the key resource for leaders, even when leaders arise from poorer segments of society (Morris and Staggenborg 2004). By leveraging data spanning several centuries, we can study the emergence of the women's rights movement from before its very beginning until it reached key milestones, such as women's suffrage in 1919. Our findings support the notion that educational institutions that foster the exchange of critical ideas and network formation can serve as important catalysts of the emergence and success of social movements.

Finally, our findings also speak to the literature studying the role of an emerging human capital elite in early-modern Europe and beyond. Here, the human capital elite constituted a herald of economic change in the lead-up to the Industrial Revolution (Diebolt and Perrin 2013; Mokyr et al. 2015; Squicciarini and Voigtländer 2015). The dispersion of this upper-tail human capital over space and time was shaped by the institutional environment such as welfare and educational policies (Dittmar and Meisenzahl 2019; Squicciarini 2020; Tabellini and Serafinelli 2019). Countries with highly educated leaders showed higher rates of economic growth (Besley et al. 2011) and democratic participation (Glaeser et al. 2007). We extend these existing studies in two dimensions: first, we explicitly focus on the female human capital elite; second, we show that in the context of the emergence of the German women's rights movement, this female human capital elite through its impact on early activists' efforts to disseminate critical ideas and institutionalize the movement constituted an important determinant of social change in and of itself.

The paper is structured as follows: In Section 1.2, we discuss the historical link between finishing schools and the women's rights movement. We discuss our data sources and construction in Section 1.3, before discussing the identification assumptions of our empirical strategy in Section 1.4. In Section 1.5 we present our main findings on the finishing schools' impact on women's representation among the human capital elite. In Section 1.6, we conduct several placebo exercises to rule out confounding economic and cultural changes. In Section 1.7, we show that finishing schools facilitated the networking and immigration of women. Before concluding, we discuss the long-run results on the dissemination of critical ideas, the organization of the women's rights movement, and modern-day representation in parliaments in Section 1.8. Finally, Section 1.9 concludes the paper.

1.2 Historical Background

We begin by illustrating the links between the women's rights movement in the late nineteenth century and the emergence of religious finishing schools. In the aftermath of the Protestant Reformation, foreign Catholic women's orders began establishing finishing schools that focused on religious teachings but also included limited aspects of secular secondary education. At these finishing schools, students and teachers alike found access to critical ideas and a network of like-minded women. Several graduates eventually disseminated critical ideas in feminist newspapers and founded the women's rights movement. Religious finishing schools thus contributed to the formation of a group of pioneering women among the human capital elite, who acted as catalysts for social change.

1.2.1 Finishing schools

For the largest part of German history, only daughters from privileged families could obtain secondary education in the form of private tutoring. Access to secondary education for women improved when the orders of the Ursulines and the Congregation of Jesus, founded in Italy 1535 and Flanders 1609 respectively, expanded into Germany. In the aftermath of the Protestant Reformation, these orders aimed to strengthen women's adherence to Catholicism in religiously competitive areas of Germany: the Ursulines founded one of the first finishing schools in Cologne with the explicit goal of creating a "bulwark against emerging Protestantism" (Lewejohann 2014, p. 57), while the Congregation of Jesus established their school near Munich to educate young women in "good Christian manners, virtues and other studies [Wissenschaften]" (Riedl-Valder 2020, p. 2). In response, Pietists opened the first school in 1698, to combine biblical doctrine with a similar focus on Christian life and piety. Some ruling families took pride in sponsoring finishing schools in their territory, but compared to Catholic rulers of Bavaria and Wuerttemberg, "Prussian monarchs did not move as vigorously as others to support secondary schools for girls" (Albisetti 1988, p. 29). By and large, city governments and Prussian rulers only became active in the field of female secondary education in the nineteenth century.[5]

Finishing schools' primary goal was to strengthen women's adherence to the respective faith, while parents sent their girls to finishing schools to improve marriage opportunities. This focus on religious teachings and marketable housekeeping skills emphasizes that religious finishing schools were not established with the explicit aim of empowering women. However, these finishing schools also included limited instruction in German, foreign languages, and arithmetic, and were among the first in German history to provide education at the secondary level to women. In contrast to the rollout of secondary education in the United States (Goldin and L. F. Katz 2003), women generally received lower-quality education than men as female teachers were denied the same quality of education as male teachers. By 1850, more than 200 finishing schools provided secondary education to thousands of young women.

1.2.2 The German women's rights movement

Starting in 1848, early women's rights activists around Louise Otto-Peters publicly demanded equal access to education, equal occupational opportunities and the right to vote (Berndt 2019; Gerhard 1990; Nagelschmidt and Ludwig 1996). Similar in spirit to the agenda of contemporary women's rights movements in the US or Great Britain, they particularly emphasized the necessity of obtaining equal access to education as a key enabling factor for securing the other two central demands, the right to vote and equal occupational opportunities (Schötz 2019).

Initially, only women from the upper class formed the nucleus of the German women's rights movement. To gain broader support and turn the movement into a societal force, early women's rights activists pursued two complementary strategies: the dissemination of critical ideas about women's role in society and an institutionalization of the movement (Berndt 2019; Gerhard 1990; Nagelschmidt and Ludwig 1996). First, the movement started to publish a newspaper in 1849, *Frauen-Zeitung*, to disseminate critical ideas about the role of women in society among interested women and the

[5]The establishment of finishing schools in Protestant areas only gained momentum after 1750, by which time 40 finishing schools had already been established in Catholic regions. When including covariates, we always control for religion and ruler fixed effects to capture these different tendencies. In addition, we provide a specification separating schools into 'Early' and 'Late' schools, to assess the severity of this potentially demand-driven bias.

general public alike; *Frauen-Zeitung* remained the main relay of the German women's rights movement until World War I (Schötz 2019).[6] Second, to coordinate its members, the movement started to establish associations with an increasing number of local chapters throughout Germany.

The first of these women's rights associations, "*Allgemeiner Deutscher Frauenverein*" (German Association of Female Citizens), was founded in Leipzig in 1865 and soon organized more than 20,000 women in 48 local chapters (Kaiserliches Statistisches Amt 1909). An important part of the local chapters' activity was to file petitions to (state) governments: they demanded the equality of women and men in the civil code (1876), the admission of women to universities (1876), and the improvement of the quality of teacher training for women (1887) (Schraut 2019). Reflecting the central importance of teachers, the "*Allgemeiner Deutscher Lehrerinnenverein*" (German Association of Female Teachers), founded in 1890 to advocate for equal access to education for women and adequate training for female teachers, quickly grew to a membership of more than 23,000 teachers spread across 108 local chapters by 1909.

In total, more than one million women joined women's rights associations by 1909 (Kaiserliches Statistisches Amt 1909, p. 17); many also joining political parties when the ban on female entry was lifted in 1908 (Evans 1980). In the first democratically elected parliament of the Weimar Republic (1919), at least 40% of female members of parliament had attended a finishing school and more than 50% had actively fought for womens rights in one of more than 1,200 women's rights associations in Germany.

1.2.3 Finishing schools and the women's rights movement

Several accounts by historians and the biographies of leading women's rights activists, such as the teacher Helene Lange, indicate the importance of finishing schools for the emergence of the women's rights movement in Germany (Albisetti 1988; Ringer 1989; Schaser 2000; Schötz 2019). Based on these accounts, we discuss two mechanisms that link the establishment of finishing schools to the emergence of the women's rights movement: access to critical ideas about women's role in society, and reduced cost to form and access networks of like-minded peers. In this way, finishing schools provided the "foundations upon which the whole breadth and force of the women's movement were to depend" (Strachey, 1928, p. 124, as quoted in Albisetti, 1988, p. xiii).

First, despite their general focus on religious piety, Ursuline nuns and Mary Ward sisters also critically engaged with the ecclesiastical and social discrimination against women and demanded the "spiritual" recognition of the equality of the sexes. They

[6]"*Frauen-Zeitung*" (translated: Women's Newspaper) was renamed "*Neue Bahnen*" (translated: New Ways) after it was banned by the Prussian government. However, the editorial staff and the ideological orientation remained.

also actively supported the educational and socio-political principles of the Enlightenment in the early nineteenth century and amended their religious teachings with secular subjects such as arithmetic and foreign languages.[7] Knowledge of English and French allowed women to access the critical writings of early feminist thinkers (e.g. Olympe de Gouge), which influenced the formation of the women's rights movement in Germany (Hauch 2019). Their ideas likely stimulated a critical questioning of women's role in society among the young women and teachers at finishing schools, especially when contrasting their opportunities with those afforded to their male counterparts (Albisetti 1988).

Second, finishing schools reduced the costs of forming and accessing networks of like-minded women. In contrast to life outside of schools, students at finishing schools lived together without the supervision of their families, being taught by female teachers who pursued an independent lifestyle unthinkable outside of the teaching profession. This provided young women at a formative stage in life with access to a network of students and teachers which could strengthen opposition to their status as second-class citizens (Albisetti 1988; Ringer 1989). Finishing schools thus facilitated the exchange of ideas between teachers and fueled the rapid spread of local women's rights associations across Germany, as illustrated by the more than 23,000 teachers active in the "*Allgemeiner Deutscher Lehrerinnenverein*" (German Association of Female Teachers) in 1909.

More than any other profession, female teachers at finishing schools shaped the direction and force of the women's rights movement in Germany by influencing the lives of generations of women. This does not stand in contrast to the achievements of the working-class women's movement (Evans 1980), but complements the views of Albisetti (1988, p. 249f, 303) and Wolff (2018) who emphasize the importance of the "association and print media structures built since the 1860s" Wolff (2018, p. 9, authors' translation) in carrying the demand for women's suffrage into society at large.[8]

Without finishing schools, neither teachers nor students would have had comparable access to critical ideas and a network of like-minded women. Thus, they contributed to the formation of a group of pioneering women among the human capital elite, united by their opposition against women's status as second-class citizens. Crucially, these pioneering women disseminated their ideas to the broader public and institutionalized their movement, thus acting as catalysts for societal change.

[7] Authors' translation, adapted from Conrad (1996, p. 256 and p. 262).

[8] Our findings are consistent with the idea that both the bourgeois and the working-class women's movement made important contributions to improving women's opportunities in general and suffrage. Both, female leaders of the SPD such as Clara Zetkin and leaders of the "radical wing" of the bourgeois women's movement such as Anita Augspurg, Minna Cauer, Lida Gustava Heymann, Gertrud Bäumner, either studied, received teacher training or taught at a finishing school at one point in their life.

1.3 Data

We assemble a novel dataset to study the role of secondary education in promoting the emergence of a female human capital elite. Our main outcome variable is derived from the biographies of all notable individuals born between 800 and 1950 CE within modern-day boundaries of Germany. Our explanatory variable finishing schools captures the availability of secondary education for women between 1626 and 1850 in all German cities. We combine these data to a balanced panel of cities in half-century periods, indicating the birth of notable women and the availability of secondary education at the nearest city.

Biographies of notable women. We obtain detailed microdata on the universe of notable German women and men for the period 800 to 1950 CE from the *Neue Deutsche Biographie* (NDB) to construct measures of women's representation among the human capital elite. The NDB is "considered the single most relevant biographic encyclopedia of the German language" and includes biographies detailing the professions and nobility of historically relevant men and women (Bayerische Akademie der Wissenschaften. Historische Kommission 1953).[9] It incorporates its direct predecessor, the *Allgemeine Deutsche Biographie* (ADB) (Königliche Academie der Wissenschaften, Historische Commission 1875), and in scope is comparable to the *Dictionary of National Biography* for British notable men and women.[10] We link 2,363 non-noble secular women to cities of birth within in the modern-day boundaries of Germany after 800 CE, as well as 261 women from the nobility, who we use as a placebo to ensure our estimates are not affected by differential population growth between cities. Thus, for each city and period, our data records the number of women born who later became recognized for their achievements. Of all 2,624 women, 32% became notable for being an artist, 21% for being a writer, 10% for being born into nobility, and 6% each for being an academic or a politician (Table 1.1). We use the place and date of birth of notable women alongside the reported biographical information to trace women's representation among the human capital elite across cities and periods. Our main dependent variables are (i) an indicator for whether at least one woman was born in a given city

[9]"Those personalities are to be included whose deeds and works reflect the development of German history in science, art, trade, and commerce; in short in every branch of political, intellectual and economic life." (Bayerische Akademie der Wissenschaften. Historische Kommission 1953, pp. VII-VIII). There is no evidence that editors or experts are selected based on the existence of finishing schools: "[t]he editors don't just rely on their own judgment; [the collection] bases its decisions on the advice of experts, on the advice of scientific institutes, and professional organizations. Essentially, it is assumed that the local and time-bound personalities have to be eliminated. In the areas of intellectual culture, it is primarily the independent, forward-looking achievement that decides, in the case of persons in a high position of responsibility, the impact on the general social course." (Bayerische Akademie der Wissenschaften. Historische Kommission 1953, p. IX, authors' own translation).

[10]The contents of NDB and ADB are freely available online (Historische Kommission der Bayerischen Akademie der Wissenschaften 2019).

Table 1.1: Summary statistics: Finishing schools and notable women

	Cities		
	Without finishing schools (N=259)	With finishing schools (N=129)	Percent of sample
Data: Female finishing schools in Germany			
Finishing schools	0	1.620	
Data: Neue Deutsche Biographie			
	Non-Noble Secular (NNS)		
Academic	33	131	0.063
Artists	139	712	0.324
Founders	2	9	0.004
Medicine	17	56	0.028
Not assigned	45	146	0.073
Occupations	39	136	0.067
Politics	43	122	0.063
Sports	0	5	0.002
	Teachers and writers (also NNS)		
Teacher	27	59	0.033
Writers, publishers	146	416	0.214
	Activists (also NNS)		
Activists	36	94	0.050
	Unmarried women		
Unmarried	492	1666	0.822
	Nobility		
Royals, wives, relatives	91	170	0.099
	Clergy		
Nuns	25	55	0.030
	Population (Bairoch, 1988)		
Population in 1600	5.3	10.4	

Notes: Summary statistics on finishing schools and notable women: 129 cities in our dataset established at least one finishing school by 1850, while the remaining 259 cities have built none. Among cities with finishing schools, the average number of schools is 1.62; 85 cities established exactly one school; 29 cities built two schools, and 15 cities opened three or more schools. The subsequent rows detail the absolute number of notable women in each category alongside their share among all notable women in our dataset. We classify notable women in two broad categories: (i) non-noble secular women (academics, artists, teachers, writers, etc.,) and (ii) women belonging to the nobility (royals, wives and relatives of notable men) and the clergy (nuns). Activists and unmarried women are separately coded and as such, could simultaneously belong to any of the other groups. The last row reports the average city size in thousands, indicating that cities that have established a finishing school by 1850 were nearly twice as large in 1600. While this relationship is very similar for women from the nobility (1.9) and the clergy (2.2), non-noble secular (unmarried) women are 3.6 (3.3) times more likely to originate from cities with finishing schools. We control for the difference in population by interacting "Population in 1600" with period fixed effects in all regression with control variables.

and period who became notable later in life, (ii) the log number of notable women, (iii) and the share of notable women among all notable individuals. These variables measure the extensive and intensive margin of women's representation among the human capital elite.

Finishing schools We link the birthplaces of all notable women to the historical emergence of finishing schools providing secondary education obtained from the "*Data Handbook of German Education History*". This handbook covers traditional female finishing schools constructed between 1626–1850 and their location as shown in Figure 1.1 (Zymek et al. 2005).[11] We match finishing schools to our data on notable women based on their location and opening date. The first finishing schools were established by female orders of the Catholic church who, following an invitation by the ruling houses, settled near existing monasteries to educate and "protect the women's mind from the falsities of their time."[12] Protestant or city schools only started to emerge after 1750. In total we record 209 school openings in 129 cities between 1626 and 1850, without a clear spatial pattern in location or timing (Figure 1.1).[13]

Cities Since birthplaces of notable women and the location of finishing schools do not overlap perfectly, we utilize data from Voigtländer and Voth (2012) to construct a balanced panel of 388 German cities that existed in 1300.[14] For each city, we create 50 year periods from 800 until 1950 CE to ensure a sufficient overlap between the opening of a finishing school and its effect on women becoming recognized for their achievements in our biographical database. We then merge the biographies of women and the emergence of finishing schools to the nearest city and period in our sample, thus covering all of modern Germany. This procedure has two advantages: First, it does not rely on any political or geographical boundary as the matching procedure is solely based on distance.[15] Second, we can use the rich set of covariates from Voigtländer

[11]We focus on these schools with continuous operation selected by Zymek et al. (2005) as the most comprehensive data on finishing schools ("*Höhere Töchterschulen*") in Germany before the emergence of the women's rights movement. Other schools existed, especially in later years, but Zymek et al. (2005) do not include these schools for two main reasons: First, these schools often operated only for a few years and closed down quickly for unknown reasons. Second, it is often unclear weather these schools provided a curricula that extended beyond basic primary education. Since such schools are more likely to appear in the later years of our dataset, we divide the data into 'Early Schools' prior to 1750, and 'Late Schools' post 1750 in Table 1.C.5 in the Appendix. We find no differential impact, and thus no evidence for a bias arising from the omission of these temporary existing schools.

[12]"*. . . vor allem den unteren Volksschichten das religiöse Leben (zu) heben und den Frauen Ansichten und Grundsätze (zu) vermitteln, durch die sie gegen Irrtümer ihrer Zeit gesichert und für eine gesunde Erweiterung ihres Lebensinhaltes befähigen würden*". Source: https://bit.ly/2WGKe4I, cited from Festschrift der Ursulinenschule, Köln (2014, S. 261, last accessed September 2, 2021).

[13]Some later schools might have been a response to local demands of the population. We report the same results for when using schools constructed in the period 1650–1750 or 1750–1850 in the Table 1.C.5. We also report no differential pre-trends and similar sized point estimates for every treatment period in Figure 1.F.2 and Table 1.F.2. Schools are not spatially correlated (Moran's I: 0.002, p-value 0.156), yet we follow two additional strategies to deal with any remaining spatial autocorrelation. First, we report standard errors corrected for spatial correlation in Table 1.D.1. Second, we randomly distribute the actual number of schools build in every period across Germany and show the distribution of point estimates in Figure 1.D.1.

[14]The 'extended sample' of Voigtländer and Voth (2012) includes 1,428 'towns and cities', 739 of which were mentioned before 1300. Many of these 'towns and cities' are close to a major city. For example, Voigtländer and Voth (2012) link three 'towns and cities' to Aachen: AACHEN L, town_id 1,3,4, mentioned in 930, 1118, and 870 CE who are close to the original city of Aachen (AACHEN S, town_id 5, mentioned in 400 CE). We use the latter as our reference city if it lies in present-day borders of Germany to control for spillovers from suburban towns to cities. Results are robust to changing the year a city existed to 800 (Table 1.C.1), changing to 25 year periods (Table 1.C.2), and including city$\times$period fixed effects in a panel setting with gender$\times$city$\times$period as the level of observation (Table 1.3).

[15]In an alternative approach explored in Appendix 1.C.2, we instead use administrative boundaries of territories in 1618 and merge all data based on whether city 'y' was in territory 'x'. As our results

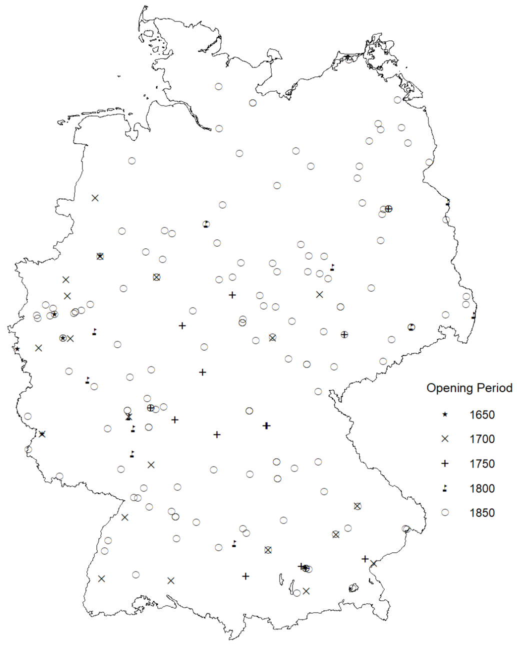


Figure 1.1: Spatial distribution of finishing schools in Germany by opening period
Notes: Spatial distribution of finishing schools by their opening period reported. In Figure 1.C.1 in the Appendix, we add the spatial distribution of notable women across cities to the figure. We depict finishing schools by opening period and religious denomination in Figure 1.J.1 in the Appendix.

and Voth (2012) to flexibly capture economic, religious, and educational factors, as measured in 1300, in every period.

1.4 Empirical Strategy

We study the role of secondary education in promoting the emergence of a female human capital elite which later formed the nucleus of the German women's rights movement. Our empirical strategy combines the staggered introduction of religious finishing schools and unique biographical microdata on the universe of notable women in German history to a balanced panel of 388 cities between 800 and 1950 CE. The key empirical challenge is then to isolate the impact of finishing schools from potential confounders that are correlated with both finishing school opening and the increase in women's representation among the human capital elite.

Cities that establish finishing schools may differ in a wide range of characteristics. Even if these schools were established for reasons that are arguably uncorrelated with local economic conditions or the demand for education, a causal interpretation of the impact of finishing schools requires that all unobservable factors that influence

remain qualitatively unchanged, we argue that sample selection does not introduce a bias in our setting.

women's representation among the human capital elite must be orthogonal to finishing school openings. However, as production technologies change, increased returns to education induce a rise in the demand for education. Similarly, wars or natural catastrophes that disproportionately affect the male population increase the demand for female labor and thus the demand for educated women. These local, often unobservable, factors can increase the adoption of educational policies and thus change the relative wages between cities. Then, cross-sectional evidence or failing to control for local factors risks overstating the true effect of finishing schools on women's representation among the human capital elite.

1.4.1 Specification

We address local differences between cities by including city and period fixed effects in a two-way-fixed-effects (TWFE) setup, capturing all observable and unobservable time-invariant factors that vary between cities and periods in our sample.

$$Y_{c,t} = \beta \, \textit{Finishing school}_{c,t} + \alpha_c + \alpha_t + \alpha_c \times t + \qquad \text{(Baseline)}$$

$$+ \sum_{\tau=800}^{T=1950} \left[\boldsymbol{X}_{e,c} \, \boldsymbol{\alpha}'_{e,\tau} + \boldsymbol{X}_{r,c} \, \boldsymbol{\alpha}'_{r,\tau} + \boldsymbol{X}_{s,c} \, \boldsymbol{\alpha}'_{s,\tau} \right] + \varepsilon_{c,t} \qquad \text{(Additional Controls)}$$

In our baseline specification, we regress a binary outcome of whether a woman who became notable later in life was born in city c and period t, on an indicator of the presence of a finishing school. We use two definitions of this indicator $\textit{Finishing school}_{c,t}$: In our main specification, this indicates whether a finishing school is present in city c at time t. In Appendix 1.F, we abstract from the variation in timing and define this variable as the classical difference-in-differences estimator, comparing 129 cities with finishing schools to 259 cities without after 1650: $\textit{Finishing school}_c \times \mathbf{1}(t \geq 1650)$.[16] We include city α_c and period α_t fixed effects as well as city-specific linear time trends $\alpha_c \times t$. This baseline set of fixed effects captures all unobservable city-specific trends that evolve linearly over time. We cluster our standard errors at the city level c and report standard errors corrected for spatial correlation in Appendix 1.D, Table 1.D.1.

To identify the impact of finishing schools on women's representation among the human capital elite, we must argue that conditional on our set of fixed effects, either school assignment is as good as random or that observed increases in women's representation among the human capital elite can only be attributed to finishing schools. Since the former is unlikely, the latter requires us to relate the increase in the number of notable women being born after the opening of the first finishing school to the long-term trends that determine women's representation among the human capital elite

[16] Using this classical difference-in-differences design we find no evidence for pre-trends (Figure 1.F.1) and similar point estimates (Table 1.F.1). Further, we find no evidence of differential pre-trends or heterogeneous treatment effects across treatment periods (Figure 1.F.2 and Table 1.F.2).

and finishing schools. Then, to identify the impact of finishing schools, cities need not exhibit different trends prior to the establishment of the first finishing school. In addition, since our baseline specification already captures differences between cities that grow linearly over time (e.g. population growth), our identifying assumption necessitates sufficiently capturing all remaining nonlinear, city-specific, confounding factors.

With our additional controls we capture three sets of potential confounders that might nonlinearly predict women's representation among the human capital elite and the opening of finishing schools: economic, religious, and educational characteristics. The first set of covariates capture the potential direct effects of economic characteristics that influence the decision to open finishing schools ($X_{e,c}$). We proxy for the economic and financial development using membership in the Hanseatic League, Jewish settlements and pogroms against Jews (Voigtländer and Voth 2012). We complement these covariates with population data in 1600 from Bairoch et al. (1988), female specific labor demand as proxied by religious battles during the 30 Years' War affecting sex-ratios and local weather conditions affecting agricultural production from Leeson and Russ (2017). Combined, these covariates, measured before the opening of the first school, capture demand factors of productivity and relative wages that may impact the decision to establish a finishing school.

The second set of covariates capture the potential influence of religion on school opening and women's representation among the human capital elite. Since almost all early finishing schools were established by religious orders, this set of covariates captures any direct effects of religious differences across cities ($X_{r,c}$). We include whether the city was a bishopric seat (Voigtländer and Voth 2012) and distance to Wittenberg to proxy for the diffusion of Protestantism (Becker and Woessmann 2009; Cantoni 2015). We determine which cities were Protestant or Catholic in 1618 by digitizing cartographic material in Engel and Zeeden (1995), and include the distance to the inner-German denominational boundary to capture religious competition between the major religious denominations. In combination, our religious controls thus address two major concerns regarding the comparison between Protestant and Catholic cities: first, early finishing schools were built by Catholic orders and Protestant cities did not establish secondary educational institutions in significant numbers until 1750. Second, as highlighted in Becker and Woessmann (2009), since Protestantism is generally associated with a greater proportion of women receiving (limited) primary education, we might wrongly attribute an effect of Protestantism to finishing schools.

Finally, we address the direct effects of differential returns to education across cities ($X_{s,c}$) by determining whether a city had a university or provided higher education for men by 1650.[17] In addition, we capture differential educational preferences across

[17]Data are obtained from https://bit.ly/2OHH4tp (last accessed on September 10, 2021) and from

heads of state by controlling for the ruling house of each city as of 1618 using Engel and Zeeden (1995).[18] Combined, male schools, universities and the educational preferences of ruling houses capture local, gender-invariant returns to education at the time the first finishing schools were established in Germany.[19]

We interact all covariates with period fixed effects to isolate the effects of finishing schools from these confounding factors.[20] Our identifying variation is thus limited to within-city, off the linear time trend of any unobservable confounding factor and the nonlinear evolution of observable economic, religious, and educational differences across time. Hence, all remaining violations of the main identifying assumption must arise from unobservable nonlinear confounding factors which explain both the opening of a finishing school as well as the subsequent increase in women's representation among the human capital elite.

1.4.2 Evaluating pre-trends

We evaluate the validity of our empirical design by testing for differential pre-trends in the event-study graph of Figure 1.2.[21] Here, we limit our sample to all cities in which a finishing school has ever been established and estimate the impact of the first finishing school four centuries before and two centuries after its opening. In Figure 1.2, we provide evidence in favor of our identification assumption as finishing schools have a precisely estimated zero impact in all periods prior to opening. We estimate the impact of finishing schools on two subgroups of women: non-noble secular women (solid line) and the nobility (dashed line). We use women from the nobility as a placebo group and separate them from the remaining notable women, since they likely had access to private tutoring and thus should not be affected by the opening of finishing schools.[22] If the establishment of finishing schools is correlated with an unobserved change in the overall likelihood of being recorded as notable (e.g. population growth or local political change), the point estimate on nobility would be significant

https://bit.ly/3mG9mRr (last accessed on September 10, 2021).

[18]An example is Prince Bishop Ferdinand of Bavaria who, in response to the religious competition, pushed for female education to win over the minds of women.

[19]In the spirit of Galor and Weil (1996) we assume that local returns to education are not impacted by directed technical change that would increase the returns to education for one specific gender. However, estimating a panel with city $\times$ year fixed effects and gender $\times$ year fixed effects in Table 1.3 captures this variation and the point estimates are not statistically different from our baseline.

[20]We explore heterogeneity along all covariates and find no heterogeneous impact or effect on our main coefficient.

[21]We estimate the event-study equivalent of our baseline equation with and without covariates:

$$Y_{c,t} = \alpha_c + \alpha_t + \sum_s \beta_s \mathbf{1}\{t - E_c = s\} + \varepsilon_{c,t}$$

$\{t - E_c = s\}$ denote relative time periods to opening of the finishing schools. Cities enter this sample 400 years prior to the establishment the first school and leave it 150 years after.

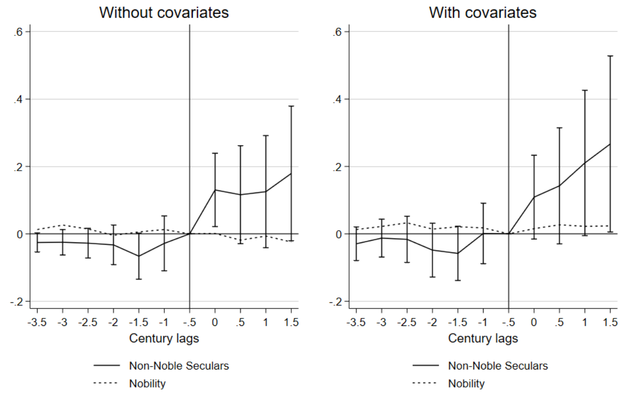
[22]We separate this group not to discredit the efforts and successes of many noble women advocating women's rights, but merely to reflect historical differences in the provision of secondary education.

in post periods. However, while we find no impact of finishing schools on women from the nobility, the probability of a non-noble secular woman being born in the city and becoming notable later in life increases immediately after the first school opened. This relationship remains robust when including all control variables nonlinearly in the right panel of Figure 1.2a.

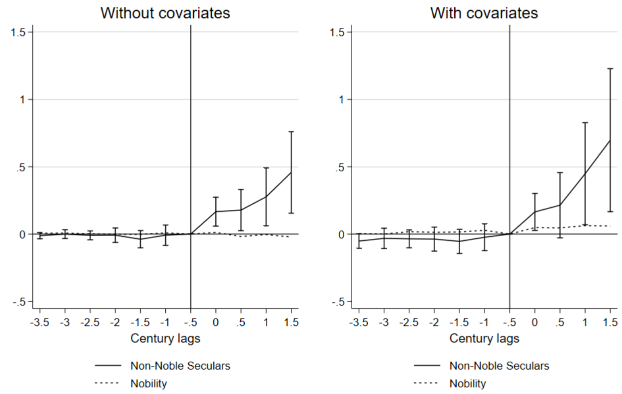
In the remaining panels of Figure 1.2, we document the absence of pre-trends when using the number of women born (Figure 1.2b) and the share of women among all notable individuals born in the same city and period (Figure 1.2c). We observe a significant treatment effect in the first period after opening that is slightly increasing in the right panels when controlling for covariates.

If this slight increase is driven by cohort-specific treatment effects, our TWFE estimator might produce biased estimates. This problem is most pressing in settings without a never-treated control group: Here, later-treated cohorts function as the control-group for earlier-treated cohorts, potentially creating negative treatment weights biasing the estimate (Goodman-Bacon 2021). Using the decomposition suggested in Goodman-Bacon (2021), we find non-negative weights and point estimates that result from the difference between never-treated cities and cities with finishing schools. We thus leverage cities that never establish a finishing schools as a pure control group in our setting and follow Baker et al. (2021) in providing three sets of evidence against heterogeneous treatment effect biasing our estimates: First, we report the main event-study graph with and without controls (Figure 1.2). Second, we assess pre-trends by treatment cohort (Figure 1.F.2) and report estimates for each treatment-cohort (Table 1.F.2). Third, in Appendix 1.E we implement the aggregation methods suggested by Chaisemartin and D'Haultfœuille (2020) and Callaway and Sant'Anna (2020), as well as include never-treated cities in the event-study design. We find no evidence of treatment-effect heterogeneity or differential pre-trends and report similar point estimates in all treatment groups and methods.

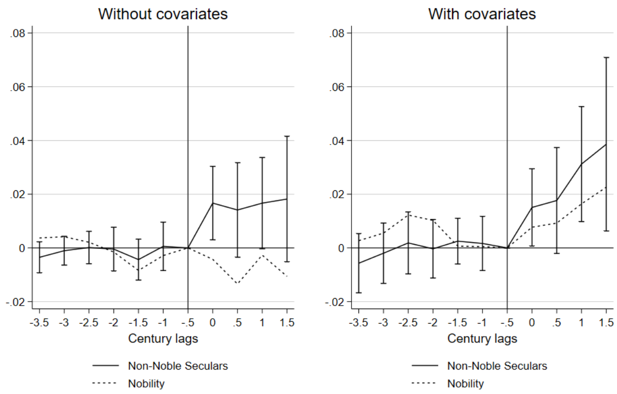
Finally, choices when creating the data might affect the observability of pre-trends. In our data, we merge women and finishing schools to a balanced panel of 388 cities, including never-treated cities, and 50-year periods. This, however, does not fully utilize the underlying premise of event studies: the exact treatment period of each school. In Appendix 1.C.3, we instead construct 10, 20, 25, and 50 year intervals around each exact opening year of finishing schools and show the resulting event-study graphs. Again, we find no evidence for a pre-trend in any specification, a significant uptick after opening, and point estimates that are not statistically different from our baseline. Thus, we use our balanced panel of cities, allowing us to include never-treated cities and control variables in a TWFE estimation, and take this result as additional evidence against pre-trends or heterogeneous effects biasing our estimates.



(a) Indicator for notable woman



(b) Log. number of notable women



(c) Share of notable women

Figure 1.2: Event-Study: Impact of finishing school establishment on notable women

Notes: Results from event-studies reported. Three different dependent variables employed: Figure (a) uses a dummy taking value 1 if a city observed the birth of at least one notable woman in a given period; Figure (b) employs the natural logarithm of the number of notable women plus 1; and Figure (c) divides the number of notable women by the total number of notable individuals in a given city and period. Zero is the normalized opening year of the first finishing school in a city. The vertical line marks the reference period – that is, 50 years before the establishment of the first finishing school. City and period fixed effects included in all panels; the full set of economic, religious, and educational controls added in all right panels. 95-percent confidence intervals based on standard errors clustered at the city level reported for non-noble secular notable women. The impact on notable women from the nobility is indistinguishable form zero in all periods and specifications. Alternative approaches are discussed in Appendix 1.E.

1.5 Finishing Schools and the Human Capital Elite

Our hypothesis is that the opening of finishing schools increased women's representation among the human capital elite. Women belong to the human capital elite of their city of birth if their names were recorded in the *Neue Deutsche Biographie*. Using data on notable women from 800 to 1950 CE, we document a sustained impact of the opening of finishing schools on an indicator of whether a notable woman was born, the number of notable women, and the share of notable women relative to their male counterparts. Using detailed occupational and biographical data, we provide additional evidence that finishing schools contributed to women entering the human capital elite as teachers and activists. These women later formed the core demographic of the women's rights movement, spreading their ideas in the *Frauen-Zeitung*, and organizing in women's rights organizations throughout the country.

We present our main results in Table 1.2, using our baseline empirical specification including all cities and periods. We report estimates from three different specifications of our dependent variable to address the sparsity in our outcome variable. In columns (1) and (2), we regress an indicator variable of whether a notable woman was born in city c at period t on our indicator variable for finishing schools that turns on after the opening of the first finishing school in city c period t. Our baseline estimate is reported in column (1) of Panel A and suggests a 23-percentage point increase (s.e. 0.029) in the propensity to observe a woman being born and becoming notable later after the establishment of the finishing school. To capture the impact of city-specific differences on the establishment of finishing schools and notable women, we interact economic, religious, and educational covariates with period fixed effects in column (2). The point estimate of 0.164 (16%, s.e. 0.033) suggests a stable impact of finishing schools on women's representation among the human capital elite, with finishing schools doubling the likelihood of observing a notable woman in periods after their establishment.[23]

In the remaining columns (3)–(6) we explore the intensive margin of the effect of finishing schools on women's representation among the human capital elite. Using the log number of women born in city c at period t, we find that the number of notable women increases by 20%, even when extensively controlling for economic, religious, and educational factors.[24]

[23]If there were a survival bias of schools and we assume schools have a positive impact, our estimates would be downward biased as control observations would be treated as well. In addition, we report reduced form estimates, unaffected by selection, using monasteries in 1300 as an instrument around 10km of the denominational divide in Figure 1.F.4 in the Appendix.

[24]Using the logarithm of a variable with a large amount of zeros is problematic as the $\log(y+1)$ transformation might introduce a bias. We are aware of this and thus refer to columns (1) and (2) as our preferred specification and report all figures using the binary definition (columns 1 and 2) as the outcome variable.

Table 1.2: Fixed-effects results on the importance of finishing schools

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.230***	0.164***	0.355***	0.204***	0.019***	0.021***
	(0.029)	(0.033)	(0.053)	(0.045)	(0.004)	(0.005)
Mean, untreated	0.150	0.149	0.272	0.272	0.018	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.194***	0.147***	0.302***	0.173***	0.011**	0.014**
	(0.030)	(0.034)	(0.049)	(0.043)	(0.005)	(0.006)
Mean, untreated	0.155	0.153	0.275	0.274	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.151***	0.104***	0.174***	0.103***	0.019***	0.017***
	(0.027)	(0.026)	(0.034)	(0.029)	(0.006)	(0.006)
Mean, untreated	0.076	0.075	0.096	0.096	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.076***	0.053***	0.064***	0.043***	0.013***	0.011**
	(0.018)	(0.018)	(0.017)	(0.015)	(0.004)	(0.005)
Mean, untreated	0.016	0.016	0.018	0.018	0.005	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.018	-0.013	-0.009	-0.007	-0.002	-0.002
	(0.016)	(0.017)	(0.015)	(0.018)	(0.008)	(0.009)
Mean, untreated	0.039	0.038	0.050	0.050	0.018	0.018
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	9,312	9,240	9,312	9,240	9,312	9,240

Notes: Results derived from main TWFE specification using all cities in all periods reported. We consider three types of dependent variables to capture the extensive and intensive margin of the birth of notable women: (i) $\mathbb{I}[Women > 0]$ is an indicator taking value 1 if a city observed the birth of at least one notable woman in a given period; (ii) "log Women" constitutes the natural logarithm of the number of women born plus one; (iii) "Share Women" divides the number of women by the number of men and women in the same category, except for "Activists", where we use the number of male politicians instead. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. Columns (1), (3), and (5) are derived from our baseline specification and include city and period fixed effects as well as city specific linear trends. In columns (2), (4), and (6) we interact city controls with period fixed effects to capture variation from economic, religious, and educational differences. We include the following controls measured in the thirteenth century: Hanseatic League and bishopric seat indicators as well as indicators for the reported presence of a Jewish community and an antisemitic pogrom. In addition, we include the following controls from 1600: distance to Wittenberg, an indicator for confessional battles in the vicinity, distance to the denominational divide, and a Catholicism indicator (as of 1618) to capture religious differences. Furthermore, we control for the average temperature in 1650 to capture differential agricultural productivity, and hence income. City-level population in 1600 is included to capture different population effects; pre-existing male schools, universities in 1650, and a ruling house indicator are included to capture differential educational preferences. All controls are interacted with period fixed effects. Standard errors clustered by city reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

Population in 1650 interacted with period fixed effects might not adequately capture the heterogeneous growth paths of German cities.[25] By using the number of notable men born in each city and period, we are able to capture differential growth in population, prosperity, and creativity, that might lead to the adoption of finishing schools and an increased representation of women among the human capital elite. In columns (5) and (6), we thus divide the number of notable women born by the total number of notable men and women in the same category and period. If the number of notable women in our sample only increased due to a discontinuous change in population, prosperity, or creativity, happening at the same time, this would increase in the number of notable men in the same category, too.[26] Relative to cities without finishing schools in which 1.8% of all notable individuals are women, the share of women among the human capital elite increased to 4% after the establishment of finishing schools.[27] The robust estimates suggest that finishing schools increased women's representation among the human capital elite and did not affect a city's population or its elite's size in particular.

Similarly to other countries (Goldin 2006), the majority of notable women were unmarried and independent. The share of unmarried women, relative to all unmarried men and women, increases from 2.2% to 3.6% after the opening of finishing schools (Table 1.2, Panel B, Column 6). While it is possible that measurement error in the data biases this point estimate, the measurement error would have to be correlated to finishing school opening to bias the point estimate upwards. Our results thus support the notion that finishing schools facilitated the emergence of a greater number of women pursuing a more independent lifestyle, free from the constraints of marriage in a patriarchal society.

In the remaining panels (C)–(E) of Table 1.2 we explore the effects of finishing schools on different subcategories of notable women based on their professions and the placebo group, women from the nobility. First, we confirm historical accounts arguing that many students went on to become teachers and writers by showing that the likelihood of a female teacher or writer being born and recorded in our data is substantially higher after the opening of a finishing school. Second, we analyze the biographies of

[25] While Aachen and Trier were some of the most important cities at the beginning of our sample period, they have been outpaced by Munich and Berlin at the end. This pattern is not predicted by initial population size or ruling houses in the seventeenth century, but due to the emergence of the Prussians and Wittelsbacher lines.

[26] The number of notable men is constructed and obtained from the same source as the number of notable women.

[27] We address the possibility that people move to neighboring towns with schools, and thus spillovers are impacting our interpretation, in two tables: We increase the catchment area of each city by only using 101 cities that already existed in 800 and show the same effect sizes (Table 1.C.1); In Table 1.D.2 we restrict our sample to 129 cities with schools and 27 non-neighboring cities in 1300. All results are robust and indistinguishable from the baseline empirical specification.

all notable women and use keywords to identify women's rights activism.[28] While we record markedly fewer women than in other categories, the relationship is robust and stable in all specifications and suggests a threefold increase in the likelihood of observing an activist after the opening of a finishing school (Panel D, columns (2) and (4)).

Finally, we estimate the impact on the subgroup of notable women from the nobility in Panel E. Again, we treat the nobility as a placebo group since the likelihood of being recorded in the *Neue Deutsche Biographie* should not benefit from the establishment of a finishing school. This subgroup captures overall trends in population growth that should equally affect all notable individuals of either category. In line with our argument that the relationship between finishing schools and women's representation among the human capital elite is not mechanically driven by population growth, we find robustly estimated insignificant null effects of finishing schools on the nobility throughout all specifications.[29]

We take the strong and robust results on non-noble secular women, and the non-existent impact on women from the nobility, as evidence that finishing schools indeed increased women's representation among the human capital elite in Germany. We conduct numerous further robustness tests in the Appendix to this paper. In Appendix 1.B, we show that our results remain qualitatively unaffected when omitting the linear time-trend, using different covariates (Table 1.B.1), or omitting outliers (Figure 1.B.1). In Appendix 1.C, we gather additional evidence against data construction choices biasing our estimates: Our results remain unchanged when using alternative sets of cities (Table 1.C.1) or alternative lengths of periods (Table 1.C.2). The estimated effect does not vary greatly by occupation (Table 1.C.3) or the timing of school opening (Table 1.C.5). We dedicate Appendix 1.D to showing that the results are unlikely to be the result of systematic SUTVA violations. To assess whether spillovers affect our interpretation, we create 200 placebo datasets using the true spatial correlation and temporal assignment and find p-values of 0.000 for all outcomes except activists (p-value: 0.020). In Appendix 1.E, we show that our point estimates are also robust to varying weighting techniques from the recent literature on the validity of event-study designs. In Appendix 1.F, we report similar estimates from a classical difference-in-differences setting, dividing cities into those that had established a finishing school by 1850 and those that had not (Table 1.F.1). There is no discernible pre-trend when using all treatment periods jointly (Figure 1.F.1) or when separately identifying pre-

[28]The top five keywords are (in order): "Frauenrecht" (Women's rights), "Frauenbewegung" (Women's movement), "Frauenverein" (Women's clubs), "Emanzipation" (emancipation), and "Feministin" (feminist). The share of women is constructed using the number of male politicians as a proxy for the politically active male population.

[29]Controlling for construction activity does not impact our results (Table 1.B.3) and is not predicted by school establishment (Figure 1.4).

trends by school opening period (Figure 1.F.2). We regard the robustness of our results as evidence against a mechanical relationship between finishing schools and notable women which could arise simply due to finishing schools improving record keeping of influential women or increasing the demand for teachers.

1.6 Placebo Exercises

To rightfully attribute the increase in women's representation among the human capital elite to the emergence of finishing schools, we discuss whether changes in the returns to education, culture, or economic activity predict a similar increase. To identify such potential confouding factors, we exploit the following city- and time-specific placebo events: In Section 1.6.1, we use the opening of secondary schools for men to capture an increase in the overall returns to education. In Section 1.6.2 we use construction activity as a proxy for economic activity; and in Section 1.6.3, we exploit the end of witch trials, the opening of female monasteries, the consecration of churches to a female saint, and the arrival of the Protestant Reformation, to capture gender-specific cultural changes at the local level. No placebo event predicts a subsequent increase in the number of notable women.[30] Unobservable nonlinear and city-specific factors are thus unlikely to confound our finding that finishing schools increase women's representation among the human capital elite.

1.6.1 Returns to education

In our first placebo exercise, we assess whether finishing schools merely capture local changes to the returns to education. We exploit cross-gender variation and show that the number of notable men and women is only affected by the opening of male and female schools, respectively. We thus argue that finishing schools are unlikely to reflect local changes in the returns to education.

To assess the importance of changes in the returns to education, we correlate the occurrence of non-noble secular men, unmarried men, and male teachers and writers, with the opening of male schools. Following Galor and Weil (1996), we interpret schools for men as an endogenous response to increased returns to education following an increased demand for skilled labor. As such, the estimated effect of male schools on the occurrence of notable men is a combination of (i) increased returns to education and (ii) education itself. By the same token, if female finishing schools were also a result of increased returns to education common to both genders, we would expect to see an increase in the number of notable men in response to the establishment of finishing schools.[31] In Panel A of Table 1.3, we limit our sample to 129 cities that

[30]These changes are however, correlated to the establishment of finishing schools, suggesting that they are relevant cultural and educational proxies to consider.

[31]In support of this argument we find that in cities that had both finishing and male schools, the male

Table 1.3: Placebo estimates on the importance of finishing schools: Differential returns to education

	Non-Noble Secular			Unmarried			Teachers & Writers		
	(1) Female	(2) Male	(3) Panel	(4) Female	(5) Male	(6) Panel	(7) Female	(8) Male	(9) Panel
Panel A: Impact of Finishing Schools									
Finishing school$_{it}$	0.096*	-0.002		0.087*	0.003		0.115**	-0.081	
	(0.054)	(0.039)		(0.052)	(0.037)		(0.049)	(0.061)	
Finishing school$_{it}$ × women			0.145**			0.100*			0.123*
			(0.059)			(0.058)			(0.066)
Panel B: Impact of Male Schools									
Male school$_{it}$	0.005	0.066		0.015	0.012		0.000	0.075**	
	(0.012)	(0.040)		(0.021)	(0.041)		(0.005)	(0.034)	
Male school$_{it}$ × men			0.088**			0.072**			0.110***
			(0.038)			(0.036)			(0.030)
City covariates × period FE	Yes	Yes		Yes	Yes		Yes	Yes	
Religious covariates × period FE	Yes	Yes		Yes	Yes		Yes	Yes	
City × period FE			Yes			Yes			Yes
Gender × period FE			Yes			Yes			Yes

Notes: Results derived from fixed-effects regressions reported where we limit our sample to a window of four centuries before and two centuries after the establishment of a finishing school (N=1,421) or male schools (N=2,161). The outcomes are indicators for the birth of notable women or men. In columns (1), (4), and (7), we estimate the impact of finishing schools on women in the sample of cities that ever established a finishing school. In columns (2), (3), and (8) we estimate the impact of finishing schools on men in the sample of cities that have ever established a finishing school. In columns (3), (6), and (9) we construct a panel in which every city × period cell has two observations: one for women and one for men. This allows us to control for city × time fixed effects and period fixed effects of the opposite gender and estimate the impact of finishing schools on women, while nonlinearly controlling for the trends in men and time-dependent city fixed effects. We employ three different outcomes: (i) an indicator taking value 1 if a city observed at least the birth of one notable women (man) in this period; (ii) an indicator taking value 1 if at least one unmarried notable individual was born in a given city and period; and (iii) an indicator taking value 1 if at least one notable teacher or writer is recorded for a given city and period. We include full economic and religious controls as defined in Table 1.2 in all regressions. Due to colinearity with our the "Male school"-treatment variable, we exclude educational controls. Standard errors clustered by city reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

ever constructed a finishing school, in a window of four centuries before and two after establishing the first school. In columns (1), (4), and (7) we estimate the impact of finishing schools on notable women, unmarried women and teachers and writers. Despite the reduction in sample size and the omission of educational covariates, the estimated coefficients in this event-study design are close to those of the fixed-effects estimation reported in Table 1.2. Finishing schools do, however, have no impact on the likelihood of observing notable men in our data (columns (2), (5), and (8)). In columns (3), (6), and (9), we construct a panel in which every city-period cell has two observations; one for women and one for men. In this setup, we are able to control for city-by-period fixed effects and gender-by-period fixed effects to estimate the impact of finishing schools on women, while nonlinearly controlling for the trends in men and city characteristics at any point in time. Our results confirm the pattern observed

school was always constructed before the finishing school.

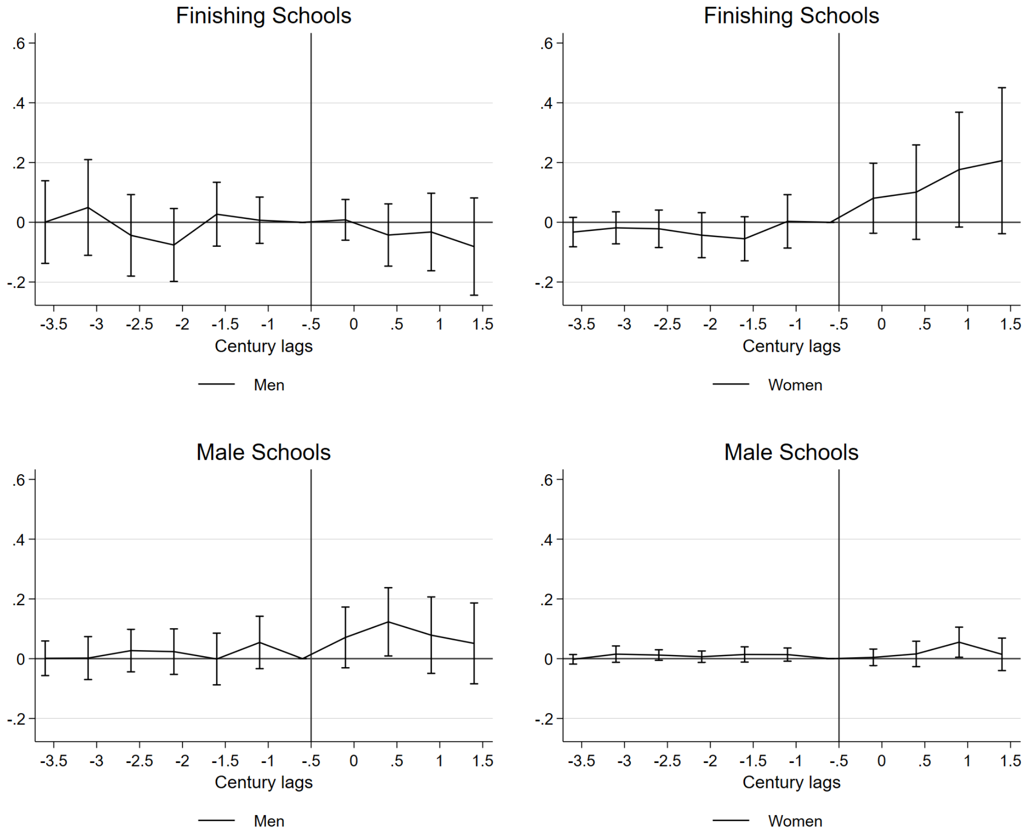


Figure 1.3: Cross-gender impact of male and female schools

Notes: Results from four event-studies reported. The outcome in the two panels on the left (right) is an indicator equal taking value 1 if a notable man (woman) was born in a given city and period. Zero is the normalized opening period of the first finishing school (top panels) or of the first male school (bottom panels) in a city. The vertical line marks the reference period – that is, 50 years before the establishment of the first school for the respective gender. All figures include the full set of economic and religious controls; educational controls are omitted. 95-percent confidence intervals derived from standard errors clustered at the city level reported.

previously as finishing schools increase the likelihood of a notable woman being born in the city. In the second panel of Table 1.3, we turn to the impact of male schools on notable women and men. The opening of a male school in a city increases the likelihood of observing a notable man (Columns (2), (5) and (8)), but the impact on women in the same city is a precisely estimated zero (Columns (1), (4), and (7)). Repeating the panel exercise and nonlinearly controlling for city characteristics confirms this pattern and suggests that male schools only had an impact on notable men in the city.

This evidence is summarized graphically in Figure 1.3, in which we treat the opening of a male school or a finishing school, respectively, as our reference period. The validity of our point estimates is supported by the absence of pre-trends and the increase in notable women and men after the opening of finishing and male schools,

Table 1.4: Placebo estimates on the importance of finishing schools: Construction activity

	$\mathbb{I}[>0]$		*Number*		*log*	
	(1)	(2)	(3)	(4)	(5)	(6)
	Any	Growth	Any	Growth	Any	Growth
Finishing school$_{it}$	-0.043	-0.017	1.805	0.939	0.034	0.133
	(0.034)	(0.066)	(1.236)	(0.644)	(0.108)	(0.111)
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Results from fixed-effects regressions reported where we limit our sample to a window of four centuries before and two centuries after the establishment of a finishing school (N= 1,421). All regressions include the full set of city and period fixed effects as well as full religious and educational covariates as defined in Table 1.2. We employ two outcomes: (i) all construction activity ("Any") in odd columns; and (ii) growth-related construction activity ("Growth") in even columns, which excludes religious, military and palace buildings. We consider three transformations of these outcomes: (i) an indicator for the construction of any building (columns 1 and 2); (ii) the raw number of buildings constructed (columns 3 and 4), and (iii) the natural logarithm of the number of buildings constructed (columns 5 and 6). Standard errors clustered by city reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

respectively (top right and bottom left). If finishing schools captured local returns to education, in the same way male schools likely do, we would observe a significant increase in the number of men as well (top left). Similarly, if we observed more notable women purely because the returns to education increased, we should observe a similar increase in women when using male schools as the source of variation (bottom right). Since we observe neither, we conclude that differential returns to education are unlikely to explain the increase in the number of notable women after the opening of a finishing school.

1.6.2 Economic growth

In the second placebo exercise, we test whether cities with a steeper growth trajectory established finishing schools earlier. Then, finishing schools merely reflect the underlying growth potential that attracted the human capital elite. Under this alternative hypothesis, the increase in women entering the human capital elite is not a response to the emergence of finishing schools, but a mere reflection of increasing income. We measure local economic activity in our panel using city-level construction data by Cantoni, Dittmar, et al. (2018). If finishing schools are merely a manifestation of increased economic growth, the establishment of finishing schools should be a good predictor of future construction activity. However, this is not borne out in our data: even when defining a subset of growth-specific construction that excludes religious, military, and palace buildings, we find no impact of finishing schools on economic activity in Table 1.4, nor in any period around the opening of finishing schools (Figure 1.4).

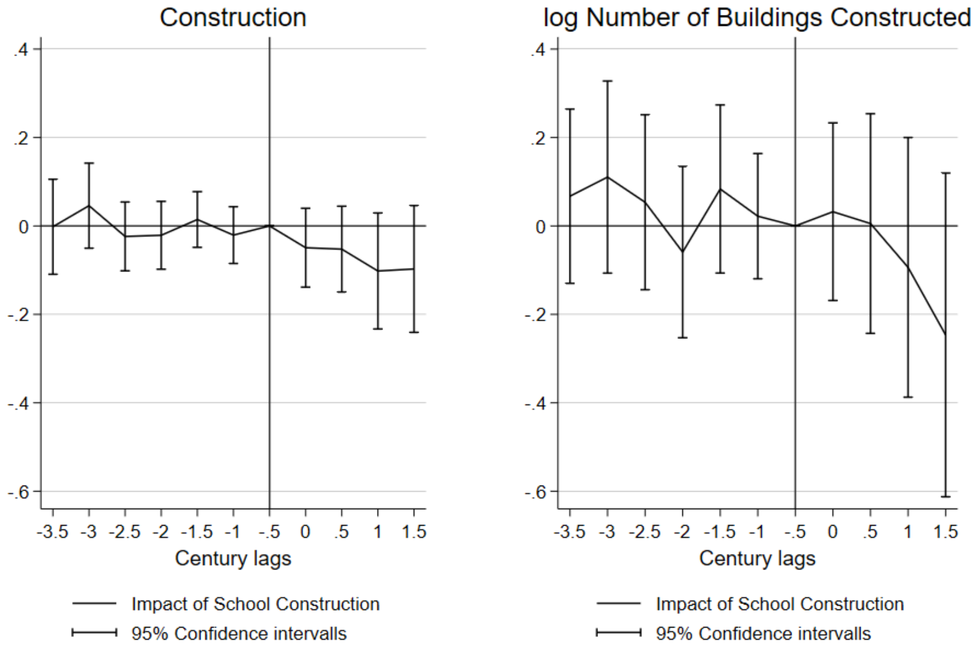


Figure 1.4: Impact of finishing schools on economic growth

Notes: Results from event-studies reported where we employ two outcomes: (i) in the left panel, we employ an indicator variable capturing construction activity in a given city and period; and (ii) in the right panel, we use the log number of buildings constructed plus one. Zero is the normalized opening year of the first finishing schools in a city. The vertical line marks the reference period – that is, 50 years before the establishment of the first finishing school. Full set of controls included in both figures. 95-percent confidence intervals derived from standard errors clustered at the city level reported.

1.6.3 Cultural change

In the last set of placebo exercises, we provide evidence against the premise that finishing schools are a reflection of broader cultural changes in society. To assess this alternative hypothesis, we exploit city and gender-specific changes in culture: the end of witch trials; the opening of female monasteries; the consecration of churches to a female saint; and the Protestant Reformation. Using event-study designs analogous to our analysis of finishing schools, we find no significant impacts on the prevalence of notable women from any of these cultural changes (Table 1.5 and Figure 1.5).

In Panel A of Table 1.5, we use data on the end of witch trials in Germany from Leeson and Russ (2017). Witch trials disproportionately targeted widows living a more independent life as well as midwives and female folk healers (Ehrenreich and English 1973; Oster 2004).[32] We thus argue that the "end of witch trials" in a city is informative

[32] Leeson and Russ (2017) collect data on 3,080 witch trials in 121 German cities, with the first and last trial recorded in 1300 and 1792. Our inclusion is motivated by the fact that 76 % of witch trials were conducted before 1648 and 23.5% of women were trialed between 1627–1633; a period in which finishing

Table 1.5: Placebo estimates on the importance of finishing schools: Changing culture

	Non-Noble Secular		Unmarried women		Teachers & Writers		Royals	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: End of witch trials								
End of Witch Trial$_{it}$	0.002	0.052	0.059*	0.062	0.014	0.005	0.031	-0.016
	(0.028)	(0.040)	(0.031)	(0.044)	(0.020)	(0.025)	(0.025)	(0.030)
Religious covariates × period FE		Yes		Yes		Yes		Yes
Panel B: Creation of a female monastery								
Female monastery opens$_{it}$	0.020**	0.012	0.027**	0.018	0.000	-0.004	-0.001	-0.006
	(0.009)	(0.008)	(0.012)	(0.012)	(0.003)	(0.004)	(0.007)	(0.010)
Religious covariates × period FE		Yes		Yes		Yes		Yes
Panel C: Church consecration to a female Saint								
Consecration to a female saint$_{it}$	0.047	0.031	0.019	-0.005	0.040*	0.041	-0.007	0.006
	(0.031)	(0.036)	(0.039)	(0.043)	(0.021)	(0.026)	(0.033)	(0.034)
Religious covariates × period FE		Yes		Yes		Yes		Yes
Panel D: Reformation happening in city								
Reformation in City$_{it}$	0.017	-0.028	0.069**	0.020	0.015	-0.009	0.030	0.036
	(0.025)	(0.019)	(0.027)	(0.033)	(0.015)	(0.016)	(0.033)	(0.041)
Religious covariates × period FE								
City covariates × period FE		Yes		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes		Yes

Notes: Results from fixed-effects regressions reported. Samples are limited to a window of four centuries before and two centuries after the end of witch trials (Panel A), the creation of a female monastery (Panel B), a church consecration to a female Saint after 1650 (Panel C), and the arrival of the Protestant Reformation in a city (Panel D). All outcomes are indicators taking value 1 if a notable woman from the respective group was born in a given city and period. All regressions include the full set of city and period fixed effects. Cities that have ever had witch trials: N=112; cities with a female monastery: N=221; cities with a church consecration to a female Saint: N=152; cities that turned Protestant: N=146. We include contros as defined in Table 1.2 in even columns. We omit religious controls in panel D, as our ruler fixed effects, the Catholicism indicator (as of 1618), and the distance to Germany's denominational divide predict whether a city becomes Protestant Protestant. Differences-in-Differences estimates confirming the observed pattern are presented in Table 1.F.3 in the Appendix. Standard errors clustered by city reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

of a change in local culture away from one of the most violent forms of discrimination against women. The threat of the stake forced midwives and folk healers to practice in secrecy. Then, the end of witch trials might have increased their likelihood of entering our sample. However, we see no impact of the end of witch trials on women becoming recognized for their achievements.

In Panel B of Table 1.5, we exploit the opening of female monasteries taken from Cantoni, Dittmar, et al. (2018) as proxies for gender-specific cultural change. Female monasteries presented women with one of the few alternatives to "traditionally advocated marriage" (Frigo and Fernandez 2019, p.1) and household roles. The establishment of such monasteries could thus be considered reflective of local culture becoming more accepting towards women choosing a comparatively independent lifestyle.[33]

schools for girls sprung up.

[33]Cantoni, Dittmar, et al. (2018) have 414 female monasteries in Germany with the average year of foundation being 1275.

However, we do not find significant impacts of the establishment of female monasteries on the number of notable women once we add economic, religious, and educational controls.

Next, we turn to the consecration of churches to female saints in Panel C of Table 1.5. We utilize data by Cantoni, Dittmar, et al. (2018) on 12,334 church construction events in Germany, and identify 1,610 events in which a church was consecrated to honor a female saint.[34] We argue that since churches could be consecrated to any saint, using a female saint might indicate a cultural shift towards the inclusion of women and thus could be correlated with a higher status of women in society. Yet, we identify a precisely estimated null effect throughout all specifications.

In Panel D of Table 1.5, we use the timing of the Protestant Reformation in each city as an indicator of a potential shift in the status of women. We follow Becker and Woessmann (2008, 2009) who argue that, since Martin Luther suggested that women needed to be able to read, Protestantism had a positive impact on female education.[35] We utilize data by Cantoni (2015) on the timing of the Reformation in cities, to proxy for a cultural shift towards the inclusion and primary education of women following Luther's teachings. Our findings suggest that Protestantism, and the associated potential shift in gender roles, cannot explain the increase in notable women, teachers, or any subcategory.[36]

The results presented in the event-study graph in Figure 1.5 support the findings from Table 1.5: It is unlikely that gender-specific cultural change contributed to the establishment of finishing schools and the following increase in notable women. We conclude that unobserved economic or cultural change is unlikely to bias our estimates on finishing schools. Instead, it is more likely that finishing schools were established by religious orders in response to religious competition or idiosyncratic shocks. Thus, finishing schools, conditional on fixed effects, can be interpreted as an exogenous shift in the supply of education for women.

1.7 Mechanism

Based on the historical literature on finishing schools (Albisetti 1988) and the women's rights movement (Schraut 2019), we derive two complementary mechanisms that link

[34] The average year of consecration in the data of Cantoni, Dittmar, et al. (2018) is 1452 in 260 cities.

[35] Note that this requirement to read was interpreted as providing basic primary schooling. Finishing schools provided secondary education that included French, arithmetic, and literature classes.

[36] We have 146 cities, 129 of which switched by the sixteenth century. We substantiate our finding in Table 1.F.3 in which we use those cities in a standard difference-in-differences setup, and find weak results on non-noble secular women, but no results on teachers, activists, or nobility. We use the log distance to Wittenberg as an instrument (Becker and Woessmann 2009) and report insignificant reduced form impacts on notable women. The OLS estimates however, suffer from a pre-trend in which cities with more notable women are more likely to become Protestant.

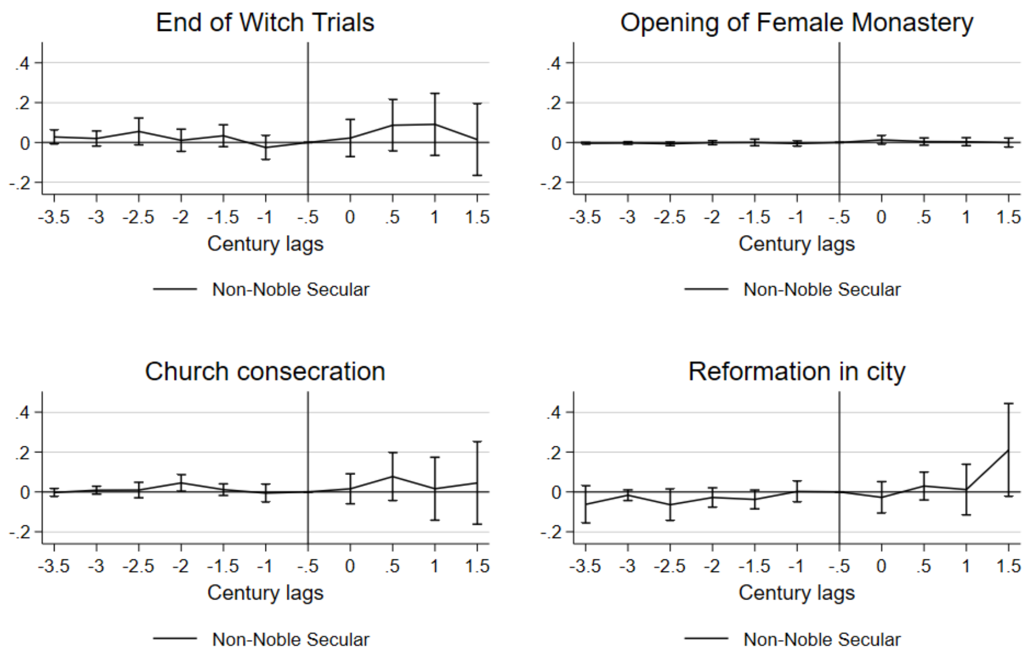


Figure 1.5: Impact of cultural change on notable women

Notes: Results from event-studies reported. The outcome in all panels is an indicator taking value 1 if a non-noble secular woman was born in a given city and period. The vertical line marks the reference period – that is, 50 years before the respective event. Economic and educational controls included in all panels. Religious controls are omitted when identifying the impact of the Protestant Reformation (bottom left panel). 95-percent confidence intervals derived from standard errors clustered at the city level reported.

the establishment of finishing schools to an emerging nucleus of the women's rights movement: access to critical ideas about women's role in society and reduced cost of forming and accessing networks of like-minded peers. We interpret our results thus far as critical ideas about women's role in society taking hold in cities with finishing schools, as more unmarried women entered the human capital elite as teachers, writers and women's rights activists. In this section, we shed light on the second mechanism: finishing schools reducing the cost of forming and accessing networks of like-minded women. We document that the establishment of finishing schools positively impacted the emergence and size of networks between notable women and increased the immigration of notable women, further contributing to network formation.

1.7.1 Networks between notable women

We construct our measure of networks between women by analyzing the biographies of women in the *Neue Deutsche Biographie*. Here, we define a connection between two women if one is mentioned in the biographical text of the other, and the younger was

Table 1.6: Fixed-effects results on the importance of finishing schools: Network formation within cities

	$\mathbb{I}[Connections > 0]$		log Connections	
	(1)	(2)	(3)	(4)
Panel A: Any network in city				
Finishing school$_{it}$	0.060***	0.043***	0.069***	0.052**
	(0.016)	(0.016)	(0.021)	(0.021)
Mean, untreated	0.015	0.015	0.020	0.020
Panel B: Network between non-noble secular women				
Finishing school$_{it}$	0.060***	0.043***	0.067***	0.052***
	(0.016)	(0.016)	(0.021)	(0.020)
Mean, untreated	0.012	0.012	0.016	0.016
Panel C: Network between politically active women				
Finishing school$_{it}$	0.016**	0.012	0.018**	0.015*
	(0.007)	(0.009)	(0.008)	(0.009)
Mean, untreated	0.003	0.003	0.003	0.003
Panel D: Network between religious women				
Finishing school$_{it}$	0.006	0.005	0.005	0.004
	(0.005)	(0.007)	(0.004)	(0.005)
Mean, untreated	0.004	0.004	0.004	0.004
Unit trend	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes
Religious covariates × period FE		Yes		Yes
Educational covariates × period FE		Yes		Yes
Observations	9,312	9,240	9,312	9,240

Notes: Results from fixed-effects regressions using all cities and periods reported. All regressions include the full set of city and period fixed effects. We consider two types of dependent variables to capture the extensive and intensive margin of connections among notable women: (i) $\mathbb{I}[Connections > 0]$ is an indicator taking value 1 if a city had at least one notable woman with a connection to another notable woman in this period; and (ii) "log Connections" is the natural logarithm of of women with connections plus 1. We regress our measures of connections between any notable women, non-noble secular notable women, politically active notable women, and religious notable women, as defined in the top row of each panel, on an indicator for the presence of a finishing school. We include covariates as defined in Table 1.2 in even columns. Standard errors clustered by city reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

at least 16 years old when the older woman died. A network thus exists in a city if at least one local woman is connected to another notable woman.[37] The size of a city's network in period t is then defined as the sum of notable women being mentioned in the biographies of all other women born in that city in period t.

In Table 1.6, we analyze the impact of finishing schools on networks between notable women. We find that finishing schools increase the likelihood of observing a network and its size four-fold (Panel A). The estimated effect, however, predictably varies by the type of network constructed: in stark contrast to networks between non-noble

[37]An example is Gertrud Bäumer: she attended the finishing school in Halle and became a teacher in Magdeburg. She was introduced to Helene Lange by an older colleague and joined the *Allgemeiner Deutscher Lehrerinnenverein* in Berlin 1898. Throughout their career, Bäumer and Lange closely collaborated on promoting women's rights, in particular women's access to education.

secular (Panel B) or politically active women (Panel C), connections between religious or noble networks are unaffected by establishing a finishing school (Panel D). The results on networks between notable women echo our main results: finishing schools increase networks only for politically active women, but not for the placebo group of the nobility.

1.7.2 Immigration of notable women

We provide further evidence on the formation of networks using the immigration of notable women. If finishing schools facilitated women to form and access networks of like-minded peers, presumably they also increased the likelihood that women migrated to the city ("pull" factor). We document migration patterns using the difference between women's places of birth and death as recorded in the *Neue Deutsche Biographie*. A total of 507 women in our data have migrated at least 10 km between birth and death. We repeat our event-study for these immigrated non-noble secular women in Figure 1.6. Again, we observe no pre-trends and a distinct increase in the likelihood of immigration after the opening of the first finishing school (left panel); a finding robust to including control variables (right panel).

To identify whether finishing schools attracted notable women, or the immigration of notable women instead facilitated the foundation of finishing schools (reverse causality), we provide two pieces of evidence: First, if the immigration of notable women increased the likelihood of finishing school opening, Figure 1.6 would show differential pre-trends. The absence of such pre-trends suggests that finishing schools had a similar effect on immigrated women as on native women, and that finishing schools are likely not a result of immigration.

Second, we build on this result and provide further support for the idea of increased networking activity using the timing of immigration, or birth, of the first notable women as our source of variation. If finishing schools increased women's representation among the human capital elite, which in turn attracted notable women from other cities, we would observe that the first native notable woman increases immigration. If, however, immigration led to the opening of finishing schools, and therewith to the formation of a female human capital elite, the first immigration event would increase the number of notable women born in a city.

We explore these alternative hypotheses in Figure 1.7, using either the first women who migrated to a city (left panel) or the first notable women born in a city (right panel) as a shifter in the likelihood of observing future notable women being born. Using the first migration event as the "treatment period" in the left panel, we report no impact on future non-noble secular women being born. In contrast, the right hand side of Figure 1.7 reveals that the first native-born notable woman induces a strong

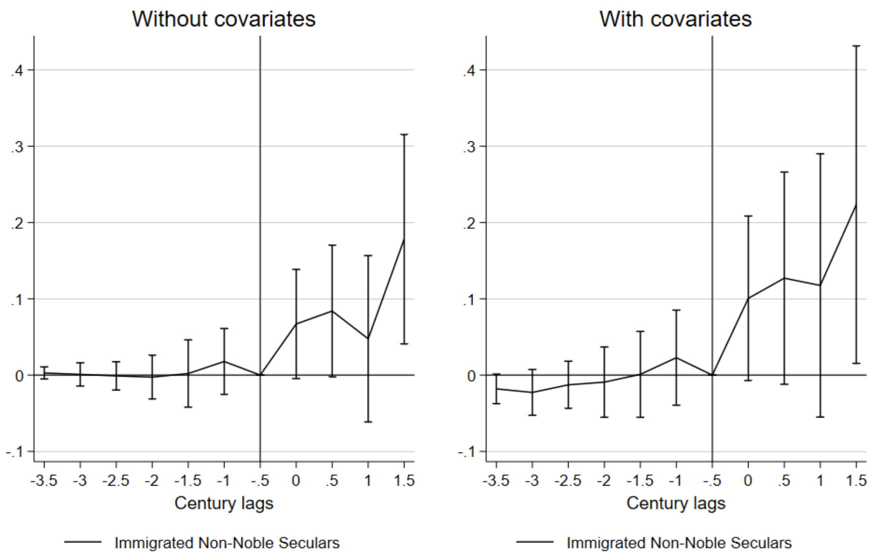


Figure 1.6: Impact of finishing school establishment on migrated women

Notes: Results from event-studes reported where we limit our sample to cities which have ever established a finishing school. The outcome is an indicator taking value 1 if a women born elsewhere migrated to a city in a given period. Zero is the normalized time of the opening of the first finishing schools in a city. The vertical line marks the reference period – that is, 50 years before the establishment of the first finishing school. Full economic, religious, and educational controls added in the right panel. Corresponding point estimates reported in Table 1.G.1 in the Appendix. 95-percent confidence intervals derived from standard errors clustered at the city level reported.

increase in immigration of other notable women from elsewhere.

Our results thus indicate that finishing schools increased women's representation among the human capital elite: women became teachers, writers and early activists, indicating that critical ideas about women's role in society took hold in cities with finishing schools. These women would eventually form networks with other women from the human capital elite and attracted other like-minded women from other cities. These early networks laid the foundation for the further dissemination of critical ideas and the institutionalization of the women's rights movement.

1.8 Finishing Schools and the Women's Rights Movement

When Dr. Martin Luther King Jr. and Susan B. Anthony spread their ideas and institutionalized their movement, they provided the social acceptance required for the civil rights and suffrage movements to succeed. German activists from the early phase of the women's rights movement pursued similar strategies to gain broader public appeal and turn their movement into a societal force (Berndt 2019; Nagelschmidt and Ludwig 1996; Schraut 2019). We measure the dissemination of critical ideas by digitizing all letters to the editor of the feminist newspaper "*Frauen-Zeitung*", in which

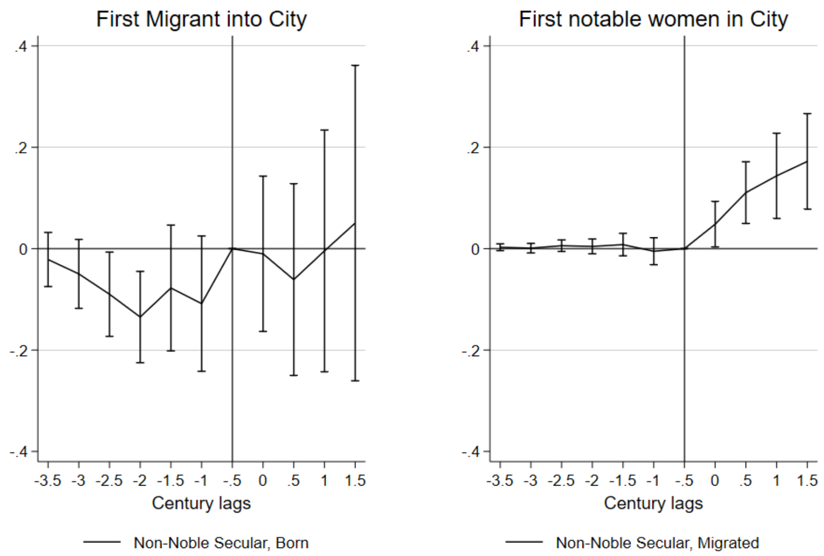


Figure 1.7: Impact of native and migrated women on subsequent notable women

Notes: Results from event-studies reported where we limit our sample to cities which have ever established a finishing school. In the left-hand panel, we asses the impact of the first notable female in-migrant to a city on the birth of "native" notable women in a city. Conversely, the right-hand panel, we depict the impact of the first "native" notable woman born in a city on the in-migration of notable women born elsewhere into the city. Zero is the normalized time of either the first in-migrated notable woman (left) or the first notable woman born in a city (right). Correspondingly, the outcome in the left panel is an indicator equal taking value 1 if a notable woman was born in a given city and period, while the outcome in the right panel is an indicator taking value 1 if at least one notable woman migrated to a city in a given period. The vertical line marks the reference period – that is, 50 years before the respective event. Full set of controls included in both figures. 95-percent confidence intervals derived from standard errors clustered at the city level reported.

women's role in society was critically discussed. To capture the increasing institutionalization, we use establishment and membership data of local chapters of the women's rights movement in 1909. Finally, we provide evidence that finishing schools, via accumulating human capital, disseminating critical ideas, and institutionalizing the movement, increased women's representation in parliaments once suffrage was achieved.

1.8.1 Empirical approach

We document the link between finishing schools and the success of the women's rights movement in a cross-sectional setting. Specifically, we show that cities c with finishing schools in 1850 send more letters to the *Frauen-Zeitung* and have more local chapters of the women's rights movement in 1909. In doing so, we estimate cross-sectional regression using specifications of the following type:

$$Y_c = \alpha + \beta \cdot \text{finishing schools}_c + \gamma \mathbf{X}_c + \varepsilon_c \qquad \text{(Cross-Section)}$$

In this cross-sectional setting, unobservable factors, previously captured by city fixed effects and linear time trends, potentially impact our interpretation. Even controlling for economic, religious and educational covariates ($\mathbf{X}_c$), unobservable factors could be correlated with the establishment of finishing schools and the women's rights movement. When schools were built in areas with greater appreciation of women's role in society or women's education, our point estimate would overstate the impact of finishing schools. We assess the magnitude of this potential bias using three complementary strategies: First, we report the bias-adjusted point estimate from a bounding exercise in the spirit of Oster (2019), comparing coefficients from a regression without any controls and restrictions to a regression with the full set of controls in areas of religious competition. Second, in Appendix 1.H we corroborate these findings and report point estimates from an instrumental variables strategy using monasteries in 1300 and religious competition as a shifter in the likelihood of establishing finishing schools. Third, we compare the effect of finishing schools using propensity score matching on all covariates in Appendix 1.H.1. All strategies reveal, if anything, a downward bias of our point estimates.

The historical literature on finishing schools suggests that religious competition was one determinant of the location of early finishing schools (Lewejohann 2014). Yet, religious competition may exhibit a direct effect on our measures, even when controlling for the distance to the religious boundary. Thus, we limit our sample to cities within 10km of the borders marking the denominational divide in 1618, i.e. to regions where religious competition was particularly pronounced in the early phases of finishing school openings. Limiting our sample to cities within 10km of the denominational divide also enhances the comparability of cities. For instance, rather than comparing Berlin to Munich (600km due south), our strategy compares the neighboring cities of Hanover and Hildesheim.

We present our results linking finishing schools with the emergence of the women's rights movement in the late nineteenth century and with political representation of women throughout the twentieth century in Table 1.7. We start by examining the link between historical finishing schools in 1850 and the dissemination of critical ideas about women's role in society to the general public (Panel A), and the institutionalization of the women's rights movement by founding local chapters and recruiting female members (panels B and C). We then turn to an important outcome of the women's rights movement, women's representation in parliaments after women achieved the right to both vote and stand for parliament in 1919 (panels D and E).

1.8.2 Dissemination of ideas

To measure dissemination of critical ideas, we digitize all letters to the editor of the first feminist newspaper in Germany, *Frauen-Zeitung* (1849-52), in Panel A. We use the place of residence of all letters and link this to the pesence of finishing schools in the nearest city. In Table 1.7 column (1), we estimate a bivariate regression without controls and restrictions, documenting an increase in the likelihood of sending a letter of 0.100 (s.e. 0.017), a 150% increase over the mean. Only 6.2% of cities without finishing schools by 1650 sent letters to the "*Frauen-Zeitung*", compared to 16.2% of cities with finishing schools. We interpret this increase as evidence that critical ideas are more common in cities with finishing schools.

To assess the potential severity of selection on unobservables, we report the bias-adjusted point estimate from a restricted estimation in column (2). Here, we include all previously defined controls and limit the sample to areas that, 200 years prior to the foundation of the "*Frauen-Zeitung*", had been religiously competitive. We estimate a similar point estimate of 0.122 (s.e. 0.037), a four-fold increase over the likelihood of sending a letter in cities without a finishing schools (0.038 in this sample). The bias-adjusted point estimate is of a similar magnitude to the baseline (0.132), indicating a slight downward bias stemming from selection on unobservable factors. In columns (3) and (4) of Table 1.7, we repeat this exercise with the number of letters sent. Again, the bias-adjusted point estimate confirms the OLS point estimate and suggests a 24% increase in the number of letters sent to the "*Frauen-Zeitung*".[38]

1.8.3 Organization of the movement

Next, we turn to studying the institutionalization of the German women's rights movement. To measure the institutionalization of networks in the second half of the nineteenth and the early twentieth century, we digitize novel data on local chapters of women's rights associations from the Imperial Statistical Office (Kaiserliches Statistisches Amt 1909). This source provides detailed establishment and membership data on more than 1,200 local chapters in 1909. The average local chapter in our dataset was established in 1898 and had approximately 1600 members. This source also allows us to differentiate between different types of associations – for example, female suffrage association and associations dedicated to improving women's educational opportunities.

We exploit this unique micro data in panels B and C of Table 1.7. Controlling for covariates in column (2), we find that an additional finishing school by 1850 increases

[38]We use the transformation $\log(y+1)$ in columns (3) and (4). Due to the sparsity of our outcome data, we refer to columns (1) and (2) for inference. We only record 242 letters from 40 cities, with five cities sending over half the letters.

Table 1.7: Long-term impact of finishing schools on the women's rights movement and political representation

	$\mathbb{I}[>0]$		log Number	
	(1)	(2)	(3)	(4)
Panel A: Leserbriefe, Frauenzeitung, 1849–1852				
Finishing schools	0.100***	0.122***	0.192***	0.241**
	(0.017)	(0.037)	(0.051)	(0.097)
R-squared	0.121	0.370	0.151	0.353
Mean, untreated	0.062	0.038	0.104	0.061
Bias-Adjusted β		0.132		0.266
Panel B: All women's rights organizations				
Finishing schools	0.150***	0.137***	1.419***	1.157***
	(0.027)	(0.050)	(0.179)	(0.306)
R-squared	0.101	0.362	0.211	0.483
Mean, untreated	0.367	0.275	444.355	155.802
Bias-Adjusted β		0.132		1.021
Panel C: Women's rights organizations to promote equal access to education				
Finishing schools	0.128***	0.074**	0.779***	0.496**
	(0.017)	(0.036)	(0.112)	(0.217)
R-squared	0.165	0.399	0.198	0.426
Mean, untreated	0.046	0.038	12.973	13.023
Bias-Adjusted β		0.046		0.337
Panel D: Member Parliament, 1919–1933				
Finishing schools	0.103***	0.101***	0.133***	0.105***
	(0.017)	(0.034)	(0.027)	(0.035)
R-squared	0.107	0.418	0.195	0.472
Mean, untreated	0.066	0.038	0.073	0.053
Bias-Adjusted β		0.100		0.091
Panel E: Member Parliament, 1949–2019				
Finishing schools	0.099***	0.091*	0.312***	0.268***
	(0.020)	(0.047)	(0.036)	(0.071)
R-squared	0.048	0.282	0.203	0.402
Mean, untreated	0.556	0.527	1.170	1.031
Bias-Adjusted β		0.088		0.241
City Covariates		Yes		Yes
Religious covariates		Yes		Yes
Educational covariates		Yes		Yes
Observations	388	183	388	183
Bandwidth		10		10

Notes: Cross-sectional results using all observations in odd columns and with our sample limited to 10 km within the inner-German denominational divide in 1618 in even columns reported. In each panel, we employ two types of outcomes: (i) an indicator variable taking value 1 if at least one letter (Panel A), women's rights organization (Panels B and C), or female member of parliament (Panels D and E) was recorded for a given city; and (ii) the natural logarithm of these variables plus 1. Our explanatory variable is the number of finishing schools in a city by 1850. We include controls as defined in Table 1.2 and limit the sample to within 10km of Germany's denominational divide in 1618 to capture areas with stronger religious competition in even columns. Bias-Adjusted β follows the procedure laid out in Oster (2019) assuming $R^{max} = 1.3\tilde{R}$ and $\delta = 1$. Standard errors clustered by city reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

the likelihood that a city has any local women's rights association by 14 percentage points (Panel B), equivalent to a 50% increase over the mean in cities without finishing schools. In particular, associations dedicated to promoting equal access to education for women exhibited stronger public support: if cities had established finishing schools by 1850, the number of members in these organizations exceeded that in cities without schools by 50% (Panel C, column 4).[39]

1.8.4 Women's representation in parliament

Our results suggest that critical ideas took hold in cities with finishing schools, leading to more members in women's rights organizations than in cities without finishing schools. First, the increasing representation of women among the human capital elite (Table 1.2) contributed to the creation of networks between cities that attracted other notable women (Figure 1.7). Second, these women were up to three times more likely to disseminate their critical ideas using the first female-led newspaper, the "*Frauen-Zeitung*", as an outlet (Table 1.7, Panel A). Finally, they organized into women's rights groups (Table 1.7, Panel B) and jointly lobbied for the core demands of the women's rights movement: equal access to education and female suffrage.

Thus, by educating young women and teachers, finishing schools contributed to the formation of a human capital elite that ultimately succeeded in achieving suffrage in 1919. Once suffrage was achieved, this larger representation of women among the human capital elite should have translated into greater female political representation in parliaments.

We explore this hypothesis in panels D and E of Table 1.7. To measure political representation, we collect the place of birth of all female members of parliament in the Weimar Republic (1919–1933, Panel D) and the Federal Republic of Germany (1949–2019, Panel E).[40] We report positive and significant coefficients when regressing an indicator for and the number of female politicians in all parliamentary elections since 1919 on the number of finishing schools in 1850.[41]

While during the Weimar Republic, only 4% of cities without finishing schools sent women to parliament, this figure rose to 53% in the Federal Republic of Germany

[39] In Appendix 1.I, we directly correlate the number of non-noble secular women in 1850 with political activity at the turn of the century: a 10% increase in the number of notable women increases political activity by 15%.

[40] Germany uses a list-based electoral system in which voters voted for the list of a party. Thus, women's representation on this list is more likely driven by the woman's preference to be nominated, than by her electorate's preference, as it would be in a system where voters directly choose their representative.

[41] The findings are robust to estimating the impact in every period separately or jointly. The findings are not driven by large cities as the top 5 cities with the most finishing schools are Munich, Berlin, Obertaunuskreis, Landshut, and Dresden. Estimates increase without the largest 10 percent of the sample in 1600.

(Panel D, column 2). In contrast, cities with historical finishing schools were 10 percentage points more likely to have sent women to parliament, equivalent to a 250% increase during the Weimar Republic and a 25% increase during the Federal Republic. Panel D and E thus highlight cities' historical advantage as "early movers" towards a more gender-equal society, gained by the establishment of finishing schools more than 300 years earlier.[42]

1.9 Conclusion

We set out to determine conditions for the emergence and success of social movements using the example of the women's rights movement in Germany. Following the literature on social movements (Markoff 2015; Tilly et al. 2020) and the history of successful movements (Dr. Martin Luther King Jr for the civil rights movement or Susan B. Anthony for the suffrage movement) we identify three key milestones. First, future leaders are educated and develop critical ideas. Second, these leaders disseminate their ideas using available mass media. Third, leaders institutionalize their movement as their ideas take root in society.

We study the importance of one form of educational institution at these three milestones, using the example of the arrival of finishing schools and the women's rights movement in Germany. In this setting, newly collected panel and cross-sectional data allow us to draw out the effect of education on the success of social movements at every step of their development. First, after cities established finishing schools, women started to represent a larger share of the political, intellectual, and economic elite ("human capital elite"), forming an activist nucleus of the women's rights movement. Second, women born in such cities also sent a disproportionate share of editorial letters to female-led newspapers, important platforms for early women's rights activism. Third, cities with historical finishing schools hosted more and larger women's rights organizations, key forces in the advancement of women's empowerment.

Using a wide range of empirical specifications our paper highlights the role of education in contributing to the emergence and success of the German women's rights movement. Further, our empirical results suggest that a world without educational institutions but significant economic and cultural changes would not see the level or pace of social change we observe throughout history.

Taken together, our findings indicate that educational institutions, which foster the exchange of critical ideas and provide the space to form networks, can function as important catalysts for the formation of a human capital elite critically engaging with

[42]We explore this 'early movers' hypothesis in more detail in Appendix Table 1.H.2. Here, a city with 50 more years of exposure to finishing schools would imply 14% more letters, twice the number of women's rights organizations and 23% more women in parliament today.

its status quo. Yet, education does not only benefit those receiving it; to the contrary, societies as a whole can benefit when committed activists fight for and bring about social change.

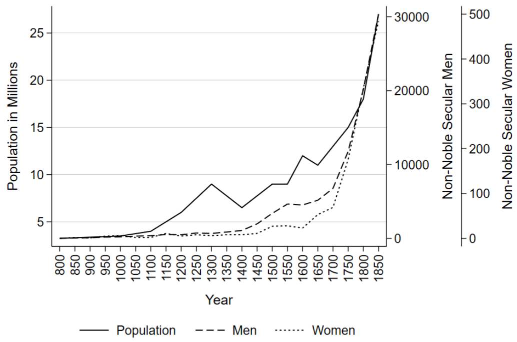
APPENDICES

1.A Record Keeping in the *Neue Deutsche Biographie (NDB)*

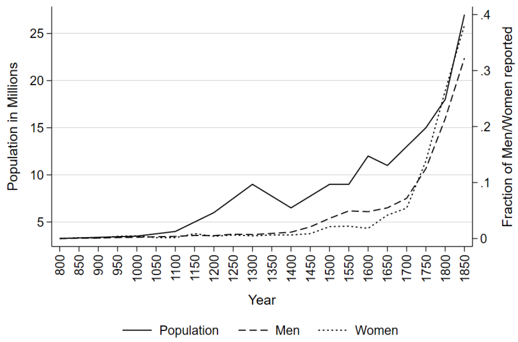
Our main results show an increase in the representation of women among the human capital elite – as measured by notable women recorded in the NDB – following the establishment of finishing schools. In this Appendix we explore whether this increased representation of women is driven by changes in reporting. If women's inclusion in the NDB increased disproportionately over time, estimates of the impact of finishing schools might be confounded by the general effect of time. In Figure 1.A.1, we provide direct evidence against this concern: the recording of notable women and men in the NDB followed the same time trend, which is, moreover, in line with general population growth. This motivates our use of the *share* of notable women among all notable women and men as dependent variable and our interpretation of the data in the main text.

In Figure 1.A.1, we compare the trends of total population in Germany based on McEvedy and R. Jones (1978) to the trends in the number of men and women recorded in the NDB. While the levels are different, all time series follow the same trend over time suggesting no change in reporting that could affect our data. The right panel in Figure in 1.A.1 shows that also the fraction of non-noble secular women among all women in our data increased similar to the increase among notable men: women's non-noble secular shares went up from 10% to 80% with the men's increase being 35% to 90%. Again, the pattern closely follows population, so that calculating the share of women born in each city and period, relative to all notable women and men in that city and period, provides a good measure of the human capital elite as it explicitly controls for trends.

A related concern is differential reporting between cities with and cities without finishing schools in the NDB. Specifically, finishing schools may have improved record keeping on notable women rather than increased women's share among the human capital elite. We offer two arguments against this interpretation: first, as shown in Figure 1.2 in the main body of Chapter 1, we find no impact of finishing schools on notable women from the nobility; if finishing schools merely improved record keeping on notable women, one might reasonably expect this to manifest also in an increased representation of women from the nobility. Second, if finishing schools merely im-



(a) Women and men in our data.



(b) Share of non-noble secular women and men.

Figure 1.A.1: Number of women and men in the NDB relative to total population

Notes: The left panel depicts the population of Germany in its modern boundaries (solid line), the number of notable men (right axis, dashed line) and the number of notable women born in each period (right axis, dotted line). All lines follow the same trend, suggesting that our estimated impacts are not driven by a change in reporting. The right panel again depicts the population of Germany in its modern boundaries as well as the share of all *non-noble secular* women (men) among all notable women (men) born in each period. This indicates that also in the subcategory of *non-noble secular* individuals the NDB exhibits no differential time trends in reporting between women and men.

proved record keeping in the NDB, this ought to show up in differential pre-trends, as a purported record-keeping effect would presumably also extend to the women who contributed to the founding of finishing schools. However, as shown in Figure 1.2 and as emphasized in Appendix 1.E we find strong evidence against differential pre-trends.

1.B Alternative Empirical Specifications and Economic Growth

We continue by documenting the robustness of our results presented in Table 1.2 in the main body of Chapter 1. To this end, we start by the most basic two-way fixed effect design, only including period and city fixed effects in column (1) of Table 1.B.1. In the four subsequent columns we individually add and remove a city-specific trend as well as city, educational, and religious covariates. As expected, the largest drop originates from city covariates, and specifically controlling for population. These covariates are responsible for almost the entire difference between the baseline and full specifications. This effect is largely an extensive margin effect, as when we drop all cities without population figures in 1600, we do not observe a change in the point estimates. The city-specific trend, while changing the point estimate significantly between columns (1) and (2), does not affect the point estimates when already controlling for covariates (columns (6) vs (7)). We thus conclude that our estimates do not rely on the inclusion of city-specific trends or a specific specification.

In a final step, we try to identify pairs of cities that only differ in the presence of finishing schools. Instead of classical matching procedures, which are usually done in cross-sectional settings, we employ increasingly parsimonious fixed effects to create smaller and smaller "cells" for cities in Table 1.B.2. We start with the full-specification including city-specific trends and all covariates interacted with period fixed effects. In column (2), we include fixed effects grouping cities into 3,244 cells according to their similarity regarding population, membership in the Hanseatic League, occurrence of anti-Jewish pogroms and religious battles within a given period. In columns (3) and (4) we slowly add similar cells for religious and educational covariates, before exactly matching on educational and economic covariates resulting in 6,580 different cells for cities to fall into. The results remain robust throughout the entire set of specifications.

Table 1.B.1: Fixed-effects results on the importance of finishing schools - Sensitivity to covariates

	Baseline	with trends	with covariates				Full
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Non-Noble Seculars, $\mathbb{I}[Women > 0]$							
Finishing school$_{it}$	0.300***	0.230***	0.177***	0.298***	0.274***	0.181***	0.164***
	(0.029)	(0.029)	(0.030)	(0.030)	(0.031)	(0.032)	(0.033)
Panel B: Non-Noble Seculars, log Women							
Finishing school$_{it}$	0.464***	0.355***	0.235***	0.460***	0.423***	0.246***	0.204***
	(0.063)	(0.053)	(0.046)	(0.063)	(0.063)	(0.048)	(0.045)
Panel C: Non-Noble Seculars, Share Women							
Finishing school$_{it}$	0.022***	0.019***	0.019***	0.022***	0.023***	0.021***	0.021***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Panel D: Unmarried women, $\mathbb{I}[Women > 0]$							
Finishing school$_{it}$	0.276***	0.194***	0.167***	0.272***	0.260***	0.173***	0.147***
	(0.029)	(0.030)	(0.031)	(0.029)	(0.031)	(0.032)	(0.034)
Panel E: Unmarried women, log Women							
Finishing school$_{it}$	0.422***	0.302***	0.215***	0.415***	0.388***	0.226***	0.173***
	(0.060)	(0.049)	(0.045)	(0.061)	(0.060)	(0.047)	(0.043)
Panel F: Unmarried women, Share Women							
Finishing school$_{it}$	0.015***	0.011**	0.015***	0.015***	0.017***	0.016***	0.014**
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.006)
Panel G: Teachers & Writers, $\mathbb{I}[Women > 0]$							
Finishing school$_{it}$	0.196***	0.151***	0.111***	0.196***	0.176***	0.117***	0.104***
	(0.026)	(0.027)	(0.024)	(0.026)	(0.026)	(0.024)	(0.026)
Panel H: Teachers & Writers, log Women							
Finishing school$_{it}$	0.220***	0.174***	0.116***	0.220***	0.194***	0.120***	0.103***
	(0.037)	(0.034)	(0.027)	(0.037)	(0.035)	(0.028)	(0.029)
Panel I: Teachers & Writers, Share Women							
Finishing school$_{it}$	0.024***	0.019***	0.018***	0.024***	0.023***	0.019***	0.017***
	(0.005)	(0.006)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)
Panel J: Activists, $\mathbb{I}[Women > 0]$							
Finishing school$_{it}$	0.077***	0.076***	0.044***	0.077***	0.078***	0.051***	0.053***
	(0.017)	(0.018)	(0.016)	(0.017)	(0.017)	(0.017)	(0.018)
Panel K: Activists, log Women							
Finishing school$_{it}$	0.066***	0.064***	0.038***	0.067***	0.066***	0.043***	0.043***
	(0.017)	(0.017)	(0.014)	(0.018)	(0.017)	(0.015)	(0.015)
Panel L: Activists, Share Women							
Finishing school$_{it}$	0.011***	0.013***	0.008**	0.011***	0.012***	0.009**	0.011**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
Unit trend		Yes					Yes
City covariates × period FE			Yes			Yes	Yes
Educational covariates × period FE				Yes		Yes	Yes
Religious covariates × period FE					Yes	Yes	Yes
Observations	9,312	9,312	9,312	9,288	9,264	9,240	9,240

Notes: Results from fixed-effects regressions using all cities and periods reported. We consider three types of dependent variables for several categories of notable women: (i) $\mathbb{I}[Women > 0]$ is an indicator taking value 1 if a city observed the birth of at least one notable woman in a given period; (ii) "'log Women"' constitutes the natural logarithm of the number of women born plus one; (iii) "'Share Women"' divides the number of women by the number of men and women in the same category, except for "Activists", where we use the number of male politicians instead. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. Column (1) denotes the absolute baseline, only including time and city fixed effects. Column (2) adds linear time trends to ascertain their impact on the point estimate. In columns (3)-(6), we add our full set of controls interacted with period fixed effects, first individually then jointly, without the linear time trends. In column (7), we then add linear time trends to show that linear time-trends do not impact the precision of our estimates. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 1.B.2: Fixed-effects results on the importance of finishing schools - Exactly matching on covariates in 1600

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: $\mathbb{I}[Women > 0]$						
Finishing school$_{it}$	0.164***	0.203***	0.214***	0.159***	0.164***	0.171***
	(0.033)	(0.038)	(0.040)	(0.045)	(0.050)	(0.047)
Panel B: log Women						
Finishing school$_{it}$	0.204***	0.224***	0.238***	0.163***	0.167***	0.175***
	(0.045)	(0.047)	(0.050)	(0.055)	(0.059)	(0.058)
Panel C: Share Women						
Finishing school$_{it}$	0.021***	0.021***	0.021***	0.014**	0.015*	0.015**
	(0.005)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Exact match on economic covariates		Yes	Yes	Yes	Yes	Yes
Exact match on religious covariates			Yes	Yes	Yes	Yes
Exact match on educational covariates				Yes	Yes	Yes
Exact match on educational and economic covariates					Yes	Yes
Exact match on educational and religious covariates						Yes
Observations	9,312	9,312	9,312	9,312	9,312	9,312
Number of Fixed Effects	1,300	3,244	3,484	5,284	6,580	5,956

Notes: Results from fixed-effects regressions reported. We consider three types of dependent dependent variables: (i) $\mathbb{I}[Women > 0]$ is an indicator taking value 1 if a city observed the birth of at least one notable woman in a given period; (ii) "'log Women"' constitutes the natural logarithm of the number of women born plus 1; (iii) "'Share Women"' divides the number of women by the number of men and women in the same category, except for "Activists", where we use the number of male politicians instead. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. Columns (1), (3), and (5) are derived from our baseline specification and include city and period fixed effects as well as city specific linear trends. In columns (2), (4), and (6) we interact our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

1.B.1 Sensitivity to Dropping Observations

In a recent paper, Broderick et al. (2020) stressed the importance of assessing the validity of results by analyzing their robustness to outliers. We implement this robustness test as follows: we drop entire sets of cities belonging to one ruling house rather than dropping individual cities (1 out of 388). With this procedure, we drop on average 18 cities, with the two largest sets of cities being ruled by the Catholic clergy (114) and the House of Hohenzollern (52). Since these two sets of cities also capture the distinction between Catholic and Protestant cities almost perfectly, the results of this analysis also document that our findings are not driven by cities from either denomination alone.

In Figure 1.B.1 and 1.B.2, we present all outcomes (in rows) in all specifications (columns) corresponding to Tables 1.2 and 1.7. The x-axis measures the ratio between a restricted estimate when a set of cities is dropped and the original estimate from the corresponding table. If the restricted estimate remains unchanged, this ratio is one. It is 1.5 if the restricted estimate is 50% larger than the original, and 0.5 if the restricted

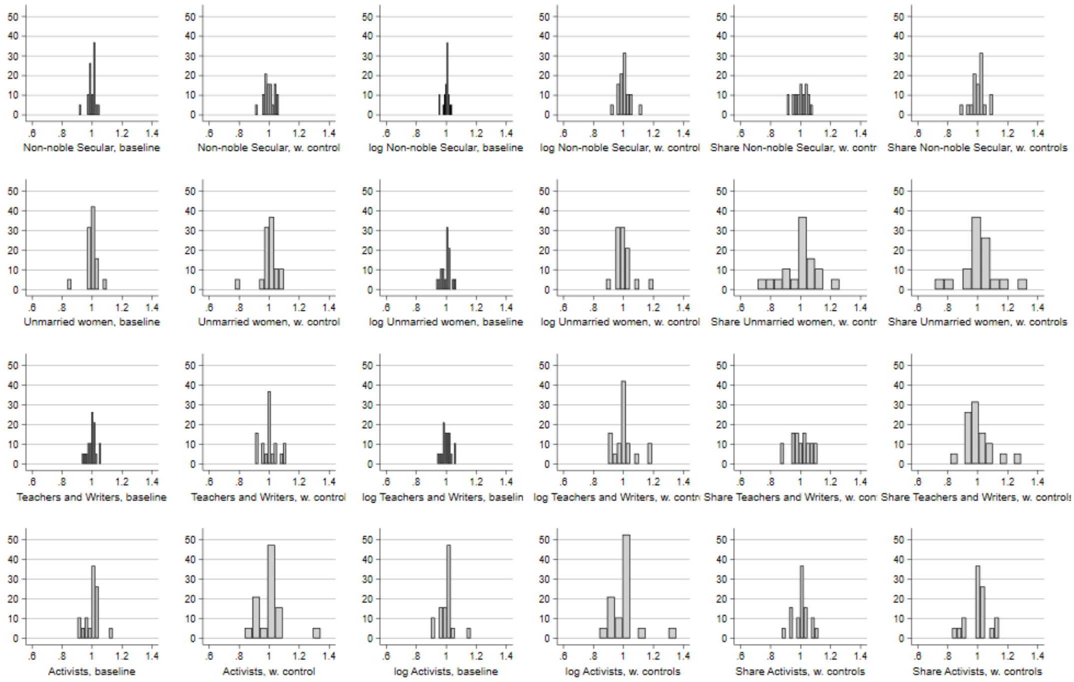


Figure 1.B.1: Sensitivity to dropping sets of cities: Panel outcomes

Notes: In each figure, the x-axis depicts the ratio between the restricted point estimate when dropping one of 22 sets of cities and the corresponding original estimate in Table 1.2. This ratio is one, if the restricted estimate is unchanged, 1.5 if the restricted estimate is 50% larger than the original, and 0.5 if the restricted estimate is 50% smaller than the original. We present all outcomes (in rows) in all specifications (in columns) corresponding to Table 1.2. In each figure, the bars add up to 100%.

estimate is 50% smaller than the original. We do this for 22 sets of cities belonging to different rulers and find a minimum of 0.7 (for the share of unmarried women) and a maximum of 1.3 (for the log number of activists) in the panel setting. These figures suggest that our panel estimates are highly robust to potential outliers as they only vary within 30% of the original effect size. The corresponding numbers for the cross-sectional regressions are 0.7 (for the log number of educational women's rights associations, with controls) and 1.6 (for the members of parliament 1949-2017, with controls). Overall, the density plots reveal a stable pattern around the estimated mean, suggesting that our results are not driven by individual cities or sample selection.

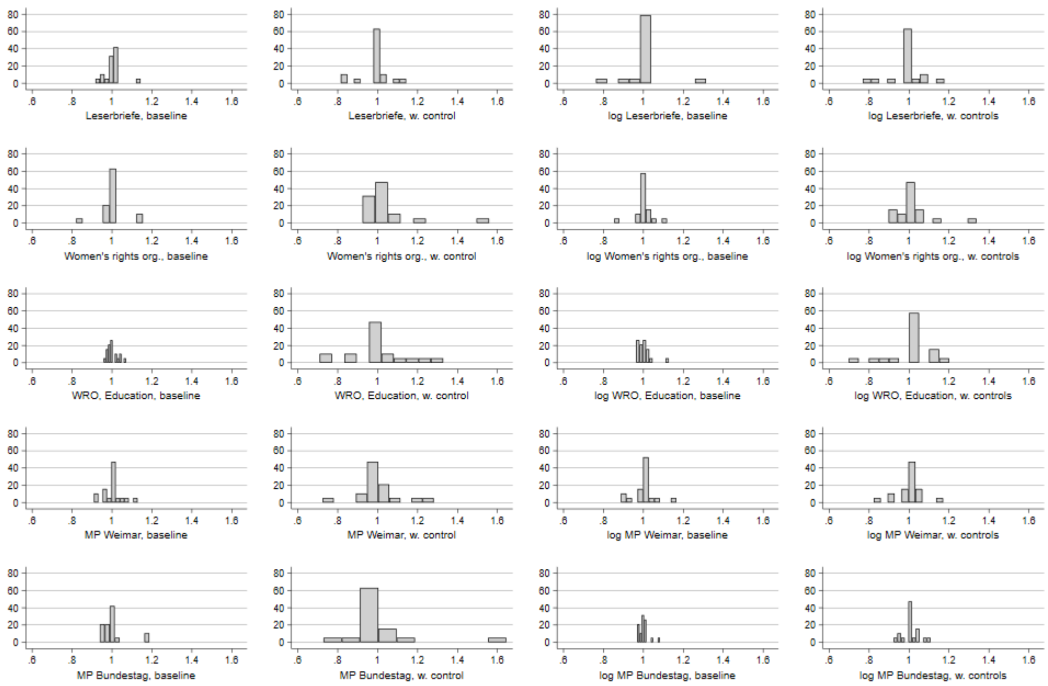


Figure 1.B.2: Sensitivity to dropping sets of cities: Long-run outcomes

Notes: In each figure, the x-axis measures the ratio between the restricted point estimate when dropping one of 22 sets of cities and the corresponding original estimate in Table 1.7. This ratio is one, if the restricted estimate is unchanged, 1.5 if the restricted estimate is 50% larger than the original, and 0.5 if the restricted estimate is 50% smaller than the original. We present all outcomes (in rows) in all specifications (in columns) corresponding to Table 1.7. In each figure, the bars add up to 100%. “WRO” in the third row denotes “women’s rights organisation”; “MP” refers to a “member of parliament”.

1.B.2 The role of economic growth: flexibly controlling for construction

Finally, we address the possibility that our city covariates do not adequately capture economic growth by including construction data from Cantoni, Dittmar, et al. (2018). Neither using the construction activity in 1650 (prior to the establishment of the first finishing school), nor the potentially endogeneous time-varying construction activity data change the point estimates significantly, as shown in Table 1.B.3. We thus conclude our identification is robust to including or excluding different sets cities, city-specific trends, or economic activity.

Table 1.B.3: Fixed-effects results on the importance of finishing schools - Controlling for construction activity

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.161***	0.169***	0.208***	0.214***	0.020***	0.021***
	(0.034)	(0.034)	(0.045)	(0.045)	(0.006)	(0.005)
Mean, untreated	0.148	0.147	0.138	0.137	0.018	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.150***	0.149***	0.182***	0.183***	0.014**	0.014**
	(0.035)	(0.035)	(0.042)	(0.042)	(0.006)	(0.006)
Mean, untreated	0.152	0.152	0.142	0.141	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.104***	0.109***	0.106***	0.112***	0.018***	0.019***
	(0.027)	(0.027)	(0.027)	(0.028)	(0.006)	(0.006)
Mean, untreated	0.075	0.075	0.059	0.059	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.065***	0.053***	0.049***	0.044***	0.015***	0.012**
	(0.018)	(0.018)	(0.015)	(0.016)	(0.005)	(0.005)
Mean, untreated	0.016	0.016	0.012	0.012	0.005	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.017	-0.015	-0.007	-0.006	-0.002	-0.002
	(0.017)	(0.016)	(0.017)	(0.015)	(0.009)	(0.008)
Mean, untreated	0.038	0.037	0.030	0.030	0.017	0.017
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Construction in 1650 × period FE	Yes		Yes		Yes	
Construction in every period × period FE		Yes		Yes		Yes
Observations	9,096	9,144	9,096	9,144	9,096	9,144

Notes: Results from fixed-effects regressions reported. We consider three types of dependent variables: (i) $\mathbb{I}[Women > 0]$ is an indicator taking value 1 if a city observed the birth of at least one notable woman in a given period; (ii) "'log Women"' constitutes the natural logarithm of the number of women born plus 1; (iii) "'Share Women"' divides the number of women by the number of men and women in the same category, except for "Activists", where we use the number of male politicians instead. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. All columns control for city and period fixed effects as well as city-specific linear trends in addition to interacting our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

1.C Dataset Construction Choices and Timing of School Establishment

In this appendix, we discuss the construction of the Thiessen Polygons around each city that existed in 1300 A.D. as taken from from Voigtländer and Voth (2012) and show that the results are robust to only using cities that existed in 800 A.D. (Appendix 1.C.1). As the cities in Voigtländer and Voth (2012) might have oversampled Jewish cities, we instead use the territories and rulers in 1618 as our baseline and reproduce the main findings of the paper and conclude that neither dataset construction nor sample selection introduced a bias in our estimates (Appendix 1.C.2). We then highlight the impact of different school establishment periods (Appendix 1.C.3).

1.C.1 Structure of the data

We take the city-level data by Voigtländer and Voth (2012) as a starting point and construct Thiessen Polygons around the center of each city in their dataset. Thiessen Polygons are constructed such that every village or town inside the polygon around city i is closer to city i than to any other city $j \neq i$. Figure 1.C.1 shows the resulting polygons alongside the location of finishing schools and the number of notable women born within each area. By construction, the city lies in the center of its polygon.

We use this data structure and the set of cities used by Voigtländer and Voth (2012) to include their rich city-level covariates and to avoid relying on county boundaries. From the entire set of cities in Voigtländer and Voth (2012), we only select those cities that are mentioned before 1300 and are the oldest town within a county. For example: Aachen has four recorded 'cities' in Voigtländer and Voth (2012): town_id 1, mentioned in 830, 13.45 km from Aachen; town_id 3, mentioned in 1118, 10.74 km from Aachen; town_id 4, mentioned in 870, 5.12 km from Aachen; and Aachen itself (town_id 5, mentioned in 400). Since these other cities are likely suburbs or dependent on Aachen's existence, we use the location of Aachen and merge all variables to Aachen. This has the advantage that our estimates are not biased by a potential rural-urban bias when including suburbs. We arrive at 388 cities by only using the oldest city within each Landkreis (town_id 5) that lies in present-day Germany.

As the NDB starts recording notable individuals born from the year 800 onwards, using cities with recorded population levels by 800 is a natural alternative, which, however, reduces the sample of cities to 101. In Table 1.C.1 we document that results for both choices (1300 vs. 800) are similar across all specifications and outcomes.

The next choice concerns the length of periods. We choose to assign notable individuals to 50-year periods based on their year of birth. There are two reasons for our 50-year period choice: First, by choosing 50-years, we ensure that on average a woman that is born in this period either did or did not have access to a finishing school. Second, the scarce number of women recorded in the NDB prior to the 15th century implies a trade-off between statistical power and assignment accuracy. If we used every birth year separately, and thus matched schools most precisely, we would end up with no variation within most city x birth-year cells. Thus, to increase power, we rely on 50-year periods, and show robustness to using 25 year intervals in Table 1.C.2. Again, our point estimates remain unaffected.

The final choice concerns the classification of notable women into different (occupational) groups: *Non-Noble Seculars, Unmarried, Teachers & Writers, Activists, and the Nobility*. We grouped women together to ensure enough variation within every city-period-occupation-cell. In Table 1.C.3, we show the consistent impact across most occupational groups. In addition to our baseline results, we show that finishing schools increase the share of unmarried women (Panel A), artists (Panel D), writers (Panel E), politicians (Panel G), academics (H), but not the share of nuns (Panel J). This evidence, especially the impact on academics, artists, and writers, reinforces the notion that finishing schools increased the share of women among the human capital elite.

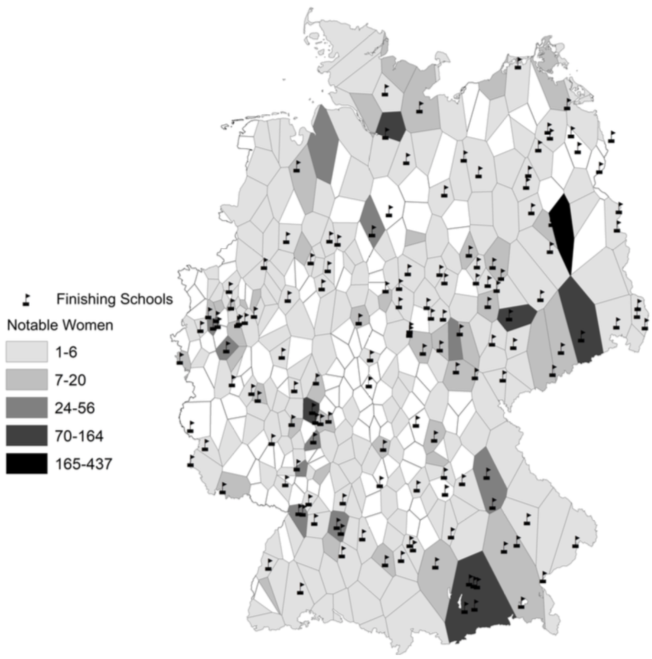


Figure 1.C.1: Thiessen Polygons, finishing schools and notable women

Notes: This figure shows our unit of observation, Thiessen polygons created around cities included in the data by Voigtländer and Voth (2012). By construction, the cities lie in the center of each Thiessen polygon. For simplicity we continue to refer to our unit of observation as "city". The figure also shows the location of finishing schools as well as the number of notable women born in each city.

Table 1.C.1: Fixed-effects results on the importance of finishing schools - Changing the Unit of observation to cities that existed in 800

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.251***	0.230***	0.465***	0.356***	0.016***	0.017*
	(0.049)	(0.064)	(0.098)	(0.100)	(0.005)	(0.009)
Mean, untreated	0.201	0.180	0.214	0.189	0.020	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.134***	0.142**	0.356***	0.272***	0.007	0.010
	(0.048)	(0.067)	(0.086)	(0.097)	(0.007)	(0.009)
Mean, untreated	0.242	0.226	0.252	0.227	0.024	0.023
Panel B: Teachers & Writers						
Finishing school$_{it}$	0.183***	0.154**	0.257***	0.179**	0.019**	0.016
	(0.048)	(0.062)	(0.067)	(0.072)	(0.008)	(0.011)
Mean, untreated	0.103	0.090	0.091	0.076	0.019	0.016
Panel C: Activists						
Finishing school$_{it}$	0.104***	0.077*	0.100***	0.058	0.016**	0.016*
	(0.032)	(0.046)	(0.031)	(0.039)	(0.006)	(0.009)
Mean, untreated	0.029	0.026	0.023	0.020	0.005	0.005
Panel D: Nobility						
Finishing school$_{it}$	-0.018	-0.056	-0.001	-0.036	0.002	-0.033
	(0.037)	(0.044)	(0.039)	(0.043)	(0.019)	(0.023)
Mean, untreated	0.105	0.098	0.092	0.083	0.045	0.041
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	2,424	2,232	2,424	2,232	2,424	2,232

Notes: Results from fixed-effects regressions reported. Instead of building our dataset from cities that existed by1300, we now consider all cities that exist in 800, resulting in a drop in the number of cities from 388 to 101. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 1.C.2: Fixed-effects results on the importance of finishing schools - Changing the Unit of observation to 25 year intervalls

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.149***	0.096***	0.212***	0.110***	0.016***	0.014***
	(0.021)	(0.021)	(0.038)	(0.030)	(0.003)	(0.004)
Mean, untreated	0.094	0.093	0.142	0.142	0.015	0.015
Panel B: Unmarried women						
Finishing school$_{it}$	0.124***	0.088***	0.182***	0.097***	0.011***	0.012***
	(0.020)	(0.022)	(0.034)	(0.028)	(0.003)	(0.004)
Mean, untreated	0.097	0.097	0.143	0.142	0.017	0.017
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.088***	0.059***	0.096***	0.057***	0.015***	0.013***
	(0.017)	(0.016)	(0.022)	(0.019)	(0.004)	(0.004)
Mean, untreated	0.044	0.043	0.050	0.050	0.013	0.013
Panel D: Activists						
Finishing school$_{it}$	0.042***	0.030***	0.034***	0.023***	0.008***	0.007**
	(0.011)	(0.011)	(0.010)	(0.009)	(0.003)	(0.003)
Mean, untreated	0.009	0.009	0.009	0.009	0.003	0.003
Panel E: Royals						
Finishing school$_{it}$	-0.014*	-0.009	-0.007	-0.005	-0.003	-0.002
	(0.008)	(0.009)	(0.008)	(0.010)	(0.004)	(0.005)
Mean, untreated	0.021	0.021	0.025	0.025	0.010	0.010
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	18,624	18,480	18,624	18,480	18,624	18,480

Notes: Results from fixed-effects regressions reported. Instead of 50-year periods, we now employ 25-year periods instead. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact our full set control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

Table 1.C.3: Fixed-effects results on the importance of finishing schools - All occupations

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Unmarried women						
Finishing school$_{it}$	0.194***	0.147***	0.302***	0.173***	0.011**	0.014**
	(0.030)	(0.034)	(0.049)	(0.043)	(0.005)	(0.006)
Mean, untreated	0.155	0.153	0.275	0.274	0.022	0.022
Panel B: Non-Royal women						
Finishing school$_{it}$	0.224***	0.164***	0.350***	0.201***	0.018***	0.018***
	(0.030)	(0.034)	(0.053)	(0.045)	(0.004)	(0.005)
Mean, untreated	0.156	0.154	0.285	0.284	0.018	0.018
Panel C: Occupation						
Finishing school$_{it}$	0.055***	0.025	0.058***	0.021	0.004	0.004
	(0.017)	(0.018)	(0.018)	(0.016)	(0.003)	(0.004)
Mean, untreated	0.019	0.019	0.020	0.020	0.004	0.004
Panel D: Artists						
Finishing school$_{it}$	0.137***	0.062**	0.187***	0.071**	0.027***	0.016**
	(0.027)	(0.028)	(0.043)	(0.033)	(0.007)	(0.007)
Mean, untreated	0.056	0.056	0.085	0.085	0.013	0.013
Panel E: Writers						
Finishing school$_{it}$	0.147***	0.099***	0.159***	0.096***	0.023***	0.020***
	(0.025)	(0.025)	(0.032)	(0.027)	(0.006)	(0.006)
Mean, untreated	0.067	0.067	0.084	0.083	0.020	0.020
Panel F: Doctors						
Finishing school$_{it}$	0.021*	-0.003	0.020**	-0.003	0.003	-0.000
	(0.011)	(0.011)	(0.009)	(0.009)	(0.003)	(0.003)
Mean, untreated	0.009	0.009	0.009	0.009	0.003	0.003
Panel G: Politicians						
Finishing school$_{it}$	0.058***	0.025	0.054***	0.018	0.011**	0.007
	(0.017)	(0.018)	(0.016)	(0.015)	(0.004)	(0.005)
Mean, untreated	0.018	0.018	0.020	0.020	0.005	0.005
Panel H: Academics						
Finishing school$_{it}$	0.080***	0.056***	0.069***	0.037***	0.009***	0.009**
	(0.015)	(0.016)	(0.014)	(0.014)	(0.003)	(0.004)
Mean, untreated	0.014	0.014	0.016	0.016	0.003	0.003
Panel I: Teachers						
Finishing school$_{it}$	0.041***	0.018	0.036***	0.014	0.006*	0.005
	(0.014)	(0.012)	(0.012)	(0.010)	(0.003)	(0.003)
Mean, untreated	0.011	0.011	0.012	0.012	0.003	0.003
Panel J: Nunns						
Finishing school$_{it}$	0.001	0.002	0.001	0.000	-0.002	-0.000
	(0.011)	(0.013)	(0.008)	(0.009)	(0.003)	(0.004)
Mean, untreated	0.012	0.012	0.012	0.013	0.004	0.004
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	9,312	9,240	9,312	9,240	9,312	9,240

Notes: Results from fixed-effects regressions using all cities and periods reported. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact our full set control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

1.C.2 Sample selection: Using a different starting point for the analysis

In our baseline data, we created a balanced panel for each city in Voigtländer and Voth (2012) using Thiessen Polygons as a starting point (Figure 1.C.1). This procedure has the advantage that it does not rely on any administrative boundary, past or present, and any covariate from Voigtländer and Voth (2012) can easily be used. However, as the original focus of this paper was on Jewish pogroms, the original data might have oversampled cities with black death and pogroms, we show robustness to using an alternative baseline source to create a balanced panel: the territories of Germany in 1619.

In Figure 1.C.2, we depict the territories of 21 different rulers, 91 ecclesiastical cities, 96 free cities and 57 imperial cities in Germany on the eve of the Thirty Years' war. We then use these administrative boundaries to create a balanced panel from 800 until 1950. The implicit assumption here is that people migrate disporporitonately within a rulers territory and only rarely migrate between competing territories. We avoid this assumption using the Voigtländer and Voth (2012) cities in combination with Thiessen polygons.

The event-study results in Figure 1.C.3 and the fixed effects results in Table 1.C.4, however, confirm our initial results. We conclude that choosing the cities from Voigtländer and Voth (2012) to create Thiessen polygons did not introduce a bias into our setting.

1.C.3 Using the exact opening time of finishing schools

In our baseline data, we created a balanced panel for each city to include never-treated cities and covariates. This decision is in line with the recent literature on event-study validity, as discussed in Appendix 1.E. In the resulting panel, we merged individuals to the closest of 50-year periods in cities. That is, if an individual is born in 1640, we merge her to the City's 1650 period, regardless of treatment status. In that setting, we have cities that switch into treatment, as well as pure-control cities in every period and can compare the three groups.

However, an event-study usually uses the exact timing to estimate the treatment effect. Ignoring never-treated cities, our data allows for such a fine-grained distinction. In this Appendix, we normalize the time period for every city to zero at the exact time the first school was opened. That is, if the first school opens in 1626 for the city of Aachen, we create city-specific period lags of arbitrary length. Yet, there are two problems associated with this: First, we are unable to merge control cities to this framework, and thus the comparison is strictly within treated cities only. Second, the choice of omitted period is not innocuous: Women that are born 10 years prior to the opening of a Finishing schools still benefit from its construction, while not having had any say in its establishing. We thus need to normalize at an earlier period at which

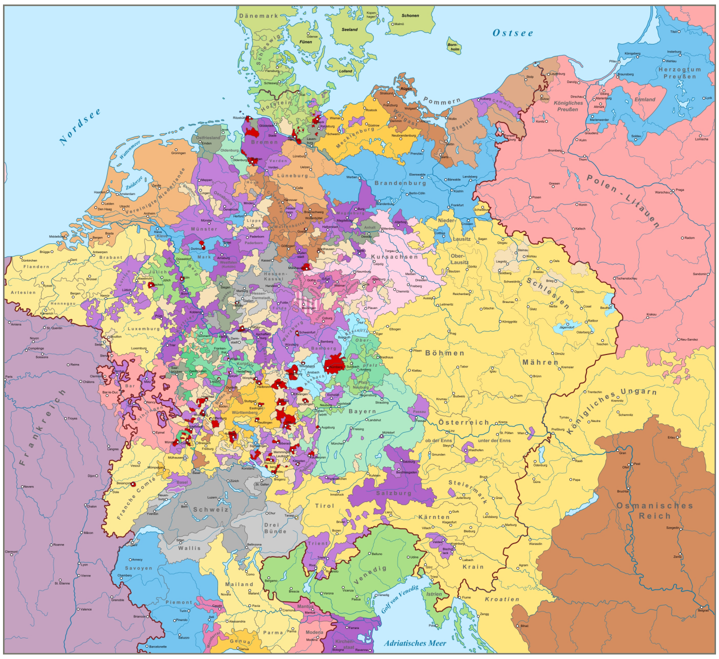
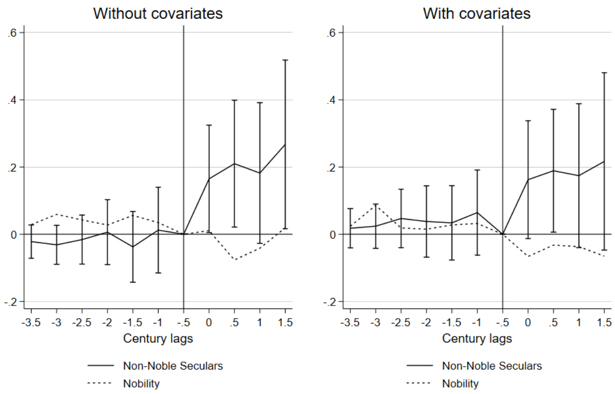
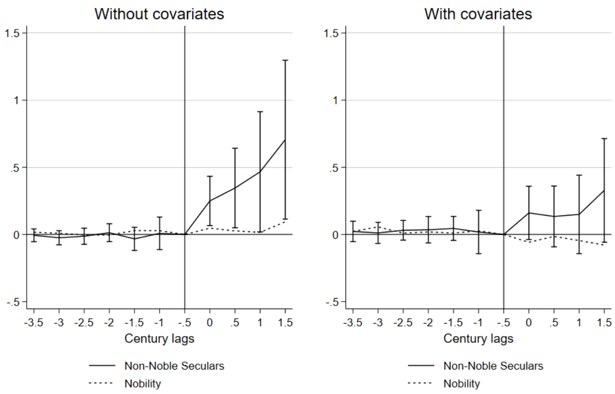


Figure 1.C.2: German territories and rulers in 1618

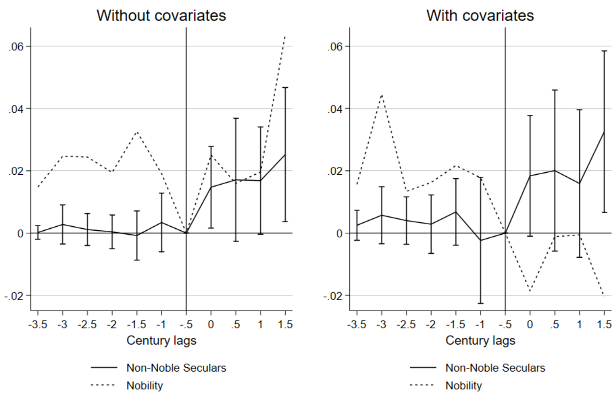
Notes: This figure shows the territories of rulers, ecclesiastical cities, free cities, and imperial cities in 1618, which we use as a baseline for the results in this section. License notice: Sir Iain. This W3C-unspecified vector image was created with Inkscape. (https://bit.ly/3LwzRos), https://bit.ly/3GQx06h.



(a) Indicator function: Notable woman born in city



(b) Log. number of notable women born in city



(c) Share of notable women born in city

Figure 1.C.3: Event-Study: Impact of finishing school establishment on notable women using territories as of 1619 as the unit of observation

Notes: Event study results for *non-noble secular* women and women from the *nobility*. In Figure a, the outcome is an indicator equal to one if a notable woman from the respective group was born in a given city and period. Figure b uses the natural logarithm of number of women born plus one. Figure c denotes the number of notable women by the number of notable individuals of all genders. Zero is the normalized time of opening of the first finishing schools in the city. The vertical line marks the reference period, which we choose to be 50 years prior to establishment of the school. City and period fixed effects included in the left figure and full economic, religious, and educational controls added in the right. 95%-confidence intervals shown only for non-noble secular, the impact on women from the nobility is indistinguishable from zero in all periods and specifications.

Table 1.C.4: Fixed-effects results on the importance of finishing schools - Changing the unit of observation to territories existing in 1618.

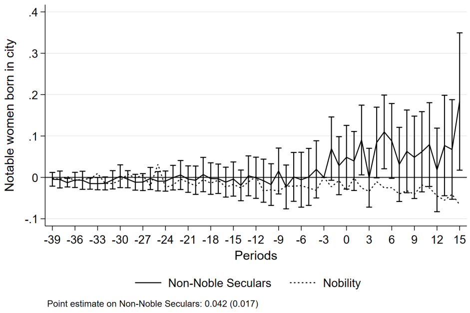
	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.376***	0.188***	0.728***	0.270***	0.024***	0.017***
	(0.039)	(0.046)	(0.096)	(0.065)	(0.004)	(0.006)
Mean, untreated	0.081	0.078	0.141	0.128	0.011	0.011
Panel B: Unmarried women						
Finishing school$_{it}$	0.283***	0.154***	0.603***	0.244***	0.017***	0.018***
	(0.040)	(0.045)	(0.081)	(0.070)	(0.005)	(0.007)
Mean, untreated	0.086	0.082	0.157	0.134	0.013	0.013
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.283***	0.097**	0.381***	0.105***	0.033***	0.018**
	(0.039)	(0.038)	(0.065)	(0.036)	(0.007)	(0.008)
Mean, untreated	0.037	0.035	0.057	0.048	0.008	0.008
Panel D: Activists						
Finishing school$_{it}$	0.151***	0.060**	0.146***	0.057**	0.021***	0.014**
	(0.029)	(0.028)	(0.032)	(0.023)	(0.005)	(0.007)
Mean, untreated	0.007	0.006	0.008	0.006	0.002	0.002
Panel E: Nobility						
Finishing school$_{it}$	-0.070**	-0.052*	-0.035	-0.044*	-0.009	-0.018
	(0.029)	(0.028)	(0.025)	(0.024)	(0.012)	(0.012)
Mean, untreated	0.029	0.025	0.043	0.031	0.012	0.011
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	6,360	6,216	6,360	6,216	6,360	6,216

Notes: Results from fixed-effects regressions reported. Instead of using the cities in Voigtländer and Voth (2012), we use the territories (as of 1618) shown in Figure 1.C.2 as the unit of observation. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

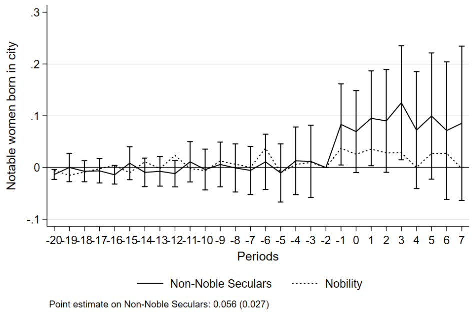
women could not have benefited from the future presence of finishing schools. While these considerations average out at 50-year intervals, they matter greatly at smaller intervals.

In Figure 1.C.4, we use the opening time of the first finishing school in our 129 cities with schools and create various lags around it. In all Panels, we aim to reference the estimates to a previous generation of women who could no longer benefit from education: parents. In Panel a), we create 10-year lag windows around each school and omit women born between 30 and 39 years prior to school opening. We omit women born between 20 and 39 years before in the 20-year Panel b), 25 and 50 years before in the 25-year Panel c) and 50-100 years before in Panel d). We find no evidence for a pre-trend in any specification, a significant uptick after the opening, and point estimates that are not statistically different from our baseline.

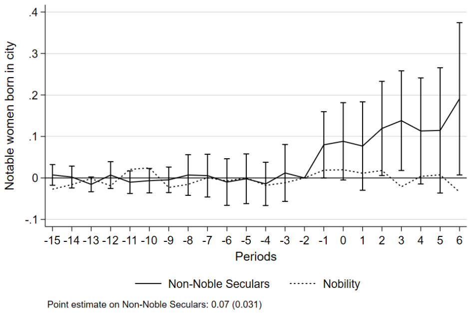
Yet, as we discuss in Appendix 1.E, the inclusion of never-treated cities allows for a clean comparison between treatment and control, as well as a classical difference-in-differences setup (Appendix 1.F). These benefits, along with the possibility to merge covariates and the unchanged point estimates, motivate our choice to match women and schools to a balanced panel of cities, instead of using this exact-timing setup.



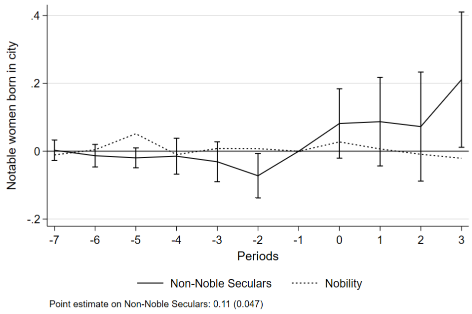
(a) 10 year periods



(b) 20 year periods



(c) 25 year periods



(d) 50 year periods

Figure 1.C.4: Event-Study: Impact of finishing school establishment on notable women

Notes: Results from event-studies reported. We leverage the exact timing of the first finishing school in every city to create 10-year periods (Panel a), 20-year periods (Panel b), 25-year periods (Panel c) and 50-year periods (Panel d). We include fifty-year period fixed effects in all regressions to uphold comparability across panel. Results are robust to using year-fixed effects that include $\geq$ 645 fixed effects for every year. As a result of the exact matching on years of birth, we observe a significant increase in period -1 in Panels a–c: If a woman was 10 years old when the first finishing school opened, she attended this school and became notable for her achievements, we assign her to the -10 years bin. Thus, this "lead" constitutes an artifact of the way the dataset is constructed and is unlikely to reflect anticipation effects. Confidence intervals derived from standard errors clustered at the city level reported.

1.C.4 Timing of school construction

When taking historical accounts at face value, the establishment of *early* finishing schools by foreign Catholic women's orders constituted a shift in the supply of women's education as opposed to a local shift in the demand for education.

In this Appendix, we assess the severity of a potential bias in our estimates that would arise if the establishment of the *later* finishing schools in our data were largely driven by increasing demand for women's education. If the *later* schools (constructed between 1800 and 1850, i.e. after the fall of the Holy-Roman-Empire) accounted for all the impact on women's representation among the human-capital elite, this would call into question our interpretation that the establishment of finishing schools constituted a supply-side shift. However, our results largely remain robust when only using schools constructed before 1800 in the odd columns of Table 1.C.5. In addition, the point estimates on *early* and *late* schools are not statistically different from each other in most specifications.

Moreover, in Table 1.C.6 we compare the impact of the first versus the second school constructed in a city and show that most of the impact indeed comes from the first established school. Combined with the impact of multiple schools shown in Figure 1.C.5, this suggests that indeed the first, arguably exogenous school opening, is responsible for the increase in the share of women among the human capital elite of German cities. This finding is confirmed in the difference-in-differences setting, where all periods produce similar estimates (Figure 1.F.2 and Table 1.F.2).

Table 1.C.5: Fixed-effects results on the importance of finishing schools - Early vs Late Schools

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1) Early	(2) Late	(3) Early	(4) Late	(5) Early	(6) Late
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.095**	0.185***	0.279***	0.246***	0.016**	0.023***
	(0.041)	(0.044)	(0.100)	(0.057)	(0.007)	(0.007)
Mean, untreated	0.147	0.148	0.137	0.138	0.019	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.050	0.180***	0.231**	0.217***	0.004	0.018***
	(0.046)	(0.044)	(0.092)	(0.053)	(0.010)	(0.007)
Mean, untreated	0.148	0.152	0.137	0.141	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.095**	0.129***	0.166**	0.124***	0.011	0.022***
	(0.041)	(0.032)	(0.081)	(0.032)	(0.008)	(0.007)
Mean, untreated	0.074	0.074	0.058	0.059	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.053*	0.066***	0.070	0.051***	0.004	0.014**
	(0.029)	(0.022)	(0.043)	(0.018)	(0.004)	(0.006)
Mean, untreated	0.018	0.016	0.013	0.012	0.006	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.022	-0.014	0.004	-0.012	-0.002	-0.003
	(0.039)	(0.019)	(0.035)	(0.016)	(0.018)	(0.009)
Mean, untreated	0.031	0.037	0.024	0.030	0.015	0.017
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,984	8,400	6,984	8,400	6,984	8,400

Notes: Results from fixed-effects regressions following our main specification reported, comparing effect sizes between *early* (1650–1750) and *late* (1800–1850) finishing schools. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. In all columns we interact our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 1.C.6: Fixed-effects results on the importance of finishing schools - Comparing the impact of the first to the second school

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
First finishing school$_{it}$	0.164***	0.156***	0.204***	0.147***	0.021***	0.020***
	(0.033)	(0.034)	(0.045)	(0.044)	(0.005)	(0.006)
Second finishing school$_{it}$		0.040		0.279**		0.003
		(0.058)		(0.109)		(0.008)
Mean, untreated	0.149	0.149	0.139	0.139	0.018	0.018
Panel B: Unmarried women						
First finishing school$_{it}$	0.147***	0.154***	0.173***	0.137***	0.014**	0.016**
	(0.034)	(0.035)	(0.043)	(0.043)	(0.006)	(0.006)
Second finishing school$_{it}$		-0.039		0.180*		-0.008
		(0.056)		(0.097)		(0.007)
Mean, untreated	0.153	0.153	0.143	0.143	0.022	0.022
Panel C: Teachers & Writers						
First finishing school$_{it}$	0.104***	0.082***	0.103***	0.065**	0.017***	0.015**
	(0.026)	(0.027)	(0.029)	(0.027)	(0.006)	(0.006)
Second finishing school$_{it}$		0.110**		0.191**		0.010
		(0.047)		(0.077)		(0.011)
Mean, untreated	0.075	0.075	0.059	0.059	0.019	0.019
Panel D: Activists						
First finishing school$_{it}$	0.053***	0.053***	0.043***	0.039**	0.011**	0.015***
	(0.018)	(0.020)	(0.015)	(0.016)	(0.005)	(0.005)
Second finishing school$_{it}$		-0.004		0.019		-0.017***
		(0.036)		(0.037)		(0.006)
Mean, untreated	0.016	0.016	0.012	0.012	0.005	0.005
Panel E: Nobility						
First finishing school$_{it}$	-0.013	-0.010	-0.007	-0.001	-0.002	-0.000
	(0.017)	(0.018)	(0.018)	(0.018)	(0.009)	(0.009)
Second finishing school$_{it}$		-0.015		-0.027		-0.008
		(0.035)		(0.026)		(0.014)
Mean, untreated	0.038	0.038	0.031	0.031	0.018	0.018
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,240	9,240	9,240	9,240	9,240	9,240

Notes: Results from fixed-effects regressions following our main specification reported, comparing effect sizes between the first and the second finishing school. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. In all columns we interact our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

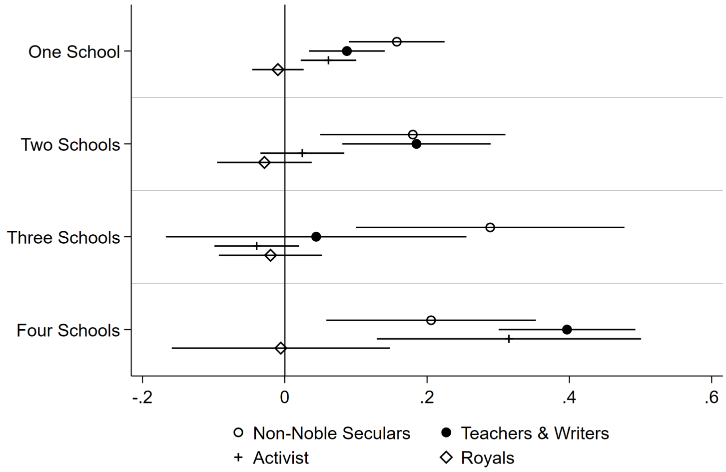


Figure 1.C.5: The impact of multiple schools

Notes: The cumulative impact of cities having one, two, three, or more school in the fixed effect estimation on the occurrence of notable women reported. The outcome is an indicator taking value 1 if a notable woman from the respective group was born in a given city and period. Full set of control variables included.

1.D Spatial Dependence and SUTVA

In this appendix, we address the potential threat of spatial correlation, possible violations of the Stable Unit Treatment Value Assumption (SUTVA), and discuss spatial noise (Kelly 2020).

We show that standard errors accounting for spatial correlation are slightly smaller than cluster-robust standard errors at the city level (Table 1.D.1). To address potential violations of SUTVA, we exclude all cities that border a city with finishing schools in Table 1.D.2. If migration from cities without finishing schools to cities with such schools drove our findings, an increase in the "cost of migration" by increasing control cities' distance to the next school city should result in significantly smaller estimates. As expected, we find no evidence that migration impacts our point estimates.

A recent literature has focused on how estimates indicating persistent effects of past events on more recent outcomes can be driven by spatial noise (Kelly 2020). To address the potential severity arising from this line of thought, we report a low Moran's I of 0.002 with a p-value of 0.156. In addition, we conduct an exercise where we randomly distribute schools across Germany in each period, holding the number of schools constant. The results in Figure 1.D.1 reveal that our results are clear outliers in this distribution, with the largest fraction of absolute values greater than our estimate at a mere 0.02 (for the results on *Activists*).

Taken together, the results presented in this Appendix suggest that our estimates are unlikely to be driven by spatial dependence and potential violations of SUTVA.

Table 1.D.1: Fixed-effects results on the importance of finishing schools - Standard errors corrected for spatial dependence

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.230***	0.164***	0.355***	0.204***	0.019***	0.021***
	(0.026)	(0.028)	(0.033)	(0.030)	(0.004)	(0.004)
Mean, untreated	0.150	0.149	0.140	0.139	0.018	0.018
Panel B: Unmarried women						
Finishing school$_{it}$	0.194***	0.147***	0.302***	0.173***	0.011**	0.014***
	(0.028)	(0.029)	(0.033)	(0.029)	(0.004)	(0.005)
Mean, untreated	0.155	0.153	0.144	0.143	0.022	0.022
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.151***	0.104***	0.174***	0.103***	0.019***	0.017***
	(0.019)	(0.020)	(0.018)	(0.020)	(0.005)	(0.005)
Mean, untreated	0.076	0.075	0.060	0.059	0.019	0.019
Panel D: Activists						
Finishing school$_{it}$	0.076***	0.053***	0.064***	0.043***	0.013***	0.011***
	(0.015)	(0.014)	(0.012)	(0.010)	(0.004)	(0.004)
Mean, untreated	0.016	0.016	0.012	0.012	0.005	0.005
Panel E: Nobility						
Finishing school$_{it}$	-0.018	-0.013	-0.009	-0.007	-0.002	-0.002
	(0.014)	(0.015)	(0.012)	(0.013)	(0.006)	(0.007)
Mean, untreated	0.039	0.038	0.031	0.031	0.018	0.018
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	9,312	9,240	9,312	9,240	9,312	9,240

Notes: Results from fixed-effects regressions following our main specification reported. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. In all columns we interact our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors corrected for spatial dependence within 100km as in Hsiang et al. (2013) reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

Table 1.D.2: Fixed-effects results on the importance of finishing schools - Comparing towns with schools to non-neighboring towns without schools

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school$_{it}$	0.164***	0.167***	0.204***	0.156***	0.018**	0.021***
	(0.033)	(0.047)	(0.045)	(0.055)	(0.007)	(0.005)
Mean, untreated	0.149	0.164	0.139	0.158	0.017	0.016
Panel B: Unmarried women						
Finishing school$_{it}$	0.147***	0.140***	0.173***	0.122**	0.011	0.014**
	(0.034)	(0.049)	(0.043)	(0.056)	(0.008)	(0.006)
Mean, untreated	0.153	0.180	0.143	0.176	0.021	0.021
Panel C: Teachers & Writers						
Finishing school$_{it}$	0.104***	0.108**	0.103***	0.090**	0.023**	0.017***
	(0.026)	(0.044)	(0.029)	(0.043)	(0.009)	(0.006)
Mean, untreated	0.075	0.085	0.059	0.066	0.017	0.017
Panel D: Activists						
Finishing school$_{it}$	0.053***	0.039	0.043***	0.026	0.012*	0.011**
	(0.018)	(0.026)	(0.015)	(0.019)	(0.006)	(0.005)
Mean, untreated	0.016	0.011	0.012	0.008	0.003	0.003
Panel E: Nobility						
Finishing school$_{it}$	-0.013	-0.010	-0.007	0.007	0.001	-0.002
	(0.017)	(0.028)	(0.018)	(0.034)	(0.015)	(0.009)
Mean, untreated	0.038	0.068	0.031	0.056	0.029	0.029
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Non-Spillover sample		Yes		Yes		Yes
Observations	9,240	3,696	9,240	3,696	3,696	9,240

Notes: Results from fixed-effects regressions following our main specification reported, comparing effect sizes between the full sample and a sample where all neighboring cities without finishing schools are dropped. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. In all columns we interact our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

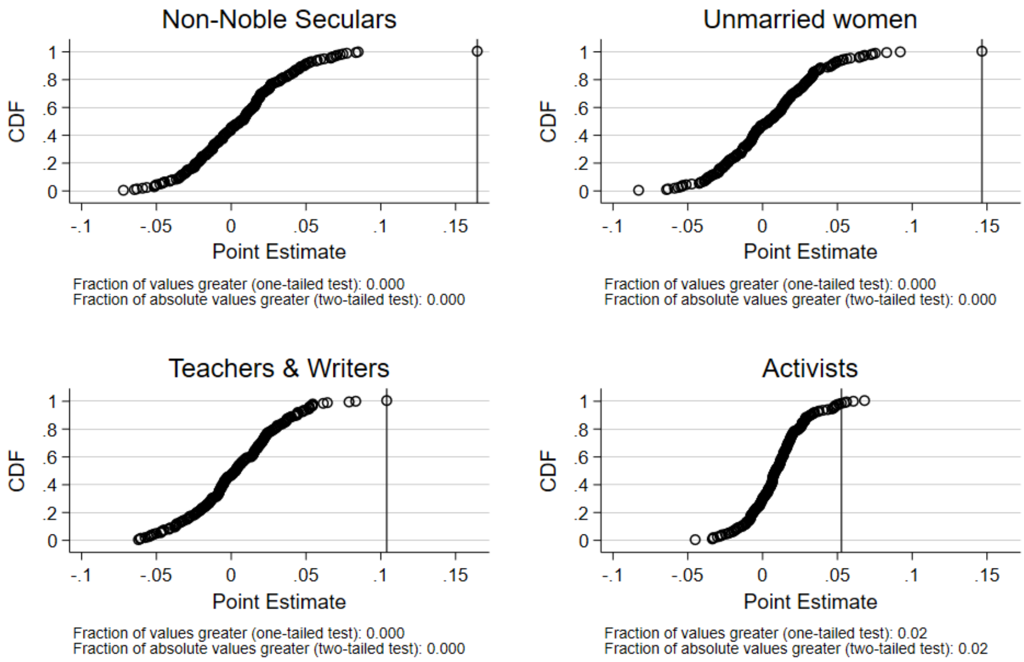


Figure 1.D.1: Placebo estimates: Distributing schools across Germany and centuries

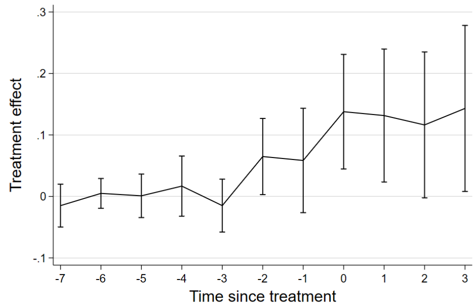
Notes: Each figure reports the point estimates from 200 randomization exercises that proceed as follows: We use the number of schools in every period and randomly distribute them across Germany. This is repeated for every period and used as a new explanatory variable in a regression with the full set of controls. The outcome is an indicator equal to one if a city observed the birth of at least one notable woman from the respective category in a given period. The vertical line marks the baseline estimate in Table 1.2 column (2).

1.E Recent Advances in Event-Study Designs: DID with Multiple Time Periods or Heterogeneous Treatment Effects

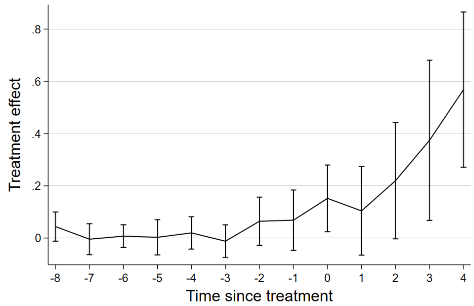
There has been a rich recent debate in the literature on how to interpret the average treatment effect on the treated in event-study designs. Following these developments, Baker et al. (2021) argue that "staggered treatment timing and treatment effect heterogeneity, either accross groups or over time, leads to biased Two-Way-Fixed-Effects DID [TWFE] estimates for the ATT", and propose three methods to assess the severity of this bias. First, show the event-study graph without controls (Figure 1.2) and by treatment group (Figure 1.F.2). Second, implement the method by Chaisemartin and D'Haultfœuille (2020) to assess whether heterogeneous treatment effects bias the estimate (Figure 1.E.1a). Third, implement the method by Callaway and Sant'Anna (2020) to assess whether treatment heterogeneity by treatment period bias our estimates (Figure 1.E.1b). Finally, show the implied weights following Goodman-Bacon (2021), showing that the main effect is derived from the comparison treatment versus control (Figure 1.E.2). All methods provide no evidence of different pre-trends and provide similar point estimates, highlighting the validity of our empirical approach.

Another way to assess the validity of our approach is by estimating the implied weight of each treatment period. In a classical event study design where one focuses on cities that ever establish treatment, late treatment cities are the implied control cities for early treatment cities (Goodman-Bacon 2021). Then, TWFE estimates are a weighted sum of individual treatment effects estimated for every city and period. Since these weights can be negative, inference can be affected. Using the approach suggested by Goodman-Bacon (2021), we show in Table 1.E.1 that the weight of the effect comes from the comparison between treated and never-treated. This result is confirmed in Figure 1.E.2, where the DID estimate is almost exclusively derived from the differences between cities without and with finishing schools, thus validating our approach.

Figure 1.E.2 suggests that the point estimate in our TWFE estimation stems from the difference between never-treated cities and cities with finishing schools. We thus provide additional evidence for the parallel trends assumption including all cities. In our main Figure 1.2, we show parallel trends in the set of cities that ever established finishing schools. In Figure 1.E.3, we complement this evidence by including cities that never established a finishing school. The results speak in favor of the parallel trends assumption: When controlling extensively for economic, religious and educational covariates, the estimated leads are centered around zero and show no difference between cities with and without finishing schools.



(a) Chaisemartin and D'Haultfœuille (2020)



(b) Callaway and Sant'Anna (2020)

Figure 1.E.1: Alternative treatment effect aggregators

Notes: Implementing the approaches introduced by Chaisemartin and D'Haultfœuille (2020) (Panel a) and Callaway and Sant'Anna (2020) where we employ an indicator taking value 1 if a city observed the birth of at least one notable woman in a given period as the dependent variable. The average treatment effect on the treated (ATT) in Panel (a) (0.146, s.e. 0.052) is slightly smaller than the ATT in Panel (b) (0.284, s.e. 0.054). These point estimates are very similar to baseline ATT reported in Figure 1.2 (0.146, s.e. 0.049).

Table 1.E.1: Goodman-Bacon (2021) decomposition of difference-in-differences estimation with variation in treatment timing

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	Weight	Av. DID Est.	Weight	Av. DID Est.	Weight	Av. DID Est.
Earlier Treatment vs. Later Control	0.071	0.160	0.071	0.227	0.071	0.015
Later Treatment vs. Earlier Control	0.013	0.028	0.013	-0.171	0.013	0.007
Treatment vs Never treated	0.915	0.315	0.915	0.492	0.915	0.023
Difference-in-differences estimate:		0.300		0.464		0.022

Notes: Applying the decomposition introduced by Goodman-Bacon (2021) to our setting. The table reports the weights and corresponding difference-in-differences estimates for three different outcomes: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians.

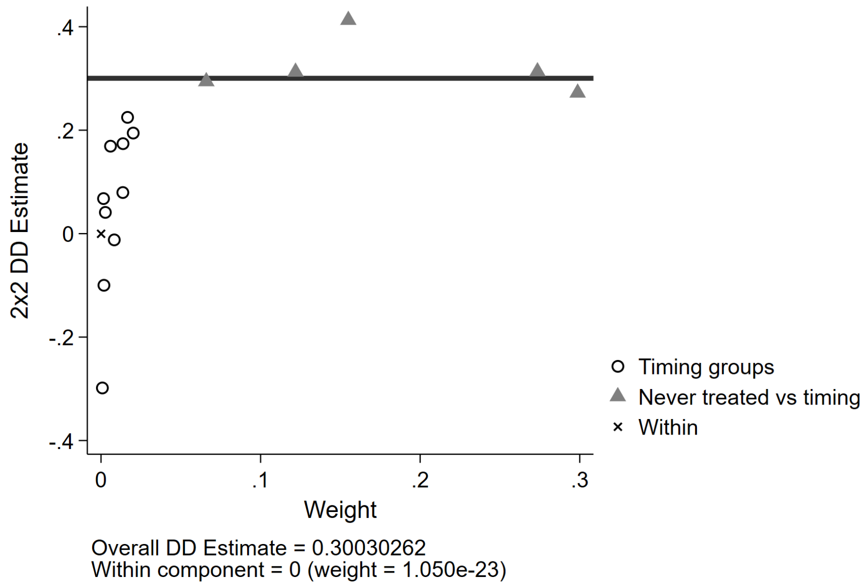
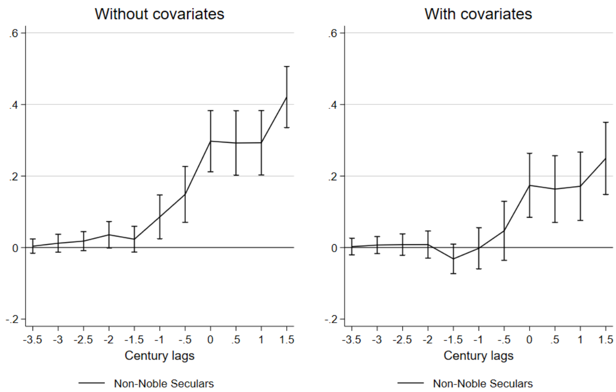
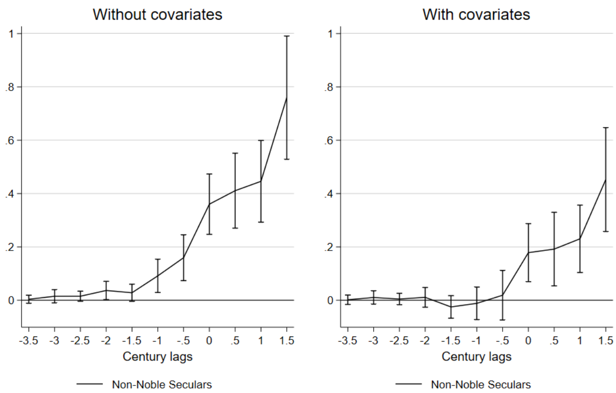


Figure 1.E.2: Goodman-Bacon (2021) decomposition of difference-in-differences estimation with variation in treatment timing

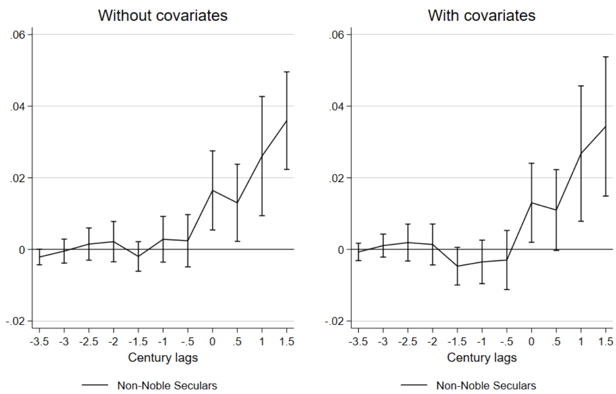
Notes: Implied weights against the treatment effect derived from Goodman-Bacon's (2021) decomposition exercise reported. The dependent variables is an indicator taking value 1 if a city observed the birth of at least one notable woman in a given period. The treatment effect is almost entirely estimated from the comparison of treated to untreated cities.



(a) Indicator: Notable woman born in city



(b) Log. number: notable women born in city



(c) Share: notable women born in city

Figure 1.E.3: Event-Study using never treated cities: Impact of finishing school establishment on notable women

Notes: Results from event-studies adding all cities which never established a finishing school as a further control group reported. Three different dependent variables employed: Figure (a) uses a dummy taking value 1 if a city observed the birth of least one notable woman in a given period; Figure (b) employs the natural logarithm of the number of notable women plus 1; and Figure (c) divides the number of notable women by the total number of notable individuals in a given city and period. Zero is the normalized opening year of the first finishing school in a city; -4 is the omitted period and includes all cities that never established a finishing school. City and period fixed effects included in all panels; the full set of economic, religious, and educational controls added in all right panels. 95-percent confidence intervals based on standard errors clustered at the city level reported.

1.F Standard Difference-in-Differences Estimates and Possible Instruments in the Panel Setting

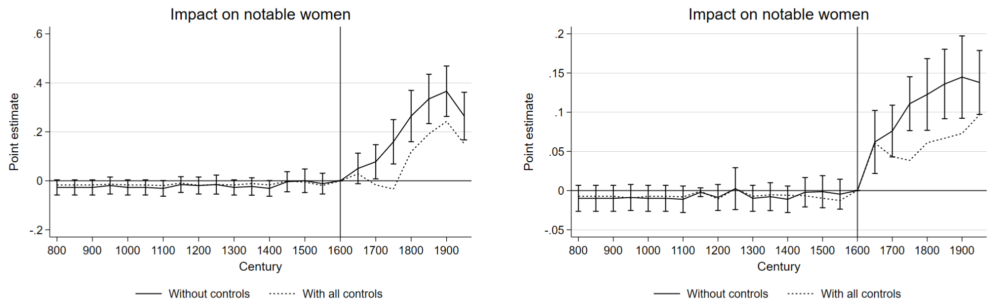
In this appendix, we show results from a standard difference-in-differences design, comparing cities without finishing schools (control group) with cities that establish a finishing school by 1850 (treatment group) to complement our assessment of pre-trends in the event-study setting and assess whether specific periods impact the estimates disproportionately. We then continue and analyze whether the diffusion of Protestantism threatens the interpretation of our findings (Becker and Woessmann 2009). We conclude this Appendix with a complementary empirical strategy using monasteries established before 1300 as an instrument for finishing schools. We document local average treatment effects that are very similar to the main results presented in the paper.

1.F.1 Standard difference-in-differences

We start by splitting our sample into cities that established finishing schools by 1850 and cities which did not and compare women's representation among the human capital elite in these two sets of cities before and after 1650, the period in which the first finishing school was founded. While this strategy allows for a more standard analysis of pre-trends than an event-study strategy, it also combines many treament periods into one, and thus likely underestimates the true impact. In Figure 1.F.1, we document the absence of significant pre-trends for both the extensive margin (establishing a school) and the intensive margin (number of schools). Yet, both panels reveal an increase in women's representation among the human capital elite in the periods after the first finishing school was established (1626). Point estimates are reported in Table 1.F.1 for both margins. First, the point estimates are very similar to the baseline results reported in Table 1.2 and are stable across specifications. Second, the point estimates on the intensive and extensive margin do not differ in most cases.

We continue and analyze the pre-trends for each treatment period separately in Figure 1.F.2. Again, we see no differential pre-trend in any pre-treatment period and significant impacts of schools only after the schools have been established. The results are somewhat stronger for the first and last schools, yet reveal no differential DID-estimate in Table 1.F.2. Here, we jointly estimate all treatment periods as compared to cities that never establish schools and find similar impacts across all types of schools. The only insignificant period is 1750, in which only three schools were established. Yet, even here the point estimate is statistically indistinguishable from the other periods.

We take this as evidence that our conclusion that finishing schools increase the share



(a) Any finishing school **(b) Number of schools**

Figure 1.F.1: Difference-in-differences estimation: Comparing cities with and without finishing schools over time

Notes: Results from difference-in-differences estimation reported. The figure split the sample into cities that have ever establish at least one finishing school by 1850 and those which have not and compare those before and after 1650. The outcome is an indicator taking value 1 if at least one notable woman was born in a given city and period. The left panel reports the point estimates when using a dummy for the presence of at least one finishing school and the right panel the number of finishing schools to compute the difference-in-differences estimate. The omitted period in both panels is 1600, the period before the first schools were opened. Estimates without (solid line) and with (dashed line) all controls shown. 95-percent confidence intervals based on standard errors clustered at the city level reported.

of women among the human capital elite is not driven by the functional form, identification strategy, or any period in particular. Also, while one could reasonably assume that the lack of variation in the outcome in the periods leading up to 1650 makes a pre-trend assessment problematic, the pre-trends are also insignificant in periods with more outcome variation such as the years 1600-1800 for the cities that establish finishing schools only in the 1850 period.

The effects in Figure 1.F.2 also indicate that the main effect in our baseline estimate is not driven by unobserved characteristics of the set of cities ever receiving finishing schools, which generally affect women's representation among the human capital elite in these cities after 1600. The temporal correspondence between the establishment of finishing schools and the timing of the effects (and the absence of pre-trends) certainly cannot alleviate all concerns about the potential endogeneity of the timing of school opening; however, it clearly points to an important nexus between the opening of finishing schools and the subsequent increase in women's representation among the human capital elite.

Table 1.F.1: Difference-in-Differences Estimation: Establishing finishing schools in cities

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school × Post 1650	0.182***	0.073***	0.264***	0.101***	0.010***	0.005*
	(0.024)	(0.022)	(0.041)	(0.030)	(0.003)	(0.003)
# Finishing schools × Post 1650	0.103***	0.063***	0.192***	0.148***	0.006***	0.004***
	(0.009)	(0.011)	(0.025)	(0.031)	(0.001)	(0.001)
Panel B: Unmarried women						
Finishing school × Post 1650	0.131***	0.044*	0.219***	0.079**	0.001	-0.001
	(0.024)	(0.025)	(0.038)	(0.032)	(0.004)	(0.004)
# Finishing schools × Post 1650	0.069***	0.033***	0.164***	0.126***	0.000	-0.001
	(0.013)	(0.012)	(0.023)	(0.028)	(0.001)	(0.002)
Panel C: Teachers & Writers						
Finishing school × Post 1650	0.113***	0.043***	0.122***	0.046***	0.014***	0.007*
	(0.018)	(0.016)	(0.022)	(0.016)	(0.003)	(0.004)
# Finishing schools × Post 1650	0.064***	0.039***	0.092***	0.073***	0.007***	0.004*
	(0.010)	(0.011)	(0.018)	(0.022)	(0.002)	(0.002)
Panel D: Activists						
Finishing school × Post 1650	0.036***	0.017*	0.032***	0.015*	0.004*	0.001
	(0.009)	(0.010)	(0.009)	(0.008)	(0.002)	(0.002)
# Finishing schools × Post 1650	0.027***	0.021***	0.028**	0.025**	0.002*	0.000
	(0.007)	(0.007)	(0.011)	(0.012)	(0.001)	(0.001)
Panel E: Nobility						
Finishing school × Post 1650	-0.012	-0.020	-0.000	-0.006	-0.002	-0.007
	(0.016)	(0.017)	(0.014)	(0.015)	(0.007)	(0.008)
# Finishing schools × Post 1650	0.004	0.003	0.015*	0.016*	0.006	0.005
	(0.008)	(0.009)	(0.009)	(0.009)	(0.004)	(0.005)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Religious covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	9,312	9,240	9,312	9,240	9,312	9,240

Notes: Results from a "standard" difference-in-differences setup reported. We present extensive (at least one finishing school by 1850) and intensive (number of finishing schools by 1850) margin effects by interacting our finishing school variable with a post-1650 indicator (the first period with finishing schools). We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact our full set of control variables with period fixed effects to capture variation from economic and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

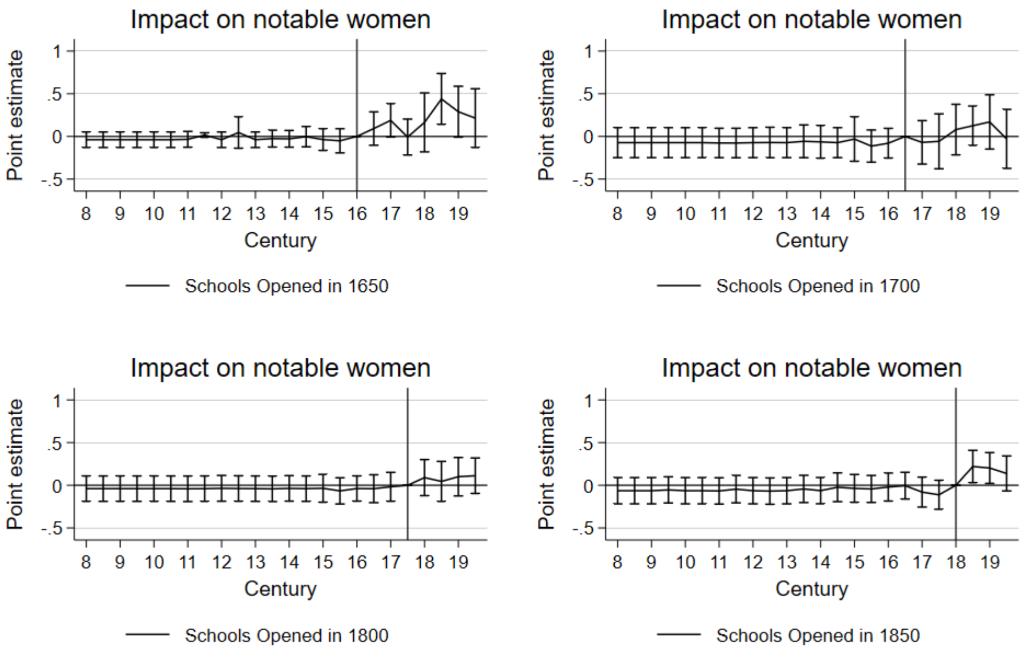


Figure 1.F.2: Parallel Trends Analysis: Lead-Lag figure by treatment cohort

Notes: Results from a difference-in-differences estimation reported. Each panel depicts the lead-lag graph for the indicated treatment group relative to the group of never treated cities. In all panels, we employ an indicator taking value 1 if a city observed the birth of at least one notable woman in a given period. No controls included. 95% confidence intervals derived from standard errors clustered at the city level reported.

Table 1.F.2: Difference-in-Differences Estimation: Establishing finishing schools in different periods

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Finishing school by 1650 × post 1650	0.294***	0.190***	0.415***	0.228**	0.023***	0.021**
	(0.060)	(0.058)	(0.114)	(0.105)	(0.008)	(0.008)
Finishing school by 1700 × post 1700	0.248***	0.138**	0.350***	0.180**	0.014***	0.014**
	(0.076)	(0.061)	(0.105)	(0.084)	(0.005)	(0.006)
Finishing school by 1750 × post 1750	0.159*	0.069	0.855*	0.699*	0.025*	0.024
	(0.083)	(0.072)	(0.437)	(0.366)	(0.015)	(0.017)
Finishing school by 1800 × post 1800	0.195***	0.134**	0.347***	0.190**	0.015**	0.019**
	(0.047)	(0.052)	(0.099)	(0.087)	(0.007)	(0.008)
Finishing school by 1850 × post 1850	0.249***	0.203***	0.248***	0.137*	0.023***	0.023***
	(0.050)	(0.052)	(0.067)	(0.074)	(0.009)	(0.009)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes		Yes
Educational covariates × period FE		Yes		Yes		Yes
Observations	9,312	9,240	9,312	9,240	9,312	9,240

Notes: Results from a "standard" difference-in-differences setup reported. We divide the sample with respect to whether a city had a finishing school in the indicated year and interact this variable with a post-year indicator to obtain the corresponding difference-in-differences estimate. All coefficiencts are jointly estimated. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact our full set of control variables with period fixed effects to capture variation from economic and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.

1.F.2 Protestantism as a confounding factor

Next, we turn to the diffusion of Protestantism as a potential confounding factor. Martin Luther advocated the education of women to enable their independent study of the Bible (Becker and Woessmann 2009). It is important to note, however, that he only argued for primary education (particularly reading), and not the secondary education and teacher training provided by finishing schools. We thus do not expect a significant impact of the Protestant Reformation on women's representation among the human capital elite. In order to obtain a causal estimate that is not confounded by the potentially endogeneous decision to adopt Protestantism, we also provide estimates using an instrumental variables strategy based on a city's distance to Wittenberg, the Reformation's epicenter.

We assess the impact of the Protestant Reformation on women's representation among the human capital elite in Figure 1.F.3. In the left-hand panel, we report estimates from an OLS regression of an indicator whether a notable woman was born in a given city and period on an indicator for whether a certain city adopted Protestantism by 1650. The lead-lag estimates suggest no consistently significant and positive effect of the Protestant Reformation on women's representation among the human capital elite until 1900. In the right-hand panel, we report estimates from a reduced form exercise where we replace the indicator for having adopted Protestantism by 1650 with the distance to Wittenberg, the city from which Protestantism spread across Germany. Again, we find no consistent positive effect on notable women. Taken together, Figure 1.F.3 suggests that our main results on the nexus between finishing schools and women's increasing representation among the human capital elite are unlikely to merely reflect the effects of the Protestant Reformation. The difference-in-differences estimates (odd columns) and reduced form estimates (even columns) in Table 1.F.3 confirm this pattern as they do not reveal a significant impact of the Reformation on women among the human capital elite.[43]

[43] We also find no evidence of a heterogeneous effect of the Reformation on the number of notable women.

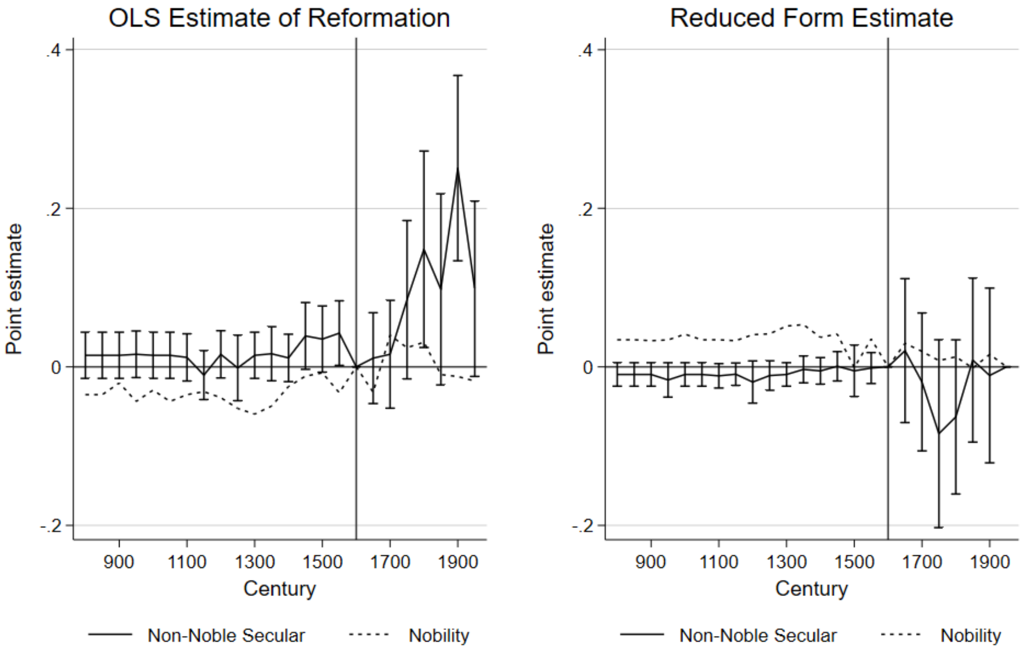


Figure 1.F.3: Using the Protestant Reformation as explanatory variation

Notes: Results from a lead-lag exercise reported. We employ an indicator for whether a city observed the birth of at least one notable woman in a given period as the dependent variable in both panels. In the left panel, we regress this variable on an indicator for whether a certain city adopted Protestantism by 1650 and its leads and lags. In the right panel, we report estimates from a reduced form exercise where we replace the indicator for having adopted Protestantism by 1650 with the distance to Wittenberg, the city from which Protestantism spread across Germany. We exclude religious controls in both panels. 95%-confidence intervals derived from standard errors clustered at the city level only reported for non-noble secular; the impact on women from the nobility is indistinguishable from zero in all periods and specifications.

Table 1.F.3: Difference-in-Differences Estimation: Switch to Protestantism as a cultural shock to the role of women in society

	$\mathbb{I}[Women > 0]$		log Women		Share Women	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Noble Seculars						
Reformation in City × post 1600	0.056**		0.068*		0.003	
	(0.023)		(0.035)		(0.003)	
log Distance to Wittenberg × post 1600		-0.041*		-0.046		-0.003
		(0.022)		(0.039)		(0.003)
Panel B: Unmarried women						
Reformation in City × post 1600	0.083***		0.088**		0.003	
	(0.027)		(0.037)		(0.004)	
log Distance to Wittenberg × post 1600		-0.009		-0.023		-0.001
		(0.028)		(0.040)		(0.003)
Panel C: Teachers & Writers						
Reformation in City × post 1600	0.030*		0.028		0.004	
	(0.018)		(0.018)		(0.004)	
log Distance to Wittenberg × post 1600		-0.032		-0.031		-0.004
		(0.022)		(0.024)		(0.003)
Panel D: Activists						
Reformation in City × post 1600	0.014		0.010		0.001	
	(0.010)		(0.007)		(0.003)	
log Distance to Wittenberg × post 1600		-0.005		-0.005		-0.000
		(0.008)		(0.008)		(0.002)
Panel E: Nobility						
Reformation in City × post 1600	0.026		0.026		0.012	
	(0.022)		(0.019)		(0.010)	
log Distance to Wittenberg × post 1600		0.017		0.010		0.005
		(0.012)		(0.010)		(0.005)
Unit trend	Yes	Yes	Yes	Yes	Yes	Yes
City covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates × period FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,288	9,288	9,288	9,288	9,288	9,288
F-Test		16.227		16.227		16.227

Notes: Results from a differences-in-differences estimation reported. In odd columns, we use an indicator variable whether a city has adopted Protestantism by 1650 to compute the difference-in-differences estimate. In even columns, we use the log distance to Wittenberg, the Reformation's epicenter, as a proxy for whether a city switched to Protestantism in a reduced form exercise. The corresponding first-stage effect exhibits an F-Stat of 16.23. We consider three types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator equal to one if a city observed the birth of at least one notable woman in a given period. "log Women" constitutes the natural logarithm of the number of women born plus one. "Share Women" denotes the number of women by the number of men and women in the same category, except for Activists, where we use the number of male politicians. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact our full set of control variables with period fixed effects to capture variation from economic and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels. $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.

1.F.3 Monasteries as an instrument

Finally, we discuss a potential instrument for the establishment of finishing schools. Historical accounts suggest that most of the early finishing schools were founded by Catholic nuns (Albisetti 1988). These nuns were often invited by rulers of German states and settled in available space in and around existing monasteries. We use monasteries that were established by 1300, more than 300 years prior to the opening of the first finishing school, as an instrument for finishing school establishment. With this instrument we exploit variation in the supply of buildings which could be converted to (or expanded to include) finishing schools at fairly low cost. By additionally limiting our analysis to cities in close vicinity to the inner-German denominational divide between Protestants and Catholics as of 1618, we hold religious competition constant. Thus, we estimate effects net of any direct impact of religious competition which the historical literature on finishing schools suggests as an important determinant of finishing school establishment (Lewejohann 2014). The key identification assumption is then that the number of monasteries established by 1300 in areas which were to become religiously competitive around the year 1600 only affects women's representation among the human capital elite via the construction of finishing schools. Figure 1.F.4 summarizes our findings. Using monasteries as an instrument provides reliable reduced form estimates that suggest a relevant instrument that is independent of the chosen bandwidth around the religious divide.

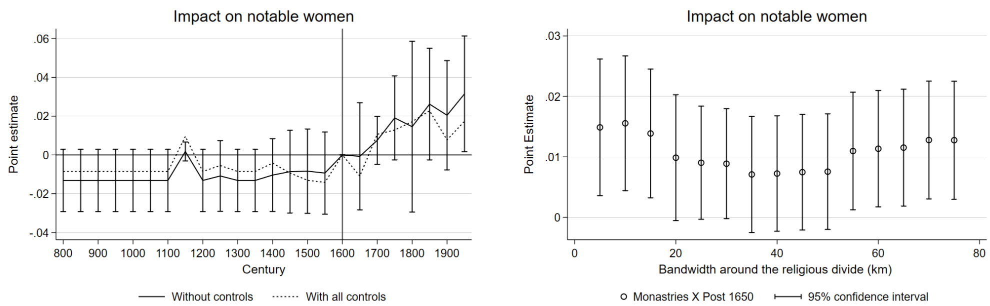


Figure 1.F.4: Reduced Form Estimates: Using monasteries in 1300 as an instrument

Notes: In the left panel, we report results from a lead-lag estimation. The dependent variable is an indicator taking value 1 if a city observed the birth of at least one notable woman in a given period. We regress this variable on the number of monasteries existing by 1300 and its leads and lags. We employ all cities within 10km of the religious divide in all time periods. In the right panel, we employ the same dependent variable to conduct a simple pre-post comparison using different bandwidths around the denominational divide. Controls include the full set of educational, economic, and religious covariates employed throughout this paper. 95-percent confidence intervals derived from standard errors clustered at the city level reported.

1.G Accumulation and Role-Model Hypothesis

In this appendix, we discuss whether finishing schools served as a pull factor motivating women from the human capital elite to migrate into a city. In contrast to the rest of the paper, where we link notable women to cities based on their place of *birth*, for this exercise, we leverage information on notable women's place of *death* to measure whether finishing schools attracted notable women from elsewhere. We thus investigate whether finishing schools contributed to a local accumulation of notable women, potentially via the mechanism that local notable women served as role-models in attracting others. In Table 1.G.1 we show that upon the establishment of the first finishing school in a city, more women from the human capital elite born in other cities moved to the city with the newly established finishing school. It is important to note that our rich data on notable women's places of birth and places of death allow us to distinguish the in-migration of notable women born elsewhere from spillover effects, which we discuss in Appendix 1.D of this appendix. Our data also allow us to document that finishing schools attracted the in-migration of women from the human capital elite to these cities while ruling out that finishing schools were established in response to the in-migration of women from the human capital elite as evidenced by the clear absence of differential pre-trends in Figure 1.6 in the main body of Chapter 1.

A further concern is that most of the positive effect of finishing schools on the in-migration of women from the human capital elite might be mechanical since finishing schools were primary employers for notable women. We test for this in the second Panel of Table 1.G.1: we find that once we add our control variables and thus adequately control for initial differences between cities, we see no significant effect of finishing schools on the number of notable teachers who migrated to a city with a finishing school. This suggests that a potential mechanical effect for teachers alone cannot account for the main effect shown in the first Panel of Table 1.G.1.

Taken together, the evidence presented in this Appendix suggests that finishing schools indeed served as a pull factor which attracted notable women born elsewhere.

Table 1.G.1: Testing role-model and accumulation hypotheses

	$\mathbb{I}[Women > 0]$		log Women	
	(1)	(2)	(3)	(4)
Panel A: Immigration of Non-Noble Seculars				
Finishing school$_{it}$	0.114***	0.059**	0.134***	0.049*
	(0.023)	(0.024)	(0.033)	(0.028)
Mean, untreated	0.042	0.042	0.034	0.034
Panel B: Immigration of Teachers & Writers				
Finishing school$_{it}$	0.049***	0.016	0.052**	0.015
	(0.018)	(0.019)	(0.022)	(0.019)
Mean, untreated	0.020	0.020	0.016	0.016
Unit trend	Yes	Yes	Yes	Yes
City covariates × period FE		Yes		Yes
Religious covariates × period FE		Yes		Yes
Educational covariates × period FE		Yes		Yes
Observations	9,312	9,240	9,312	9,240

Notes: Results from fixed-effects regressions using cities and periods reported. We consider two types of dependent variables: $\mathbb{I}[Women > 0]$ is an indicator taking value 1 if a city observed the immigration of at least one notable woman born elsewhere in a given period. "log Women" constitutes the natural logarithm of immigrated women plus 1. We regress our dependent variables on a dummy taking value 1 if a finishing school existed in a given city and period. Columns (1), (3), and (5) constitute the baseline and include city and period fixed effects as well as city-specific linear trends. In columns (2), (4), and (6) we interact our full set of control variables with period fixed effects to capture variation from economic, religious, and educational differences. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

1.H Specification and Robustness in the Cross-Sectional Setting

In this appendix, we highlight that our cross-sectional results is robust to using an instrumental variables estimation, to estimating effects of a city's length of exposure to finishing schools, and to matching on observables.

First, we discuss a potential instrument for the establishment of finishing schools. As pointed out earlier, historical accounts suggest that most of the early finishing schools were founded by Catholic nuns (Albisetti 1988). These nuns were often invited by rulers of German states and settled in available space in and around existing monasteries. We use monasteries that were established by 1300, more than 300 years prior to the opening of the first finishing school, as an instrument for finishing school establishment. With this instrument we exploit variation in the supply of buildings which could be converted to (or expanded to include) finishing schools at fairly low cost. By additionally limiting our analysis to cities in close vicinity to the inner-German denominational divide between Protestants and Catholics as of 1618, we can hold religious competition constant and thus estimate effects net of any direct impact of religious competition which the historical literature on finishing schools suggests as an important determinant of finishing school establishment (Lewejohann 2014). The key identification assumption is then that the number of monasteries established by 1300 in areas which were to become religiously competitive around the year 1600 only affects women's representation among the human capital elite via the construction of finishing schools.

In Table 1.H.1, we show that indeed using the number of monasteries existing in 1300 as an instrument for the number of finishing schools in 1850 produces consistent estimates throughout all outcomes and main specifications (columns 1 and 4). Changing the cutoff year for pre-existing monasteries closer to 1648, the end of the Thirty Years' War, produces similarly sized estimates, yet smaller F-statistics (columns 2, 3, 5, and 6).

Finally, we estimate effects of a city's length of exposure to finishing schools (instead of the absolute number of finishing schools). In Table 1.H.2, we show that changing the independent variable to years since first opening produces very similar results in a wide range of specifications. Here, we define '0' as having no school in 1850, and progressively move back in time to '224', indicating the school was build in 1626. In Table 1.H.2 we thus investigate whether more time elapsed since the establishment of the first finishing school in city – and thereby a greater representation of women among the human capital elite – is associated with stronger support of the women's rights movement.

At a mean of 20 years of exposure to finishing schools, increasing the number of years by 10% (2 years), increases the number of letters to *Frauen-Zeitung* by 0.56%, the number of women's rights associations by 5% and the number of female members of parliament by 0.25% and 0.95% respectively. Or to put it differently, had a city opened a finishing school in 1800 (instead of never) and thus had 50 years more exposure to such a school, this would imply a 250% increase in exposure compared to the mean of 20 years. This city would have sent 14% more letters, hosted twice the number of women's rights organizations, and sent 24% more women to postwar parliaments. These are sizable effects, for a relatively small change in exposure.

Table 1.H.1: Long-term impact of finishing schools on political outcomes - IV estimates using different timings of the *monastery instrument*

	$\mathbb{I}[>0]$			log Number		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Leserbriefe, Frauenzeitung, 1849–1852						
Finishing schools	0.249**	0.274**	0.297**	0.412***	0.492***	0.444**
	(0.098)	(0.108)	(0.121)	(0.158)	(0.187)	(0.192)
Mean, untreated	0.038	0.038	0.038	0.061	0.061	0.061
Panel B: All women's rights organizations						
Finishing schools	0.378*	0.378	0.258	2.868*	2.844	2.308
	(0.223)	(0.241)	(0.219)	(1.680)	(1.835)	(1.678)
Mean, untreated	0.275	0.275	0.275	155.802	155.802	155.802
Panel C: Women's rights organizations to promote equal access to education						
Finishing schools	0.333**	0.340*	0.393**	2.099**	2.123**	2.504**
	(0.159)	(0.178)	(0.178)	(0.851)	(0.940)	(0.966)
Mean, untreated	0.038	0.038	0.038	13.023	13.023	13.023
Panel D: Member Parliament, 1919–1933						
Finishing schools	0.164*	0.122	0.137	0.227**	0.193**	0.226**
	(0.093)	(0.090)	(0.104)	(0.090)	(0.093)	(0.093)
Mean, untreated	0.038	0.038	0.038	0.053	0.053	0.053
Panel E: Member Parliament, 1949–2019						
Finishing schools	0.237	0.236	0.179	0.471**	0.480*	0.524*
	(0.174)	(0.189)	(0.192)	(0.223)	(0.247)	(0.269)
Mean, untreated	0.527	0.527	0.527	1.031	1.031	1.031
City Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes	Yes	Yes
Observations	183	183	183	183	183	183
Bandwidth	10	10	10	10	10	10
Monastery Year	1300	1500	1648	1300	1500	1648
F-Stat first stage	8.906	7.177	8.435	8.906	7.177	8.435

Notes: Results from two-stage least-squares (2SLS) regressions reported. We instrument the number of finishing schools in 1850 by the number of monasteries by 1300 (Columns 1 and 4), by 1500 (Columns 2 and 5), or by 1648 (Columns 3 and 6). We further limit the sample to cities within 10 km of the inner-German denominational divide to hold religious competition as good as constant. In each panel, we employ two types of outcomes: (i) an indicator variable taking value 1 if at least one letter (Panel A), women's rights organization (Panels B and C), or female member of parliament (Panels D and E) was recorded for a given city; and (ii) the natural logarithm of these variables plus 1. We employ our full set of educational, economic, and religious control variables as defined in Table 1.2 in all columns. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 1.H.2: Long-term impact of finishing schools on political outcomes: Years since opening of the first finishing school as explanatory variable

	$\mathbb{I}[>0]$				log Number			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Leserbriefe, Frauenzeitung, 1849–1852								
Years since first opening	0.001*	0.001			0.002**	0.001		
	(0.000)	(0.001)			(0.001)	(0.001)		
log Years since first opening			0.024**	0.025*			0.056***	0.061*
			(0.011)	(0.015)			(0.021)	(0.035)
Mean, untreated	0.062	0.038	0.062	0.038	0.105	0.061	0.105	0.061
Panel B: All women's rights organizations								
Years since first opening	0.002***	0.002***			0.015***	0.015***		
	(0.000)	(0.001)			(0.003)	(0.005)		
log Years since first opening			0.072***	0.096***			0.535***	0.634***
			(0.015)	(0.023)			(0.098)	(0.148)
Mean, untreated	0.366	0.275	0.366	0.275	447.696	155.802	447.696	155.802
Panel C: Women's rights organizations to promote equal access to education								
Years since first opening	0.001*	0.000			0.005*	0.003		
	(0.001)	(0.001)			(0.003)	(0.004)		
log Years since first opening			0.029**	0.017			0.169***	0.100
			(0.011)	(0.015)			(0.062)	(0.081)
Mean, untreated	0.047	0.038	0.047	0.038	13.074	13.023	13.074	13.023
Panel D: Member Parliament, 1919–1933								
Years since first opening	0.001**	0.001			0.001***	0.001*		
	(0.000)	(0.001)			(0.000)	(0.001)		
log Years since first opening			0.023**	0.022			0.025**	0.020*
			(0.011)	(0.014)			(0.010)	(0.011)
Mean, untreated	0.066	0.038	0.066	0.038	0.074	0.053	0.074	0.053
Panel E: Member Parliament, 1949–2019								
Years since first opening	0.001	0.001*			0.002*	0.004**		
	(0.001)	(0.001)			(0.001)	(0.002)		
log Years since first opening			0.044***	0.063***			0.095***	0.137***
			(0.015)	(0.022)			(0.025)	(0.036)
Mean, untreated	0.556	0.527	0.556	0.527	1.163	1.031	1.163	1.031
City Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	385	183	385	183	385	183	385	183
Bandwidth	400	10	400	10	400	10	400	10

Notes: Results from cross-sectional regressions reported. We employ two different explanatory variables: (i) time elapsed since the opening of the first finishing school in a city until 1850 (measured in years) and (ii) the natural logarithm plus 1 of time elapsed since the first finishing school. Hence, time elapsed since the first school opening is zero for cities which have never established a finishing school. In each panel, we employ two types of outcomes: (i) an indicator variable taking value 1 if at least one letter (Panel A), women's rights organization (Panels B and C), or female member of parliament (Panels D and E) was recorded for a given city; and (ii) the natural logarithm of these variables plus 1. We employ our full set of educational, economic, and religious control variables as defined in Table 1.2 in all columns. We report results using two different bandwidths around the inner-German denominational divide: 400km in odd columns and 10km in even columns. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

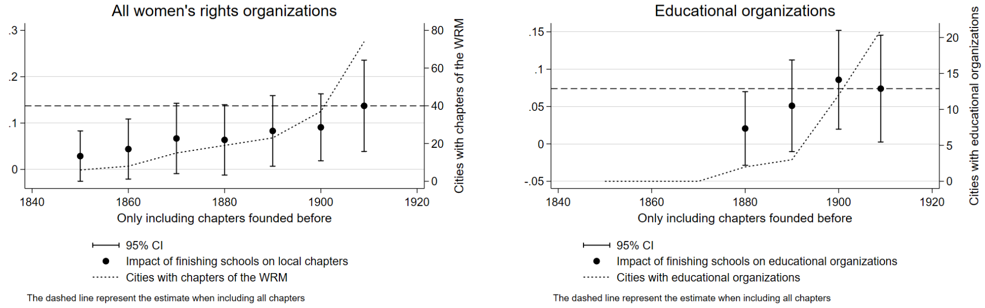


Figure 1.H.1: The impact of finishing schools on chapters of the Women's Rights Movement - Time varying effects

Notes: Results from cross-sectional regressions reported. In both figures, the sample is limited to cities within 10km of the inner-German denominational divide. The left figure shows the impact of finishing schools on whether any local chapter of the women's rights movement was founded in a city by 1850, 1860, 1870, 1880, 1890, 1900, and 1909, respectively. The right figure shows the same impact on local chapters devoting their efforts to promoting equal access to education for women. The All educational, economic, and religious controls included in both figures. 95-percent confidence intervals derived from standard errors clustered at the city level reported.

1.H.1 Comparison to propensity score matching

As a final step, we show robustness of our results to matching each city to its closest counterparts based on observable characteristics. The point estimates in columns (3) and (6) are not statistically different from the OLS (columns 1 and 4) or the sample of cities that lie within 10 km of the religious divide (columns 2 and 5). In addition, the matched sample shows no signs of imbalances across all covariates (Table 1.H.4, column 6).

Table 1.H.3: Long-term impact of finishing schools on political outcomes: Comparison to *matching estimators*

	$\mathbb{I}[>0]$			log Number		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Leserbriefe, Frauenzeitung, 1849–1852						
Finishing schools	0.095***	0.122***	0.144***	0.183***	0.241**	0.209***
	(0.020)	(0.037)	(0.018)	(0.055)	(0.097)	(0.076)
Panel B: All women's rights organizations						
Finishing schools	0.064**	0.137***	-0.003	0.800***	1.157***	0.532***
	(0.027)	(0.050)	(0.023)	(0.160)	(0.306)	(0.194)
Panel C: Women's rights organizations to promote equal access to education						
Finishing schools	0.083***	0.074**	0.055*	0.549***	0.496**	0.510***
	(0.017)	(0.036)	(0.029)	(0.113)	(0.217)	(0.194)
Panel D: Member Parliament, 1919–1933						
Finishing schools	0.067***	0.101***	0.042	0.100***	0.105***	0.107**
	(0.018)	(0.034)	(0.027)	(0.029)	(0.035)	(0.049)
Panel E: Member Parliament, 1949–2019						
Finishing schools	0.060**	0.091*	0.012	0.246***	0.268***	0.280***
	(0.024)	(0.047)	(0.025)	(0.040)	(0.071)	(0.055)
City Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Religious covariates	Yes	Yes	Yes	Yes	Yes	Yes
Educational covariates	Yes	Yes	Yes	Yes	Yes	Yes
Propensity score matching			Yes			Yes
Observations	385	183	318	385	183	318
Bandwidth		10			10	

Notes: Results from cross-sectional regressions reported. We presents results from three sets of specifications: (i) using the full set of controls (columns 1 and 4); (ii) using the full set of controls when we limit the sample to cities within 10 km of the inner-German denominational divide (columns 2 and 5); and (iii) using propensity score matching (columns 3 and 6). In each panel, we employ two types of outcomes: (i) an indicator variable taking value 1 if at least one letter (Panel A), women's rights organization (Panels B and C), or female member of parliament (Panels D and E) was recorded for a given city; and (ii) the natural logarithm of these variables plus 1. We employ our full set of educational, economic, and religious control variables as defined in Table 1.2 in all columns. Standard errors clustered at the city level reported in parentheses. Significance levels: $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.

Table 1.H.4: Balance in the matched Sample

	Unmatched sample			Matched sample		
	β	s.e.	p-value	β	s.e.	p-value
log(Distance Wittenberg)	-0.080	0.040	0.046	0.084	0.079	0.291
log(Distance religious divide)	0.214	0.065	0.001	0.029	0.098	0.769
log(Population in 1650)	0.422	0.058	0.000	0.047	0.034	0.167
Temperature in Spring 1650	0.011	0.041	0.783	0.011	0.068	0.871
Temperature in Summer 1650	0.079	0.048	0.097	0.010	0.075	0.892
Temperature in Fall 1650	-0.002	0.036	0.947	-0.022	0.052	0.668
Temperature in Winter 1650	-0.119	0.048	0.014	-0.078	0.065	0.227
Hanse city	0.044	0.020	0.031	-0.016	0.039	0.689
Bishop seat	0.036	0.017	0.033	-0.030	0.022	0.184
Jewish settlement	0.081	0.025	0.001	0.021	0.039	0.598
Progrom	0.044	0.023	0.057	0.036	0.039	0.350
Battle during 30-years war	0.062	0.021	0.003	-0.049	0.049	0.314
Boy school in 1605	0.018	0.017	0.279	0.036	0.030	0.221
University in 1650	0.005	0.008	0.557	-0.004	0.011	0.701
Catholic region	0.012	0.023	0.597	0.014	0.042	0.746

Notes: Results from balance test of covariates in 1650 reported. Balancing is assessed using the regression $X_c = \alpha + \beta \cdot \text{Schools}_{c,1850} + \varepsilon_c$. The unmatched sample contains all cities in 1650, whereas the matched sample selects a nearest neighbor – that is a city that is comparable with respect to observables – for each city with at least one finishing. While cities with finishing schools are closer to Wittenberg, further away from the inner-German denominational divide and have larger population in 1650, these differences disappear when matching cities to their nearest neighbor.

1.I Impact of Notable Women in 1850 on Local Political Activity

In this appendix, we directly ask what is the correlation between an additional non-noble secular women in 1850 and subsequent political activity in the subsequent one hundred years. To this end, we estimate the following equation in Table 1.I.1:

$$Y_c = \alpha + \beta \cdot \log(\text{Number Non-Noble Seculars+1})_{c,1850} + \gamma \boldsymbol{X}_c + \varepsilon_c \tag{1.1}$$

Being well aware of the endogeneity concerns associated with this equation, we nevertheless present estimates for their interpretability: a 10% increase in the number of notable women in a city is associated with a 2% increase in correspondence (Panel A), a 15% increase in women's rights associations (Panels B&C), and a 2% (4.6%) increase in the number of female members of parliament during the Weimar Republic (Federal Republic).

We conduct two exercises to assess the reliabilty of these correlations. First, we present point estimates with (odd columns) and without (even columns) controls, limited to 10 km of the religious boundary. The estimates remain stable throughout all specifications. Second, we instrument the number of notable women by the number of existing monasteries in 1300 and provide the 2SLS coefficient, the p-value and F-statistic below the OLS estimates. However, as the exclusion restriction, monasteries only affect political outcomes through their impact on finishing schools' impact on notable women, is likely to fail, we take these estimates with a caution. All 2SLS estimates are significant and larger than the OLS estimates with a strong first stage of 14: a 10% increase in the number of notable women in each city is associated with a 8% increase in correspondence (Panel A), a 40% increase in women's rights associations for education (Panel C), and a 4% increase in the number of female members of parliament during the Weimar Republic (Panel D).[44]

Both extensively controlling for confounding factors and instrumenting non-noble secular women by historical finishing schools suggest that a larger representation of women among the human capital elite increases women's political activity. Yet, as neither finishing schools nor non-noble secular women are likely randomly allocated to German cities in 1850, these estimates represent an informative correlation.

[44] A similar exercise using finishing schools as an instrument can be conducted. It yields qualitatively similar results with a stronger first stage of 22.

Table 1.I.1: Impact of notable women in 1850 on political activity of the Women's Rights Movement

	$\mathbb{I}[> 0]$		log Number	
	(1)	(2)	(3)	(4)
Panel A: Leserbriefe, Frauenzeitung, 1849–1852				
log(Number Non-Noble Seculars)	0.221***	0.190***	0.381***	0.227***
	(0.028)	(0.058)	(0.078)	(0.076)
Implied 2SLS coefficient	0.246	0.483	0.285	0.800
P-value 2SLS coefficient	0.005	0.013	0.038	0.018
First stage F-statistic	29.640	14.916	29.640	14.916
Panel B: All women's rights organizations				
log(Number Non-Noble Seculars)	0.262***	0.133*	2.598***	1.511***
	(0.030)	(0.076)	(0.178)	(0.488)
Implied 2SLS coefficient	0.695	0.734	5.737	5.563
P-value 2SLS coefficient	0.000	0.074	0.000	0.052
First stage F-statistic	29.640	14.916	29.640	14.916
Panel C: Women's rights organizations to promote equal access to education				
log(Number Non-Noble Seculars)	0.300***	0.263***	1.813***	1.516***
	(0.025)	(0.055)	(0.144)	(0.303)
Implied 2SLS coefficient	0.531	0.646	2.840	4.073
P-value 2SLS coefficient	0.000	0.000	0.000	0.000
First stage F-statistic	29.640	14.916	29.640	14.916
Panel D: Member Parliament, 1919–1933				
log(Number Non-Noble Seculars)	0.206***	0.204***	0.257***	0.198***
	(0.030)	(0.050)	(0.044)	(0.049)
Implied 2SLS coefficient	0.422	0.319	0.461	0.440
P-value 2SLS coefficient	0.000	0.036	0.000	0.004
First stage F-statistic	29.640	14.916	29.640	14.916
Panel E: Member Parliament, 1949–2019				
log(Number Non-Noble Seculars)	0.197***	0.199***	0.610***	0.469***
	(0.027)	(0.070)	(0.051)	(0.109)
Implied 2SLS coefficient	0.418	0.460	0.935	0.914
P-value 2SLS coefficient	0.000	0.120	0.000	0.005
First stage F-statistic	29.640	14.916	29.640	14.916
City Covariates		Yes		Yes
Religious covariates		Yes		Yes
Educational covariates		Yes		Yes
Observations	388	183	388	183
Bandwidth		10		10

Notes: Results from cross-sectional regressions reported. Our explanatory variable in these regressions is the natural logarithm of the number of non-noble secular notable women in a given city by 1850. We also report the point estimate, p-value and F-statistic from an 2SLS regression below the OLS coefficient for convenience. To obtain the 2SLS estimate, we instrument the log number of notable women in city c with the number monasteries existing by 1300. We further enhance the comparability of cities, in particular with respect to historical levels of religious competition, by limiting our sample to cities within 10 km of the inner-German denominational divide in odd columns. In each panel, we employ two types of outcomes: (i) an indicator variable taking value 1 if at least one letter (Panel A), women's rights organization (Panels B and C), or female member of parliament (Panels D and E) was recorded for a given city; and (ii) the natural logarithm of these variables plus 1. We employ our full set of educational, economic, and religious control variables as defined in Table 1.2 in all columns. Standard errors clustered at the city level reported in parentheses. Significance levels: $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

1.J Additional History on Finishing Schools

In Figure 1.J.1, we depict the spatial distribution of finishing schools in Germany separately by denomination – that is, by indicating which school was Catholic and which was Protestant. As becomes apparent from the figure, the first schools were exclusively Catholic. In fact, the first Protestant school opened in 1698. The first school funded by city authorities opened in 1800. The observed acceleration in the roll-out of finishing schools in the period 1800-1850 is likely driven by the dissolution of the Holy Roman Empire (800-1806), freeing up resources from previous inner-German conflicts. More than 100 schools were built between 1825 and 1850 alone, most of them in Prussia. Interestingly, Prussia recruited many of its female teachers from Catholic Bavaria. Comparing treatment effects for early and late periods (Table 1.C.5) and treatment periods (Table 1.F.2) suggest no differential treatment effects with respect to time.

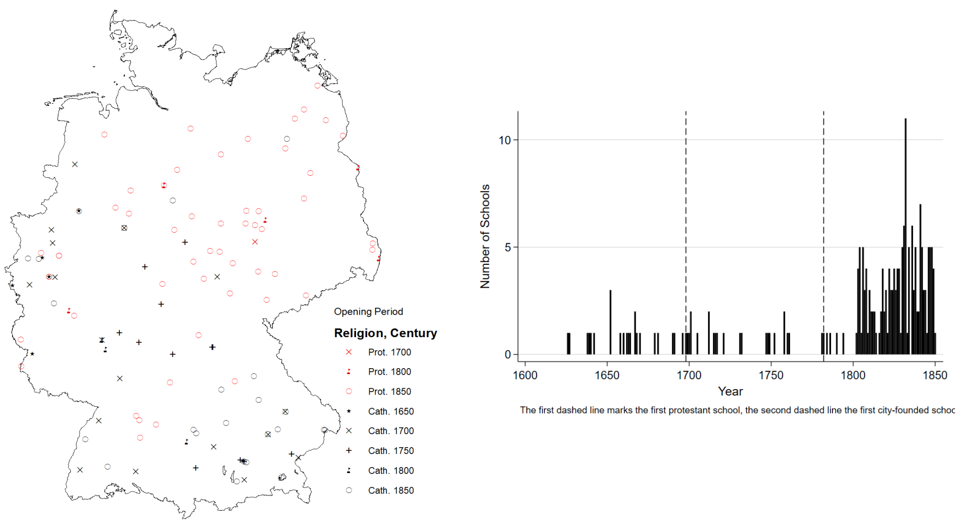


Figure 1.J.1: Opening years of finishing schools in Germany

Notes: The left map displays the spatial distribution Catholic and Protestant finishing schools in Germany. The right figure reports the opening year of each finishing school in our dataset.

CHAPTER 2

ANTICIPATED PEER EFFECTS

2.1 Introduction

What motivates us to act prosocially? A prominent literature in economics has documented that individuals are more likely to act prosocially if their behavior is observable to others and that *social image effects* are an important motive explaining these behavioral responses.[1] In this paper, we propose a complementary explanation as to why individuals are more inclined to act prosocially under observability: *anticipated peer effects*. While social image effects imply that individuals care about how their behavior is perceived by others, anticipated peer effects capture the idea that individuals are also motivated by an anticipation that their own (prosocial) behavior may exhibit a peer effect on others.

We argue that anticipated peer effects are a relevant motive for decision making in many situations where our actions generate positive externalities if those around us follow suit: examples range from signing up early for a health screening to nudge our more present-biased friends, to ordering vegetarian food during a group meal or wearing an "I voted" button to signal the importance of eco-friendly behavior or voting, respectively. In all these situations other motives such as social and self image concerns clearly play important roles, but anticipated peer effects frequently push the benefits of a prosocial action above its costs, such as when we stop at a red traffic light to be a role model to younger observers. Despite their potential importance, empirical evidence on the existence of anticipated peer effects in prosocial settings is scarce, largely due to the difficulty of disentangling them from social image effects.

In this paper, we causally identify anticipated peer effects in the decision whether to register for a COVID-19 vaccination, separating them conceptually and empirically from social image effects. Using a survey-based online field experiment, we document that individuals' willingness to register for the vaccination increases sharply when re-

[1] Bénabou and Tirole (2006) provide the seminal theoretical exposition of social signaling in the context of prosocial behavior. Bursztyn and Jensen (2017) offer an extensive review of experimental evidence on how observability shapes behavior in various domains including voting, donations to charity, financial decision making, or schooling decisions.

alizing that they can influence a peer's decision. Our results highlight that individuals anticipate and internalize their potential to lead by example in consequential decision environments.

Our experimental design groups survey participants into pairs, where one participant takes on the role of "Sender" (she) and the other acts as "Receiver" (he). To isolate anticipated peer effects, we experimentally vary (1) the *observability* of the Sender's decision to her Receiver and (2) the *timing* when the Receiver is informed. In the baseline condition *"not informing partner"*, we told Senders that their decision whether to register for a vaccination would *not* be reported to their partner. In the *"informing partner after"* condition, Senders learned that their decision would be shared with their Receiver, but only *after* the Receiver himself had already decided whether to register. Finally, in the *"informing partner before"* condition, we told Senders that their Receiver would be informed *before* his own registration decision. We expect anticipated peer effects to influence the behavior of Senders in the *"informing partner before"* condition, while the *"informing partner after"* condition serves as a control group which holds other behavioral factors – in particular social image effects – constant.

Anticipated peer effects almost double Senders' likelihood of registering for a COVID-19 vaccination: 9 percent of Senders in the *"informing partner before"* condition completed the official registration process during our online experiment compared to 5 percent in the *"informing partner after"* condition. We exploit the official state-wide online registration and appointment allocation system for COVID-19 vaccinations in the German state of Bavaria, which constituted the only pathway to receiving a COVID-19 vaccination for Bavarian citizens at the time of the experiment in early April 2021. This allowed us to elicit a verifiable revealed preference measure of individuals' willingness to register for a vaccination. We also document that Senders in the *"informing partner before"* condition were substantially more likely to believe that their registration decision could influence their partner's decision. These Senders arguably internalized that their registration decision might generate externalities – with respect to their partner's personal health as well as herd immunity – if their partners followed their lead.[2]

The interaction between Senders and Receivers in our experimental design is explicitly designed to be anonymous and to rule out future interactions between partners to further minimize the influence of social image effects. We recruited participants for our online survey using a professional online panel provider sampling from the adult population of Bavaria. After an elicitation of baseline beliefs and attitudes (e.g.,

[2]An alternative two-stage least squares (2SLS) estimation supports this interpretation: if we use our key experimental manipulation as an instrument for Senders' beliefs about the likelihood of exerting a peer effect on their partner, we find statistically significant effects of this belief on Senders' likelihood to sign up for a COVID-19 vaccination.

regarding vaccination safety and efficacy) participants learn that they will be grouped into pairs to collaborate on a brief joint problem solving task, where they can interact via chat but merely learn each other's nickname, age, gender and state of residence. As intended, the joint task is successful in inducing social proximity, i.e., in establishing the Receiver as a relevant – if temporary – peer to the Sender. However, the interaction between Senders and Receivers is limited to the brief exchange in the joint task and the anonymous online survey setting forestalls future interactions outside of the experiment, which precludes Senders from influencing their partner after the experiment.[3] After the joint task and an elicitation of social proximity, participants proceed to the treatment stage where we vary the *observability* of the Sender's decision and the *timing* of when the Receiver is informed about it. Subsequently, we begin by eliciting *stated* preference outcomes, e.g., asking participants whether they would like to register for a COVID-19 vaccination. We elicit our main *revealed* preference outcome by giving participants the option of actually registering for a COVID-19 vaccination using Bavaria's official online registration platform and subsequently return to our survey, where we verify their registration.[4]

Our experimental setting allows us to rule out various alternative explanations including (i) social image effects, (ii) experimenter demand, (iii) strategic lying, and (iv) cheating. First, we can hold social image effects constant since we identify anticipated peer effects by comparing the share of participants who registered for a COVID-19 vaccination between the *"informing partner before"* and the *"informing partner after"* conditions. Senders in both conditions are subject to being judged by their partner, but only Senders in the *"informing partner before"* condition should infer that they can influence their partner during the experiment. Moreover, a comparison of the *"informing partner after"* condition with the baseline condition *"not informing partner"* reveals no difference in registration shares between those two groups, suggesting that social image effects only play a minor role in our setting, as expected due to our experiment's highly anonymous and one-shot peer interaction. Second, experimenter demand is

[3]In a related lab experiment, Karlan and McConnell (2014) use a similar set of experimental manipulations to tease out the impact of leading by example on prosocial behavior. Yet, in their experiment neither the interaction between Senders and Receivers nor the decision taken by Senders are one-shot in nature. This limits the ex-ante potential for identifying anticipated peer effects since Senders in the *informing partner after* condition may also anticipate that they will influence their partner's behavior after the experiment.

[4]We verify registrations by asking participants to provide us with specific information from the official confirmation email they would have received from the Bavarian health authorities. To motivate participants to take on these additional time costs, we incentivized the verification via a lottery of Amazon vouchers. An analysis of timing patterns reveals that participants whose registration we verify in this way did not complete the registration process only after they learned that they could win a voucher: on average they were inactive in our survey for six minutes at the stage where we expected them to register (before learning about the incentive) – a realistic duration for completing the official online registration – while they were much faster in providing the verifiable information. These timing patterns did not differ between the relevant treatment conditions as documented in Figure 2.6 and the corresponding Table 2.B.5 in Appendix 2.B.

unlikely to play a major role in our setting: we show that *stated* registration willingness does not differ between the *"informing partner before"* and the *"informing partner after"* conditions, while *actual* (verified) registration willingness clearly does. This renders it unlikely that an *"informing partner before"*-specific experimenter demand effect biases our main result on the revealed-preference outcome; instead, moving from *stated* to *actual* willingness seems to weed out a general experimenter demand effect regarding registration willingness present in all experimental conditions. Third, this comparison between *stated* and *actual* registration willingness shows that strategic lying does not drive our results: Senders in the *"informing partner before"* condition do not merely pretend to register; instead, they actually follow through, arguably to signal the benefits of taking the vaccine to their partner. Finally, we show that differential incentives to cheat are unlikely to explain our findings, addressing two variants of this concern: either Senders in the *"informing partner before"* condition only signed up after they had learned about the monetary incentives for verifying their registration status; or, Senders in this condition were more likely to tap other sources, e.g., the internet, to pass our verification procedure. Using data on how much time Senders spent on each survey page, we demonstrate that neither of these alternative explanations challenge our findings.

Heterogeneity analyses further support our interpretation that Senders in the *"informing partner before"* condition internalize an anticipated peer effect and that this constitutes a prosocial motive: our main treatment effect of interest – the difference in registration shares between Senders in the *"informing partner before"* and the *"informing partner after"* conditions – is positive and significant for Senders who have a strong pre-treatment belief in the safety and efficacy of COVID-19 vaccines, while it is negative among those who do not believe in the vaccines' safety and efficacy. This result suggests that Senders who can influence their partner indeed choose to lead by example, signalling what they believe is best for the other.[5] This interpretation is further reinforced by the finding that the strength of the treatment effect seems to increase with the level of social proximity between Sender and Receiver: Senders who can influence their partner are slightly more likely to sign up for a vaccination when they "feel closer" to their partner.

In a final set of results, we document that Senders are not successful in influencing their partners – contrary to their own anticipation. Receivers who are informed about their Sender's registration decision *before* they can decide themselves are no more likely to make the same choice as their partner than those who learn about their Sender's choice only *after* their own decision. This finding can be explained by our

[5]In our specific vaccination registration setting, Senders' beliefs about the vaccination certainly influence their view on which decision – to register or not – would entail a positive versus a negative externality if their partner followed suit.

specific setting in which Senders do not observe Receivers' decisions, implying that Senders cannot verify whether they indeed exert a peer effect. Moreover, social image effects – a potential channel for conformity – play no role on the Receivers' side, as Senders do not observe their decision. We can learn from this result, however, that there are important decision environments in which individuals overestimate their anticipated peer effect; hence, the materialization of an "actual" peer effect is not always necessary to activate people's desire to lead by example.[6]

In sum, our findings indicate that anticipated peer effects can play a substantial role in decision settings with a prosocial component, i.e., where our actions generate positive externalities if those around us follow suit. This has implications for policy: where anticipated peer effects are a meaningful behavioral motive, policymakers wishing to encourage prosocial behavior may have another "social carrot" at their disposal – leveraging our desire to lead by example – and need not revert to "social sticks". For example, as an alternative to imposing a fine on citizens who miss their vaccination appointment, governments may also target people's willingness to set a good example in order to increase vaccination rates.[7] At a more general level, our findings suggest that anticipated peer effects may constitute one potential channel through which social change propagates: if a desire to lead by example – perhaps based on an overestimation of our impact on others – motivates us to bear the private costs of prosocial behavior, such as participating in a protest for civil liberties in an autocracy, anticipated peer effects may add to our understanding of why people, despite the low chances of being pivotal, are willing to accept these private costs.

The idea that individuals incorporate into their decision making an anticipation of how their behavior might influence others has been an implicit theme in a wide variety of papers in economics. To our knowledge, however, the present paper is the first to offer field experimental evidence that people act upon a desire to lead by example in a prosocial setting. The paper most closely related to ours is a lab experiment by Karlan and McConnell (2014), who hypothesize – in a similar vein as we do – that a desire to influence others might be one reason why donations are higher under observability. Based on a comparable design they find that a desire to influence others does *not* seem to increase donations. As the authors point out themselves, however, their study does not offer "dispositive evidence" due to a lack of precise null effects as well as concerns regarding the external validity and particular features of their lab setting,

[6] That anticipated peer effects influence Senders' behavior despite their partners not following suit implies that Senders have out-of-equilibrium beliefs. Due to reasons of statistical power we have to limit ourselves to documenting these misperceptions in this paper. However, we believe that the endogenous formation of such misperceptions constitutes an interesting avenue for future research.

[7] Recently, policy makers in Germany discussed whether to introduce fines for citizens who missed their COVID-19 vaccination appointments (see, e.g., media coverage by the Süddeutsche Zeitung: https://bit.ly/3zuoQO9, last accessed August 24, 2021).

e.g., the difficulty of ruling out future interactions between subjects drawn from the same peer group. We benefit from these insights and explicitly design our experiment as a one-shot interaction between two anonymous partners in a consequential decision environment, thereby limiting Senders' ability to influence their partner to one specific moment.

Our research also relates to a large body of literature that studies whether and under which conditions leading by example is successful in increasing contributions in public goods games played in the lab.[8] For example, Potters et al. (2007) show that leading by example increases public goods contribution under asymmetric information, i.e., when an informed leader can signal information about the value of contributing to an uninformed follower, a result consistent with earlier theoretical work (e.g., Hermalin 1998 and Vesterlund 2003). Leading by example "works" since followers are very likely to copy the decisions of leaders and leaders tend to correctly anticipate followers' responses, contributing more themselves. Our paper shares with this literature the idea that leaders anticipate that their behavior might influence the behavior of followers. A key difference in these public goods games is, however, that leaders always have a first-order monetary incentive to lead by example: their own monetary payoff is higher if they convince others to follow. We complement this literature by abstracting from such first-order incentives and adding an explicit focus on studying the desire to lead by example in a prosocial field setting.

Finally, we speak to a prominent literature on social signaling in the context of prosocial behavior. At least since the seminal contribution by Bénabou and Tirole (2006), a large body of literature in economics has highlighted that individuals' prosocial behavior depends on the visibility of their actions to others and that social image effects are an important motive explaining these behavioral responses.[9] Early theoretical predictions have been confirmed by a series of field experiments investigating social image effects (Bursztyn, Fujiwara, et al. 2017; Bursztyn and Jensen 2017; DellaVigna, List, and Malmendier 2012; DellaVigna, List, Malmendier, and Rao 2017; Perez-Truglia and Cruces 2017). We add to this literature by highlighting a social signaling motive that is distinct from social image concerns, insofar as it depends on the anticipation that our behavior can have a peer effect on others. Our paper is thus also related to the study of social signaling in the context of childhood immunization by Karing (2021), in which she highlights the distinction between social signals as transmitters of information about others' actions on the one hand and as a means to signal one's type on the other. Our results have a similar potential for informing policymakers aiming

[8] Important contributions include Arbak and Villeval (2013), Cappelen et al. (2016), Dannenberg (2015), Drouvelis and Nosenzo (2013), Gächter, Nosenzo, et al. (2012), Gächter and Renner (2018), Güth et al. (2007), Haigner and Wakolbinger (2010), and Potters et al. (2007).

[9] In a related earlier model, Bernheim (1994) argues that people's status concerns can generate conformity of behavior.

to promote prosocial behavior, e.g., by increasing timely vaccination take-up. The results from our anonymous setting indicate that such policies need not conflict with privacy concerns: revealing anonymous information may suffice to facilitate prosocial behavior via anticipated peer effects.

This paper is organized as follows: in Section 2.2, we discuss our experimental design employed to identify anticipated peer effects in a prosocial setting. Then, in Section 2.3, we present our main results from the experiment and address potential concerns about our findings. Section 2.4 concludes the paper.

2.2 Experimental Setup

The objective of our experimental design is to separate two complementary motives why people are more inclined to act prosocially if they are observed by others: anticipated peer effects and social image effects. In this section, we illustrate our experiment's setting and the sample employed and discuss the main features of our experimental design. In the final subsection, we show that Senders' predetermined characteristics are balanced across treatment conditions.[10]

2.2.1 Setting and sample

We conduct a survey-based online field experiment studying decision making in the context of COVID-19 vaccinations in the German state of Bavaria. We examine individuals' willingness to register for a COVID-19 vaccination via the state-wide central appointment allocation system BayIMCO, which at the time of the experiment in April 2021 constituted the only pathway for obtaining a vaccination in Bavaria.[11] Owing to vaccine supply shortages, which prevailed until ca. July 2021, the official vaccination regulations categorized individuals into several priority groups depending on their age and pre-existing health conditions. However, all Bavarian residents had the possibility to register online[12] from January 2021 onwards, regardless of their prioritization status. Once vaccine supply and their prioritization status allowed, registered residents received a vaccination appointment through the central system.

We recruited the participants for our survey from the professional online panel provider CINT. During our experiment's field time, approximately 15 percent of the

[10] We pre-registered all features of our experimental design at the AEA RCT registry under ID AEARCTR-0007437 before the experiment commenced. The experiment described here was approved by the Ethics Committee of the Department of Economics at LMU Munich, protocol 2021-01. For the technical implementation of our online experiment, we used the open-source software oTree (Chen et al. 2016).

[11] Later in the vaccination campaign, the central system was complemented by a decentralized system relying on local doctors' offices. However, as of July 2021, the central system still accounts for roughly 60 percent of all vaccinations in Germany (Bundesministerium für Gesundheit 2021).

[12] Only in exceptional cases was registration via phone also possible.

Table 2.1: Summary statistics for full sample (Senders and Receivers)

Statistic	Mean	St. Dev.	Min	Max	N
Demographics					
Age	40.90	14.35	18.00	79.00	1,857
Female	0.55	0.50	0.00	1.00	1,857
Monthly household income (net)	2,907.78	1,597.37	1,100.00	7,500.00	1,857
Upper secondary degree	0.39	0.49	0.00	1.00	1,857
Local characteristics*					
Mean incidence rate (second wave)	138.73	40.67	65.64	301.07	1,857
Population in zip	14.81	9.85	0.60	48.05	1,857
Lives in urban area (≥100k)	0.29	0.46	0.00	1.00	1,857
Turnout in 2017	77.52	4.31	59.90	90.20	1,857
AfD vote share in 2017	12.23	3.07	5.49	26.42	1,857
Unemployment rate	2.37	0.93	0.05	5.50	1,857
Beliefs about vaccine					
Safety	3.41	1.96	1.00	7.00	1,857
Efficacy	3.77	1.96	1.00	7.00	1,857
Social desirability	3.62	2.25	1.00	7.00	1,857
Severity of freerider problem	3.26	2.07	1.00	7.00	1,857
Willingness to take vaccine in state (%)	59.11	20.16	0.00	100.00	1,857
Preferences					
Own willingness to take vaccine (%)	51.31	37.09	0.00	100.00	1,857
Altruism	0.01	0.83	−1.96	2.25	1,857
Desire to influence	0.08	0.98	−2.90	1.72	1,857
Social image concern	0.03	1.00	−1.81	2.34	1,857
Social proximity					
Social proximity	0.02	1.00	−1.04	2.27	1,526

Notes: Variables marked with * vary on the zip code, county ("Landkreis") or town ("Gemeinde") of residence level and not on the individual level.

Bavarian population had already received at least one vaccination and a further 30 percent had registered in the central system. We exclude both of these groups from our experiment by screening them out at the start of our survey. In total, 1,857 participants completed our experiment. We report summary statistics on participant characteristics in Table 2.1: 51 percent of our participants were willing to get vaccinated (elicited pre-treatment), which is – due to our exclusion of already vaccinated and registered individuals – somewhat lower than the vaccination willingness of 65 percent elicited in a nationally representative study at the same point in time (Betsch et al. 2020; COSMO – COVID-19 Snapshot Monitoring 2021). With respect to other key characteristics such as gender, age, and income, our study participants are suitably representative of the Bavarian population as a whole.[13]

[13]Roughly half of our sample is female; mean age and monthly net income are 40.9 years and €2,907, respectively, compared to the official state averages of 43.7 years in 2017 (Bayerisches Landesamt für Statistik 2019) and €2,549 in 2018 (GESIS – Leibniz-Institut für Sozialwissenschaften 2019).

2.2.2 Experimental design

Our experiment revolves around the interaction within teams consisting of one *Sender* and one *Receiver* and aims to isolate anticipated peer effects from social image effects. It evolves over seven stages, which we detail below.[14]

1. Introduction We begin by screening out all subjects who had already been vaccinated or registered for a COVID-19 vaccination. From all remaining participants we collect basic demographic information as well as a rich set of attitudes, beliefs, and preferences related to the vaccination (e.g., beliefs about vaccine safety and efficacy).

2. Joint problem solving task Subsequently, we build teams consisting of two randomly paired participants. Within teams, subjects are randomly assigned either to the role of Sender (she) or Receiver (he). Before teams enter the main stage of the experiment, they work on a joint problem solving task adopted from Goette and Tripodi (2020), which we use to induce social proximity between the partners, i.e., to establish the Receiver as a relevant peer to the Sender. The task consists of four consecutive questions, in which teams are presented with historical paintings and are asked to select the corresponding artist from a list. Each correct answer increases participants' probability of winning an Amazon voucher, but only if their partner selects the correct artist as well. To allow for coordination between partners, we provide them with the option to exchange text messages.[15] Participants are informed as to whether they won any of the vouchers on the final page of the survey.

3. Social proximity After the joint task, we elicit a measure of social proximity between partners using the "oneness" scale (Cialdini et al. 1997; Gächter, Starmer, et al. 2015), again following Goette and Tripodi (2020).[16] We find that the joint problem solving task performs well in establishing social proximity between partners: according to this scale, at least half of the participants perceive their partner in the experiment as an "acquaintance" and 25 percent even think of their partner as a "non-close friend" (for details on the scale, see Gächter, Starmer, et al. 2015).

4. Treatment Next, teams enter the experiment's treatment stage, where we use two experimental manipulations to isolate the impact of anticipated peer effects on Senders' decisions to sign up for a vaccination: we vary (1) the observability of the Sender's decision to her partner and (2) the timing of when the partner is informed about the Sender's decision.

[14] We provide the complete survey instrument in Appendix 2.D.

[15] We provide a screenshot of the joint problem solving task showing the chat window in Figure 2.C.1 in Appendix 2.A.

[16] The oneness scale is computed as the unweighted mean of the Inclusion of Other in the Self (IOS) scale (Aron et al. 1992) and the (ii) WE scale (Cialdini et al. 1997). We provide screenshots of how we elicited the oneness scale in Figure 2.C.2 in Appendix 2.C.

The main intuition of our design is illustrated in Figure 2.1. For each treatment condition, we report the key treatment message shown to the Sender and the corresponding decision sequence as implemented in the experiment. Irrespective of the condition to which we assigned teams, Senders were always offered the opportunity to sign up for the vaccination before the Receiver and no Sender learned about the decision of the Receiver.

In the *"not informing partner"* condition, we inform Senders that their decision on whether to register for a vaccination will not be reported to their partner. As a result, neither anticipated peer effects nor social image effects affect Senders' decisions.

In the *"informing partner after"* condition, Senders learn that their decision will be shared with their partner. However, we highlight to Senders that their partner will only be informed about their decision once he (the partner) has himself already decided whether to register. Therefore, while social image effects might arise, Senders cannot influence their partner's decision within the experiment and, consequently, anticipated peer effects should play no role in this condition.

In the third and final condition, *"informing partner before"*, we inform Senders that their partner will learn about their decision *before* he is given the opportunity to register for a vaccination. As above, Senders in this condition are subject to social image effects. In addition, however, they should infer that they can now influence their partner's decision. More formally, in the present condition Senders' beliefs about the likelihood of exerting a peer effect on their partner should, in expectation, be higher than in the *"informing partner after"* condition. Hence, by comparing Senders' willingness to sign up for the vaccination between Senders who can and those who cannot influence their partner, we can isolate anticipated peer effects from social image effects.

5. First stage As laid out above, the strength of anticipated peer effects is governed by Senders' beliefs about how likely it is that they can influence their partner's decision. As such, changes in this particular belief constitute the "first stage" of our experiment. To measure whether our experimental manipulations indeed induce an upward shift in this first-stage belief, we ask Senders how likely they think it is that they can influence their partner's decision of whether to sign up for the vaccination. To elicit this belief, we use a slider ranging from 0 to 100. We pose this question to Senders after the treatment module and before eliciting the main outcome.

6. Main outcome Next, we elicit our main outcome by asking participants whether they wished to sign up for a COVID-19 vaccination right away. If participants answered "yes", they were forwarded to the BayIMCO website outside of our survey.[17]

[17] The official registration website (BayIMCO) provided by the Bavarian Ministry of Health can be accessed at https://impfzentren.bayern/citizen/. We provide screenshots illustrating how we elicit and verify the registration decision in Appendix 2.C.3.

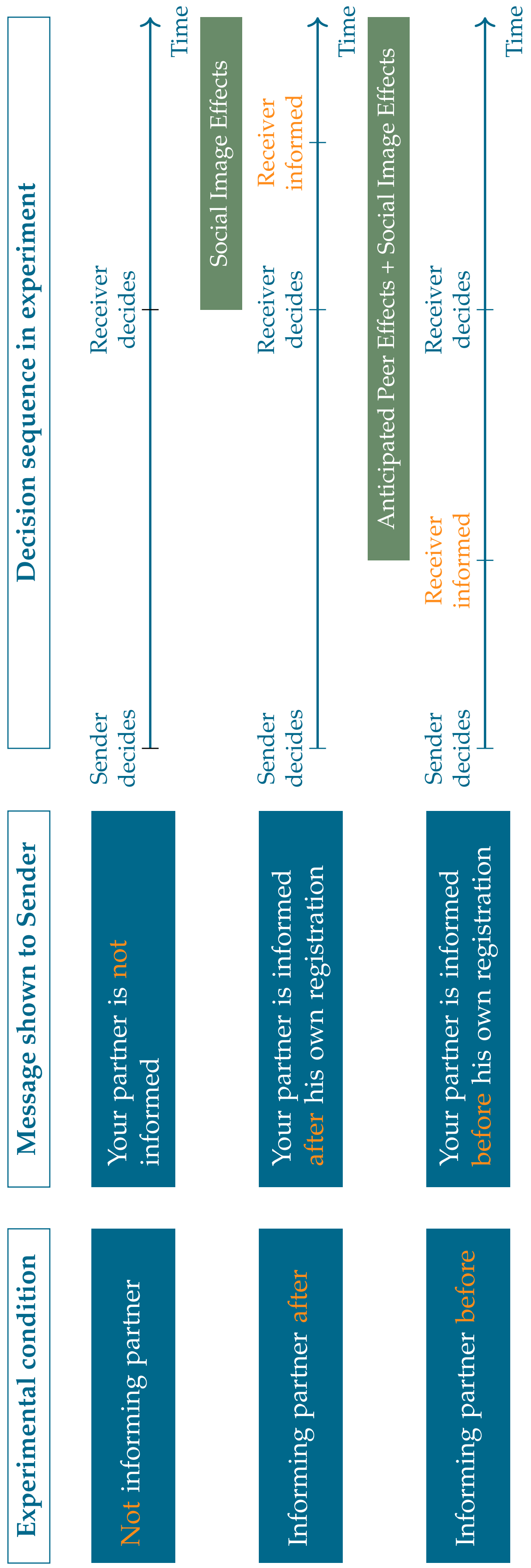


Figure 2.1: Treatment messages and corresponding implementation in the survey

Participants who responded "no" were forwarded to the next page of our survey. On average, it took participants in our experiment five to six minutes to complete the online registration form. Once participants completed the form, they obtained an email from BayIMCO officially confirming their registration. We use this confirmation email to verify whether participants indeed registered for a vaccination by asking them to enter the sending address and the subject line of the confirmation email in a survey form.[18]

The timing of the steps we used to elicit whether participants actually signed up for the vaccination is crucial in this context: when we offered participants the opportunity to sign up for the vaccination, participants did not know that we would ask them to provide proof of their registration. We only informed participants about the confirmation and the corresponding remuneration only after they had reported to us that they successfully completed the registration. Hence, participants did not have an incentive to misreport their registration in order to qualify for one of the vouchers. Still, one may worry that participants misreporting their registration status tried to find out the address and the subject line of the confirmation email to nevertheless qualify for one of the vouchers. It is, however, very unlikely that participants successfully managed to cheat, since the address from which the confirmation email was sent changed over time. Thus, even if participants found a screenshot of the confirmation email by searching the Internet, the screenshot had to be fairly recent to keep up with the changes of the confirmation email over time.[19]

7. Further outcomes Finally, we collect post-treatment attitudes and beliefs related to the COVID-19 vaccination, including participants' stated willingness to take the vaccine alongside with their beliefs regarding the safety and efficacy of the vaccine, its social desirability, and associated free riding problems.[20] In addition, we collected further demographic information including income, education, county and zip code of residence. On the final page of the survey, we revealed payoffs to participants and provided them with the opportunity to comment on the survey.

[18]We incentivized participants by informing them that by reporting both pieces of information correctly they would qualify for one of 30 additional €20 Amazon vouchers. Once participants had entered their information, their responses were checked by our system. If both answers were correct, a lottery determined whether participants obtained one of the Amazon vouchers. Participants only learned whether they had won any of the Amazon vouchers after they had answered all questions, i.e., on the final page of the survey.

[19]As we detail in Section 2.3.4, we find no differential indication of Senders successfully bypassing our verification process or completing their registration only after they had learned about the vouchers between experimental conditions.

[20]We also collect the same set of beliefs before the treatment to analyze within individual changes arising from the treatment.

2.2.3 Additional steps taken to identify anticipated peer effects

In order to identify anticipated peer effects, our design aims to maximize the difference in Senders' beliefs about their ability to influence their partner between the "*informing partner before*" and "*informing partner after*" conditions. To achieve this, we designed both the decision Senders take as well as the interaction with their partner to be "one-shot". To ensure that the interaction is one-shot in nature, we paired individuals who had never met before and upheld anonymity throughout the experiment. Anonymity facilitates identifying anticipated peer effects as it limits Senders' chances of influencing their partner to that particular encounter: Senders in the *"informing partner before"* condition should realize that their opportunity to influence their partner's decision is either now, by sending a signal in the experiment, or never. Of course, Senders' decisions within the experiment may influence Receivers' behavior after the experiment has ended, as Receivers may contemplate their partner's decision in the experiment for a while and register for a vaccination at some later point in time. In principle, Senders in the *"informing partner after"* condition may realize as well that their actions during the experiment might influence Receivers' behavior *after* the experiment. If that was the case, anticipated peer effects would also motivate Senders in this condition, potentially attenuating behavioral differences relative to the *"informing partner before"* condition.[21]

As a further step towards identifying anticipated peer effects, we deliberately limited the scope for social image effects from the onset. To this end, Senders in our experiment interacted with individuals they had never met before and not with their neighbors (as in Bursztyn, González, et al. 2020) or classmates (as in Bursztyn and Jensen 2015). Our design further limits the potential impact of social image effects by informing only one individual about the Sender's decision. In existing paradigms, the number of Receivers is usually much larger (e.g., in Perez-Truglia and Cruces 2017). Ultimately, by upholding anonymity throughout the experiment, we rule out future interactions between partners and thereby shut down most instrumental motives underlying social image effects.[22] Taken together, we expect only a weak impact of social image effects on Senders' behavior in this particular context.

[21]Moreover, the fact that the decision itself – and thus its potential externality on the Receiver – is one-shot, may render it more salient from the perspective of the Sender. Combined, the one-shot decision and the one-shot interaction help us identify anticipated peer effects. The role of these design features also suggests a reason why Karlan and McConnell (2014) – who used a similar set of experimental manipulations – did not find evidence for anticipated peer effects: to conduct their experiment, they recruited participants from the same peer group (college students from the same university). As a result, Senders might have already known Receivers and might have anticipated to meet them again in the future, reducing the relative importance of the signal sent within the experiment. A similar logic applies to the decision they studied: they asked Senders to decide about a donation to a university institution, a decision which Senders could take multiple times in the future.

[22]See Bursztyn and Jensen (2017) for a discussion of the distinction between instrumental and hedonic motives underlying social image effects.

2.2.4 Experimental assignment and sample balancing

We used a two-stage random procedure to assign participants into treatment conditions: first, we assigned teams to one of the three treatment conditions *"not informing partner"*, *"informing partner after"*, or *"informing partner before"*. Second, within the teams, we further randomized who was assigned the role of Sender and Receiver, respectively. We report the resulting assignment into experimental conditions in Table 2.2.[23]

Table 2.2: Number of Senders and Receivers assigned to each group

Condition	Treatments	Senders	Receivers
(1) **Not** informing partner	Observability = 0	328	–
(2) Informing partner **after**	Observability = 1 Informed before = 0	554	236
(3) Informing partner **before**	Observability = 1 Informed before = 1	519	220

Since we are primarily interested in Senders' decisions, we opted for an implementation using fewer Receivers than Senders in each group: in some teams a Sender's partner was another Sender and not a Receiver. To avoid deception, the experimental instructions thus involved a degree of uncertainty regarding whether a participant's decision would be shared with their partner. Therefore, we could use the same experimental instructions for all Senders in the same condition irrespective of whether Senders' partner was another Sender or an "actual" Receiver, while still only employing factually true information.[24] To further reduce the number of Receivers in our experiment, we paired Senders in the *"not informing partner"* condition always with other Senders. Since Senders' decisions in this condition were not shared with their partner from the joint problem solving task anyways, these Senders' partners could also be other Senders without introducing deception.

To assess whether Senders' predetermined characteristics are balanced across experimental conditions, we conducted pairwise comparisons of 21 predetermined characteristics across all three conditions using bivariate regressions.[25] In Table 2.3, we

[23]The discrepancy between the number of participants in the *"informing partner after"* and *"informing partner before"* conditions is an artefact of the specific randomization procedure used. We used "on the fly" randomization to assign participants into experimental conditions as they entered the survey. Due to the random nature of the assignment process, the effective share in each condition slightly deviates from the target shares we specified in our pre-analysis plan.

[24]To be precise, we informed Senders that their partner would learn about their decision only "with high probability".

[25]We use regressions of the following form to compare predetermined characteristics between pairs of conditions: $characteristic_i = \alpha + \beta \cdot treat_i + \epsilon_i$, where $treat_i$ is a dummy variable corresponding to either the *"informing partner after"* or the *"informing partner before"* condition, and where we omit one condition

Table 2.3: Sender's predetermined characteristics compared across treatment conditions

	Group means			Test for equal means: p-values			
	Before	After	Not	Before vs. After	Before vs. Not	After vs. Not	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Attrition							
Completed survey	0.73	0.74	0.76	0.87	0.24	0.30	1892
Demographics							
Age	40.67	41.36	40.43	0.43	0.82	0.36	1401
Female	0.56	0.54	0.52	0.42	0.23	0.61	1401
Monthly household income (net)	2846.82	2850.90	2990.55	0.97	0.21	0.21	1401
Upper secondary degree	0.37	0.40	0.40	0.43	0.41	0.88	1401
Local characteristics							
Avg. incidence rate (during second wave)	138.37	140.61	136.96	0.37	0.61	0.20	1401
Population in zip	14.21	15.17	14.91	0.11	0.29	0.71	1401
Lives in urban area ($\geq$100k)	0.29	0.32	0.31	0.31	0.44	0.91	1401
Turnout (%)	77.42	77.60	77.52	0.50	0.74	0.79	1401
AfD vote share (%)	12.29	12.17	12.18	0.55	0.64	0.96	1401
Unemployment rate (%)	2.36	2.41	2.37	0.40	0.91	0.53	1401
Beliefs							
Safety	3.37	3.40	3.42	0.80	0.69	0.86	1401
Efficacy	3.76	3.73	3.76	0.84	0.99	0.87	1401
Social desirability	3.58	3.64	3.62	0.69	0.81	0.92	1401
Severity of freerider problem	3.29	3.10	3.40	0.12	0.46	0.03**	1401
Willingness to take vaccine in state (%)	58.37	58.41	59.83	0.97	0.30	0.30	1401
Preferences							
Own willingness to take vaccine (%)	50.78	51.40	49.57	0.78	0.65	0.48	1401
Altruism	-0.01	0.04	-0.01	0.34	0.97	0.39	1401
Desire to influence	0.10	0.03	0.10	0.24	0.99	0.30	1401
Social image concerns	0.04	0.02	0.02	0.77	0.80	0.99	1401
Social proximity							
Oneness	-0.03	0.07	-0.04	0.13	0.92	0.14	1140
Test for joint significance							
				0.59	0.93	0.44	

Notes: Group means of Senders' predetermined characteristics alongside p-values testing for equal means reported. p-values are derived from the following regressions comparing predetermined characteristics between pairs of conditions: $characteristic_i = \alpha + \beta \cdot treat_i + \epsilon_i$, where $treat_i$ is a dummy variable corresponding to either the "*informing partner after*" or the "*informing partner before*" condition, and where we omit one condition from our sample for every pair-wise comparison. *Not* refers to the *not informing partner* condition. All variables classified as "local characteristic" do not vary on the individual but on the zip code or town ("Gemeinde") of residence level. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

report the group means separately for each condition alongside the p-values obtained from these regressions.[26] Out of the 63 estimates reported in Table 2.3, only one is significant at the 5 percent level, suggesting that Senders' predetermined characteristics are well balanced across treatment conditions.[27] These results thus minimize the risk of wrongly attributing potential treatment effects to our experimental manipulations rather than to pre-existing differences.

from our sample for every pair-wise comparison.

[26]We report the corresponding balancing table for Receivers in Table 2.B.6 in Appendix 2.B.

[27]This is supported by the p-values obtained from tests for joint significance of all predetermined characteristics reported at the bottom of the table.

2.3 Empirical Analysis

In this section, we first discuss our empirical strategy to separate anticipated peer effects from social image and other behavioral motives. Subsequently, we document that our experimental variation generated the desired first-stage effect, i.e., it manipulated Senders' beliefs about the likelihood of exerting a peer effect. After discussing our main result – that anticipated peer effects more than doubled Senders' likelihood of signing up for a COVID-19 vaccination – we move on to addressing several potential concerns including experimenter demand, strategic lying and cheating. We also corroborate our interpretation that Senders acted upon a prosocial anticipated peer effect mediated by the possibility of influencing their partner in a heterogeneity analysis and a 2SLS estimation. We end this section by documenting that Senders' anticipated peer effects did not translate into "actual" peer effects, i.e., behavioral changes among Receivers.

2.3.1 Regression specification

To identify experimental treatment effects, we estimate the following regression model:

$$y_i = \beta_0 + \beta_1 \cdot \text{informing partner}_i + \beta_2 \cdot \text{informing partner before}_i + \boldsymbol{X_i}\,\gamma' + \epsilon_i \quad (2.1)$$

where y_i corresponds to the relevant outcome of interest for Sender i. In our main specifications, y_i is a dummy variable indicating whether Sender i registered for a COVID-19 vaccination and could provide proof of her registration. When testing for the first-stage effect of our experiment, we instead use Sender i's belief about the likelihood of her being able to influence her partner as the outcome variable y_i. In alternative specifications, we also consider Sender i's self-reported registration status and willingness to take the vaccine as well as further measures of her decision to sign up for the vaccination, such as whether Sender i clicked on the link forwarding participants to the BayIMCO website, as dependent variables.

The variables *informing partner*$_i$ and *informing partner before*$_i$ capture the impact of our two experimental manipulations. First, *informing partner*$_i$ is an indicator variable taking value 1 if Sender i learned that we would report her registration decision to her partner in the experiment. Second, *informing partner before*$_i$ takes value 1 if Sender i learned that her partner would be informed about her registration decision *before* her partner himself would have the opportunity to sign up for the vaccination. When using both indicators *informing partner*$_i$ and *informing partner before*$_i$ simultaneously as specified in Equation 2.1, β_1 captures the social image effect and β_2 the additional anticipated peer effect that only occurs if a Sender's partner was informed *before* rather than *after* his own registration decision. Finally, in some specifications we include control variables: $\boldsymbol{X}_i$ is a vector that includes Senders' predetermined characteristics.[28]

2.3.2 First stage

We first test for the presence of the intended first-stage effect: are Senders' beliefs about the likelihood of exerting a peer effect on their partner shifted upwards if they learn that we will inform their partner *before* rather than *after* the partner has the opportunity to register for the COVID-19 vaccination? The left-hand panel of Figure 2.2 reports the mean belief in each of the three experimental conditions. When we compare the upper (*"not informing partner"*) to the middle bar (*"informing partner after"*), we find that Senders in the latter group are slightly more likely to think that they can influence their partner. This is consistent with the idea that these Senders anticipate that their decision in the experiment might influence Receivers' registration behavior *after* the survey, even though their signal arrives too late for Receivers' decisions *within* the experiment.[29] More importantly, however, when contrasting the middle with the lower bar (*"informing partner before"*), we discover that Senders who learned that their partner was informed *before* rather than *after* are significantly more likely to believe that they can exert a peer effect on their partner.

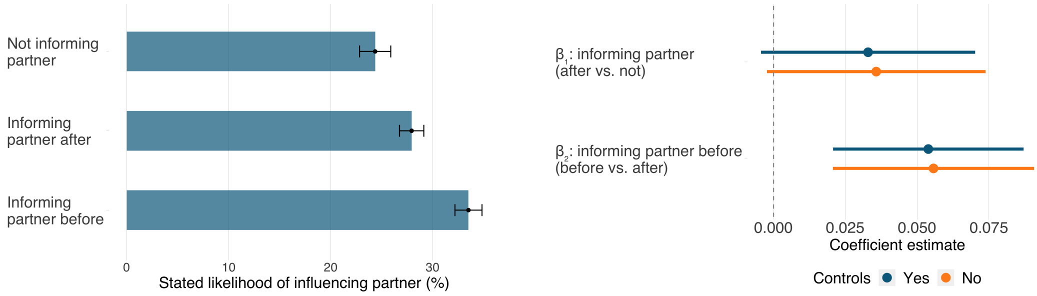
This finding is confirmed by our regression results depicted in the right-hand panel of Figure 2.2. In this figure, we report coefficient estimates and the corresponding 95-percent confidence intervals which we obtained from regressions following the specification depicted in Equation 2.1. Here, we employ Senders' beliefs about the likelihood of exerting a peer effect on their partners as the dependent variable. We present results from regressions both with and without controls.[30] The upper pair of coefficients in Figure 2.2 depicts our estimates for β_1 which corresponds to the difference between the upper and the middle bar in the left-hand panel of the figure. Our estimates for β_1 range from 3.29 (with controls, se: 1.90) to 3.58 (without controls, se: 1.94) percentage points and are both significant at the 10 percent level, corresponding to a 13 percent increase over the likelihood stated by Senders whose partners were not informed.

We estimate even larger treatment effects for β_2 (the lower pair of coefficients) which correspond to the difference in first-stage beliefs between the middle and the lower bar. Our estimates for β_2 range from 5.39 (se: 1.69) to 5.57 (se: 1.79) percentage points

[28] We use all Sender characteristics reported in Table 2.1 with the exception of our measure of social proximity as control variables. We exclude social proximity due to a number of missing observations for this measure from participants who have skipped the corresponding survey items. Our results do not change when including it as an additional control variable.

[29] Arguably, if all Senders in the *"not informing partner"* condition had fully understood the experimental instructions, they should have reported that they cannot influence their partner at all. That this belief is not zero is likely explained by some degree of inattention among participants which is not uncommon for this type of online experiment.

[30] Full regression results are reported in columns (3) and (4) of Table 2.B.1 in Appendix 2.B.



(a) Conditional means **(b) Coefficient estimates**

Figure 2.2: Treatment effects on Senders' first-stage beliefs

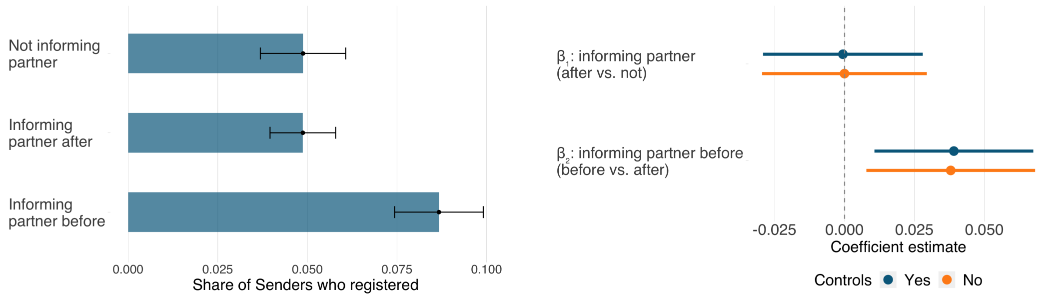
Notes: Panel (a) plots Senders' mean stated likelihood of influencing their partner, in percent, by treatment condition, alongside the corresponding 95-percent confidence intervals. Panel (b) plots coefficient estimates and 95 percent confidence intervals from regressions as laid out in Equation 2.1 where we employ Senders' first-stage beliefs as the dependent variable. Controls include the full set of variables reported in Table 2.1 with the exception of social proximity.

and are significant at the 1 percent level, irrespective of whether we include controls or not, corresponding to a 20 percent increase over the *"informing partner after"* condition. Taken together, these findings suggest that our experimental manipulations successfully induced the desired shift in beliefs: making Senders' decisions observable to their partners increased Senders' mean beliefs about the likelihood of being able to influence their partners. Crucially, however, informing Senders' partners before rather than after we offer them the opportunity to register induced a significant wedge in Senders' first-stage beliefs, which we leverage to isolate anticipated peer effects from social image effects.

2.3.3 Separating anticipated peer effects from social image effects

Next, we present treatment effects on Senders' likelihood to sign up for a COVID-19 vaccination. The left-hand panel of Figure 2.3 displays the share of Senders who verifiably registered across our three experimental conditions. We find that among Senders in the *"not informing partner"* and *"informing partner after"* conditions, 5 percent decided to sign up for a vaccination during our experiment. When contrasting this with Senders in the *"informing partner before"* condition, we find that Senders in this condition are roughly 80 percent more likely to register (9 vs. 5 percent).

We assess whether the differences in the share of Senders who signed up are statistically significant by running regressions of the form specified in Equation 2.1. We use a dummy variable taking value 1 for Senders who verifiably registered for a COVID-19 vaccination as the dependent variable. The upper pair of coefficients reported in Figure 2.3, right panel, corresponds to β_1 and as such captures the difference between the upper and the middle bar in the left-hand panel. Irrespective of whether we use con-



(a) Conditional means **(b) Coefficient estimates**

Figure 2.3: Treatment effects on Senders' likelihood to register for a COVID-19 vaccination

Notes: Panel (a) plots the share of Senders who registered for a vaccination and could provide proof of their registration by treatment condition, alongside the corresponding 95-percent confidence intervals. Panel (b) plots coefficient estimates and 95 percent confidence intervals derived from regressions as laid out in Equation 2.1 where we employ Senders' verified registrations as the dependent variable. Controls include the full set of variables reported in Table 2.1 with the exception of social proximity.

trols or not, we obtain precisely estimated zero effects for β_1 (se: 0.01-0.02).[31] This implies that Senders who knew that their partner was informed *after* he had obtained the opportunity to sign up for a vaccination (*"informing partner after"*) are not more likely to register than Senders who knew that their partner was not informed at all (*"not informing partner"*). Senders' decisions on whether to sign up for a COVID-19 vaccination thus seem not to be affected by social image concerns due to being observed by their partner in the experiment. In stark contrast to this, we obtain point estimates for β_2 of approximately 4 percentage points (se: 0.01-0.02), both with and without controls, which are highly significant ($p < 0.02$), implying that anticipated peer effects significantly affect Senders' likelihood to sign up.[32] These finding are confirmed by results from Fisher permutation tests summarized in Figure 2.A.1 in Appendix 2.A, which yield a p-value of 0.97 for β_1 and of 0.02 for β_2.[33]

Combined, our findings depicted in Figure 2.3 thus suggest that observability per se does not induce a change in Senders' behavior. However, once Senders can influence their partners' decision whether to sign up for a COVID-19 vaccination within the experiment, they are almost twice as likely to register themselves. In this particular setting, anticipated peer effects thus seem to explain why Senders are more likely

[31] We report full regression results in columns (3) and (4) of Table 2.B.2 in Appendix 2.B.

[32] Given the absence of difference in the share of Senders who signed up between the *"informing partner after"* and the *"not informing partner"* condition, our estimates for β_2 also correspond to an 80 percent increase over the mean in the *"not informing partner"* condition.

[33] We pre-specified both conventional t-statistics as well as permutation tests for statistical inference in our pre-analysis plan. To derive Fisher p-values, we randomly assign "placebo treatment" status to Senders in our experimental conditions in 5,000 iterations and calculate a distribution of "placebo estimates" for both β_1 and β_2. We then compare the size of the treatment effects we find using the actual treatment assignment (the "true" estimate) to the distribution of "placebo estimates".

to act prosocially if their behavior can be observed by others. The absence of social image effects and the relative strength of anticipated peer effects in this context results from the fact that the scope for social image effects was limited by design: instead of leveraging a considerable number of individuals from the Senders' peer group as Receivers as in comparable studies interested in identifying social image effects, we matched Senders with only one stranger and let them interact in a quasi-anonymous online setting without the chance of future interactions.

2.3.4 Robustness

2.3.4.1 Experimenter demand

Taking a COVID-19 vaccine, and by extension also signing up for a vaccination, is generally perceived as a socially desirable action. Thus, we expect a certain baseline level of experimenter demand effects to be present in all experimental conditions. This type of experimenter demand does, however, not constitute a potential threat to our interpretation of the findings as long as the extent of experimenter demand is uniform across conditions. Yet, if Senders in the *"informing partner before"* condition inferred with a higher probability from our instructions that our experiment's main hypothesis was to find a higher share of them signing up for a COVID-19 vaccination, our estimates would, at least partially, reflect stronger experimenter demand in the *"informing partner before"* condition.

Previous research by de Quidt et al. (2018) and Haaland et al. (2021) found that self-reported outcomes are more prone to suffer from experimenter demand effects than revealed preference outcomes since the latter impose an actual economic cost on experimental subjects. We thus compare our estimates for β_1 and β_2 between regressions where we employed our revealed preference outcome (verified registrations) as the dependent variable and those where we use one of the following self-reported outcomes: first, a dummy taking value 1 if a Sender reported that she signed up, which we elicit after participants were offered the opportunity to register for a COVID-19 vaccination via BayIMCO; second, a dummy taking value 1 if a Sender clicked on the link forwarding her to the BayIMCO website; third, a dummy taking value 1 if a Sender replied that she is planning to sign up, which we elicit after Senders saw the treatment messages, yet before we offered them the opportunity to sign up; fourth, the change in a Sender's self-reported willingness to take the vaccine from before to after the treatment.

In Figure 2.4, we plot coefficient estimates and corresponding confidence intervals for both β_1 and β_2 obtained from regressions as specified in Equation 2.1 using our full set of controls.[34] All outcomes were scaled using the corresponding mean in the *"not*

[34]Coefficients and confidence intervals are barely affected by using control variables. Yet, estimates

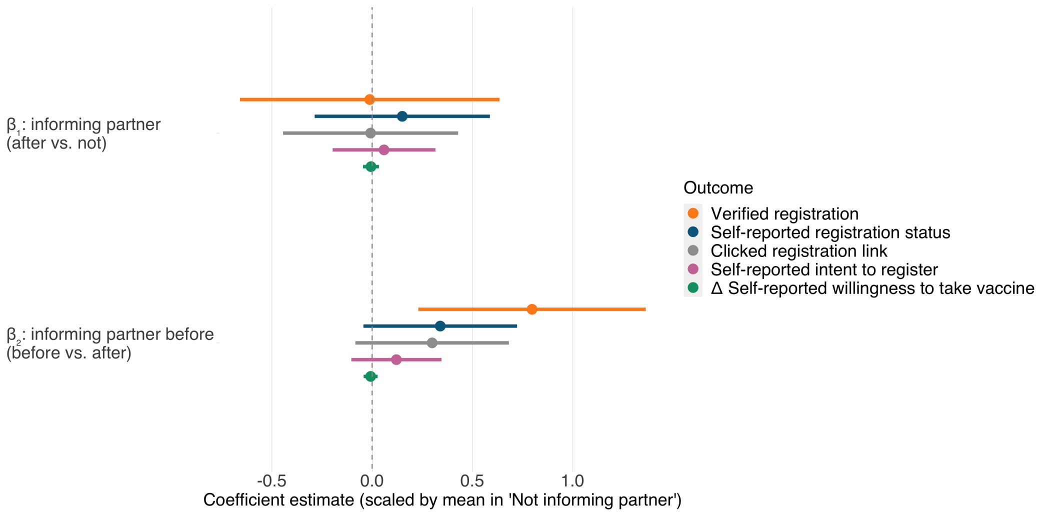


Figure 2.4: Comparing treatment effects across outcomes

Notes: Coefficient estimates derived from regressions as laid out in Equation 2.1 reported. Full set of controls reported in Table 2.1 with the exception of social proximity employed. We use the following dependent variables: (i) a Sender's verified registration status; (ii) a dummy variable taking value 1 if a Sender reported that she had registered (elicited before verification); (iii) a dummy variable taking value 1 if a Sender clicked on the registration link forwarding her to BayIMCO; (iv) dummy variable taking value 1 if a Sender's reported to be willing to register (elicited before verification); and (v) the change in a Sender's self-reported willingness to take the vaccine (pre/post treatment). All outcomes are scaled using the corresponding mean in the *"not informing partner"* condition. 95-percent confidence intervals reported.

informing partner" condition to facilitate the interpretation of coefficient sizes. Regardless of whether we look at self-reported or verified outcomes, we find fairly precisely estimated zero effects for β_1. In contrast, while we find that our estimates for β_2 are positive and highly statistically significant if we employ our revealed preference outcome (verified registrations), we obtain insignificant and considerably smaller estimates when using self-reported outcomes. For example, when employing Senders' *self-reported* registration status as an outcome, we estimate that β_2 only corresponds to about a 30 percent increase over the mean (compared to about an 80 percent increase over the mean when using verified registrations as the dependent variable). We obtain even smaller estimates for β_2 for any of the other self-reported outcomes.[35] The pattern we observe in Figure 2.4 thus suggests that our experimental manipulations did not generate additional experimenter demand in the *"informing partner before"* condition that goes beyond any baseline experimenter demand present in all conditions.

obtained from regressions using our full set of control variables are slightly more precise. Since smaller confidence intervals would, in this particular exercise, work against us when looking at self-reported outcomes, we decided to present results obtained from regressions with controls.

[35]We report full regression results in Table 2.B.3 in Appendix 2.B.

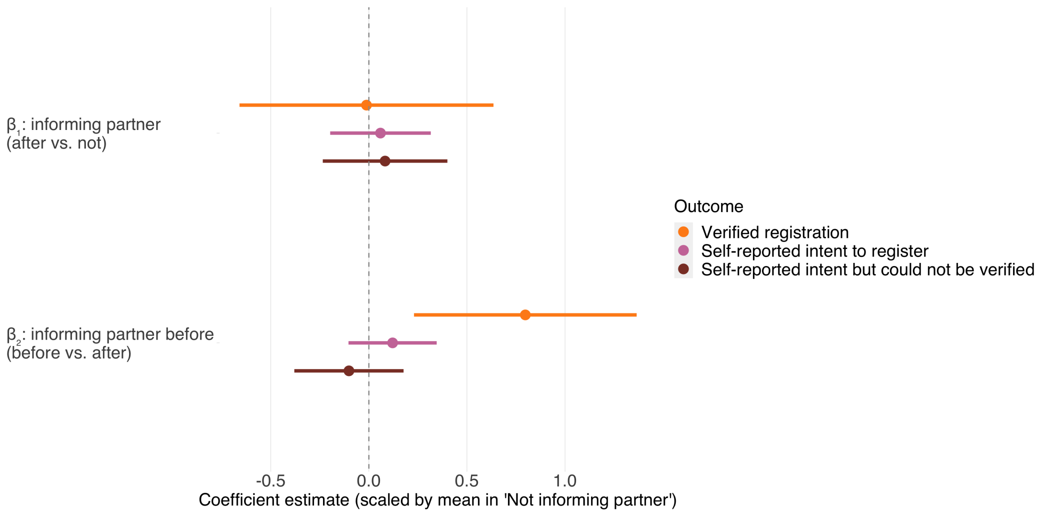


Figure 2.5: Treatment effects on "strategic" lying

Notes: Coefficient estimates derived from regressions as laid out in Equation 2.1 reported. Full set of controls reported in Table 2.1 with the exception of social proximity employed. We use the following dependent variables: (i) a Sender's verified registration status; (ii) dummy variable taking value 1 if a Sender's reported to be willing to register (elicited before verification); (iii) a dummy variable taking value 1 if a Sender who reported that shed had signed up but failed to provide proof of her registration. All outcomes are scaled using the corresponding mean in the *"not informing partner"* condition. 95-percent confidence intervals reported.

2.3.4.2 Strategic lying

A related alternative explanation is strategic lying, given that the decision of whether to take the COVID-19 vaccine, and by extension whether to sign up for a vaccination, represents a collective action problem. While the vaccine entails several important benefits, including for society, it also comes with private costs for individuals, e.g., in terms of potential side effects or opportunity costs. Therefore, Senders have an incentive to state that they are willing to register – to nudge their partner to take the vaccine – without actually following through with the registration themselves. Strategic lying poses a threat to our interpretation if the extent of such behavior is more pronounced among Senders in the *"informing partner before"* condition.

In Figure 2.5, we construct a measure of strategic lying – a dummy variable taking value 1 for Senders who reported that they had signed up but failed to provide proof of their registration – and use it as the dependent variable in regressions following the general setup specified in Equation 2.1. We compare the coefficient estimates for β_1 and β_2 obtained for this measure of strategic lying with the corresponding estimates for verified registrations and Senders' self-reported intent to register.[36] To facilitate the comparison of effect sizes across outcomes, we scaled all three outcomes with the

[36]For full regression results, please consult the corresponding Table 2.B.4 in Appendix 2.B.

respective mean in the *"not informing partner"* condition. We find fairly precisely estimated zero effects for β_1, irrespective of whether we use controls or not. In contrast, we obtain positive and highly significant estimates for β_2 when employing verified registrations as the dependent variable, whereas we find no significant coefficients when using our measure of strategic lying as the dependent variable. The estimate for β_2 is small, statistically indistinguishable from zero, and if anything, negative. It is thus not the case that Senders in the *"informing partner before"* condition are merely more likely to state that they would like to register while failing to follow through with their registration than Senders in other conditions. Taken together, anticipated peer effects seem to be a sufficiently strong behavioral motive which reflects a true preference for prosociality rather than a strategic, and thus selfish, concern.

2.3.4.3 Cheating

A third potential threat concerns differential incentives to "cheat" which may arise from differences in treatment instructions or survey items employed across experimental conditions.[37] There are two related variants of this concern: first, there may exist differential incentives to sign up for a vaccination to qualify for the additional remuneration offered to participants who passed our verification process. Second, Senders may face differential incentives to search the Internet for the information required to qualify for the remuneration.

To address this type of concern, we exploit the fact that either variant should manifest in similar patterns with respect to how much time Senders devote to each of the survey pages post treatment. We would expect Senders who cheat to spend only a short time on the survey page where the registration should have taken place and considerably more time on the survey page where they are asked to provide proof of their registration: either because they need to register for the vaccination online ex post, i.e., after we had offered them to start their registration from within the survey on the previous page, or because they need to retrieve the address and the subject line of the confirmation email from the Internet.

We compare time spent on each survey page after receiving the treatment message in Figure 2.6. We hereby focus only on Senders who could provide proof of their registration to assess the potential severity of (successful) cheating. We provide Senders the opportunity to sign up for a COVID-19 on the "Registration" page and ask them to provide verifiable information on the next page ("Confirmation"), where we also informed them about the additional remuneration.[38] Therefore, any increase in time spent due to cheating would manifest on the "Confirmation" page. Yet, contrary to

[37] In our particular setting, survey items were identical across the *"informing partner before"* and the *"informing partner after"* conditions. Thus, differential incentives to cheat must arise from slight differences in the wording of experimental instructions.

[38] Note that there was no possibility to "return" to a previous page throughout the entire survey.

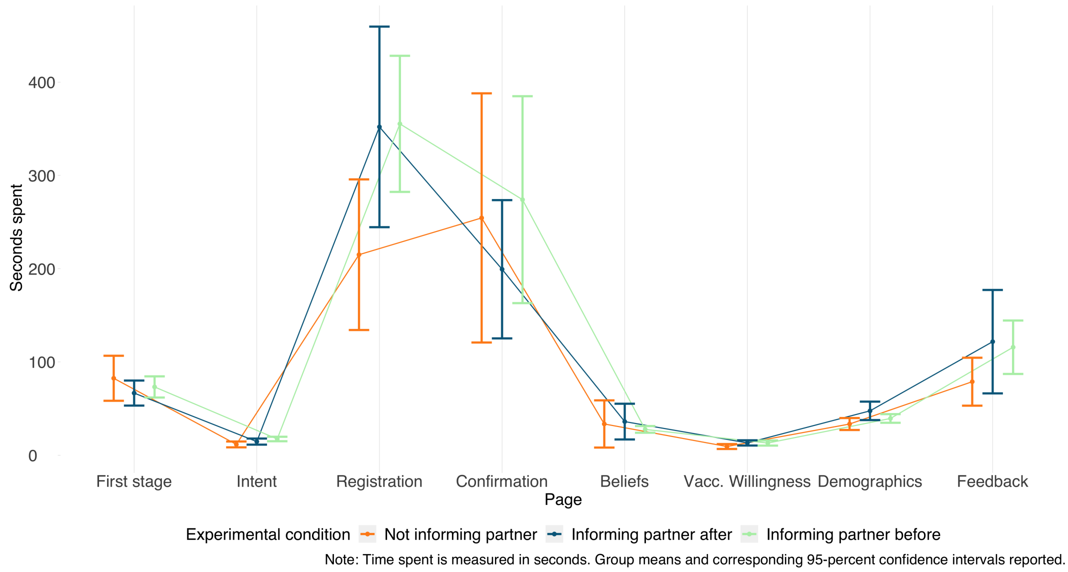


Figure 2.6: Time spent on each page post treatment by experimental condition

Notes: Senders' mean time spent on all survey pages after the treatment module alongside 95-percent confidence intervals by treatment condition reported. Time spent on each page is measured in seconds. The sample of Senders' is limited to those who could provide proof of their registration.

the notion of differential incentives to cheat explaining our findings, we find that participants in both relevant groups spent ca. six minutes inactive on our "Registration" page, a realistic duration to switch to the websites of the Bavarian health authorities and conduct the official registration there. Furthermore, as we document in Figure 2.6 in Appendix 2.A and Table 2.B.5 in Appendix 2.B, Senders in the *"informing partner before"* condition did not spend significantly more (or less) time on any of the pages post treatment than Senders in the *"informing partner after"* condition. Together, these results thus speak against the idea that differential incentives to cheat explain our main finding.

2.3.5 Internal consistency

2.3.5.1 Heterogeneities

We now investigate potential explanations for why Senders are more willing to incur the cost of signing up during the experiment when they know that they can influence their partner. One such explanation is that Senders wish to send a signal about the quality of the vaccine to their partner, who they may suspect to be less informed. To assess this explanation, we analyze whether treatment effects depend on Senders' own beliefs about the quality of the vaccine. Specifically, we now limit our estimation sample to Senders in the *"informing partner before"* and *"informing partner after"* conditions

Table 2.4: Treatment effects conditional on beliefs about quality of the vaccine

	Verified registration	
	(1)	(2)
Beliefs quality	0.05***	0.01
	(0.01)	(0.01)
Informing partner before	0.04***	0.04***
	(0.01)	(0.01)
Informing partner before x Beliefs quality	0.05***	0.05***
	(0.02)	(0.02)
Controls		Yes
Mean 'Verified registration' (control)	0.05	0.05
Mean 'Beliefs quality' (control)	0	0
SD 'Beliefs quality' (control)	1	1
Observations	1,073	1,073
R^2	0.10	0.12

Notes: Results from regressions as laid out in Equation 2.2 reported. Full set of controls reported in Table 2.1 with the exception of social proximity employed in Column 2. We use Senders' verified registration status as the dependent variable. Estimation sample limited to Senders in the *"informing partner before"* and *"informing partner after"* conditions. Thus, *'control'* refers to the *"informing partner after"* condition. *Beliefs quality* is standardized using the mean and standard deviation in the control group. Robust standard errors reported in parentheses: $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.

and estimate the following regression model:

$$\begin{aligned} y_i = \gamma_0 + \gamma_1 \cdot \text{beliefs quality}_i + \gamma_2 \cdot \text{informing partner before}_i \\ + \gamma_3 \cdot \text{informing partner before} \times \text{beliefs quality}_i + \boldsymbol{X_i}\, \boldsymbol{\theta'} + \epsilon_i \end{aligned} \tag{2.2}$$

where y_i and *informing partner before*$_i$ are defined as in Equation 2.1.[39] To proxy Sender i's beliefs about the quality of the vaccine, we employ the average of her pre-treatment beliefs about the safety and the efficacy of the vaccine.[40] The interaction term between our treatment indicator and Senders' beliefs about the quality of the vaccine (γ_3) captures whether the likelihood of registering due to anticipated peer effects becomes stronger if Senders are more convinced about the quality of the vaccine.

We report the main results from this exercise in Table 2.4 in Appendix 2.B and Figure 2.A.2 in Appendix 2.A where we standardize Senders' beliefs about the quality of the vaccine using the mean and standard deviation in the control group. Our estimates for γ_2 and γ_3 are positive and highly significant. Together, these estimates imply that Senders are more likely to sign up for a vaccination once they can influence their partner if they indeed believe that the vaccine is safe and effective. Conversely, Senders with the lowest levels of trust in the vaccine deliberately decided not to sign up dur-

[39] See Section 2.3.1 for a detailed description of these variables.

[40] For both survey items we employed a 1–7 scale, where higher numbers represent stronger beliefs in the safety or efficacy of the vaccine.

Table 2.5: Treatment effects conditional on social proximity between partners

	Verified registration	
	(1)	(2)
Social proximity	0.01	0.01
	(0.01)	(0.01)
Informing partner before	0.05***	0.05***
	(0.02)	(0.02)
Informing partner before x Social proximity	0.02	0.01
	(0.02)	(0.02)
Controls		Yes
Mean 'Verified registration' (control)	0.05	0.05
Mean 'Social proximity' (control)	0	0
SD 'Social proximity' (control)	1	1
Observations	877	877
R^2	0.02	0.13

Notes: Results from regressions as laid out in Equation 2.2 reported. Full set of controls reported in Table 2.1 with the exception of social proximity employed in Column 2. We use Senders' verified registration status as the dependent variable. Estimation sample limited to Senders in the *"informing partner before"* and *"informing partner after"* conditions. Thus, *'control'* refers to the *"informing partner after"* condition. *Social proximity* is standardized using the mean and standard deviation in the control group. Controls include the full set of variables reported in Table 2.1 with the exception of social proximity. Robust standard errors reported in parentheses. Significance levels: $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

ing the experiment if they knew that they could influence their partner. This finding underlines our interpretation that anticipated peer effects constitute a prosocial motive in our setting: Senders who can influence their partners indeed choose to lead by example, signalling what they believe is best for their partner. Anticipated peer effects thus seem to arise if people think that leading by example sends an informative signal about the desirability of a certain action to observing individuals.

Naturally, one may expect that our desire to lead by good example is more pronounced if observing individuals include friends and family or other individuals we care about. To test whether perceived social proximity affects the strength of anticipated peer effects in a prosocial setting, we use a measure of Sender i's perception of social proximity between herself and her partner (instead of her beliefs about the vaccine's quality) as the conditioning variable. Results from this exercise are summarized in Table 2.5 in Appendix 2.B and in the corresponding Figure 2.A.3 in Appendix 2.A, where we again standardized the conditioning variable using its mean and standard deviation in the control group. Although we find tentative evidence that anticipated peer effects increase in perceived social proximity, the interaction term between the dummy variable indicating that Sender i was assigned to the *"informing partner before"* condition and the perceived social proximity to her partner is not significant. However, in combination with the fact that more than 50 percent of Senders perceive their partner at least as an "acquaintance"[41], this finding nevertheless supports the notion that anticipated peer effects are more likely to matter for prosocial behavior if people care about observing individuals.

2.3.5.2 2SLS estimates

The estimated impact of our experimental manipulations on Senders' likelihood to sign up for a COVID-19 vaccination we discussed so far constitutes a "reduced form" effect. Yet, as argued in previous sections, Senders' beliefs about the potential impact of their own decision on their partners – their first-stage belief – may be particularly important for the strength of anticipated peer effects. Therefore, we now investigate the relationship between our experimental manipulations, Senders' first-stage belief, and their willingness to sign up for a COVID-19 vaccination more systematically: to this end, we limit our estimation sample to Senders in the the *"informing partner before"* and *"informing partner after"* conditions and leverage our experimental manipulation as an instrument for Senders' beliefs about the likelihood of being able to exert a peer effect on their partner in a 2SLS framework. We thus estimate a local average treatment effect on those Senders whose beliefs were actually shifted by the experimental intervention.

We report the results from this exercise in Table 2.6. Columns (1) to (4) summarize our findings discussed in previous sections: Senders in the *"informing partner before"* condition exhibit a significantly higher probability to believe that they can influence their partner (Columns (1) and (2)) and are significantly more likely to sign up for a COVID-19 vaccination (Columns (3) and (4)) than Senders in the *"informing partner after"* condition. Then, in Columns (5) and (6) we leverage our experimentally induced variation in Senders' beliefs about the likelihood of influencing their partner to compute the 2SLS estimate of this belief on Senders' likelihood to sign up for a COVID-19 vaccination. Irrespective of whether we use controls or not, we obtain positive and significant estimates for Senders' beliefs about the likelihood of exerting a peer effect on their partner. This confirms our view that anticipated peer effects are governed by Senders' beliefs about their chances of influencing their partner.

2.3.6 "Actual" peer effects on Receivers

As we have documented in the previous subsection, Senders' beliefs about the likelihood of exerting a peer effect on their partner play a central role in the strength of anticipated peer effects. Yet, it remains an open question as to whether Senders correctly anticipate such a peer effect on their partner. To investigate this question, we analyze whether Senders' decision to sign up for a COVID-19 vaccination actually influenced Receivers' behavior by running the following regression model with our full

[41] For an explanation of the social proximity scale, see Section 2.2.

Table 2.6: Treatment effects estimated using 2SLS

	First Stage Likelihood of influencing partner		Reduced Form Verified registration		Second Stage	
	(1)	(2)	(3)	(4)	(5)	(6)
Informing partner before	5.57***	5.42***	0.05***	0.05***		
	(1.78)	(1.69)	(0.02)	(0.02)		
Likelihood of influencing partner					0.01**	0.01**
					(0.00)	(0.00)
Controls		Yes		Yes		Yes
Mean 'Likelihood of influencing partner' (control)	27.93	27.93	27.93	27.93	27.93	27.93
Mean 'Verified registration' (control)	0.05	0.05	0.05	0.05	0.05	0.05
F-Statistic for 1st stage			9.74	10.29		
Observations	911	911	911	911	911	911

Notes: Results from first-stage (Columns 1 and 2), reduced form (Columns 3 and 4), and 2SLS (Columns 5 and 6) regression reported. Estimation sample was limited to Senders in the *"informing partner before"* and *"informing partner after"* conditions. Thus, *'control'* refers to the *"informing partner after"* condition. Controls include the full set of variables reported in Table 2.1 with the exception of social proximity. Robust standard errors reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

sample of Receivers:[42]

$$y_i = \phi_0 + \phi_1 \cdot \text{informed before}_i + \boldsymbol{X_i} \, \boldsymbol{\zeta'} + \epsilon_i, \tag{2.3}$$

where y_i is a dummy variable which equals 1 if the Receiver decides in the same way as his partner and 0 otherwise. We use Receivers' decision to sign up for a COVID-19 as our main outcome and complement it, among others, with Receivers' self-reported intention to register. Our main explanatory variable informed before$_i$ is a dummy which equals one if Receiver i was informed about his partner's decision before we offered him the opportunity to sign up for a vaccination. Finally, in some specifications we include control variables which we denote by $\mathbf{X}_i$.[43]

We present regression results in Figure 2.7.[44]

The dependent variable is a dummy taking value 1 if the Receiver decided the same way as the Sender. Irrespective of whether we consider revealed-preference or self-reported outcomes, we find small and insignificant estimates throughout.[45]

[42] As we document in Table 2.B.6 in Appendix 2.B, Receivers' predetermined characteristics are also well balanced. The test for joint significance of all predetermined characteristics yields a p-value of 0.6. Thus, we can be fairly confident that the treatment effects we estimate can actually be attributed to our experimental manipulations and did not arise to differences in predetermined characteristics.

[43] We use the same set of control variables as for our analysis of anticipated peer effects, i.e., all characteristics reported in Table 2.1 with the exception of our measure of social proximity.

[44] Full regression results underlying Figure 2.7 can be found in Table 2.B.7 in Appendix 2.B.

[45] The same pattern emerges when we look at changes in attitudes or beliefs, which we report in Table 2.B.8 in Appendix 2.B: three out of four coefficient estimates are statistically indistinguishable from zero. Only for the change in Receivers' beliefs about the severity of the free-riding problem in the roll-out of the mass immunization program we obtain a negative and statistically significant coefficient (p-value < 0.08).

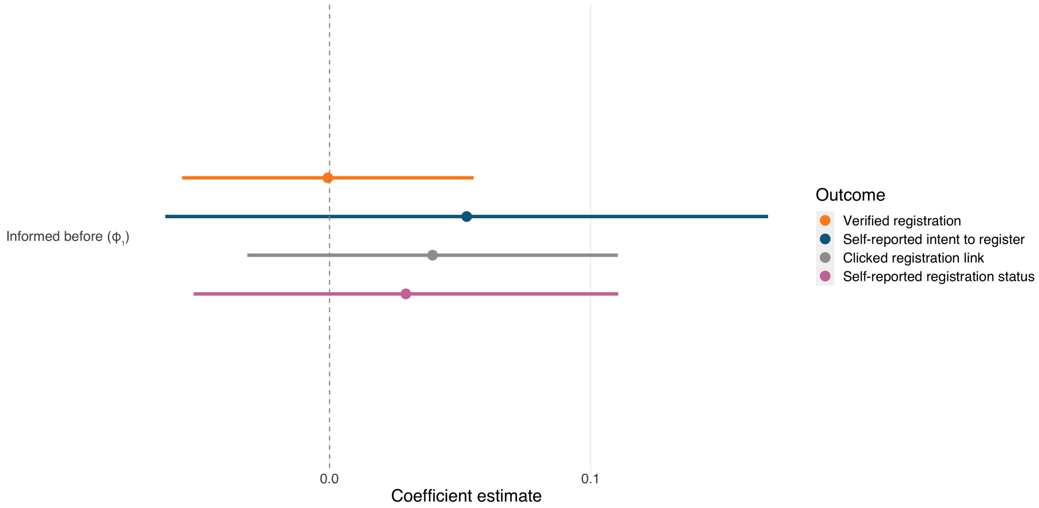


Figure 2.7: Comparing treatment effects on Receivers across experimental conditions
Coefficient estimates derived from regressions as laid out in Equation 2.3 reported. Full set of controls reported in Table 2.1 with the exception of social proximity employed. We use the following dependent variables which are defined as dummies taking value 1 if the Receiver decides the same way as the Sender: (i) verified registration status; (ii) self-reported intent to register (elicited before verification); (iii) whether the link forwarding to BayIMCO was clicked; and (iv) self-reported registration status. 95-percent confidence intervals reported.

Contrary to their own anticipation, Senders are thus not successful in influencing Receivers' behavior and attitudes. The absence of actual peer effects can be explained by the specific setting we study: Receivers learned about Senders' decisions, yet not vice versa. As a result, Senders could not verify whether they indeed exerted a peer effect on their partner and Receivers were thus not subject to social image concerns which could explain the missing conformity of Receivers' behavior. At a more general level, this suggests that in certain decision environments people might perceive themselves as more pivotal than they actually are, such that anticipated peer effects can arise even in absence of "actual" peer effects. Hence, leveraging people's desire to lead by example as a measure to promote prosocial behavior can work even if observing individuals do not follow suit.

2.4 Conclusion

We provide evidence that anticipated peer effects constitute a relevant motive for prosocial behavior in a consequential decision environment. Leveraging a survey-based online experiment in the context of the COVID-19 vaccination campaign in Germany, we find that individuals' willingness to register for the vaccination almost doubles when informed that they can influence a peer's decision. We further document a strong first-stage effect of our treatment on subjects' beliefs about the chances of influencing their peer's decision, implying that individuals anticipate and internalize their potential to lead by example. Anticipated peer effects constitute a complementary behavioral mechanism explaining why people are more inclined to act prosocially if they can be observed by others, which operates independently of social image effects. Our findings further highlight that individuals are willing to incur considerable costs to send an encouraging signal to observing peers to follow their lead if they are convinced that an action can generate positive externalities. In line with this interpretation, we find that anticipated peer effects only increase individuals' willingness to register for a vaccination if they are sufficiently convinced about the quality of the vaccine.

The behavioral relevance of anticipated peer effects can hold relevant implications for policy makers seeking to promote prosocial behavior: instead of having to resort to "social sticks" in the form of enforcement or punishment (e.g., fines for missing vaccination appointments or maintenance of personal restrictions for unvaccinated people), they might leverage people's desire to lead by example as a "social carrot", e.g., by encouraging people to publicly signal their decision to get vaccinated. Increasing the benefits of behaving prosocially – rather than raising the costs of failing to do so – is also likely also beneficial from a welfare perspective.

While in this paper we provided evidence for the existence and empirical relevance of anticipated peer effects in a prosocial setting, future work should focus on exploring the underlying mechanisms in more detail. We can think of at least two potential drivers behind people's willingness to lead by good example: first, individuals might simply feel good about shaping the behavior of others and receive a hedonic payoff from leading by example. Second, in the spirit of theories of pure altruism (Andreoni 1989; Bénabou and Tirole 2006), individuals might care about the total provision of a public good (e.g., contributing to ending the pandemic). In that case, Senders could be motivated by an observability-dependent form of altruism, pushing them to set an example of prosocial behavior if they expect that others might follow suit and contribute to the public good as well.

Finally, our results have highlighted that anticipated peer effects can arise even without translating into peer effects, i.e., without actually affecting the decision of observing individuals. This has two interesting implications: first, it indicates that in some settings the mere potential of being able to influence others can be sufficient to promote prosocial behavior. Second, it implies that individuals might hold out-of-equilibrium beliefs about their impact on others. Understanding the sources of such misperceptions as well as their potential importance in motivating people to assume the responsibility of being social leaders – bearing the private costs of prosocial behavior without knowing that others might follow suit – constitutes another interesting avenue for future research.

APPENDICES

2.A Additional Figures

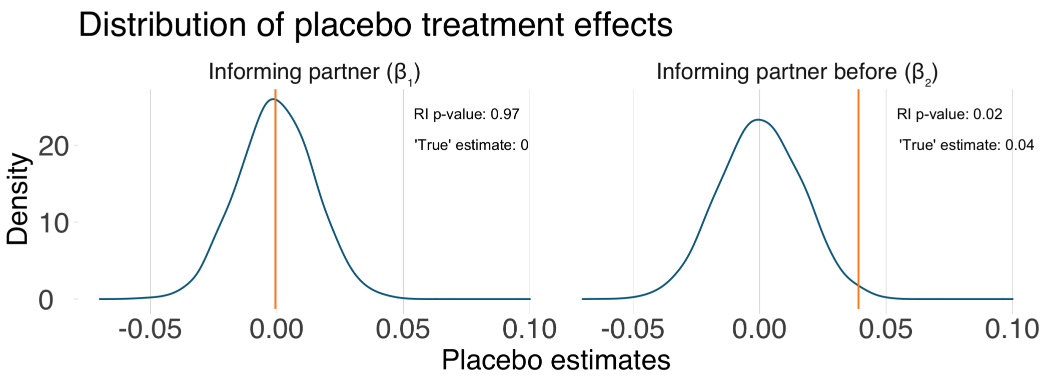


Figure 2.A.1: Results from randomization inference

Notes: Distribution of placebo estimates derived from randomly re-assigning Senders to placebo treatment groups for 5,000 times and calculating the share of "placebo treatment effects" that exceed the "true treatment effect" in (absolute) magnitude reported. Panel (a) reports the resulting distribution and Fisher exact p-value for β_1 and Panel (b) for β_2 based on Equation 2.1. The outcome in both panels is Senders' verified registration status.

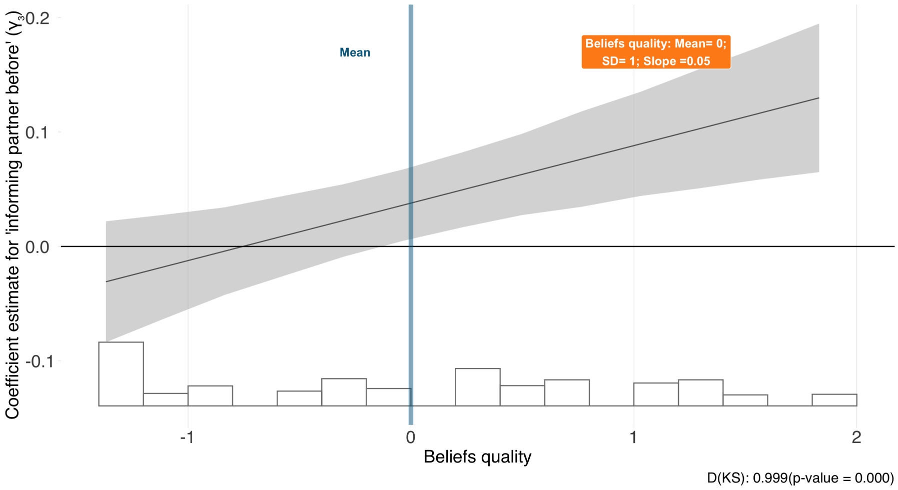


Figure 2.A.2: Treatment effect for *informing partner before* conditional on beliefs about the quality of the vaccine

Notes: Treatment effect heterogeneity based on Equation 2.2 reported. Beliefs about the quality of the vaccine are employed as the conditioning variable and are measured as the average of Senders' belief about the safety and the efficacy of the vaccine. The horizontal axis depicts the distribution of this belief among Senders. The intercept of the line corresponds to our estimates of γ_2 and the slope to our estimates of γ_3 reported in Table 2.4. Full set of controls listed in Table 2.1 with the exception of social proximity employed. 95-percent confidence bands reported.

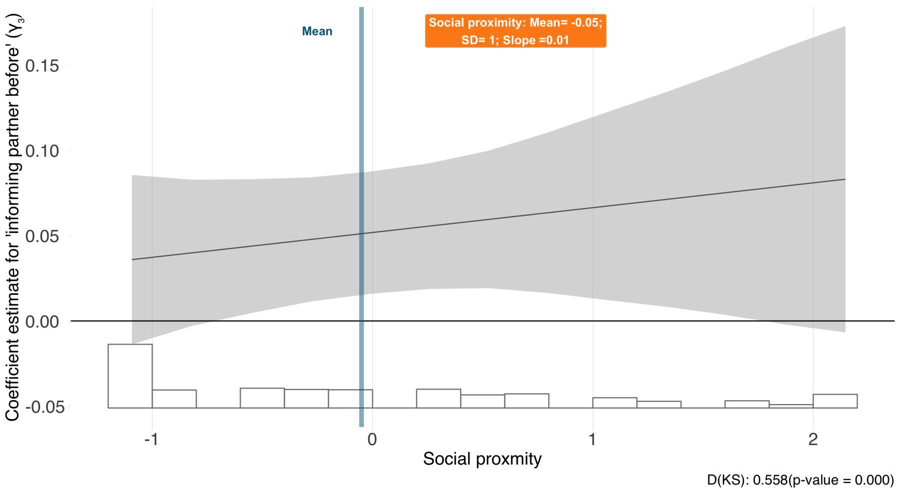


Figure 2.A.3: Treatment effect for *informing partner before* conditional on perceived social proximity to partner

Notes: Treatment effect heterogeneity based on Equation 2.2 reported. Social proximity to the partner is employed as the conditioning variable and is measured using the *Oneness* scale. The horizontal axis depicts the distribution of social proximity among Senders. The intercept of the line corresponds to our estimates of γ_2 and the slope to our estimates of γ_3 reported in Table 2.5. Full set of controls listed in Table 2.1 employed. 95-percent confidence bands reported.

2.B Additional Tables

Table 2.B.1: Treatment effects on first stage beliefs

	Likelihood that partner can be influenced (%)			
	(1)	(2)	(3)	(4)
Informing partner	6.26***	5.90***	3.58*	3.29*
	(1.77)	(1.73)	(1.94)	(1.90)
Informing partner before			5.57***	5.39***
			(1.79)	(1.69)
Controls		Yes		Yes
Mean, 'Not informing partner'	24.36	24.36	24.36	24.36
Mean, 'Informing partner after'	27.93	27.93	27.93	27.93
Observations	1,194	1,194	1,194	1,194
R^2	0.01	0.11	0.02	0.12

Notes: Results derived from regressions as laid in Equation 2.1 where we employ Senders' beliefs about the likelihood that their partner can be influenced as the dependent variable. Controls include the full set of variables reported in Table 2.1 with the exception of social proximity. Robust standard errors reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 2.B.2: Treatment effects on signing up for a COVID-19 vaccination

	Verified registration			
	(1)	(2)	(3)	(4)
Informing partner	0.02	0.02	−0.00	−0.00
	(0.01)	(0.01)	(0.02)	(0.01)
Informing partner before			0.04**	0.04***
			(0.02)	(0.01)
Controls		Yes		Yes
Mean, 'Not informing partner'	0.05	0.05	0.05	0.05
Mean, 'Informing partner after'	0.05	0.05	0.05	0.05
Observations	1,401	1,401	1,401	1,401
R^2	0.00	0.10	0.01	0.11

Notes: Results derived from regressions as laid in Equation 2.1 where we employ Senders' verified registration status as the dependent variable. Controls include the full set of variables reported in Table 2.1 with the exception of social proximity. Robust standard errors reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

2.B.1 Addressing potential concerns

Table 2.B.3: Comparing treatment effects across outcomes

	Verified registration	Δ Self-reported willingness to take vaccine	Self-reported intent to register	Clicked reg. link	Self-reported registration status
	(1)	(2)	(3)	(4)	(5)
Informing partner	−0.00	−0.00	0.01	−0.00	0.01
	(0.01)	(0.01)	(0.03)	(0.02)	(0.02)
Informing partner before	0.04***	−0.00	0.02	0.03	0.03*
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Controls	Yes	Yes	Yes	Yes	Yes
Mean, 'Not informing partner'	0.049	0.494	0.198	0.088	0.095
Mean, 'Informing partner after'	0.049	0.505	0.217	0.088	0.11
Observations	1,401	1,401	1,401	1,401	1,401
R^2	0.11	0.85	0.21	0.12	0.13

Notes: Results derived from regressions as laid in Equation 2.1. We employ the following dependent variables: (Column 1) a dummy variable taking value 1 if Sender reported that she registered for a vaccination and could provide proof of her registration; (Column 2) the change in a Sender's self-reported willingness to take the vaccine (pre/post treatment); (Column 3) dummy variable taking value 1 if a Sender's reported to be willing to register (elicited before verification); (Column 4) a dummy variable taking value 1 if a Sender clicked on the registration link forwarding her to BayIMCO; and (Column 5) dummy variable taking value 1 if a Sender's reported that she had registered (elicited before verification). Controls include the full set of variables reported in Table 2.1 with the exception of social proximity. Robust standard errors reported in parentheses. Significance levels: $*\ p < 0.10$, $**\ p < 0.05$, $***\ p < 0.01$.

Table 2.B.4: Strategic lying

	Self-reported intent to register	Verified registration	Self-reported intent not verified
	(1)	(2)	(3)
Informing partner	0.01	-0.001	0.01
	(0.03)	(0.01)	(0.02)
Informing partner before	0.02	0.04^{***}	-0.02
	(0.02)	(0.01)	(0.02)
Controls	Yes	Yes	Yes
Mean, 'Not informing partner'	0.198	0.049	0.149
Mean, 'Informing partner after	0.194	0.044	0.149
Observations	1,401	1,401	1,401
R^2	0.21	0.11	0.11

Notes: Results derived from regressions as laid in Equation 2.1. We employ the following dependent variables: (Column 1) dummy variable taking value 1 if a Sender's reported to be willing to register (elicited before verification); (Column 2) a dummy variable taking value 1 if a Sender reported that she registered for a vaccination and could provide proof of her registration; (Column 3) a dummy variable taking value 1 if a Sender who reported that she had signed up but failed to provide proof of her registration. Controls include the full set of variables reported in Table 2.1 with the exception of social proximity. Robust standard errors reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

Table 2.B.5: Cheating (time spent on survey pages in seconds)

	Before vs. After		Before vs. Not		After vs. Not	
	Diff.	p-value	Diff.	p-value	Diff.	p-value
First Stage						
First stage	6.57	0.448	-9.3	0.460	-15.87	0.229
Main Outcomes						
Intent	2.785	0.165	5.815^{***}	0.003	3.03	0.169
Waitpage	-0.104	0.177	0.044	0.160	0.148^{**}	0.040
Registration	3.274	0.959	140.311^{***}	0.009	137.037^{**}	0.040
Confirmation	82	0.215	19.563	0.814	-62.438	0.388
Additional Outcomes						
Beliefs	-8.37	0.378	-5.833	0.624	2.537	0.867
Vacc. Willingness	-0.141	0.942	3.732^{**}	0.047	3.873^{**}	0.042
Demographics						
Demographics	-8.156	0.130	5.962	0.118	14.118^{**}	0.017
Feedback						
Feedback	-8.541	0.779	34.387^{*}	0.068	42.928	0.155

Notes: Differences in Senders' mean time spent (in seconds) on all pages after the treatment module alongside p-values testing for equal means reported. Differences are calculated as follows: $\text{time spent}_i = \alpha + \beta \cdot treat_i + \epsilon_i$, where $treat_i$ is a dummy variable either corresponding to either the *informing partner after* (*After*) or *informing partner before* (*Before*) condition, omitting one condition for every pair-wise comparison. *Not* refers to the *not informing partner* condition. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

2.B.2 Balancing receivers

Table 2.B.6: Receivers' predetermined characteristics compared across treatment conditions

	Group means		p-value	
	Before	After	Before vs. After	N
	(1)	(2)	(3)	(4)
Attrition				
Completed survey	0.73	0.71	0.50	635
Demographics				
Age	39.76	42.03	0.10	456
Female	0.56	0.56	0.86	456
Income	2861.59	3103.39	0.12	456
Upper secondary degree	0.37	0.37	0.92	456
Local characteristics				
Avg. incidence rate (during second wave)	139.21	137.11	0.58	456
Population in zip	15.30	14.70	0.52	456
Lives in urban area ($\geq$100k)	0.29	0.23	0.20	456
Turnout (%)	77.45	77.57	0.76	456
AfD vote share (%)	11.99	12.53	0.06*	456
Unemployment rate (%)	2.39	2.25	0.10	456
Beliefs				
Safety	3.50	3.42	0.68	456
Efficacy	3.92	3.79	0.48	456
Social desirability	3.74	3.59	0.48	456
Severity of freerider problem	3.37	3.26	0.59	456
Willingness to take vaccine in state (%)	60.74	59.84	0.64	456
Preferences				
Own willingness to take vaccine (%)	53.22	52.91	0.93	456
Altruism	-0.01	-0.01	0.93	456
Desire to influence	0.06	0.15	0.31	456
Social image concerns	0.00	0.05	0.60	456
Social proximity				
Oneness	0.16	-0.05	0.04**	386
Test for joint significance				
			0.6	456

Notes: Group means of Receivers' predetermined characteristics alongside p-values testing for equal means reported. p-values are derived from the following regressions: $\text{characteristic}_i = \alpha + \beta \cdot \text{informed before}_i + \epsilon_i$, where informed before_i is a dummy taking value 1 for all Receivers in the *informed before* condition. All variables classified as "local characteristic" do not vary on the individual but on the zip code or town ("Gemeinde") of residence level. Significance levels: $* \, p < 0.10$, $** \, p < 0.05$, $*** \, p < 0.01$.

2.B.3 "Actual" peer effects on Receivers

Table 2.B.7: Treatment effects on Receivers: Registration outcomes

	Dummy == 1 if Receiver decides the same way as Sender			
	Verified registration	Self-reported intent to register	Clicked reg. link	Self-reported registration status
	(1)	(2)	(3)	(4)
Informed before	−0.00	0.04	0.03	0.02
	(0.02)	(0.04)	(0.03)	(0.03)
Controls	Yes	Yes	Yes	Yes
Mean dependent variable, 'informed after'	0.919	0.712	0.86	0.835
Observations	456	456	456	456
R^2	0.03	0.07	0.06	0.03

Notes: Results derived from regressions as laid out in Equation 2.3 reported. Across all columns, the dependent variable is a dummy variable taking value 1 if a Receiver decided the same way as the Sender he was matched with. We consider the following outcomes: (Column 1) verified registrations; (Column 2) self-reported intent to register (elicited before verification); (Column 3) clicked on link forwarding Receiver to BayIMCO; and (Column 4) self-reported registration status (elicited before verification). Controls include the full set of variables reported in Table 2.1 with the exception of social proximity. Robust standard errors reported. Significance levels: $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.

Table 2.B.8: Treatment effects on Receivers: Changes in atitudes and beliefs

	Change in attitudes/beliefs			
	Δ Self-reported willigness to take vaccine	Δ Beliefs safety & efficacy	Δ Beliefs freeriding	Δ Beliefs image
	(1)	(2)	(3)	(4)
Informed before	−0.00	−0.08	−0.19*	−0.23
	(0.01)	(0.08)	(0.10)	(0.16)
Controls	Yes	Yes	Yes	Yes
Mean dependent variable, 'informed after'	0.002	0.307	0.089	1.924
Observations	456	456	456	456
R^2	0.02	0.06	0.04	0.39

Notes: Results derived from regressions as laid out in Equation 2.3 reported. We consider the following dependent variables which are all defined as changes from before to after the treatment: (Column 1) willingness to take the vaccine; (Column 2) beliefs about safety and efficacy of the vaccine; (Column 3) beliefs about the severity of free-riding in the context of the vaccination program; and (Column 4) beliefs about the social desirability of the vaccine. Controls include the full set of variables reported in Table 2.1 with the exception of social proximity. Robust standard errors reported. Significance levels: $*$ $p < 0.10$, $**$ $p < 0.05$, $***$ $p < 0.01$.

2.C Screenshots

2.C.1 Joint problem solving task

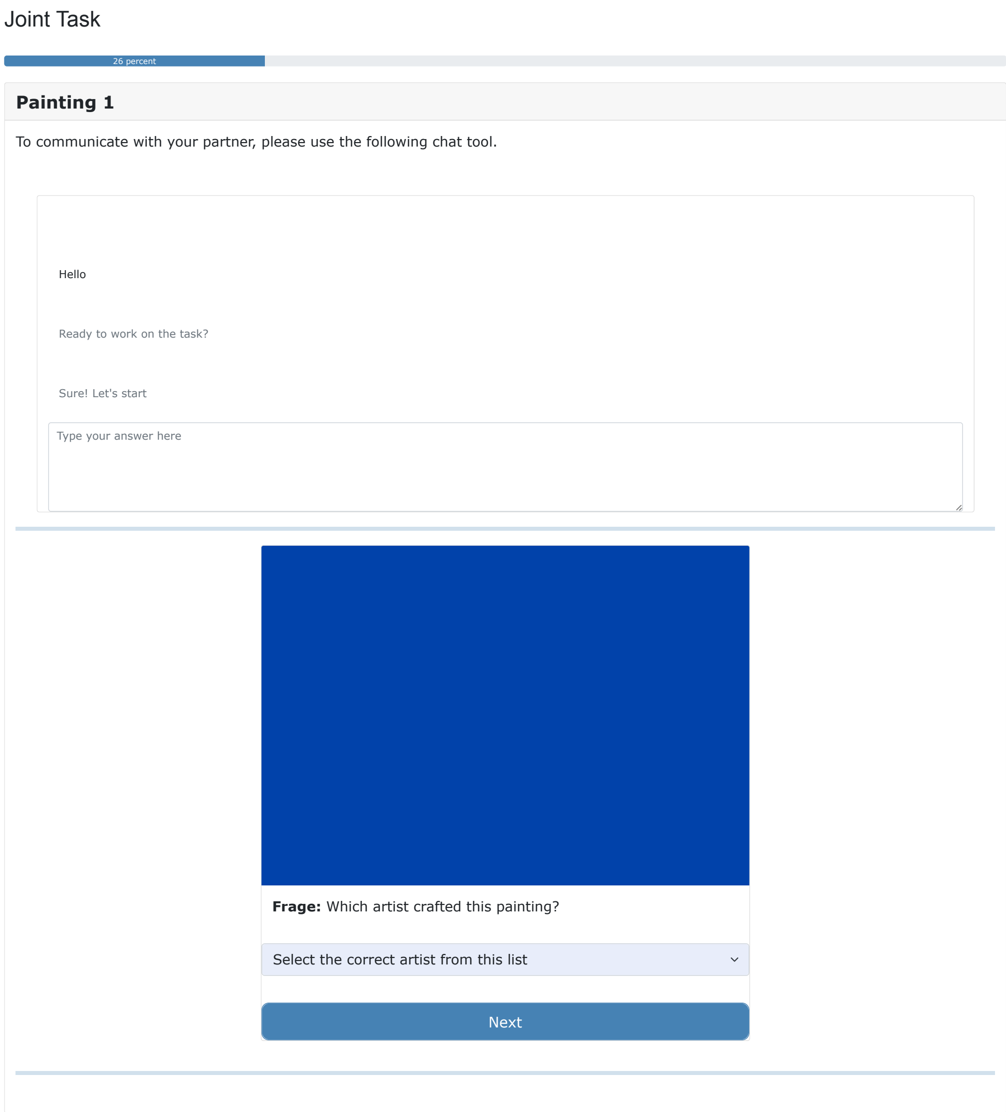
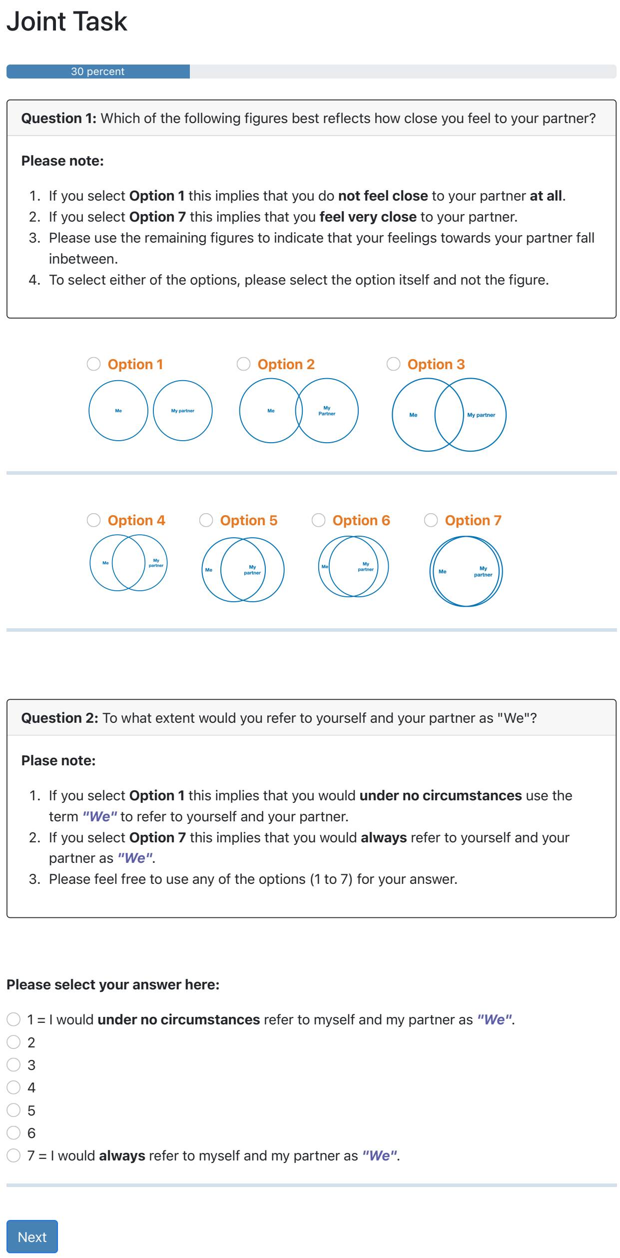


Figure 2.C.1: Survey page showing chat window and first historical painting

2.C.2 Oneness elicitation



Joint Task

30 percent

Question 1: Which of the following figures best reflects how close you feel to your partner?

Please note:

1. If you select **Option 1** this implies that you do **not feel close** to your partner **at all**.
2. If you select **Option 7** this implies that you **feel very close** to your partner.
3. Please use the remaining figures to indicate that your feelings towards your partner fall inbetween.
4. To select either of the options, please select the option itself and not the figure.

Question 2: To what extent would you refer to yourself and your partner as "We"?

Plase note:

1. If you select **Option 1** this implies that you would **under no circumstances** use the term ***"We"*** to refer to yourself and your partner.
2. If you select **Option 7** this implies that you would **always** refer to yourself and your partner as ***"We"***.
3. Please feel free to use any of the options (1 to 7) for your answer.

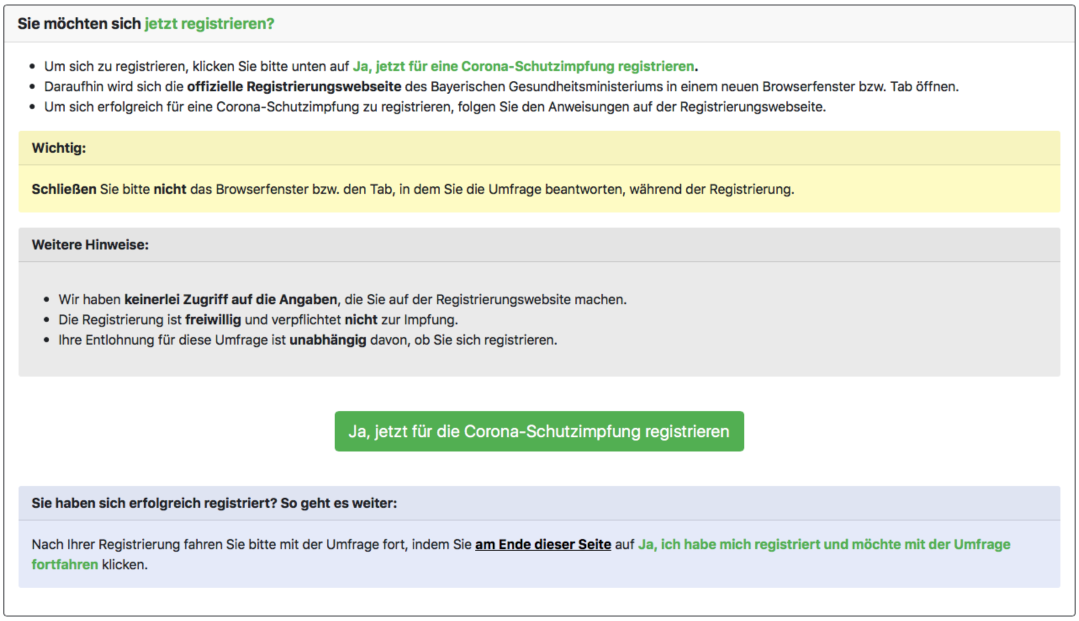
Please select your answer here:

- 1 = I would **under no circumstances** refer to myself and my partner as ***"We"***.
- 2
- 3
- 4
- 5
- 6
- 7 = I would **always** refer to myself and my partner as ***"We"***.

Next

Figure 2.C.2: Survey page showing oneness elicitation

2.C.3 Registration for COVID-19 vaccination



Sie möchten sich jetzt registrieren?

- Um sich zu registrieren, klicken Sie bitte unten auf **Ja, jetzt für eine Corona-Schutzimpfung registrieren**.
- Daraufhin wird sich die **offizielle Registrierungswebseite** des Bayerischen Gesundheitsministeriums in einem neuen Browserfenster bzw. Tab öffnen.
- Um sich erfolgreich für eine Corona-Schutzimpfung zu registrieren, folgen Sie den Anweisungen auf der Registrierungswebseite.

Wichtig:

Schließen Sie bitte **nicht** das Browserfenster bzw. den Tab, in dem Sie die Umfrage beantworten, während der Registrierung.

Weitere Hinweise:

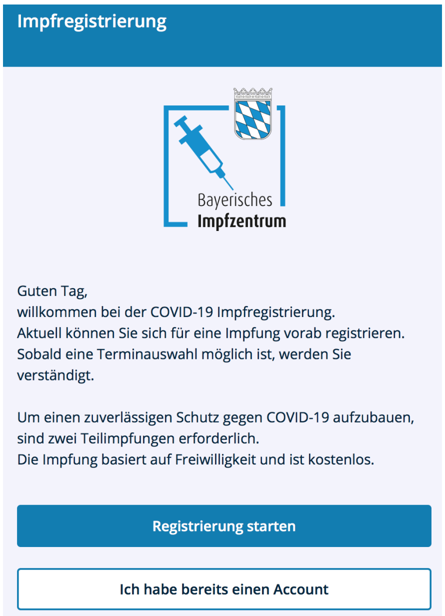
- Wir haben **keinerlei Zugriff auf die Angaben**, die Sie auf der Registrierungswebsite machen.
- Die Registrierung ist **freiwillig** und verpflichtet **nicht** zur Impfung.
- Ihre Entlohnung für diese Umfrage ist **unabhängig** davon, ob Sie sich registrieren.

Ja, jetzt für die Corona-Schutzimpfung registrieren

Sie haben sich erfolgreich registriert? So geht es weiter:

Nach Ihrer Registrierung fahren Sie bitte mit der Umfrage fort, indem Sie **am Ende dieser Seite** auf **Ja, ich habe mich registriert und möchte mit der Umfrage fortfahren** klicken.

Figure 2.C.3: Survey page eliciting intended willingness to register and providing link to official registration website (BayIMCO)


Impfregistrierung

Bayerisches
Impfzentrum

Guten Tag,
willkommen bei der COVID-19 Impfregistrierung.
Aktuell können Sie sich für eine Impfung vorab registrieren.
Sobald eine Terminauswahl möglich ist, werden Sie verständigt.

Um einen zuverlässigen Schutz gegen COVID-19 aufzubauen, sind zwei Teilimpfungen erforderlich.
Die Impfung basiert auf Freiwilligkeit und ist kostenlos.

Registrierung starten

Ich habe bereits einen Account

Figure 2.C.4: Starting page of the official registration process (BayIMCO)


Bestätigen Sie nun Ihre Registrierung

Sie haben angegeben, dass Sie sich **gerade** online für eine Corona-Schutzimpfung **registriert haben**.

- Sie sollten eine **Bestätigungs-Email** nach Abschluss der Registrierung erhalten haben.
- Wir bitten Sie um die zwei folgenden Angaben aus der Bestätigungs-Email des Impfzentrums:
  1. **Email-Adresse**
  2. **Betreff**

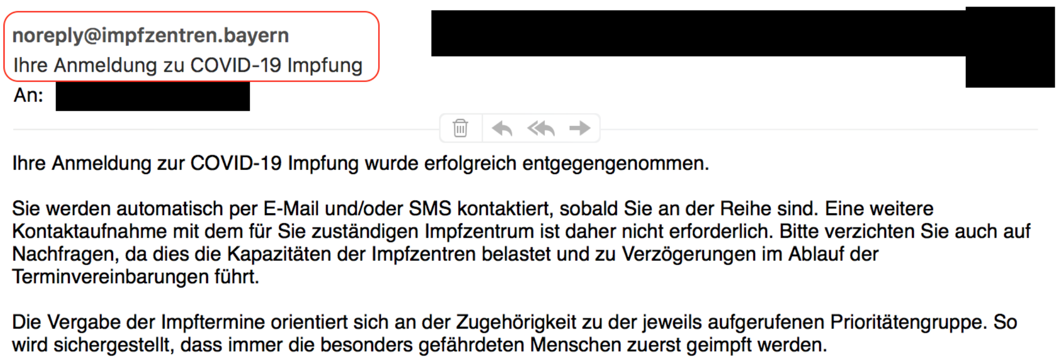
Gewinnspiel:

- **Wenn Ihre Angaben beide richtig sind**, können Sie einen von **30 Amazon-Gutscheinen im Wert von 20€** gewinnen.
- Sie müssen die Umfrage beenden, um an der Verlosung teilnehmen zu können.

Weitere Hinweise:

- Diese Angaben lassen keinerlei Rückschlüsse auf Sie als Person zu. **Sie bleiben** weiterhin **vollständig anonym**.
- Sie können auch ohne Beantwortung der Fragen mit der Umfrage fortfahren. Dann können Sie aber nicht an der Verlosung teilnehmen.

Figure 2.C.5: Survey page explaining verification of registration



noreply@impfzentren.bayern [redacted]
Ihre Anmeldung zu COVID-19 Impfung
An: [redacted]

Ihre Anmeldung zur COVID-19 Impfung wurde erfolgreich entgegengenommen.

Sie werden automatisch per E-Mail und/oder SMS kontaktiert, sobald Sie an der Reihe sind. Eine weitere Kontaktaufnahme mit dem für Sie zuständigen Impfzentrum ist daher nicht erforderlich. Bitte verzichten Sie auch auf Nachfragen, da dies die Kapazitäten der Impfzentren belastet und zu Verzögerungen im Ablauf der Terminvereinbarungen führt.

Die Vergabe der Impftermine orientiert sich an der Zugehörigkeit zu der jeweils aufgerufenen Prioritätengruppe. So wird sichergestellt, dass immer die besonders gefährdeten Menschen zuerst geimpft werden.

Figure 2.C.6: Confirmation email highlighting sending address and subject line

2.D Survey Instrument

I Basic demographic information

Question 1: Are you male or female?
Question 2: How old are you?
Question 3: In which federal state do you live?

new page

Since the end of last year (December 2020), *vaccinations against the coronavirus* (COVID-19 vaccinations) have been administered in Germany.

Question: Have you already received a COVID-19 vaccination? Reply options: *Yes or No*

new page

Did you know that?
In Bavaria, it is possible to register for a COVID-19 vaccination already, even though the actual vaccination may not take place for a few months. Registration takes place either online or by telephone at the Bavarian vaccination centres.

Question: Have you already registered for a COVID-19 vaccination? Reply options: *Yes or No*

new page

II Attitudes towards the COVID-19 vaccination

We would like to start by asking you a few basic questions regarding how you feel about the COVID-19 vaccination.
There are now several vaccines against the coronavirus on the German market. Vaccination is officially recommended for adults of all ages (exception: not during pregnancy and breastfeeding for the time being, as no data on safety and efficacy are yet available).

To what extent do you agree with the following statements?

- **Statement 1:** I have full confidence that vaccination against COVID-19 is safe. Reply options: *Likert scale (1-7) with 1: do not agree at all, 7: agree completely.*

- **Statement 2:** I have full confidence that vaccination against COVID-19 is effec-

tive.
Reply options: *Likert scale (1-7) with 1: do not agree at all, 7: agree completely.*

- **Statement 3:** I see vaccination as a collective effort against the spread of COVID-19.
  Reply options: *Likert scale (1-7) with 1: do not agree at all, 7: agree completely.*
- **Statement 4:** If everyone is vaccinated against COVID-19, I don't need to get vaccinated too.
  Reply options: *Likert scale (1-7) with 1: do not agree at all, 7: agree completely.*

Question 1: How likely is it that you will get vaccinated against COVID-19?
Instruction: Please use the bar/slider for your answer. Click on the bar at the bottom to reveal the slider. Then move the slider to give your answer. 0 percent means "definitely not willing to get vaccinated". 100 percent means "definitely willing to get vaccinated".

Question 2: What do you think? What proportion of people in Bavaria are willing to get vaccinated against COVID-19?
Instruction: Please use the bar/slider for your answer. Click on the bar at the bottom to reveal the slider. Then move the slider to give your answer. 0 percent means "no one is willing to get vaccinated". 100 percent means "everybody is willing to get vaccinated".

——— *new page* ———

III Broader set of attitudes

How well do the following statements apply to you as a person?

- **Statement 1:** I like it when people accept my suggestions.
  Reply options: *Likert scale (1-7) with 1: do not agree at all and 7: agree completely*.
- **Statement 2:** I like it when my ideas and opinions have an impact on other people.
  Reply options: *Likert scale (1-7) with 1: do not agree at all and 7: agree completely.*
- **Statement 3:** I would like the feeling of having influenced other people's lives.
  Reply options: *Likert scale (1-7) with 1: do not agree at all and 7: agree completely.*

——— *new page* ———

How well do the following statements apply to you as a person?

- **Statement 1:** It is important to me to impress others.
  Reply options: *Likert scale (1-7) with 1: do not agree at all and 7: agree completely*.

- **Statement 2:** I think a lot about whether I am good enough compared to others.
  Reply options: *Likert scale (1-7) with 1: do not agree at all and 7: agree completely.*

- **Statement 3:** It is important to me how I am perceived by others.
  Reply options: *Likert scale (1-7) with 1: do not agree at all and 7: agree completely.*

new page

We now ask you about your behavior in certain situations.

Question: How much would you be willing to give to a good cause without expecting anything in return?
Reply options: *0: Not at all willing, 10: Extremely willing*

Imagine the following situation: Today you unexpectedly received 1,000 EUR.
Question: How much of the money would you donate to a good cause? *Note: You can enter whole numerical values from 0 to 1,000 here.*

new page

IV Joint task

Please read the following instructions carefully before proceeding with the survey.

- In the next section of our survey, we ask you to solve a short task together with another participant of this survey.

- Your task is to match famous pieces of art to the respective artist together with your partner.

- In this task, you can win one of 30 Amazon vouchers worth €10.

- You can communicate with your fellow player by means of a chat.

- To facilitate communication, please enter your first name or a nickname below.

Question: What is your first name or nickname?

Hint:

- In order to remain anonymous, please make sure to enter only your first name.

- You can also choose another name here. However, the name should correspond to your gender.

new page

We ask you to solve the upcoming task together with your partner.
Your partner is: [*name*]

[He/she] is [*xx*] years old. [He/she] lives in Bavaria.

Task: Together with your partner, match the following four pieces of art with the correct artist.

Hints:

1. You and your partner have 60 seconds for each piece of art.
2. If you and your partner correctly match at least three pieces of art, you can win one of 30 Amazon vouchers worth €10.
3. You must complete the full survey to qualify for one of the vouchers.
4. To increase your chances of winning, it is important that you and your partner work together.
5. You will receive points only if you both give the correct answer.
6. Use the chat window to communicate with your partner via text messages and coordinate your answers. The chat window is available for the entire task.
7. Its a good idea to introduce yourself to your partner with a short message right away.

[***Chat window***]

Final hints before the tasks begins: You may have to wait for a moment until your partner [*name*] has read the instructions and responds to you.
Reminder: You can win one of 30 Amazon vouchers worth €10.

new page

[*Painting is shown for 1 Minute.*]

Question: Which artist painted this piece of art?
Reply options: *Participants can choose one artist from a drop-down menu.*

[*This process is repeated four times. During this time the participants have the option to use the chat window to communicate.*]

new page

Question: Which of the following figures best reflects how connected you feel with your partner [*name*]?

Hints:

1. Option 1 means that you do not feel connected to your partner [*name*] at all.
2. Option 7 means that you feel very close to your partner [*name*].
3. Use the remaining options (2-6) to grade your answer.
4. To select one, click on the option in the header and not on the image.

new page

Please still think of your partner [*name*].
Question: To what extent would you refer to yourself and your partner [*name*] as ″we″.

Hints:

1. Option 1 means that you would definitely not refer to the two of you as ″we″.
2. Option 7 means that you would definitely speak refer to the two of you as ″we″.
3. Use the remaining options (2-6) to grade your answer.

new page

V Explanations on the survey

Instructions: In the following, we would like to ask you about your willingness to get vaccinated against COVID-19. Specifically, we would like to know whether you are willing to register for a COVID-19 vaccination right away. With that we are referring to the official registration process required for residents of Bavaria to be able to obtain an appointment at a vaccination center. In this survey, we will provide you with the opportunity to switch to the official registration website of the Bavarian Ministry of Health to complete the registration. Of course, the registration is voluntary and you can also complete the survey without registering.

Task: Confirm that you have understood these instructions by selecting the correct answer below.

Question: During this survey, will you be able to switch to the official registration website of the Bavarian Ministry of Health to complete the registration for a COVID-19 vaccination?
Reply options: *Yes or No*

new page

V.A Instructions Senders *"not informing partner"*

Instructions:
The survey proceeds as follows:

Step 1: You decide whether you want to register for a COVID-19 vaccination right away.
Step 2: Your partner [*name*] decides whether [he/she] wants to register for a COVID-19 vaccination right away.

Important: We do not tell your partner [*name*] whether you want to register for a vaccination.

You do not find out about the decision of your partner [*name*].

Task: Confirm that you have understood the instructions by selecting the correct answer below.
Question: Will your partner [*name*] find out whether you want to register?
Reply options: *Yes/No*

V.B Instructions Senders *"informing partner after"*

Instructions:
We will tell your partner [*name*] with a high probability whether you want to register for a vaccination. This proceeds as follows:

Step 1: You decide whether you want to register for a COVID-19 vaccination right away.
Step 2: Your partner [*name*] decides whether [he/she] wants to register for a COVID-19 vaccination right away.
Step 3: We tell your partner [*name*] whether you want to register for a vaccination.
Important: Your partner [*name*] will find out about your registration decision **only after** [he/she] has already decided whether [he/she] wants to register.

You do not find out about the decision of your partner [*name*].

Task: Confirm that you have understood the instructions by selecting the correct answers below.

Question 1: Will your partner [*name*] find out with a high probability whether you want to register?
Reply options: *Yes/No*
Question 2: When will your partner [*name*] find out about your registration decision? **Directly before** or **only after** [he/she] can register for a COVID-19 vaccination?
Reply options: *Directly before/Only after*

V.C Instructions Senders *"informing partner before"*

Instructions:
We will tell your partner [*name*] with a high probability whether you want to register for a vaccination. This proceeds as follows:

Step 1: You decide whether you want to register for a COVID-19 vaccination right away.
Step 2: We tell your partner [*name*] whether you want to register for a vaccination.
Important: Your partner [*name*] will find out about your registration decision **directly before** [he/she] can decide whether [he/she] wants to register.
Step 3: Your partner [*name*] decides whether [he/she] wants to register for a COVID-19 vaccination right away.

You do not find out about the decision of your partner [*name*].

Task: Confirm that you have understood the instructions by selecting the correct answers below.

Question 1: Will your partner [name] find out with a high probability whether you want to register?
Reply options: *Yes/No*
Question 2: When will your partner [*name*] find out about your registration decision? **Directly before** or **only after** [he/she] can register for a COVID-19 vaccination?
Reply options: *Directly before/Only after*

V.D Instructions Receivers *"informed before"* and *"informed after"*

Instructions: The survey proceeds as follows:

Step 1: Your partner [*name*] decides whether [he/she] wants to register for a COVID-

19 vaccination right away.
Step 2: You decide whether you want to register for a vaccination now. Since you are the second to decide you may have to wait for a moment.

We do not tell your partner [*name*] whether you want to register for a vaccination.

Task: Please confirm that you have understood these instructions by selecting the correct answer below.
Question: Will your partner find out about your decision?
Reply options: *Yes/No*

new page

VI Vaccination willingness

VI.1.A First stage Senders *"not informing partner"*

Reminder: Below we will provide you and your partner [*name*] with the opportunity to go to the official registration website of the Bavarian Ministry of Health to complete the registration process.

Your partner [*name*] will not know whether you wish to register for a COVID-19 vaccination.

Remember: Your partner [*name*] **will not** learn about your registration decision.
Question 1: What do you think? How likely is it that your decision to register or not to register will influence your partner's decision?

Hints:

- Click on the bar at the bottom to reveal the slider.
- Then move the slider to give your answer.
- 0 percent means "there is no way I can influence my partner with my decision".
- 100 percent means "I can definitely influence my partner with my decision".

Remember: Your partner [*name*] **will not** learn about your registration decision.
Question 2: What do you think? How likely is it that your partner will make the same decision as you?

Hints:

- Click on the bar at the bottom to reveal the slider.
- Then move the slider to give your answer.

- 0 percent means “my partner will definitely not decide the same way I do”.
- 100 percent means “my partner will definitely decide like me” .

VI.1.B First stage Senders *“informing partner after”*

Reminder: Below we will provide you and your partner [*name*] with the opportunity to go to the official registration website of the Bavarian Ministry of Health to complete the registration process.

Your partner [*name*] will learn with a high probability whether you wish to register for a COVID-19 vaccination.

Remember: Your partner [*name*] will learn about your registration decision only after [he/she] has already decided whether to register for COVID-19 vaccination now.
Question 1: What do you think? How likely is it that your decision to register or not to register will influence your partner's decision?

Hints:

- Click on the bar at the bottom to reveal the slider.
- Then move the slider to give your answer.
- 0 percent means “there is no way I can influence my partner with my decision”.
- 100 percent means “I can definitely influence my partner with my decision”.

Remember: Your partner [*name*] will learn about your registration decision only after [he/she] has already decided whether to register for COVID-19 vaccination now.
Question 2: What do you think? How likely is it that your partner will make the same decision as you?

Hints:

- Click on the bar at the bottom to reveal the slider.
- Then move the slider to give your answer.
- 0 percent means “my partner will definitely not decide the same way I do”.
- 100 percent means “my partner will definitely decide like me” .

VI.1.C First stage Senders *'informing partner before'*

Reminder: Below we will provide you and your partner [*name*] with the opportunity to go to the official registration website of the Bavarian Ministry of Health to complete the registration process.

Your partner [*name*] will learn with a high probability whether you wish to register for a COVID-19 vaccination.

Remember: Your partner [*name*] will learn about your registration decision right before [he/she] decides whether to register for a COVID-19 vaccination.
Question 1: What do you think? How likely is it that your decision to register or not to register will influence your partner's decision?

Hints:

- Click on the bar at the bottom to reveal the slider.
- Then move the slider to give your answer.
- 0 percent means "there is no way I can influence my partner with my decision".
- 100 percent means "I can definitely influence my partner with my decision".

Remember: Your partner [*name*] will learn about your registration decision right before [he/she] decides whether to register for a COVID-19 vaccination.
Question 2: What do you think? How likely is it that your partner will make the same decision as you?

Hints:

- Click on the bar at the bottom to reveal the slider.
- Then move the slider to give your answer.
- 0 percent means "my partner will definitely not decide the same way I do".
- 100 percent means "my partner will definitely decide like me" .

new page

VI.2.A Registration intent Senders *"not informing partner"*

Reminder: if you live in Bavaria and want to get vaccinated, this registration is required to get a vaccination appointment at a Bavarian vaccination center.

Your partner [*name*] **will not** learn if you want to register for a COVID-19 vaccination.

Question: Would you like to register for a COVID-19 vaccination?
Reply options: *Yes/No*

VI.2.B Registration intent Senders *'informing partner after'*

Reminder: if you live in Bavaria and want to get vaccinated, this registration is required to get a vaccination appointment at a Bavarian vaccination center.
Your partner [*name*] will learn with a high probability if you wish to register for a COVID-19 vaccination.

Important: Your partner [*name*] will learn about your registration decision only after [he/she] has already decided whether to register for a COVID-19 vaccination.

Question: Would you like to register for a COVID-19 vaccination?
Reply options: *Yes/No*

VI.2.C Registration intent Senders *'informing partner before'*

Reminder: if you live in Bavaria and want to get vaccinated, this registration is required to get a vaccination appointment at a Bavarian vaccination center.
Your partner [*name*] will learn with a high probability if you wish to register for a COVID-19 vaccination.

Important: Your partner [*name*] will learn about your registration decision directly before [he/she] decides whether to register for a COVID-19 vaccination.

Question: Would you like to register for a COVID-19 vaccination?
Reply options: *Yes/No*

VI.2.D Registration intent Receivers *'informed after'*

Reminder: if you live in Bavaria and want to get vaccinated, this registration is required to get a vaccination appointment at a Bavarian vaccination center.
Your partner will not know if you want to register.

Question: Would you like to register for a COVID-19 vaccination?
Reply options: *Yes/No*

VI.2.E Registration intent Receivers *'informed before'*

Reminder: if you live in Bavaria and want to get vaccinated, this registration is required to get a vaccination appointment at a Bavarian vaccination center.
Your partner will not know if you wish to register.

Important: Your partner [*name*] [would like/would not like] to register for a COVID-19 vaccination.

Question: Would you like to register for a COVID-19 vaccination?
Reply options: *Yes/No*

new page

VI.3 Registration for COVID-19 vaccine

Would you like to register now?
To register, please click on **Yes, register now for a COVID-19 vaccination** below.
This will open the official registration website of the Bavarian Ministry of Health in a new browser window or tab. To successfully register for a COVID-19 vaccination, follow the instructions on the registration website.

Important: Please do not close the browser window or tab in which you are answering the survey during registration.

Additional Notes: We do not have any access to the information you provide on the registration website. Registration is voluntary and it does not entail an obligation to get vaccinated. Your reward for this survey is independent of whether you register.

Button: *Yes, register for the COVID-19 vaccination right away.*

[*Opens the link to the official registration website.*]

Have you successfully registered?

Here's how to proceed: once you have registered, please continue with the survey by clicking **Yes, I have registered and would like to continue with the survey** at the bottom of this page.

Don't want to register now?
If you do not wish to register now, you will not be penalized in any way, for example by being paid less for this survey. To continue with the survey, please click **No, I have not registered and would like to continue with the survey** at the bottom of this page.

To continue with the survey, please answer the following question:
Question: have you just register for the COVID-19 vaccination?

Reply options:

- *No, I have not registered and would like to continue with the survey*
- *Yes, I have registered and would like to continue with the survey*

new page

VI.4 Confirmation of registration for COVID-19 vaccination

Now confirm your registration: You have indicated that you have just registered online for a Corona vaccination.
You should have received a confirmation email after completing your registration.
Please provide the following two pieces of information from the confirmation email sent out by the vaccination center:

1. Email Address
2. Subject

Lottery: If both of your answers are correct, you can win one of 30 Amazon vouchers worth 20€.

You must complete the survey to qualify for the lottery.

Further notes: Providing this information does not allow us to infer anything about you as a person. You remain completely anonymous. You can also continue with the survey without answering the questions. However, you will then not be able to participate in the lottery draw.

Question 1: What is the email address from which you received the confirmation email?

Question 2: What is the subject of the confirmation email you received from the vaccination center?

new page

VI.5 What do you think about the COVID-19 vaccine?

Question 1: What do you think? How safe is the COVID-19 vaccination?
Reply option: *Likert scale (1-7) with 1: not at all safe, 7: extremely safe.*

Question 2: What do you think? How effective is the COVID-19 vaccination?
Reply option: *Likert scale (1-7) with 1: not at all effective, 7: extremely effective.*

Question 3: What do you think? To what extent is it socially desirable to get vaccinated against COVID-19?
Reply option: *Likert scale (1-7) with 1: not at all socially desirable, 7: extremely socially desirable*

Question 4: To what extent do you agree with the following statement? Statement: if everyone is vaccinated against COVID-19, I don't need to get vaccinated too.
Reply option: *1: do not agree at all, 7: agree completely*

new page

Question: How likely are you to get vaccinated against COVID-19?
Please use the bar/slider for your answer.

- Click on the bar at the bottom to reveal the slider.
- Then move the slider to make your selection.
- 0 percent means "definitely not willing to get vaccinated."
- 100 percent means "definitely willing to get vaccinated."

new page

VII Further demographic information

To conclude this survey, please provide some general information.

Question 1: What county do you live in?
Question 2: What is your zip code?
Question 3: What was your household's monthly net income last year?
Note: We mean the sum that results from wages, salaries, income from self-employment,

pensions, income from public aid, income from letting, housing allowances, child benefits and all other incomes, after the deduction of taxes and social security contributions.
Reply options:

- Less than 1,100 EUR
- 1.100 - 1.500 EUR
- 1,501 - 2,000 EUR
- 2,001 - 2,600 EUR
- 2,601 - 4,000 EUR
- 4,001 - 7,500 EUR
- More than 7,500 EUR

Question 4: What is your highest educational degree (general or vocational)?

new page

VIII End of survey

Thank you for participating in our survey!

In the following, we list your performance in the task in which you had to assign artworks to artists together with your partner and inform you whether you have won one of the Amazon vouchers. Afterwards, we ask you to answer two more questions about this survey yourself and give you the opportunity to give us feedback on the survey.

- Unfortunately, you have not won one of the raffled Amazon vouchers./Congratulations, you have won one of the raffled Amazon vouchers.
- If you would like to know how you and your partner did on your shared task, please click here. [*Upon clicking the button, participants' answers and the corresponding solutions open in the same window.*]
- *For Receivers 'informed after':* Finally, we would like to inform you that your partner [*name*] [registered/did not register] for a COVID-19 vaccination.
- Thank you again for participating in our survey.

Please answer the following questions to complete the survey:

Question 1: What do you think? What was the purpose of this survey?
Question 2: Where on the political spectrum would you place this survey?
Hints: Please use the slider to tell us the extent to which you felt this survey was leaning more toward the political right or toward the political left.
Click on the bar below to reveal the slider. Then move the slider to make your selection.

Feedback If you would like to give us any feedback on the survey, please feel free to do so here.

Would you like to close the survey now?

*Click on **Close survey***

CHAPTER 3

CAN GRASSROOTS ORGANIZATIONS REDUCE SUPPORT FOR RIGHT-WING POPULISM VIA SOCIAL MEDIA?

3.1 Introduction

Right-wing populism has been on the rise throughout Western democracies. The academic literature studying this trend has pointed to a number of contributing factors and, in particular, to economic and cultural grievances.[1] Other scholars suggest that the growing importance of social media spurred the rise of right-wing populism, for example, via the spread of fake news or the creation of "echo-chambers".[2] At the same time, due to their "low barriers to entry and reliance on user-generated content" (Zhuravskaya et al. 2020, p. 416), social media has reduced the costs for grassroots movements to enter the political arena and achieve significant outreach (Zhuravskaya et al. 2020). Indeed, we observe a growing number of grassroots efforts exploiting social media to contend against right-wing populism across Western countries.[3] In light of these trends, the question emerges whether grassroots organizations can reduce support for right-wing populism via social media.[4]

We study this question in the context of an experimentally controlled, randomized Facebook campaign by a German grassroots organization during a series of recent elections. The campaign aimed to reduce electoral support for the "*Alternative for Ger-*

[1] Anelli et al. (2019) study the consequences of the loss of jobs in manufacturing; Autor et al. (2020), Colantone and Stanig (2018), and Dippel, Gold, et al. (2021) analyze the effect of trade exposure; Halla et al. (2017) and Steinmayr (2021) study the role of immigration; Cantoni, Hagemeister, et al. (2020) assess the impact of changes in the supply of political platforms; and Inglehart and Norris (2016) and Margalit (2019) discuss the role of cultural grievances.

[2] Allcott and Gentzkow (2017), Tufekci (2018), or Zhuravskaya et al. (2020) offer different perspectives on this hypothesis.

[3] This phenomenon is, e.g., covered in the following media articles: Mayer (2017, *Huffington Post*), Manjoo (2017, *New York Times*), and *The Guardian*, and Tsakiridis (2021, *BR24*).

[4] In his account of the political far right, Mudde (2019), for example, concludes that it is unclear whether grassroots efforts such as protests are successful in reducing support for right-wing populists.

many" (AfD); a German right-wing populist party which has enjoyed considerable electoral success since 2016.[5] Similar to right-wing populists elsewhere, the AfD campaigns on a national-conservative, anti-immigrant, and at times even xenophobic platform (Cantoni, Hagemeister, et al. 2020; Häusler 2018; Schellenberg 2018). Exploiting experimental variation as to where the organization disseminated its Facebook ads, we find that the organization's campaign did not significantly affect the AfD's vote share and turnout: our treatment effect estimates are small in magnitude, precisely estimated, and robust to an array of empirical specifications. In combination with the high statistical power of our experiment, our estimates are thus more likely to reflect the "true" absence of any meaningful treatment effects than insufficient statistical power. Using a complementary online survey experiment, we show that this finding can be explained by insufficient outreach on Facebook and the lack of individual-level responses of attitudes and behavior to the campaign. We further demonstrate that the campaign's effectiveness could not have been increased by highlighting common identity traits between the organization and its audience.

We conducted these experiments in collaboration with the organization "*Kleiner Fünf*" (K5), which is part of a broader grassroots movement contending against right-wing populism in Germany. K5 tries to reduce the AfD's vote share and, simultaneously, to increase turnout of non-AfD partisans by the means of distributing political advertisements on Facebook directly placed on users' feeds (hereafter called campaign ads). We embedded a pre-registered field experiment in K5's Facebook campaign during the run-up to the 2019 state elections in Brandenburg, Saxony, and Thuringia, for which polls predicted that the AfD could emerge as the strongest political force.

Our field experiment leverages the detailed geographical targeting options for Facebook advertisements to distribute K5's campaign ads only to a random subset of postal districts in Brandenburg, Saxony, and Thuringia. We first stratified postal districts into groups of four, based on predetermined characteristics, and then assigned exactly half of postal districts in each stratum to the treatment group and the remainder to the control group. While Facebook users in treated postal districts were exposed to K5's campaign ads, no such ads were disseminated in control postal districts. Campaign ads consisted of short videos, illustrations, and texts addressing various aspects of the AfD's political agenda. Hence, by comparing election outcomes between treatment and control postal districts, we can assess the causal impact of K5's Facebook campaign on electoral support for the AfD and turnout.

[5] The AfD satisfies several of the criteria the political science literature has developed to classify parties as "right-wing populist" (Golder 2016; Häusler 2018; Mudde 2004, 2019): first, the AfD takes typical right-wing stances on immigration, security, and foreign policy. Second, the AfD frequently employs the stylized antagonisms of the "the true people" vs. "the corrupt elite", a key characteristic of populists. Other studies referring to the AfD as a right-wing populist party include, for example, Cantoni, Hagemeister, et al. (2020). See Häusler (2018) for a detailed description of the rise of the AfD.

We find that K5's Facebook campaign did not meaningfully affect election outcomes: contrary to K5's aims, we estimate that the AfD's vote share was 0.05 percentage points *higher* and turnout was 0.26 percentage points *lower* in postal districts exposed to K5's Facebook campaign. Our estimates are robust to the inclusion of stratum fixed effects and computing treatment effects using differences in means. Standard errors are of the same magnitude as point estimates and Fisher exact p-values are virtually identical to p-values derived from cluster-robust standard errors, suggesting that our estimates are fairly precise. To assess the magnitude of treatment effects, we follow DellaVigna and Gentzkow (2010) and compute persuasion rates of 0.74 to 2.14 percent for AfD voting and turnout, respectively. This puts K5's campaign at the lower end of the distribution of persuasion rates observed in similar contexts (e.g. DellaVigna and Gentzkow 2010). In combination with the fact that our experiment was designed to detect effect sizes starting at about 3 percent of a standard deviation 80 percent of the time, our estimates are thus more likely to reflect "true" zero effects than insufficient statistical power.

We further explore whether these average treatment effects hide systematic heterogeneities: ex ante, we expected stronger treatment effects in areas with a large pool of citizens at the margin of voting at all and of voting for the AfD in particular. To assess this hypothesis, we compare treatment effects on AfD voting between postal districts with a strong history of AfD voting and high turnout and those with low AfD support and turnout. Yet, regardless of the outcome considered, we find no statistically significant treatment effects for this particular set of postal districts either.

Using a complementary online survey experiment conducted with a sample of around 1,700 voting-age individuals from the same three states, we explore potential explanations for the absence of significant treatment effects. This experiment yields three sets of results: first, by comparing the share of survey participants who had seen K5's campaign ads before commencing with the survey, we document that the effective outreach of K5's campaign on Facebook was insufficient to induce changes in aggregate voting behavior. This finding is most likely the result of K5's main donor withdrawing its funding right before the launch of the campaign, resulting in a campaign budget one order of magnitude smaller than expected.

Second, by exposing a random subset of participants to K5's campaign ads during our survey experiment, we find that individual-level treatment effects on attitudes and self-reported voting behavior only weakly point toward reduced support for right-wing populism. Our estimates are, at best, modest in size, short-lived, and most importantly, insignificant most of the time. We further document the absence of significant treatment effects on two revealed preference outcomes (donations and intended signatures of a petition), implying that K5's campaign ads were not able to meaningfully shape individual-level outcomes.

Third, we test whether highlighting identity traits that K5 and its audience share boosts the impact of K5's campaign ads. In investigating this strategy we follow a recent strand of the literature arguing that populists' frequent usage of the antagonism between "the true people" and "the elites" is key to understanding their success.[6] In this stylized view of the world, grassroots organizations such as K5, with their many college-educated supporters from urban centers, are part of "the elites" and as such, their identities overlap only little with a considerable fraction of AfD supporters.[7] This raises the question whether K5's campaign ads exhibit stronger effects if shared identity traits are highlighted. In our online survey experiment, we thus varied participants' perceptions of K5's (regional) identity by informing half of participants that K5 is based in Berlin and the remainder that K5 has many supporters in the participants' state of residence. We find that this additional treatment does not boost the impact of K5's campaign ads on attitudes and (self-reported) behavior. In sum, our individual-level results suggest that – even in a scenario where K5 generated sufficient outreach on Facebook – its campaign ads would most likely not have been able to significantly affect aggregate election outcomes, irrespective of whether shared identity traits are highlighted or not.

Our study relates to several research agendas in economics and political science: first, our paper adds a new perspective to the burgeoning literature on the rise of (right-wing) populism. A prominent view in this literature is that growing economic insecurity resulting from the demise of traditional manufacturing and the threats posed by increasing globalization and immigration together account for a significant portion of the rise of right-wing populism.[8] Other scholars instead highlight the role of cultural factors: Inglehart and Norris (2016) and Margalit (2019), for example, argue that the recent populist successes can be best understood as a backlash against progressive cultural change. In our study, we focus on the flip-side of this development by asking how civil society responds to the rise of right-wing populism. In particular, we observe an increasing number of grassroots efforts to contend against right-wing populism, both in the streets and online.[9] Yet, what remains unclear is whether these grassroots efforts are successful in reducing support for right-wing populism. We provide new field experimental evidence on this question by studying one such grassroots campaign, which leverages Facebook ads to limit electoral support for Ger-

[6]For variants of this argument please see, e.g., Golder (2016), Mudde (2019), and Müller (2017).

[7]Decker (2016, 2020) and Hambauer and Mays (2018) provide detailed accounts of which segments of society support the AfD.

[8]Scholars advocating this view include Anelli et al. (2019), Autor et al. (2020), Halla et al. (2017), and Rodrik (2021).

[9]In 2018, an estimated 250,000 people protested against the far right in Berlin and between 3 and 5 million participated in the Women's Marches against Donald Trump across the United States in 2017 which followed a viral campaign on Facebook (Mayer 2017; Mudde 2019).

many's main outlet of right-wing populism, the AfD. As such, our results also inform a growing body of literature examining the impact of the arrival of social media on politics more generally (Bond et al. 2012; Bursztyn, Egorov, Enikolopov, et al. 2019; Zhuravskaya et al. 2020).

Second, our study contributes to the long-standing debate on the effectiveness of political advertisements in shaping election outcomes (DellaVigna and Gentzkow 2010; Gentzkow 2006; Gerber et al. 2009; Pons 2018; Spenkuch and Toniatti 2018). The closest to our own paper is Hager (2019), who examines a large national field experiment in Europe and shows that online ads can indeed have an impact on aggregate election results. Our own results, however, are more in line with a recent review by Kalla and Broockman (2018), who conclude that, on average, advertising does not affect candidate choice in general elections in the US. Beyond adding another set of estimates, we extend this literature by studying a different type of political interest group: grassroots organizations. While we find no evidence that the particular campaign we study was able to shape election outcomes, other campaigns may generate larger effects.

The remainder of this paper is organized as follows: in the next section, we discuss the design and results from our field experiment on Facebook. Then, in Section 3.3, we describe the design and summarize the results from the complementary online survey experiment, which we employ to explain our findings from the field experiment. Section 3.4 concludes this paper.

3.2 Field Experiment

3.2.1 Context and timeline

We conducted a pre-registered field experiment on Facebook during the run-up to the 2019 state elections in Brandenburg, Saxony, and Thuringia.[10] The experiment was split into two waves: the first wave ran in Brandenburg and Saxony in the last week of August until the state elections on September 1, 2019; the second wave took place in Thuringia in the week leading up to the elections on October 27, 2019.

To carry out this experiment, we partnered with *Kleiner Fünf* (K5), which is part of a larger network of grassroots organizations contending against right-wing populism in Germany.[11] K5 is a civil-society organization and as such, is predominantly financed by donations. K5's active supporters number in the hundreds; most of them are stu-

[10] We pre-specified all features of our experimental design in our pre-analysis plan, which we stored at the AEA RCT registry under RCT ID AEARCTR-0004622 before the experiment commenced. The Ethics Committee of the Department of Economics at LMU Munich approved the experimental design outlined in this section, protocol 2019-13.

[11] Other organizations that are part of this (informal) network include, for example, *Amadeu Antonio Stiftung*, *Aufstehen gegen Rassismus*, and *Offene Gesellschaft*.

dents and young professionals living in urban centers throughout Germany.[12] K5's main objective is to limit electoral support for Germany's most successful right-wing populist party, the Alternative for Germany (*Alternative für Deutschland*, AfD). While in its early years the AfD predominantly pursued a fiscally conservative agenda centering around the European currency crisis (2012–2014), the party has increasingly adopted a national-conservative, anti-immigrant, and at times even xenophobic platform starting in 2015 (Cantoni, Hagemeister, et al. 2020; Häusler 2018). As such, the AfD's platform is similar to right-wing populist parties in other European countries including the UK, France, or the Netherlands (Schellenberg 2018). Following the spike in immigration from non-European countries in 2015 and 2016, the AfD enjoyed several consecutive electoral successes both at the state and the national level (Häusler 2018): the most significant of which was when the AfD emerged as the third strongest force in parliament in the 2017 federal elections. The AfD was particularly successful in the eastern states, including Brandenburg, Saxony, and Thuringia where it obtained more than 20 percent of the vote (Bundeswahlleiter 2017). The 2019 state elections in these three states were thus of considerable importance for K5, especially because several polls even predicted that the AfD could emerge as the strongest force in at least two out of the three newly elected state parliaments.[13] Against this background, K5 decided to evaluate the effectiveness of its campaign by the means of a randomized experiment.

K5 estimated that its budget would be sufficient to distribute its campaign ads to a significant share of Facebook users in these states at a considerable frequency. Yet, only days before the campaign was scheduled to go live in Brandenburg and Saxony, K5's main donor for this particular campaign withdrew its funds, resulting in a fall in the campaign budget by one order of magnitude. Despite this, K5 decided against limiting its campaign to certain areas in either of the three states, and instead uniformly decreased the frequency at which its ads were displayed to Facebook users in the areas selected for the campaign.

3.2.2 Experimental design

3.2.2.1 Treatment

K5's campaign for the 2019 state elections was designed to reduce electoral support for the AfD and, simultaneously, to increase turnout of non-AfD partisans. To dissuade

[12]We conducted an additional survey among members of K5 to collect data on members' socioeconomic background and their political preferences. Please see K5's website to learn more about the organization: https://bit.ly/3knIRzo (last accessed August 24, 2021).

[13]Wikipedia lists the predictions by a wide array of polls for each of the three state elections: Brandenburg (https://bit.ly/3C32PqE, last accessed September 10, 2021), Saxony (https://de.wikipedia.org/wiki/Landtagswahl_in_Sachsen_2019, last accessed September 10, 2021), and Thuringia (https://bit.ly/3E72XHl, last accessed September 10, 2021).

citizens from casting their votes for the AfD, K5 developed campaign ads centered around the idea of what would happen if the AfD managed to successfully implement its preferred policies:[14] (i) impose tighter restrictions on migration; (ii) implement policies fostering national identity; and (iii) roll back climate change mitigation policies. For example, to address the AfD's goal of reducing efforts to mitigate climate change, K5 listed the adverse consequences of global warming for that particular region, which would include a marked increase in the frequency of droughts and floods.[15] K5's campaign ads used similar projections to draw attention to the potential consequences of the AfD entering the government in the domains of migration and national identity. To increase turnout, K5's campaign tried to leverage social multipliers – that is, to specifically target non-AfD partisans who, they suspected, would be willing to motivate their peers to vote in the election.[16]

3.2.2.2 Sample and data

Postal districts constitute both the unit of observation and randomization in our experiment. We chose postal districts because they constituted the lowest geographical level to which Facebook advertisements could be targeted at the time of the experiment. We employed 760 postal districts from Brandenburg, Saxony, and Thuringia in our experiment.[17] Their locations, alongside their treatment status, are shown in Figure 3.1, which documents that the postal districts are evenly spaced throughout the three states, ensuring that the estimated treatment effects are not driven by regional peculiarities.

For each postal district, we collected data on the 2017 federal elections ("*Bundestagswahlen*") and the 2019 state elections ("*Landtagswahlen*") from the respective election authorities in charge.[18] This includes the total number of eligible voters, the number of valid votes, and the total number of valid votes for each party. While we use results from the 2017 federal elections to stratify our sample before conducting the actual randomization, we employ the results from the 2019 state elections to construct our main dependent variables of interest: (i) the AfD's vote share and (ii) turnout.[19]

[14] The following three links forward to the AfD's manifestos for the 2019 state elections: Brandenburg ((https://bit.ly/3xYt3rD, last accessed August 20, 2021), Saxony (https://bit.ly/3syLmTk, last accessed August 20, 2021), and Thuringia (https://bit.ly/2UzDzYX, last accessed August 20, 2021).

[15] A collection of K5's campaign ads disseminated during this particular campaign can be found here: https://bit.ly/3AVCJVH (last accessed August 20, 2021).

[16] For more details on K5's strategy to increase turnout, please see K5's campaign website: https://bit.ly/2WeGgQe (last accessed August 20, 2021).

[17] In total, the three states contain 815 postal districts. However, we had to drop postal districts not fully contained in either of the three states, plus a few more due to our randomization strategy which required that the total number of postal districts must be divisible by four.

[18] We collected the official municipality-level results for the 2017 federal elections from Regionalstatistik (2017) and for the 2019 state elections from the Landeswahlleiter des Freistaates Sachsen (2019), Landeswahlleiter für Brandenburg (2019), and Thüringer Landeswahlleiter (2019).

[19] To construct these statistics, we focus on voters' list vote "*Zweitstimme*" which governs the distribu-

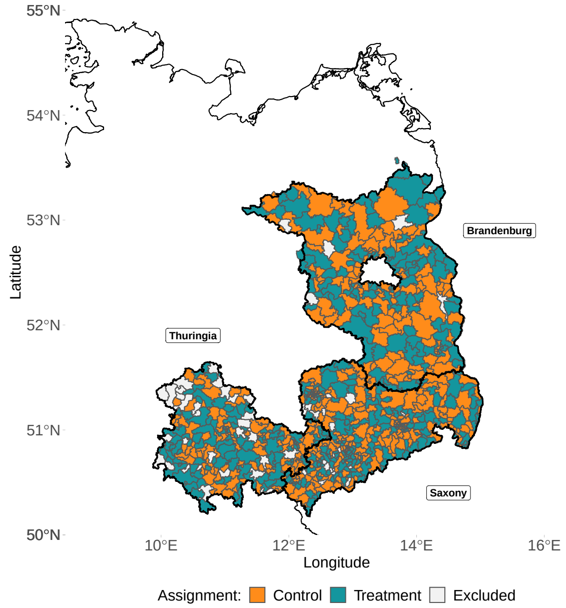


Figure 3.1: Spatial distribution of treatment and control postal districts

Notes: Location of all 760 postal districts in our sample, alongside their treatment status, reported. Facebook users living in treatment districts were exposed to K5's campaign ads, while users in control districts were not.

We combined election data with additional postal district characteristics (e.g. area or population) from an online service provider.[20]

German election authorities do not publish election results at the postal district level. Instead, results are reported at the municipality or precinct level, which do not, however, necessarily map on postal districts. Outside of larger, densely populated cities, postal districts can cover several adjacent municipalities. Yet, under the assumption that voters are homogeneously distributed across these municipalities, we can aggregate election results to the postal district level by overlaying the geospatial vectors of municipalities with those of the postal districts. For each postal code i and each municipality j, we calculated the share of the area of i that is covered by j. We use the resulting $I \times J$ matrix to aggregate data to the postal district level.[21] Municipalities with above 50,000 inhabitants required a different approach as they contain several adjacent postal districts. Thus, we employed data on the electoral precinct level to obtain election results on the postal district level. To this end, we first matched electoral precincts to postal districts using the street address of the precincts' polling stations. Second, we apportioned precinct-level results using the number of eligible voters and then aggregated the results to the postal district level.

tion of parliamentary seats to parties and thus, which parties form the state government.

[20] We obtained this data from *Suche-Postleitzahl.org* (https://bit.ly/3kigQcK, last accessed August 24, 2021).

[21] This approach was previously used by Hager (2019).

Table 3.1: Summary statistics on postal districts

Statistic	Mean	St. Dev.	Min	Max	N
ZIP code characteristics					
Area (in sqkm)	79.11	96.39	1.65	891.89	760
Population (x 1000)	10.91	8.43	0.52	44.35	760
In Brandenburg	0.28	0.45	0.00	1.00	760
In Saxony	0.48	0.50	0.00	1.00	760
In Thuringia	0.25	0.43	0.00	11.00	760
Results previous election (2017 federal elections)					
Number of valid votes (x 1000)	5.07	4.75	0.19	33.90	760
Turnout (in %)	64.65	7.10	46.49	84.50	760
CDU vote share (in %)	27.67	4.15	13.64	49.40	760
AfD vote share (in %)	26.44	6.47	9.09	44.46	760
SPD vote share (in %)	12.76	3.77	5.81	24.55	760
GRUENE vote share (in %)	3.70	2.40	0.98	18.76	760
FDP vote share (in %)	7.48	1.56	3.71	13.14	760
LINKE vote share (in %)	15.48	3.40	5.20	32.03	760

Notes: Election results on the postal district level were imputed either from the municipality level (municipalities with less than 50,000 inhabitants) or the precinct level (municipalities with more than 50,000 inhabitants).

The summary statistics for our sample of postal districts are presented in Table 3.1. The average postal district in our sample exhibits a strong history of AfD voting: in the 2017 federal elections, the AfD obtained, on average, 26.4 percent of the vote in Brandenburg, Saxony, and Thuringia, which is considerably higher than the national average of 12.6 percent (Bundeswahlleiter 2017). In Saxony, the AfD even emerged as the strongest force in 2017. When comparing the results from our postal district aggregation to the official state election statistics, we find that the resulting vote share of the AfD is only 0.1 percentage points shy of the party's official result across all three states, implying that the aggregation method performed well. More generally, our sample is suitably representative of the full set of postal districts in terms of election results, area, and population including the distribution of districts across states.[22]

3.2.2.3 Randomization

Our design randomly assigned each of the 760 postal districts either to the treatment or the control group. Following Athey and Imbens (2017), we stratified postal districts into groups of four based on predetermined characteristics, which has three main advantages over a non-stratified design: first, this stratified design limits the scope for differences in predetermined characteristics between the treatment and control group, which is especially relevant in our setting where postal districts differ markedly in terms of population size and past election outcomes. Second, ex ante stratification

[22]The average postal district in our sample covers an area of 79 square kilometers (ca. 30 square miles) and has a population of 11,000 inhabitants. Roughly half of postal districts in our sample are in Saxony, reflecting the fact that Saxony has about the same number of inhabitants as Brandenburg and Thuringia combined.

allows us to incorporate covariates in the analysis, while still being able to employ simple differences in means as an alternative to conventional regression estimates.[23] Third, and most importantly, stratification boosts the statistical power of our design: our power calculations yield that our experiment would detect effect sizes of approximately 3 percent of a standard deviation in 80 percent of iterations, corresponding to approximately a 0.25-percentage point change in the AfD's vote share.

To conduct the stratification, we built pairs of postal districts, which minimize the bilateral differences in predetermined characteristics between postal districts using the optimal matching algorithm provided by the R package *nonbimatch*.[24] Then, we used the same algorithm to generate pairs of pairs – that is, we matched each pair of postal districts with the pair which was the most similar in terms of the average of the predetermined characteristics.[25] We used the vote share of the AfD and of the CDU ("*Christian Democratic Union*") in the 2017 federal elections, as well as population size, to build strata of four postal districts. We included the AfD's past vote share because previous election results exhibited substantial persistence across election cycles. Under the assumption that this relationship carried over to the 2019 elections, the AfD's vote share in the 2017 federal elections was likely to constitute a good predictor of the AfD's electoral performance in 2019.[26] We included the vote share of the CDU in the 2017 federal elections, because exit polls after previous elections have revealed that many former CDU voters switched to the AfD, and we expected similar dynamics for the 2019 elections. Finally, we also incorporated population size in our list of stratification variables, because the distribution of population size across postal districts is heavily skewed, with only a few very large postal districts. As a result, imbalance in population size between the treatment and control group may arise despite random assignment of postal districts. In the third and final step of our randomization procedure, we randomly assigned exactly two postal districts within each stratum of four postal districts to the treatment group, while the remainder was assigned to the control group.[27]

[23] Athey and Imbens (2017) remind us that differences in means are generally preferable over regression analysis in terms of the accuracy of treatment effects and statistical inference when analyzing data drawn from randomized experiments. Specifically, Athey and Imbens (2017, p. 94) emphasize that if researchers use regressions to analyze randomized experiments, they may "end up with analyses that rely on a difficult-to-assess mix of randomization assumptions, modeling assumptions, and large sample approximations." Hence, both estimated treatment effects and inference results based on standard regression assumptions may be misleading. Therefore, we supplement our main regression estimates with simple differences in means and report Fisher exact p-values derived from permutation tests as an alternative approach to statistical inference.

[24] For more information on the *nonbimatch* function and the *nbpMatching* package, please see the package vignette at https://bit.ly/3BsJFKu.

[25] We chose this strategy as it performed slightly better in our power calculations than algorithms minimizing within-stratum differences for groups of four.

[26] As we document in Table 3.B.2 in the Appendix 3.B, the AfD's vote share in the 2017 federal elections indeed constituted an important predictor of AfD voting in the 2019 state elections.

[27] Assigning a pre-specified *number* of experimental units in each stratum to either the treatment or

Table 3.2: Predetermined characteristics compared across experimental conditions

	Group means		Test for equal means	
	Control	Treatment	Δ (stand.)	p-value
	(1)	(2)	(3)	(4)
ZIP code characteristics				
Area (in sqkm)	78.33	79.9	1.57	0.82
Population (x 1000)	10.97	10.86	-0.11	0.86
In Brandenburg	0.28	0.27	-0.01	0.75
In Saxony	0.48	0.47	-0.01	0.83
In Thuringia	0.24	0.26	0.02	0.56
Results last election (2017 federal elections)				
Number of valid votes (x 1000)	5.11	5.03	-0.08	0.82
Turnout (in %)	64.57	64.74	0.17	0.74
CDU vote share (in %)	27.65	27.68	0.04	0.90
AfD vote share (in %)	12.78	12.74	-0.05	0.87
SPD vote share (in %)	3.72	3.69	-0.04	0.83
GRUENE vote share (in %)	7.52	7.44	-0.08	0.49
FDP vote share (in %)	15.46	15.50	0.04	0.88
LINKE vote share (in %)	26.44	26.44	-0.01	0.99
Test for joint significance				
				1.00

Notes: Means of each predetermined characteristic reported by treatment condition. Facebook users living in treatment postal districts were exposed to K5's campaign ads, while those living in control districts codes were not. Δ captures the mean difference between the treatment and the control group, which we estimate using the following regression model: $characteristic_i = \alpha + \beta \cdot treat_i + \epsilon_i$, where $treat_i$ is a dummy variable taking value 1 if postal district i was assigned to the treatment group, and 0 otherwise. To enhance comparability of estimates across characteristics, all estimated differences (Δ) are standardized using the mean and standard deviation in the control group. p-values testing for equal means derived from robust standard errors reported. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.2.2.4 Sample balancing

In Table 3.2, we compare the means of thirteen predetermined characteristics between the treatment and control group. All pairwise comparisons yield no significant differences between postal districts in the treatment and control group, implying that our randomization was successful in balancing pre-determined characteristics. This finding thus minimizes the risk of wrongly attributing any potential differences in voting behavior detected for the 2019 state elections to K5's Facebook campaign instead of predetermined differences.[28]

the control group ("*complete randomization*") is preferable over a procedure in which experimental units are assigned with a pre-specified *probability* to either of the two group for two reasons. First, complete randomization avoids imbalanced treatment-control shares which may weaken statistical power. Second, complete randomization does not require a re-weighting with the inverse probability weights when conducting statistical inference. For more information please see Athey and Imbens (2017) and the DeclareDesign.org blog post on randomization techniques (https://bit.ly/3xXFm7G, last accessed August 20, 2021).

[28]In Table 3.2, we also report the p-value for a test of joint-significance of all predetermined characteristics that confirms this finding.

3.2.2.5 Implementation on Facebook

Approximately one week before each election, K5 started to disseminate its campaign ads in treatment postal districts via Facebook's business manager. To guarantee comparable treatment intensities across postal districts, the funds for the campaign were assigned in proportion to the population size of the given postal district. Considering that Hager (2019) points out that Facebook commonly reallocates funds between postal districts on the basis of users' engagement and other performance metrics, we follow Hager's (2019) strategy and generated 760 individual Facebook campaigns, each with an individual fixed budget which was proportional to the postal district population. Therefore, treatment intensities were uniform across treatment postal districts, alleviating potential concerns about effect heterogeneities arising from systematic differences in effective treatment intensities across postal districts.

3.2.3 Results

3.2.3.1 Empirical strategy

To identify the causal effect of K5's Facebook campaign on election outcomes, we estimate the following regression model:

$$y_{is} = \alpha_0 + \alpha_1 \cdot \text{Campaign ads}_i + \delta_s + \epsilon_{is} \tag{3.1}$$

where y_i is either the share of list votes ("Zweitstimmen") cast for the AfD or turnout in postal district i in stratum s; Campaign ads_i is an indicator taking value 1 if a given postal district i in stratum s was exposed to K5's campaign ads on Facebook, and 0 otherwise; and δ_s are stratum fixed effects capturing predetermined heterogeneity in terms of population size and voting behavior. We employ standard errors clustered at the postal district level throughout our analysis.[29] We complement our regression estimates with simple differences in means and Fisher exact p-values, reflecting recent advances in the analysis of data drawn from randomized experiments (Athey and Imbens 2017). We obtain Fisher exact p-values by randomly re-assigning postal districts to placebo treatment groups for 5,000 times and calculating the share of "placebo treatment effects" that exceed the "true treatment effect" in (absolute) magnitude.

[29] We observe our outcomes at the same level of aggregation at which randomization was conducted, so we do not face a standard clustering problem in our context. One may nevertheless prefer clustered standard errors in this context for two reasons. First, one may worry about spatial dependence. Second, we are using a subset of all postal districts, so some of the uncertainty in our estimates does not arise from the random assignment into experimental conditions (design-based uncertainty) but from the sampling process (Abadie et al. 2020). Thus, to be conservative, we report cluster-robust standard errors and complement these with Fisher exact p-values.

3.2.3.2 Main results

Figure 3.2 summarizes our main findings from the field experiment: the left-hand panel reports the postal-district-level average treatment effect derived from comparing the average share of votes cast for the AfD and turnout between the treatment and control group – that is, between postal districts exposed to K5's campaign ads on Facebook and those unexposed. Contrary to K5's aims, we calculate that the AfD's vote share in treatment postal districts *exceeds* that in control districts by 0.05 percentage points (Fisher exact p-value = 0.78), whereas turnout in treatment districts is 0.26 percentage points *lower* (Fisher exact p-value = 0.59).[30]

Following Athey and Imbens (2017), we complement the postal-district-level average treatment effect with the cluster-average treatment effect to leverage the full potential of ex ante stratification, which we obtain by running Equation 3.1 with stratum fixed effects. We report our point estimates for α_1 alongside the cluster-robust 95-percent confidence intervals in Figure 3.2, right panel:[31] we obtain an estimate of 0.05 percentage points (S.E. = 0.20) when employing the AfD's vote share as the dependent variable and of -0.26 percentage points (S.E. = 0.47) when using turnout, which are both insignificant. The cluster-average treatment effects and the postal-district-level treatment effects thus virtually coincide. Cluster-robust standard errors are of the same magnitude as point estimates and are almost to identical Fisher p-values, which suggests that our estimates are fairly precise. We thus conclude that K5's Facebook campaign did not, on average, exhibit any significant impact on the AfD's vote share and turnout.

To further assess the magnitude of our estimates, we employ *persuasion rates* introduced by DellaVigna and Gentzkow (2010), which they define as follows:

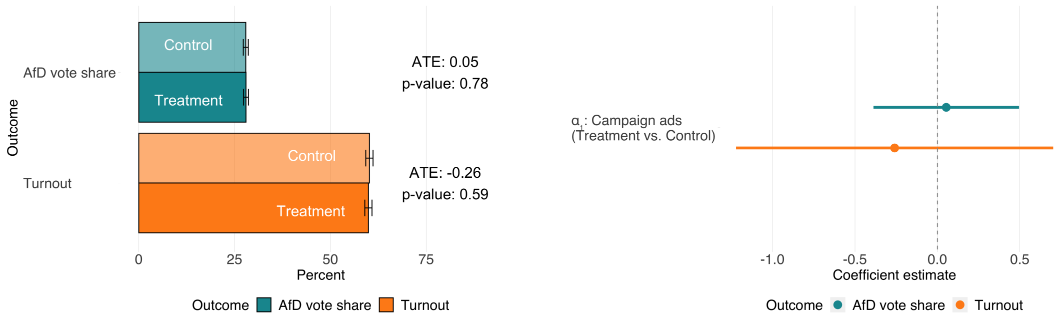
$$f = 100 \times \frac{y_T - y_C}{e_T - e_C} \times \frac{1}{1 - y_0} \tag{3.2}$$

where y_T and y_C correspond to the AfD's vote share and turnout in the treatment group and control group, respectively; e_T captures the share of the population in the treatment group that saw K5's campaign ads, which we approximate using the number provided by Facebook – that is, around four percent;[32] we follow Hager (2019) and assume that the respective share in the control group (e_C) is zero and that the AfD's

[30] We present the full distribution of placebo estimates and corresponding Fisher exact p-values in Figures 3.A.1, 3.A.2, and 3.A.3 in Appendix 3.A.

[31] We provide full regression results in Table 3.B.1 in Appendix 3.B.

[32] As we document in Section 3.3.5, the share of the voting-age population in the treatment group that recalled K5's campaign ads a few days before the election is around 0.7 percent. Yet, the share in the control group was almost identical, suggesting that $e_T - e_C$ was in reality probably much closer to zero than what Facebook's statistics suggest. To ensure that our estimated persuasion rates are nevertheless comparable to Hager (2019), we abstract from any spillovers to control districts and compute persuasion rates using the statistics provided by Facebook.



(a) Conditional means **(b) Coefficient estimates**

Figure 3.2: Treatment effects on aggregate election outcomes

Notes: Panel (a) plots the mean vote share of the AfD and the mean turnout in the 2019 state elections, in percent, for both treatment conditions, alongside the corresponding 95-percent confidence intervals. The average treatment effect (ATE) and Fisher exact p-values testing for the equality of means are also reported. Panel (b) depicts coefficient estimates derived from regressions with stratum fixed effects as laid out in Equation 3.1, where we employ the same outcomes. 95-percent confidence intervals derived from robust standard errors clustered at the postal district level reported.

vote share and turnout in the control group are a suitable proxy for y_0. Using our point estimates depicted in Table 3.B.1 in Appendix 3.B and abstracting from the unintended sign of the estimates, we calculate persuasion rates of 0.74 percent for AfD voting and of 2.14 percent for turnout. Compared to the distribution of persuasion rates observed in similar contexts (DellaVigna and Gentzkow 2010; Hager 2019; Pons 2018), our estimates rank at the bottom end of the distribution. Even when employing the upper end of the respective 95-percent confidence intervals, we find persuasion rates of 6.55 and 15.61 percent, implying that the impact of K5's campaign was, at best, moderate. Combined with the fact that our experiment was designed to detect effect sizes starting at about 3 percent of a standard deviation 80 percent of the time, this suggests that our findings reflect the absence of any meaningful treatment effects and are not the result of insufficient statistical power.

3.2.3.3 Heterogeneity

We now evaluate whether treatment effects are larger in postal districts where K5's campaign was more likely to reach certain demographics more susceptible to its ads. We expected K5's campaign to exhibit stronger effects in areas with a history of low voter turnout and strong AfD support, because this may coincide with a larger pool of individuals at the margin of either voting at all or of voting for the AfD in particular. We therefore test whether the impact of K5's Facebook campaign systematically varied by the AfD's vote share and turnout in the preceding 2017 federal elections.[33] To this

[33] We pre-registered both of these heterogeneities in our pre-analysis plan.

end, we run the following regression model:

$$\begin{aligned} y_{is} = \alpha_0 + \alpha_1 \cdot \text{Campaign ads}_i + \alpha_2 \cdot (\text{Past Turnout} > \text{median})_i \\ + \alpha_3 \cdot (\text{Campaign ads} \times \text{Past Turnout} > \text{median})_i + \delta_s + \epsilon_{is} \end{aligned} \tag{3.3}$$

where $y_i s$, Campaign ads$_i$, and δ_s are defined as in Equation 3.1; (Past Turnout $>$ median)$_i$ is an indicator taking value 1 if postal district i exhibited above-median turnout in the 2017 federal elections, and 0 otherwise.[34]

In Table 3.3, Columns 1 and 2, we report treatment effect heterogeneities with respect to past turnout when employing the AfD's vote share in the 2019 elections as the dependent variable. We obtain an estimate of -0.26 (S.E. = 0.29) for our treatment indicator and 0.63 (S.E. = 0.43) for the interaction effect (Column 2). Taken at face value, the coefficients would thus suggest that K5's campaign reduced the AfD's vote share only in districts with a history of low turnout, while it increased AfD voting in high-turnout districts. However, neither the treatment indicator nor the interaction effect surpass conventional levels of statistical significance. The same pattern emerges when employing turnout in the 2019 elections as the dependent variable (Columns 3 and 4). Again, we obtain a negative estimate for our treatment indicator (-0.84; S.E. = 0.70) and a positive interaction effect (0.99; S.E. = 0.97) which are, however, both insignificant (Column 4). When turning to heterogeneities with respect to past AfD voting, the reverse pattern emerges: we obtain positive estimates for our treatment indicator and negative interaction effects (Columns 2 and 4 in Table 3.4). If we abstracted from statistical significance, these estimates would suggest that K5's campaign had a tentative, negative effect on turnout but not on AfD voting in districts with a history of strong support for the AfD. However, neither of the coefficients of interest is statistically significant.

Taken together, our results imply that K5's Facebook campaign during the run-up to the 2019 state elections in Brandenburg, Saxony, and Thuringia did not exhibit a significant impact on AfD voting and turnout, not even in areas which we, ex ante, expected to be more susceptible to K5's campaign. We devote the next section to exploring potential explanations for the absence of significant treatment effects.

[34]We replace (Past Turnout $>$ median)$_i$ for (Past AfD $>$ median)$_i$ in Equation 3.3 when assessing treatment effect heterogeneities with respect to past AfD voting.

Table 3.3: Heterogeneities with respect to turnout in the 2017 federal elections

	AfD's vote share		Turnout	
	(1)	(2)	(3)	(4)
Campaign ads	0.05	−0.26	−0.35	−0.84
	(0.20)	(0.29)	(0.44)	(0.70)
Past turnout > median	0.31	−0.01	5.41***	4.91***
	(0.28)	(0.35)	(0.61)	(0.76)
Campaign ads x Past turnout > median		0.63		0.99
		(0.43)		(0.97)
Stratum FE	Yes	Yes	Yes	Yes
Mean dep. var., 'Control'	27.92	27.92	60.18	60.18
SD dep. var., 'Control'	6.47	6.47	9.4	9.4
Observations	760	760	760	760
R^2	0.86	0.86	0.68	0.68

Notes: Results are derived from regressions as laid out in Equation 3.3. We employ two different dependent variables: (Columns 1 and 2) the AfD's vote share; and (Columns 3 and 4) Turnout. Robust standard errors clustered at the postal district level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 3.4: Heterogeneities with respect to the AfD's vote share in the 2017 federal elections

	AfD's vote share		Turnout	
	(1)	(2)	(3)	(4)
Campaign ads	0.05	0.13	−0.26	0.07
	(0.20)	(0.28)	(0.47)	(0.66)
Past AfD vote share > median	1.38**	1.46**	0.81	1.12
	(0.61)	(0.64)	(1.57)	(1.61)
Campaign ads x Past AfD vote share > median		−0.17		−0.67
		(0.40)		(0.98)
Stratum FE	Yes	Yes	Yes	Yes
Mean dep. var., 'Control'	27.92	27.92	60.18	60.18
SD dep. var., 'Control'	6.47	6.47	9.4	9.4
Observations	760	760	760	760
R^2	0.86	0.86	0.63	0.63

Notes: Results are derived from regressions as laid out in Equation 3.3. We employ two different dependent variables: (Columns 1 and 2) the AfD's vote share; and (Columns 3 and 4) Turnout. Robust standard errors clustered at the postal district level reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

3.3 Online Survey Experiment

3.3.1 Context and timeline

We conducted a complementary, anonymous online survey experiment in parallel to the field experiment.[35] The goal of this additional experiment is two-fold: first, to explore explanations for the absence of significant treatment effects of K5's Facebook campaign on aggregate election outcomes; and second, to test whether K5's campaign ads could have been successful in shaping election outcomes if K5 highlighted that it shares certain identity traits with the campaign audience. The survey experiment comprised two waves: a pre-election survey conducted in late August (Brandenburg and Saxony) and in mid-October (Thuringia) and a post-election survey fielded about two weeks after each election. Eligibility for the post-election survey was limited to individuals who completed the pre-election survey. All experimental manipulations took place in the pre-election survey.

3.3.2 Setting and sample

We recruited a sample of 1,728 voting-age individuals through *respondi*, an online panel provider. Participation in the experiment was subject to living in either Brandenburg, Saxony, or Thuringia, explicitly consenting to answering questions on political views, and passing a simple attention check. We present summary statistics of participants' characteristics in Table 3.5: 87 percent of participants reported that they had voted in the 2017 federal elections and 17 percent replied that they had cast their vote for the AfD. These figures are broadly consistent with official statistics and are also fairly similar to those elicited by the *German General Social Survey*, "*ALLBUS*" (GESIS – Leibniz-Institut für Sozialwissenschaften 2019).[36] The remaining participant characteristics are also suitably representative of the population of Brandenburg, Saxony, and Thuringia.[37]

[35]We pre-specified all features of this additional experiment in our pre-analysis plan, which we – together with the full survey instrument – stored at the AEA RCT registry under RCT ID AEARCTR-0004623. The Ethics Committee of the Department of Economics at LMU Munich approved the experimental design outlined in this section, protocol 2019-14. We employed the open-source software oTree (Chen et al. 2016) for the technical implementation of our survey experiment.

[36]To compare self-reported to official statistics, we employ data published by the election authorities ("Landeswahlleiter") discussed in Section 3.2. We present summary statistics obtained from the 2018 ALLBUS wave, limited to participants from Brandenburg, Saxony, and Thuringia, in Table 3.B.3 in Appendix 3.B.

[37]Approximately half of our sample is female; 3 percent report being currently unemployed; mean age and monthly net income per person are 44.7 years and €1,060, respectively, compared to averages of 54.34 years and €1,470 in 2018 (GESIS – Leibniz-Institut für Sozialwissenschaften 2019).

Table 3.5: Survey participants' characteristics

Statistic	Mean	St. Dev.	Min	Max	N
Demographics					
Age	44.69	15.03	24.00	80.00	1,728
Monthly net income (x 1000)	1.06	0.49	0.46	1.83	1,728
Female	0.56	0.50	0.00	1.00	1,728
Unemployed	0.03	0.17	0.00	1.00	1,728
Lives in Brandenburg	0.25	0.43	0.00	1.00	1,728
Lives in Saxony	0.53	0.50	0.00	1.00	1,728
Lives in Thuringia	0.21	0.41	0.00	1.00	1,728
Treatment Status Field					
Exposed to Facebook campaign	0.51	0.50	0.00	1.00	1,728
Political Attitudes					
Political Self Assessment	0.00	1.00	−2.65	2.31	1,728
Trust in Institutions	0.00	1.00	−2.14	2.61	1,728
Attitudes towards migration	0.00	1.00	−2.23	3.30	1,728
Attitudes towards climate change	0.00	1.00	−2.88	1.36	1,728
Attitudes towards 'identity'	0.00	1.00	−3.11	2.98	1,728
Attitudes towards political system	0.00	1.00	−3.27	2.80	1,728
Moral values					
Morality Score	0.00	1.00	−3.52	4.05	1,728
Past voting behavior					
Voted in 2014	0.74	0.44	0.00	1.00	1,728
Voted AfD in 2014	0.08	0.28	0.00	1.00	1,728
Voted in 2017	0.87	0.33	0.00	1.00	1,728
Voted AfD in 2017	0.17	0.37	0.00	1.00	1,728

Notes: Summary statistics of survey participants' characteristics. *Political Attitudes* constitute equally weighted indices computed as laid out in Kling et al. (2007). The *Morality Score* is computed as laid out in Enke (2020) and subsequently standardized.

3.3.3 Survey design

Pre-treatment module After signing an online consent form, participants answered a set of baseline demographic questions (age, gender, state of residence, etc.), followed by a series of survey items eliciting participants' political attitudes and beliefs in the domains of migration, identity, and climate change. For instance, we asked participants whether they think that Germany benefits from immigration, whether they preferred stricter policies to mitigate climate change, or whether the government should enact policies strengthening national identity.[38] We further measured participants' trust in political institutions and in the media, collected measures of participants' political interest and knowledge, and elicited participants' views on morality using the Moral Foundations Survey questionnaire introduced to the economics literature by Enke (2020).[39] In the next section of the survey, we measured the effective outreach of K5's campaign on Facebook to obtain a measure of the first stage of our field experiment. To this end, we presented participants with a selection of campaign ads by K5, political parties, and other grassroots organizations that were also disseminated during the run up to the 2019 state elections. Subsequently, we asked them to indicate

[38]We provide the full survey instrument, including the exact wording of our experimental instructions. in Appendix 3.D.

[39]We included questions from the Moral Foundations Survey, since Enke (2020) has shown, in the US context, that citizens' views on morality predicted their tendency to vote for Donald Trump.

which of the campaign ads they had seen. We also included campaign ads by fictive organizations to detect flawed answers. Before participants entered the treatment stage of our experiment, we administered a short attention check to ensure that they were paying sufficient attention to our instructions.

Campaign ads treatment Next, we randomly assigned participants to one of two main experimental conditions: participants in the treatment condition were exposed to the same campaign ads K5 placed on Facebook users' timeline addressing AfD's stances on migration, identity, and climate change. We focused on ads aimed at reducing AfD voting, because a primary, initial objective of this survey was to assess whether K5's ads work via persuasion or mobilization.[40] Participants in the control group were not exposed to any of K5's campaign ads. Instead, to limit experimenter demand effects arising from different perceptions of the social desirability of expressing support for the AfD, participants in the control group received a placebo treatment highlighting the work of grassroots organizations contending against right-wing populism in general. We employed a series of survey items eliciting participants' attitudes in the domains of migration, identity, and climate change to evaluate whether the *campaign ads treatment* induced shifts in the latter that could plausibly manifest in changes in voting behavior. We refer to this as the first-stage effect of the *campaign ads treatment*.

Identity treatment Among participants in the treatment group, we further cross-randomized whether participants learned that K5 is based in Berlin or whether K5 has many supporters from participants' state of residence to obtain variation in participants' perceptions of K5's (regional) identity.[41] This additional layer allows us to address the question of whether grassroots organizations' social media campaigns induce stronger individual-level responses if the organization shares certain identity traits with its audience. We employed a series of (incentivized) survey items to test for the presence of a sufficiently strong first-stage effect – that is, whether informing participants about K5's support in their state of residence induced changes in participants' perceptions of K5, and in particular, to what extent participants identified with K5. To this end, we first asked participants to state their beliefs about K5's political orientation on a standard left/right (liberal/conservative) scale, for which we offered participants additional remuneration.[42] Given the risk of contaminating participants' beliefs in the control group by asking them about K5, we employed a similarly phrased survey item to elicit their best guess about the political orientation of grassroots organizations campaigning against right-wing populism in general. Subsequently, we

[40]We provide examples of the specific campaign ads used in the survey experiment in Appendix 3.C.

[41]We administered an additional survey among members of K5 to obtain factually true information about the strength of local support for K5 in each of the three states.

[42]Participants could win up to €100 in this task.

elicited the extent to which participants identify with K5 and its goals, measured their perceptions of K5's competence, and asked them what they think other survey takers replied to these questions.

Outcomes We collected two sets of main outcomes: (i) self-reported voting behavior in the upcoming 2019 state elections and (ii) revealed preference measures of opposition to right-wing populism. We elicited two such outcomes: first, we informed participants that we would be giving away €10 vouchers and then asked them, in case they won, how much of the €10 they would be willing to donate to an organization contending against right-wing populism.[43] Second, we provided participants the opportunity to sign a real petition demanding greater political representation for Muslims in Germany.[44] For privacy reasons, we are limited to observing whether participants clicked on the link forwarding them to the petition. Arguably, clicking the petition link may nevertheless constitute a more credible signal of opposition to right-wing populism than self-reported attitudes or behavior.

Post-election survey About two weeks after the elections, we administered a post-election survey re-eliciting a subset of outcomes from the pre-election survey. We asked participants whether they voted and who they voted for, as well as eliciting their attitudes in the domains of migration, identity, and climate change. In addition, we collected a set of more detailed demographic characteristics and elicited participants' economic preferences using the *Global Preferences Survey* developed by Falk et al. (2018).[45]

3.3.4 Experimental assignment and sample balancing

In total, our design features three experimental conditions: the *Control* condition which received a placebo instead of K5's campaign ads; the *Campaign ads – Berlin* condition, in which participants were exposed to K5's campaign ads and learned that K5 is based in Berlin; and finally, the *Campaign ads – Local* condition, in which participants saw K5's campaign ads and learned that K5 has many supporters from their state of residence. We assigned participants to each of the three groups with equal probabilities. We report the resulting assignment of participants to conditions in Table 3.6.[46]

To assess whether participants' pre-treatment characteristics are balanced across experimental conditions, we conducted pairwise comparisons of 22 predetermined char-

[43] We offered participants the opportunity to donate to the German civil society organization *Initiative Offene Gesellschaft e.V.*.

[44] We used an already existing petition and did not create the petition for the purpose of this experiment. The petition is archived at https://bit.ly/3mEeCHu (last accessed August 27, 2021).

[45] We provide the full survey instrument employed in the post-election survey in Appendix 3.E.

[46] We observed deviations in the share of participants assigned to either condition from the target share of one third is an artifact of the "on the fly" randomization we used.

Table 3.6: Number of participants assigned to each experimental condition

Condition	Treatments	Brandenburg	Saxony	Thuringia	Total
(1) Control	*Campaign ads* = 0 *Identity* = 0	140	285	131	556
(2) Campaign ads – Berlin	*Campaign ads* = 1 *Identity* = 0	150	329	126	605
(3) Campaign ads – Local	*Campaign ads* = 1 *Identity* = 1	145	310	112	567

acteristics across all three experimental conditions using bivariate regressions.[47] In Table 3.7, we report the differences in means between each of the experimental conditions alongside the corresponding p-values. Out of the 88 estimates reported in Table 3.7, nine are significant at the ten-percent level. While predetermined characteristics seem to be suitably balanced in general, we nevertheless present results where we employ the full set of participant characteristics listed in Table 3.5 as controls to further limit the risk of wrongly attributing potential treatment effects to pre-existing differences.

3.3.5 Outreach on Facebook

We start exploring potential explanations for the absence of significant treatment effects on aggregate election outcomes by assessing whether K5's campaign generated sufficient outreach on Facebook. To this end, we compare the share of participants in our survey who reported that they had previously seen K5's campaign ads on Facebook outside of the survey between those living in treatment and control postal districts. This provides us with an estimate of the first stage of our field experiment, which captures the effective penetration of the population with K5's campaign ads.

Empirical specification To assess this, we estimate the following regression model:

$$y_{ips} = \beta_0 + \beta_1 \cdot \text{Campaign ads}_p + \delta_s + \boldsymbol{X}_i\, \boldsymbol{\phi}' + \epsilon_{ips} \tag{3.4}$$

where y_i is an indicator taking value 1 if participant i reported that s/he had seen K5's campaign ads outside of the experiment before, and 0 otherwise; Campaign ads$_p$ is a dummy variable taking value 1 if participant i lives in a given postal district p in stratum s where K5's campaign ads were disseminated on Facebook, and 0 otherwise; δ_s are stratum fixed effects; and $\boldsymbol{X}_i$ is a vector of participant characteristics containing the full set of variables displayed in Table 3.5. Because we assigned entire postal districts instead of individual participants to the treatment and control group in the field ex-

[47] To compare participants' predetermined characteristics across conditions, we run the following regression model: $characteristic_i = \alpha + \beta \cdot treat_i + \epsilon_i$ where $treat_i$ is an indicator variable either corresponding to condition *Campaign ads - Berlin*, *Campaign ads - Local*, or both conditions simultaneously when comparing characteristics between the treatment and control condition.

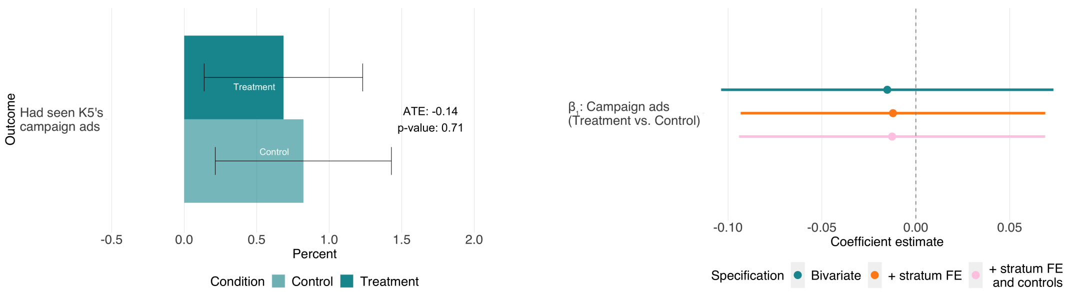
Table 3.7: Respondents' predetermined characteristics compared across conditions

	Campaign vs. Control		Local vs. Berlin		Local vs. Control		Berlin vs. Control		
	Δ	p-value	Δ	p-value	Δ	p-value	Δ	p-value	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Attrition									
Completed 1st wave	0.02	0.26	0.01	0.11	0.01*	0.09	0.00	0.91	1817
Started 2nd wave	0.04	0.11	-0.01	0.69	0.02	0.44	0.03	0.24	1817
Completed 2nd wave	0.65	0.40	0.00	0.91	0.03	0.18	0.04	0.14	1817
Demographics									
Age	0.05**	0.04	-0.64	0.47	0.32	0.72	0.96	0.28	1728
Monthly net income (x 1000)	-0.01	0.63	0.05*	0.08	0.08***	0.01	0.03	0.33	1728
Female	-0.02**	0.02	0.00	0.94	-0.01	0.71	-0.01	0.65	1728
Unemployed	0.00	1.00	0.01	0.24	-0.02*	0.10	-0.03***	0.01	1728
Lives in Brandenburg	0.03	0.20	0.01	0.76	0.00	0.88	0.00	0.88	1728
Lives in Saxony	-0.03	0.13	0.00	0.92	0.03	0.25	0.03	0.29	1728
Lives in Thuringia	-0.02	0.53	-0.01	0.65	-0.04	0.12	-0.03	0.26	1728
Treatment status field									
Exposed to Facebook campaign	-0.09*	0.09	-0.03	0.38	-0.03	0.32	0.00	0.89	1728
Political attitudes									
Political Self Assessment	0.03	0.57	0.06	0.29	-0.06	0.36	-0.12**	0.05	1728
Trust in Institutions	0.00	0.97	0.00	0.99	0.03	0.62	0.03	0.62	1728
Attitudes towards migration	-0.05	0.36	-0.04	0.47	-0.02	0.68	0.02	0.74	1728
Attitudes towards climate change	-0.01	0.82	0.05	0.41	-0.02	0.69	-0.07	0.24	1728
Attitudes towards 'identity'	-0.04	0.49	0.05	0.40	0.01	0.83	-0.04	0.54	1728
Attitudes towards political system	-0.03	0.57	-0.09	0.13	-0.08	0.18	0.01	0.92	1728
Morality									
Morality Score	0.02	0.30	-0.02	0.76	-0.04	0.52	-0.02	0.72	1728
Past voting behavior									
Voted in 2014	0.01	0.34	0.01	0.63	0.03	0.25	0.02	0.50	1728
Voted AfD in 2014	0.02	0.20	0.02	0.34	0.02	0.20	0.01	0.73	1728
Voted in 2017	0.01	0.74	-0.02	0.19	0.01	0.63	0.03*	0.08	1728
Voted AfD in 2017	0.01	0.74	0.01	0.56	0.01	0.57	0.00	1.00	1728
Test for joint significance									
		0.27		0.14		0.34		0.09	

Notes: Mean differences in participants' predetermined characteristics (Δ), alongside p-values testing for equal means, reported by experimental condition. We estimate Δ using the following regression model: $characteristic_i = \alpha + \beta \cdot treat_i + \epsilon_i$ where $treat_i$ is an indicator variable either corresponding to condition *Campaign ads - Berlin*, *Campaign ads - Local*, or both conditions simultaneously when comparing characteristics between the *Campaign ads* and the *Control condition* (Column 1). For each pairwise comparison between the *Campaign ads - Berlin*, *Campaign ads - Local*, and the *Control* condition we drop the remaining third condition from the sample. p-values testing for equal means are derived from robust standard errors. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

periment, we report robust standard errors clustered at the postal district level, which we complement with Fisher exact p-values.

Results We summarize the results from this analysis in Figure 3.3. In its left-hand panel, we depict the share of participants who reported that they had previously seen K5's campaign ads outside of the experiment separately by treatment conditions. We find that, on average, only 0.8 percent of participants had seen the ads before, which is considerably smaller than the share of the population that Facebook reports was *exposed* to K5's ads ($\approx$ 4 percent). Considering the fact that our sample is broadly representative of the voting-age population in these three states, our own estimate is more likely to reflect the effective penetration of the population with K5's ads, since it measures the share of the population that could recall K5's ads right before the election. However, effective penetration rates do not significantly differ between participants living in treatment and control postal districts (Fisher exact p-value = 0.71), which is confirmed by our regression estimates for β_1 depicted in Figure 3.3, right-hand



(a) Conditional means **(b) Coefficient estimates**

Figure 3.3: Treatment effects on Facebook penetration rates

Notes: Panel (a) plots the share of survey participants who reported that they had seen K5's campaign ads before, in percent, for both treatment conditions, alongside the corresponding 95-percent confidence intervals. The average treatment effect (ATE) and Fisher exact p-values testing for equal means are also reported. Panel (b) depicts coefficient estimates from regressions as laid out in Equation 3.4, where the employ the same outcome variable. We provide results from three types of specifications: (i) bivariate; (ii) with stratum fixed effects; and (iii) with stratum fixed effects plus the full set of participant controls listed in Table 3.5. 95-percent confidence intervals derived from robust standard errors clustered at the postal district level reported.

panel.[48] Irrespective of the specification – that is, bivariate, with stratum fixed effects, or with stratum fixed effects and participant controls – we obtain very small and statistically insignificant estimates for β_1.[49] Given the likely presence of spillovers on control districts resulting from imperfect spatial targeting of ads by Facebook (Hager 2019), the penetration of Facebook users in treatment districts was apparently insufficient to generate significant differences in ad exposure. Hence, the absence of significant treatment effects on aggregate election outcomes can, at least partly, be explained by insufficient outreach on Facebook.

3.3.6 Effectiveness of campaign ads

Next, we analyze whether K5's Facebook campaign could have significantly affected aggregate election outcomes under the assumption of sufficient outreach on Facebook. To this end, we analyze the effects of the *campaign ads treatment* implemented in our survey, which exposed a random subset of survey participants to K5's campaign ads. We consider three sets of outcomes. First, to obtain an estimate of the first stage of the *campaign ads treatment*, we assess whether K5's campaign ads induced a shift in attitudes in domains addressed in K5's ads. Second, we test whether K5's campaign ads affected self-reported voting behavior. Finally, we evaluate whether any potential treatment effects carry over to revealed preference outcomes.

[48] We report results in regression format in Table 3.B.4 in Appendix 3.B. In this table, we also report the corresponding Fisher exact p-values depicted in the left-hand panel of Figure 3.3.

[49] As we document in Table 3.B.4 in Appendix 3.B, this result also holds when studying the interaction effect of reporting to be a Facebook user and living in a treatment postal district.

Empirical specification To conduct these analyses, we run the following regression model:

$$y_i = \gamma_0 + \gamma_1 \cdot \text{Campaign ads}_i + \boldsymbol{X}_i\, \boldsymbol{\phi}' + \epsilon_i \tag{3.5}$$

where y_i either captures participant i's attitudes, her/his self-reported voting behavior, or one of our revealed preference outcomes; Campaign ads$_i$ is a dummy variable taking value 1 if participant i was exposed to K5's campaign ads as part of the survey experiment and 0 otherwise; X_i is the same vector of participant characteristics as employed in Equation 3.4. We employ robust standard errors throughout.

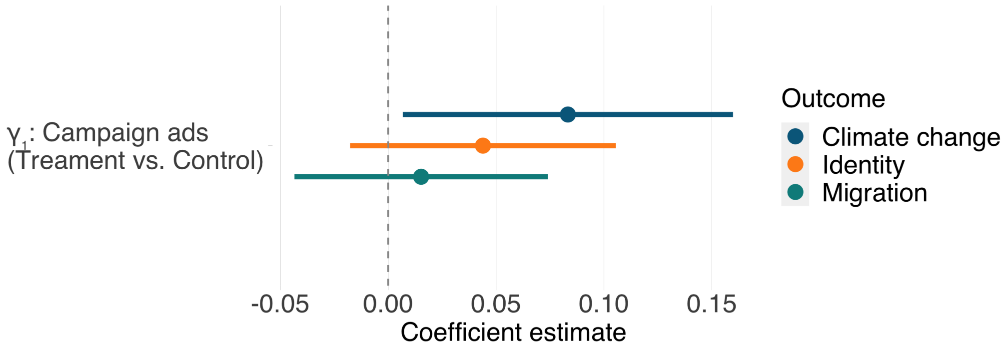


Figure 3.4: Treatment effects on posterior attitudes elicited *pre* election day

Notes: Coefficient estimates from regressions as laid out in Equation 3.5 with the full set of control variables listed in Table 3.5 reported. We employ the following dependent variables: participants' posterior attitudes in the domains of (i) *Climate change*, (ii) *Identity*, and (iii) *Migration*. All attitudes were elicited in our pre-election survey and are standardized (mean = 0, sd = 1). 95-percent confidence intervals reported.

First stage – Attitudes We begin by discussing treatment effects on participants' posterior attitudes in the domains of migration, identity, and climate change as measured by equally weighted, standardized indices with mean zero and standard deviation one.[50] Coefficient estimates can thus be interpreted as changes in standard deviations. We present coefficient estimates for γ_1 using our full set of control variables and corresponding 95-percent confidence intervals in Figure 3.4.[51] We obtain small and statistically insignificant estimates when analyzing attitudes in the domains of migration and identity. In contrast, we estimate that participants exposed to K5's campaign ads exhibit an eight-percent of a standard deviation (S.E. = 0.04) higher support for climate change mitigation policies which is significant at the 5-percent level. The magnitude of this effect is well in line with effect sizes detected in related survey experiments (Haaland et al. 2021). However, as we document in Figure 3.A.4 in Appendix 3.A, this

[50] To obtain these indices, we follow Kling et al. (2007) and sum up participants' numeric answers to each survey item and scaled the resulting index using its mean and standard deviation.

[51] We report results in regression format in Table 3.B.5 in Appendix 3.B.

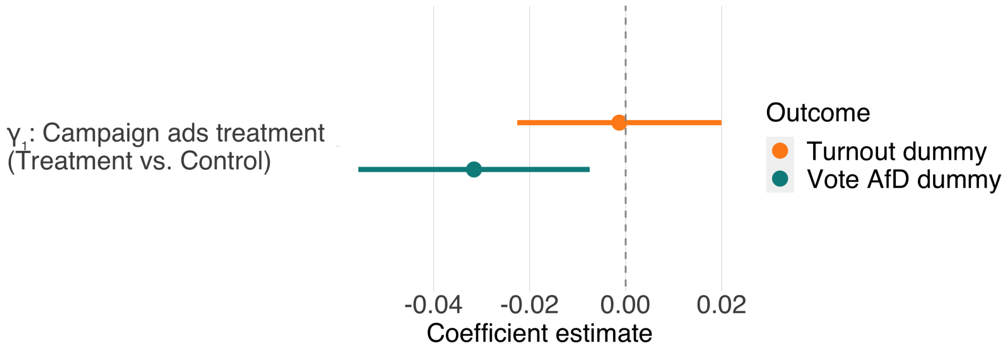


Figure 3.5: Treatment effects on *intended* voting behavior in the 2019 state elections
Notes: Coefficient estimates from regressions as laid out in Equation 3.5 with the full set of control variables listed in Table 3.5 reported. We employ two different dependent variables: (i) a dummy variable taking value 1 if a participant reported to be planning to vote (*Turnout dummy*); (ii) a dummy variable taking value 1 if a participant reported to be planning to vote for the AfD (*Vote AfD dummy*). Both variables were elicited in our pre-election survey and thus, capture *intended* voting behavior. 95-percent confidence intervals reported.

effect is only short-lived: when employing the same index elicited in our post-election survey as the dependent variable, we can no longer detect any significant differences regarding this particular set of attitudes. K5's campaign ads thus did not exhibit a sufficiently strong first-stage effect on attitudes that would be likely to translate into changes in actual behavior.

Voting behavior We present our treatment effect estimates of K5's campaign ads on self-reported voting behavior in Figure 3.5.[52] We obtain very small and statistically insignificant treatment effects on participants' self-reported tendency to vote in the 2019 state elections, which might, however, reflect the focus of the campaign ads on reducing support for the AfD. On the contrary, we estimate that exposure to K5's campaign ads reduces participants' self-reported likelihood to vote for the AfD by 3 percentage points (S.E. = 0.01) when elicited before the election, corresponding to a 9-percent of a standard deviation decrease relative to the control group. This effect does, however, not persist until our post-election survey conducted shortly after the election as we show in Figure 3.A.5 in Appendix 3.A and Table 3.B.7 in Appendix 3.B. We further explore the impact of K5's campaign ads on self-reported voting behavior in Tables 3.B.6 and 3.B.7 in Appendix 3.B using alternative outcomes such as the candidate vote ("*Erststimme*"), and specific subsamples (e.g., only Thuringia). These tables broadly support the notion that K5's campaign ads had no impact on self-reported turnout decisions and, at best, a moderately negative impact on participants' stated tendency to vote for the AfD.

[52]The corresponding regression results are reported in Table 3.B.6 and 3.B.7 in Appendix 3.B.

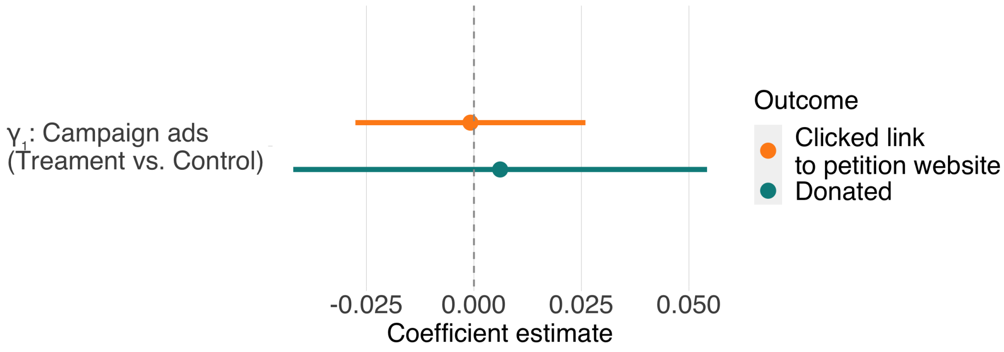


Figure 3.6: Treatment effects on revealed preference outcomes

Notes: Coefficient estimates from regressions as laid out in Equation 3.5 with the full set of control variables listed in Table 3.5 reported. We employ two types of revealed preference outcomes: (i) a dummy variable taking value 1 if a participant clicked on the link forwarding her/him to a website where s/he could sign a petition demanding greater political representation for Muslims in Germany (*Clicked link to petition website*); and (ii) a dummy variable taking value 1 if a participant donated a positive amount to another grassroots organization campaigning against right-wing populism (*Donated*). 95-percent confidence intervals reported.

Revealed preference outcomes Considering the risk of experimenter demand inflating treatment effect estimates (de Quidt et al. 2018), we now analyze the effect of K5's campaign ads on our revealed preference measures of opposition to right-wing populism. We employ two such measures: first, a dummy variable taking value 1 if participant i clicked a link forwarding her/him to a website where s/he could sign a petition demanding greater political representation for Muslims in Germany; and second, a dummy variable taking value 1 if participant i was willing to donate to another grassroots organization campaigning against right-wing populism. We summarize corresponding results in Figure 3.6 and provide full regression results alongside alternative outcomes in Table 3.B.8 in Appendix 3.B. Regardless of which outcome we consider, we only find very small and statistically insignificant effects. This implies that the significant estimates we obtained for a selection of posterior attitudes and participants' self-reported tendency to vote for the AfD are more likely to reflect experimenter demand effects than actual changes in attitudes and behavior.[53] In sum, the main insight from this subsection is that even with sufficient outreach on Facebook, K5's campaign ads would most likely not have been able to shape aggregate election outcomes.

[53] An alternative explanation would be multiple hypothesis testing, suggesting that a certain number of regression estimates surpasses conventional levels of statistical significance despite the absence of any "true" effect if the number of hypothesis tests conducted is considerably large.

3.3.7 The role of identity

In this final subsection, we now discuss whether highlighting certain shared identity traits between K5 and its audience could have magnified the impact of K5's campaign. To assess this hypothesis, we exploit variation in participants' perceptions of K5's regional identity induced by cross-randomizing additional information on the strength of local support for K5 in participants' state of residence (*identity treatment*).

Empirical specification To carry out this analysis, we estimate the following regression model:

$$y_i = \delta_0 + \delta_1 \cdot \text{Campaign ads}_i + \delta_2 \cdot \text{Local}_i + \boldsymbol{X}_i\, \boldsymbol{\phi}' + \epsilon_i \tag{3.6}$$

where y_i captures participant i's identification with K5 or her/his (self-reported) attitudes and behavior such as her/his willingness to vote for the AfD. The dummy variables Campaign ads_i and Local_i capture the impact of our two experimental manipulations in the survey: Campaign ads_i takes value 1 for all participants who were exposed to K5's campaign ads during the survey, and 0 otherwise – that is, for those participants in the *Campaign Ads – Berlin* and *Campaign Ads – Local* condition. In contrast, Local_i takes value 1 only if participant i was assigned to the *Campaign ads – Local* condition who, instead of learning that K5 is based in Berlin, were informed that K5 has many supporters from participants' state of residence. Because we are primarily interested in the additional effect of aligning regional identities, our discussion focuses on δ_2, which captures differences in participants' outcomes in the *Campaign Ads – Berlin* and *Campaign Ads – Local* conditions.

First stage To test for the presence of a first-stage effect, we analyze participants' responses to the following survey items: (i) "Do you think K5 is competent?"; (ii) "Do you share K5's goals?"; (iii) "Do you identify with K5?"; and (iv) "Do you think other survey takers identify with K5?" We summarize the results from this exercise in Figure 3.7, where we scaled coefficient estimates for δ_2 using the corresponding mean and standard deviation in the "*Campaign ads – Berlin*" condition.[54] We obtain small and statistically insignificant estimates for all outcomes, suggesting that informing participants that K5 has many supporters in participants' state of residence did not significantly increase participants' identification with K5.

Main result Finally, we compare posterior attitudes, self-reported voting behavior, and revealed-preference outcomes between participants in the *Campaign ads – Local* and the *Campaign ads – Berlin* conditions. We summarize our estimates for δ_2 in Figure 3.8, where we again use the corresponding mean and standard deviation in the

[54]Results in regression format are reported in Table 3.B.9 in Appendix 3.B.

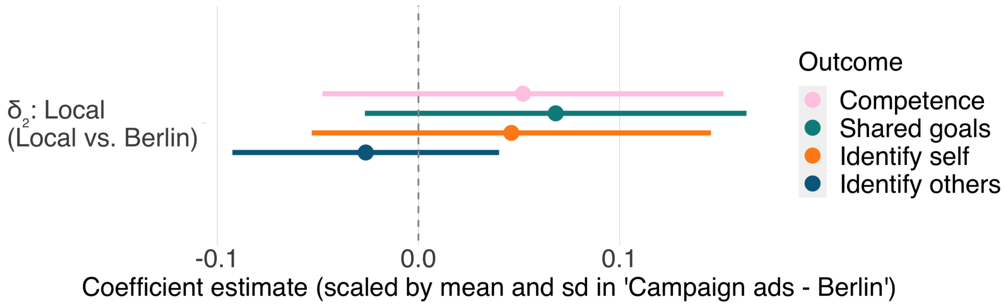


Figure 3.7: Treatment effects of the *identity treatment* on first stage

Notes: Coefficient estimates from regressions as laid out in Equation 3.6 with the full set of control variables listed in Table 3.5 reported. We employ participants' answers to the following questions as dependent variables: (i) "Do you think K5 is competent?" (*Competence*); (ii) "Do you share K5's goals?" (*Shared goals*); (iii) "Do you identify with K5?" (*Identify self*); and (iv) "Do you think other survey takers identify with K5?" (*Identify others*). All outcomes are standardized using the corresponding mean and standard deviation in the "*Campaign ads – Berlin*" condition. 95-percent confidence intervals reported.

'*Campaign ads – Berlin*' condition to scale estimates.[55] All of our estimates are small and statistically indistinguishable from zero. This implies that highlighting that K5 has many supporters in participants' state of residence did not boost the individual-level impact of K5's campaign ads. We can thus conclude that even in a scenario with sufficient outreach on Facebook and a campaign highlighting shared identity traits, K5 would most likely not have been able to shape election outcomes.

3.4 Conclusion

We provide experimental evidence on the question of whether grassroots organizations can reduce support for right-wing populism using social media. We derive this evidence from a field experiment embedded in the Facebook campaign of K5; a German grassroots organization which aims to reduce electoral support for Germany's most successful right-wing populist party, the AfD. Exploiting the detailed geographic targeting options for advertisements available on Facebook, we distributed K5's campaign ads to a random subset of postal districts during a series of recent elections in Germany and subsequently, compared election outcomes between treated and untreated postal districts. We find no statistically significant differences in the AfD's vote share and turnout between treatment and control districts. Our estimates are small in magnitude, precisely estimated, and robust to several different empirical specifications. Thanks to the statistical power of our stratified design, we can rule out with high probability that treatment effects on the AfD's vote share and turnout are larger than 3 percent of a standard deviation. Further analyses confirm the absence of sig-

[55] We provide full regression results in Table 3.B.10 in Appendix 3.B.

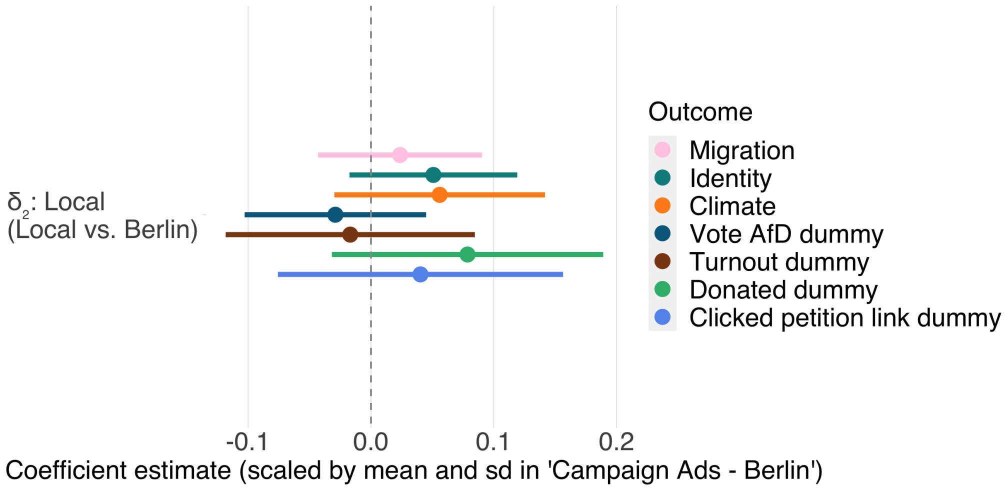


Figure 3.8: Treatment effects of the *identity treatment* on all outcomes

Notes: Coefficient estimates from regressions as laid out in Equation 3.6 with the full set of control variables listed in Table 3.5 reported. We employ the following three sets of outcomes defined in previous figures: (i) posterior attitudes (*Climate change*, *Identity*, and *Migration*) elicited in our pre-election survey; (ii) self-reported voting behavior in the 2019 state elections elicited in our pre-election survey (*Vote AfD dummy* and *Turnout dummy*); (iii) revealed preference measures of opposition to right-wing populism (*Donated dummy* and *Clicked petition link dummy*). All outcomes are standardized using the corresponding mean and standard deviation in the "*Campaign ads – Berlin*" condition. 95-percent confidence intervals reported.

nificant treatment effects also for subsets of postal districts with an ex ante higher susceptibility to campaigns against right-wing populism as well.

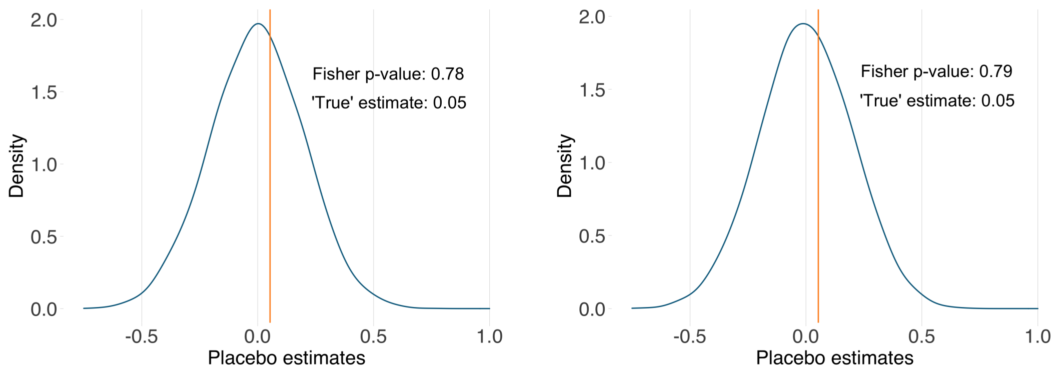
Drawing on data from an additional online survey experiment, we provide two complementary explanations for why K5 was not successful in leveraging social media to contend against right-wing populism: first, we document that K5's outreach on Facebook was insufficient to exhibit a detectable impact on aggregate election results. Second, we show that even under the assumption of sufficient outreach on Facebook, K5's campaign ads are unlikely to have significantly altered voting behavior. The same holds if we combine K5's campaign ads with an additional treatment highlighting identity traits K5 and its audience share.

We view the results discussed in this paper as one piece of evidence but not as a definitive answer to the question of whether grassroots organizations can successfully leverage social media to reduce support for (right-wing) populism, because they are based on one campaign in one particular context. Hence, the absence of significant treatment effects documented for this campaign does not imply that K5 or any other organization are generally not effective in contending against right-wing populism using social media. In contrast, we require more evidence to draw more definitive conclusions. First and foremost, we see the following main open questions for future research: do campaigns on other social media platforms with different target audiences exhibit larger effects? What types of campaign ads are most effective? In particular, does Hager's (2019) finding that programmatic ads perform better than ads featuring specific individuals carry over to campaigns by grassroots organizations?

Appendices

3.A Additional Figures

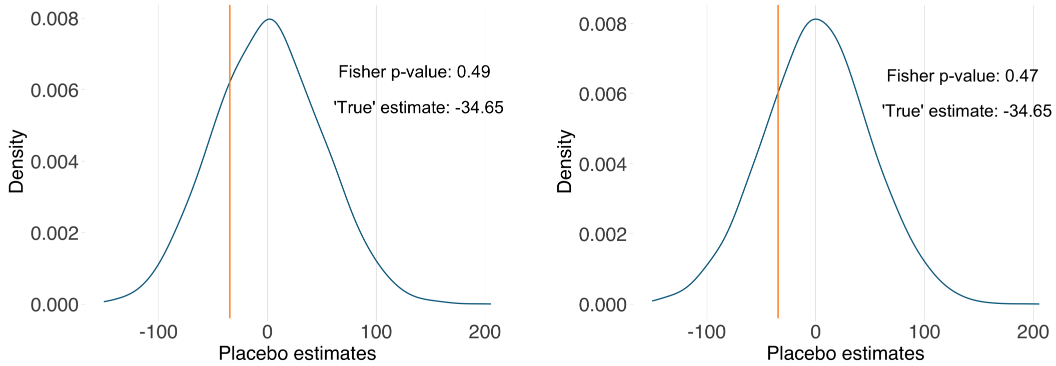
3.A.1 Field experiment



(a) ***Without*** **stratum fixed effects** **(b)** ***With*** **stratum fixed effects**

Figure 3.A.1: Distribution of placebo estimates for ***AfD vote share***

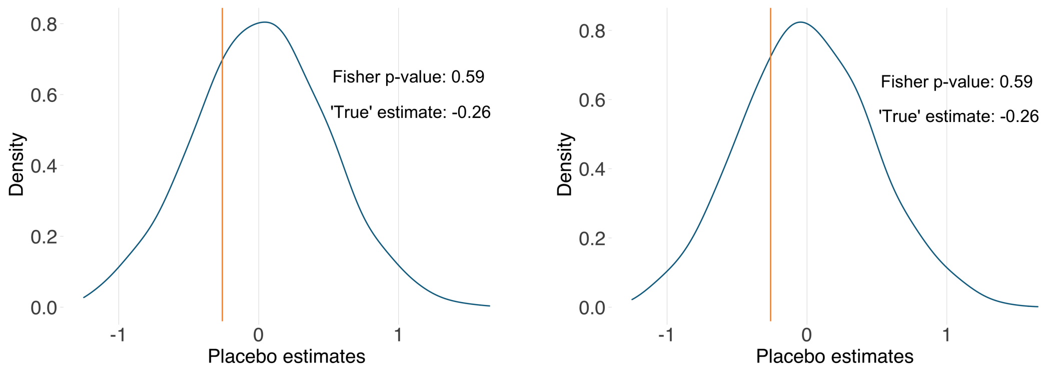
Notes: Distribution of placebo estimates derived from randomly re-assigning postal districts to placebo treatment groups for 5,000 times and calculating the share of "placebo treatment effects" that exceed the "true treatment effect" in (absolute) magnitude reported. Panel (a) depicts the resulting distribution and Fisher exact p-value when running Equation 3.1 *without* stratum fixed effects and Panel (b) *with* stratum fixed effects. The outcome in both panels is the *AfD's vote share* in the 2019 state elections.



(a) ***Without*** **stratum fixed effects** **(b)** ***With*** **stratum fixed effects**

Figure 3.A.2: Distribution of placebo estimates for ***absolute number of votes for the AfD***

Notes: Distribution of placebo estimates derived from randomly re-assigning postal districts to placebo treatment groups for 5,000 times and calculating the share of "placebo treatment effects" that exceed the "true treatment effect" in (absolute) magnitude reported. Panel (a) depicts the resulting distribution and Fisher exact p-value when running Equation 3.1 *without* stratum fixed effects and Panel (b) *with* stratum fixed effects. The outcome in both panels is the *absolute number of votes for the AfD* in the 2019 state elections.



(a) ***Without*** **stratum fixed effects** **(b)** ***With*** **stratum fixed effects**

Figure 3.A.3: Distribution of placebo estimates for ***turnout***

Notes: Distribution of placebo estimates derived from randomly re-assigning postal districts to placebo treatment groups for 5,000 times and calculating the share of "placebo treatment effects" that exceed the "true treatment effect" in (absolute) magnitude reported. Panel (a) depicts the resulting distribution and Fisher exact p-value when running Equation 3.1 *without* stratum fixed effects and Panel (b) *with* stratum fixed effects. The outcome in both panels is *turnout* in the 2019 state election.

3.A.2 Survey experiment

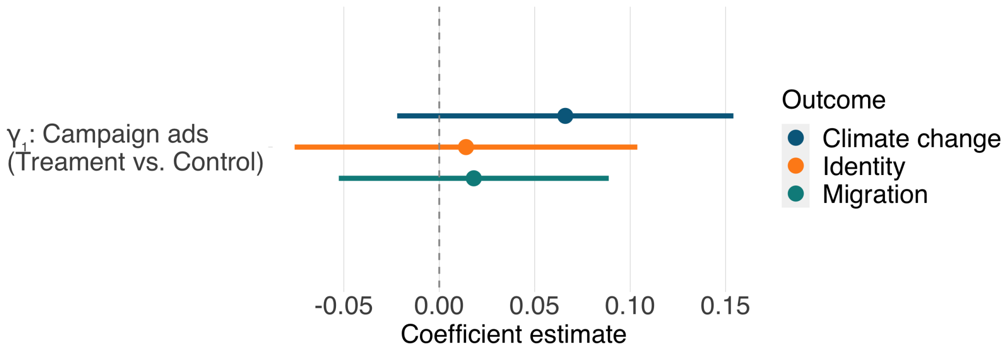


Figure 3.A.4: Treatment effects on posterior attitudes elicited *after* the elections

Notes: Coefficient estimates from regressions as laid out in Equation 3.5 with the full set of control variables listed in Table 3.5 reported. We employ the following dependent variables: participants' posterior attitudes in the domains of (i) *Climate change*, (ii) *Identity*, and (iii) *Migration*. All attitudes were elicited in our post-election survey and are standardized (mean = 0, sd = 1). 95-percent confidence intervals reported.

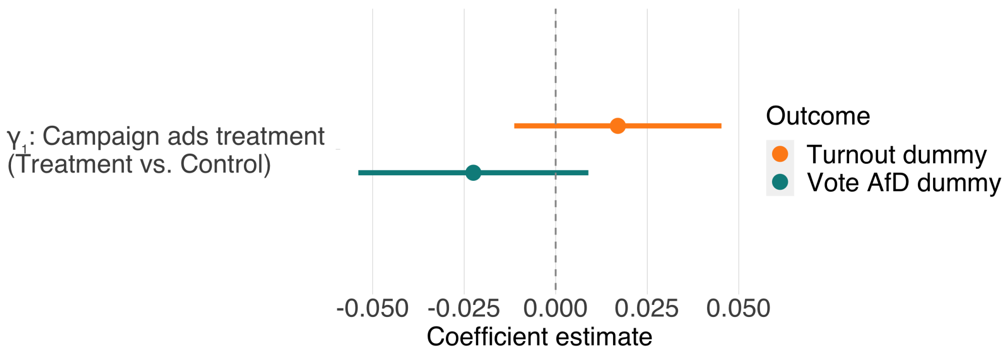


Figure 3.A.5: Treatment effects on self-reported voting behavior elicited *after* the elections

Notes: Coefficient estimates from regressions as laid out in Equation 3.5 with the full set of control variables listed in Table 3.5 reported. We employ two different dependent variables: (i) a dummy variable taking value 1 if a participant reported that s/he had votd (*Turnout dummy*); (ii) a dummy variable taking value 1 if a participant reported that s/he had votd for the AfD (*Vote AfD dummy*). Both variables were elicited in our post-election survey and thus, capture *self-reported*, retrospective voting behavior. 95-percent confidence intervals reported.

3.B Additional Tables

3.B.1 Field experiment

Table 3.B.1: Treatment effects on AfD voting and turnout

	AfD's vote share		Abs. number of AfD votes		Turnout	
	(1)	(2)	(3)	(4)	(5)	(6)
Campaign ads	0.05	0.05	−34.65	−34.65	−0.26	−0.26
	(0.46)	(0.20)	(71.81)	(49.55)	(0.68)	(0.47)
	$p = 0.78$	$p = 0.79$	$p = 0.49$	$p = 0.47$	$p = 0.59$	$p = 0.59$
Stratum FE		Yes		Yes		Yes
Mean dep. var., 'Control'	27.92	27.92	1187.45	1187.45	60.18	60.18
SD dep. var., 'Control'	6.47	6.47	1018.31	1018.31	9.4	9.4
Observations	760	760	760	760	760	760
R^2	0.00	0.86	0.00	0.64	0.00	0.63

Notes: Results from regressions as laid out in Equation 3.1 reported. We employ three different dependent variables: (Columns 1 and 2) the *AfD's vote share*; (Columns 3 and 4) the *absolute number of AfD votes*; and (Columns 5 and 6) *turnout*. Robust standard errors clustered at the postal district level reported in parentheses and p-values obtained from Fisher permutation tests beneath. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 3.B.2: Determinants of the AfD's vote share in the 2019 state elections

	AfD vote share														
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Area (in sqkm)	0.05														0.03***
	(0.03)														(0.01)
Population (x 1000)		−0.27***													−0.01
		(0.04)													(0.02)
In Brandenburg			−0.19***												0.21***
			(0.04)												(0.02)
In Saxony				0.35***											−0.01
				(0.03)											(0.02)
In Thuringia					−0.20***										
					(0.03)										
Nr. valid votes (x 1000)						−0.37***									−0.02
						(0.04)									(0.05)
Turnout (in %)							−0.09**								−0.05***
							(0.04)								(0.01)
CDU vote share (in %)								−0.00							−0.09**
								(0.05)							(0.04)
SPD vote share (in %)									−0.49***						−0.06*
									(0.03)						(0.04)
GRUENE vote share (in %)										−0.72***					−0.25***
										(0.03)					(0.03)
FDP vote share (in %)											−0.22***				−0.06***
											(0.04)				(0.02)
LINKE vote share (in %)												−0.53***			−0.05
												(0.04)			(0.04)
AfD vote share (in %)													0.91***		0.76***
													(0.02)		(0.06)
Number of valid votes for AfD														−0.10***	−0.03
														(0.03)	(0.05)
Observations	760	760	760	760	760	760	760	760	760	760	760	760	760	760	760
R^2	0.00	0.07	0.04	0.12	0.04	0.14	0.01	0.00	0.24	0.52	0.05	0.28	0.82	0.01	0.91

Notes: Results from regressions of AfD's vote share on all predetermined postal district characteristics listed in Table 3.1 reported. All explanatory variables are standardized using their mean and standard deviation. Robust standard errors clustered at the postal district level reported in parentheses. Significance levels: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

3.B.2 Survey experiment

Table 3.B.3: Characteristics of participants in the 2018 ALLBUS survey

Statistic	Mean	St. Dev.	Min	Max	N
Demographics					
Age	54.34	16.79	18.00	94.00	698
Female	0.50	0.50	0.00	1.00	699
Montly net income (x 1000)	1.47	1.01	0.09	15.00	524
Unemployed	0.03	0.17	0.00	1.00	684
Lives in Brandenburg	0.30	0.46	0.00	1.00	699
Lives in Saxony	0.44	0.50	0.00	1.00	699
Lives in Thuringia	0.25	0.44	0.00	1.00	699
Political attitudes					
Political self assessment	4.27	3.36	−9.00	10.00	699
Past voting behavior					
Voted in 2017	0.87	0.34	0.00	1.00	699
Voted AfD in 2017	0.12	0.33	0.00	1.00	699

Notes: Selection of characteristics of participants in the 2018 ALLBUS survey who live in Brandenburg, Saxony, or Thuringia.

Table 3.B.4: Treatment effects on Facebook outreach

	Had seen K5 campaign ads before survey				
	(1)	(2)	(3)	(4)	(5)
Campaign ads	−0.001	−0.001	−0.001	−0.001	−0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)
	$p = 0.713$	$p = 0.795$	$p = 0.793$	$p = 0.792$	$p = 0.548$
Facebook user			0.002	0.001	−0.00002
			(0.005)	(0.005)	(0.008)
Campaign ads x Facebook user					0.002
					(0.009)
Stratum FE		Yes	Yes	Yes	Yes
Participant controls				Yes	Yes
Mean dep. var., 'Control'	0.008	0.008	0.008	0.008	0.008
SD dep. var., 'Control'	0.090	0.090	0.090	0.090	0.090
Observations	1,729	1,729	1,729	1,729	1,729
R^2	0.0001	0.111	0.111	0.118	0.118

Notes: Results from regressions as laid out in Equation 3.4 reported. We employ a dummy variable taking value 1 if a participant in our survey reported that s/he had seen K5's campaign ads before the survey commenced as the dependent variable. Robust standard errors clustered at the postal district level reported in parentheses and p-values obtained from Fisher permutation tests beneath. The Fisher exact p-value testing whether the coefficients for *Campaign ads* in Columns (4) and (5) are identical is 0.72 . Participant controls include the full set of variables reported in Table 3.5. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 3.B.5: Treatment effects on posterior attitudes

	Pre election			Post election		
	Migration	Identity	Climate change	Migration	Identity	Climate change
	(1)	(2)	(3)	(4)	(5)	(6)
Campaign ads	0.02	0.04	0.08**	0.02	0.00	0.07
	(0.03)	(0.05)	(0.04)	(0.04)	(0.06)	(0.04)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep. var., 'Control'	0.00	0.00	0.00	0.00	0.00	0.00
SD dep. var., 'Control'	1.00	1.00	1.00	1.00	1.00	1.00
Observations	1,728	1,728	1,728	1,312	1,312	1,312
R^2	0.67	0.00	0.45	0.66	0.00	0.45

Notes: Results are derived from regressions as laid out in Equation 3.5, where we employ participants' posterior attitudes in the domains of *climate change* (Columns 1 and 4), *identity* (Columns 2 and 4), and *migration* (Columns 3 and 6) as dependent variables. We report results based on the pre- and the post-election survey. All outcomes have a mean of zero and a standard deviation of one. Controls include the full set of variables reported in Table 3.5. Robust standard errors reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 3.B.6: Treatment effects on AfD voting and turnout in the 2019 state elections elicited *before* the elections

	AfD Voting						Turnout	
	Ever vote for AfD		Intended candidate vote		Intended list vote		Intended to vote	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
Facebook treatment	-0.00	-0.01	-0.01	-0.02	-0.03	-0.03**	0.01	-0.00
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Mean dep. var., 'Control'	0.26	0.26	0.18	0.18	0.19	0.19	0.93	0.93
SD dep. var., 'Control'	0.39	0.39	0.39	0.39	0.39	0.39	0.25	0.25
Observations	1,728	1,728	1,728	1,728	1,728	1,728	1,728	1,728
R^2	0.00	0.65	0.00	0.59	0.00	0.60	0.00	0.27
Panel B: Brandenburg & Saxony								
Facebook treatment	-0.01	-0.02	-0.02	-0.03*	-0.04*	-0.05***	-0.00	-0.01
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Mean dep. var., 'Control'	0.28	0.28	0.20	0.20	0.21	0.21	0.94	0.94
SD dep. var., 'Control'	0.40	0.40	0.40	0.40	0.41	0.41	0.23	0.23
Observations	1,359	1,359	1,359	1,359	1,359	1,359	1,359	1,359
R^2	0.00	0.66	0.00	0.59	0.00	0.61	0.00	0.26
Panel C: Thuringia								
Facebook treatment	0.03	0.02	0.03	0.01	0.03	0.01	0.04	0.02
	(0.04)	(0.02)	(0.04)	(0.02)	(0.04)	(0.02)	(0.03)	(0.02)
Mean dep. var., 'Control'	0.20	0.20	0.11	0.11	0.13	0.13	0.90	0.90
SD dep. var., 'Control'	0.35	0.35	0.32	0.32	0.34	0.34	0.30	0.30
Observations	369	369	369	369	369	369	369	369
R^2	0.00	0.62	0.00	0.64	0.00	0.62	0.01	0.38
Controls		Yes		Yes		Yes		Yes

Notes: Results are derived from regressions as laid out in Equation 3.5. We employ the following dependent variables elicited in our pre-election survey: (Columns 1 and 2) participants' self-reported likelihood to ever vote for the AfD (0-1) (*Ever vote for AfD*); (Columns 3 and 4) a dummy variable taking value 1 if a participant reported that s/he is planning to vote for an AfD candidate (*Intended candidate vote*); (Columns 5 and 6) a dummy variable taking value 1 if a participant reported that s/he is planning to vote for the AfD (*Intended list vote*); and (Columns 7 and 8) a dummy variable taking value 1 if a participated reported that s/he is planning to vote in the elections (*Intended to vote*). Controls include the full set of variables reported in Table 3.5. Robust standard errors reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 3.B.7: Treatment effects on AfD voting and turnout in the 2019 state elections elicited *after* the elections

	AfD Voting						Turnout	
	Ever vote for AfD		Self-reported candidate vote		Self-reported list vote		Voted	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Full Sample								
Facebook treatment	0.01	-0.00	-0.02	-0.03*	-0.02	-0.02	0.03	0.02
	(0.02)	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)
Mean dep. var., 'Control'	0.27	0.27	0.21	0.21	0.22	0.22	0.91	0.91
SD dep. var., 'Control'	0.39	0.39	0.41	0.41	0.41	0.41	0.29	0.29
Observations	1,312	1,312	1,212	1,212	1,212	1,212	1,312	1,312
R^2	0.00	0.64	0.00	0.57	0.00	0.61	0.00	0.25
Panel B: Brandenburg & Saxony								
Facebook treatment	0.00	-0.01	-0.03	-0.04**	-0.05	-0.05**	0.02	0.01
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)
Mean dep. var., 'Control'	0.28	0.28	0.23	0.23	0.24	0.24	0.92	0.92
SD dep. var., 'Control'	0.4	0.4	0.42	0.42	0.43	0.43	0.28	0.28
Observations	1,016	1,016	942	942	942	942	1,016	1,016
R^2	0.00	0.64	0.00	0.57	0.00	0.60	0.00	0.26
Panel C: Thuringia								
Facebook treatment	0.03	0.02	0.02	-0.00	0.08	0.05**	0.05	0.04
	(0.04)	(0.03)	(0.05)	(0.03)	(0.05)	(0.03)	(0.04)	(0.03)
Mean dep. var., 'Control'	0.22	0.22	0.14	0.14	0.15	0.15	0.88	0.88
SD dep. var., 'Control'	0.36	0.36	0.35	0.35	0.36	0.36	0.33	0.33
Observations	296	296	270	270	270	270	296	296
R^2	0.00	0.69	0.00	0.62	0.01	0.70	0.01	0.26
Controls		Yes		Yes		Yes		Yes

Notes: Notes: Results are derived from regressions as laid out in Equation 3.5. We employ the following dependent variables elicited in our post-election survey: (Columns 1 and 2) participants' self-reported likelihood to ever vote for the AfD (0-1) (*Ever vote for AfD*); (Columns 3 and 4) a dummy variable taking value 1 if a participant reported that s/he had voted for an AfD candidate (*Self-reported candidate vote*); (Columns 5 and 6) a dummy variable taking value 1 if a participant reported that s/he had voted for the AfD (*Self-reported list vote*); and (Columns 7 and 8) a dummy variable taking value 1 if a participated reported that s/he had voted in the elections (*Voted*). Controls include the full set of variables reported in Table 3.5. Robust standard errors reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

Table 3.B.8: Treatment effects on revealed preference outcomes

	Clicked donation link		Donated		Amount donated		Clicked petition link	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Campaign ads	−0.01	−0.01	0.01	0.01	−0.05	−0.05	0.00	0.00
	(0.01)	(0.01)	(0.02)	(0.02)	(0.18)	(0.18)	(0.02)	(0.02)
Controls		Yes		Yes		Yes		Yes
Mean, untreated	0.08	0.08	0.61	0.61	3.31	3.31	0.1	0.1
SD, untreated	0.27	0.27	0.49	0.49	3.58	3.58	0.29	0.29
Observations	1,511	1,511	1,511	1,511	1,511	1,511	1,511	1,511
R^2	0.02	0.02	0.19	0.19	0.23	0.23	0.09	0.09

Notes: Results are derived from regressions as laid out in Equation 3.5. We employ the following revealed preference outcomes: (Columns 1 and 2) a dummy variable taking value 1 if a participant clicked on the link forwarding her/him to the website of another grassroots organization contending against right-wing populism for which s/he could donate during the survey (*Clicked donation link*); (Columns 3 and 4) a dummy variable taking value 1 if a participant donated a positive amount to this grassroots organization (*Donated*); Columns 5 and 6) the amount a participant donated to this organization (*Amount donated*); and (Columns 6 and 9) a dummy variable taking value 1 if a participant clicked on the link forwarding her/him to a website where s/he could sign a petition demanding greater political representation for Muslims in Germany (*Clicked petition link*). Controls include the full set of variables reported in Table 3.5. Robust standard errors reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 3.B.9: Treatment effects of *identity treatment* on first-stage attitudes and beliefs about K5

	Competence	Shared goals	Identify self	Identify others
	(1)	(2)	(3)	(4)
Campaign ads	−0.35***	−0.04	−0.17	1.71
	(0.13)	(0.14)	(0.16)	(1.66)
Local	0.14	0.21	0.14	−1.42
	(0.14)	(0.15)	(0.16)	(1.83)
Controls	Yes	Yes	Yes	Yes
Mean dep. var., 'Control'	4.52	5.04	4.02	5.33
SD dep. var., 'Control'	2.63	2.96	3.3	3.27
Observations	1,729	1,729	1,729	1,728
R^2	0.33	0.35	0.31	0.02

Notes: Results from regressions as laid out in Equation 3.6 reported. We employ participants' answers to the following questions as dependent variables: (Column 1) "Do you think K5 is competent?" (*Competence*) (Column 2) "Do you share K5's goals?" (*Shared goals*); (Column 3) "Do you identify with K5?" (*Identify self*); and (Column 4) "Do you think other survey takers identify with K5? (*Identify others*)". Controls include the full set of variables reported in Table 3.5. Robust standard errors reported in parentheses. Significance levels: $* \ p < 0.10$, $** \ p < 0.05$, $*** \ p < 0.01$.

Table 3.B.10: Comparison of treatment effects of *identity treatment* across outcomes

	Attitudes			Voting behavior		Revealed preference	
	Migration	Climate	Identity	Vote AfD dummy	Turnout dummy	Donated dummy	Clicked petition link
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Campaign ads	0.00	0.06	0.02	-0.02^{*}	0.00	-0.01	-0.01
	(0.03)	(0.04)	(0.04)	(0.01)	(0.01)	(0.03)	(0.02)
Local	0.02	0.05	0.05	-0.01	-0.00	0.04	0.01
	(0.03)	(0.04)	(0.04)	(0.01)	(0.01)	(0.03)	(0.02)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean, untreated	0	-0.02	-0.02	0.19	0.93	0.61	0.08
SD, untreated	1	1.03	0.97	0.39	0.25	0.49	0.28
Observations	1,729	1,729	1,729	1,729	1,729	1,512	1,729
R^2	0.67	0.45	0.64	0.60	0.27	0.19	0.08

Notes: Results from regressions as laid out in Equation 3.6 reported. We employ the following three sets of outcomes defined in previous tables: (i) posterior attitudes (*Climate change* in Column 1, *Identity* in Column 2, and *Migration* in Column 2) elicited in our pre-election survey; (ii) self-reported voting behavior in the 2019 state elections elicited in our pre-election survey (*Vote AfD dummy* in Column 4 and *Turnout* dummy in Column 5); (iii) revealed preference measures of opposition to right-wing populism (*Donated dummy* in Column 6 and *Clicked petition link dummy* in Column 7). Controls include the full set of variables reported in Table 3.5. Robust standard errors reported in parentheses. Significance levels: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$.

3.C Campaign Adds

Figure 3.C.1: K5’s campaign ad addressing *identity*

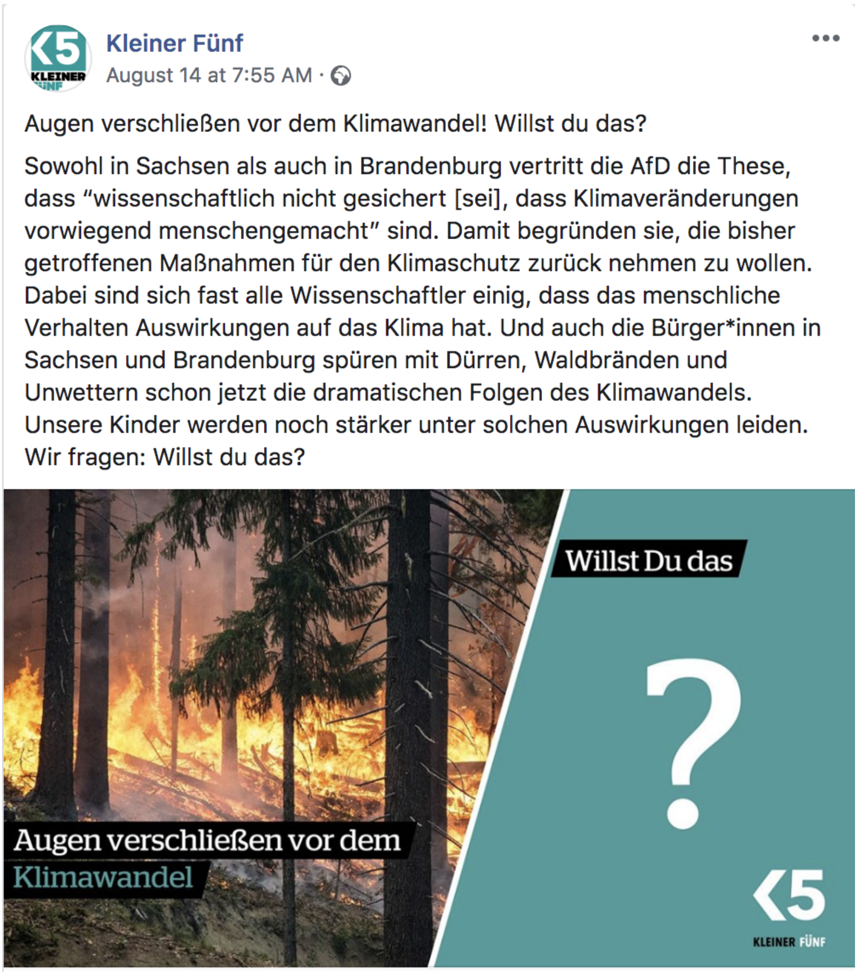


Figure 3.C.2: K5's campaign ad addressing *climate change*

3.D Survey Instrument Pre-Election Survey

3.D.1 Basic demographics

Question: How old are you?
Answer options: Under 18, 18-29, 30-44, 45-59, 60-74, 75+.

Question: What is your zip code?

Question: What was your own GROSS income in the last year (i.e. before deduction of taxes, contributions to pension, health, long-term care and unemployment insurance)?
Answer options: less than 10.000€, 10.000€-24.999€, 25.000-39.999€, 40.000€ and more

Question: Which gender do you identify with?
Answer options: Male, Female

Question: In which federal state do you live?

new page

Question: Are you eligible to vote in the upcoming state elections in Brandenburg?
Answer options: Yes, No

new page

Question: How would you rate your current household income? With the current income I/we (can)...
Answer options: Live comfortably, get by, have a hard time getting by, have a very hard time getting by.

Question: Which of the following categories applies to your current employment?
Answer options: Full time employed, Part time employed, Self-employed, In vocational training or student, Not employed, Registered as unemployed.

Question: What is the name of the place (city/town) you live in?

new page

3.D.2 Moral Foundations Survey

Question: When deciding whether something is right or wrong, to what extent are the following considerations important to you? Please rate each statement using a scale. ”Not at all relevant” means: This consideration has absolutely nothing to do with my judgment of whether something is right or wrong. ”Extremely relevant” means: This is one of the most important factors when I decide whether something is right or wrong. You can use the options in between to grade your opinion.
Answer options: Not at all relevant, not very relevant, a little relevant, fairly relevant, very relevant, extremely relevant.

1. Whether someone’s feelings are hurt.
2. Whether some people are treated differently than others.
3. Whether actions are done out of love of country.
4. Whether someone has shown a lack of respect for authority.
5. Whether someone has violated decency and purity.
6. Whether someone has performed well in mathematics.
7. Whether someone stands up for another vulnerable and weak person.
8. Whether someone acts unjustly.

new page

1. Whether someone has done something to betray his or her group and deceive them.
2. Whether someone has adhered to the traditions of society.
3. Whether someone has done something disgusting.
4. Whether someone was cruel.
5. Whether someone was denied his or her rights.
6. Whether someone shows a lack of loyalty.
7. Whether someone’s actions have caused chaos and disorder.

8. Whether someone has acted in a way that God would approve of.

——— *new page* ———

Task: Please also read through the following statements and indicate how much you agree or disagree with them.
Answer options: Disagree at all, Somewhat disagree, Somewhat agree, Totally agree.

1. Compassion for those who suffer is the most important virtue.
2. When the government makes laws, they should always be designed so that everyone is treated fairly.
3. I am proud of my country's history.
4. All children should learn respect for authority.
5. People should not do things that are disgusting, even if no one is disturbed or hurt in the process.
6. It is better to do good things than bad things.
7. Hurting a defenseless animal is one of the worst things a person can do.
8. Justice is the most important cornerstone for a society.

——— *new page* ———

1. People should be loyal to their family members even if they have done something wrong.
2. Men and women should take on different roles in society.
3. I would call certain acts wrong because they are unnatural.
4. It can never be right to kill a human being.
5. I find it morally reprehensible that rich children inherit a lot of money while poor children inherit nothing.
6. It is more important to be a good team player than to self actualize.
7. As a soldier, if I disagreed with my superior's orders, I would still follow them out of duty.
8. Chastity is an important and valuable virtue.

——— *new page* ———

3.D.3 Political attitudes, beliefs, and knowledge

In this section of our survey, we will ask you some questions about politics.

Question: How interested are you in politics? Are you...
Answer options: Very interested, fairly interested, not very interested, not interested at all.

Question: Who of the following people is the current German Minister of Interior?

Question: In general, how often do you talk about politics with your peers (family, friends, acquaintances)?
Answer options: Very often, often, sometimes, rarely, never

new page

Question: Please use the scale below to indicate how much you trust each of the public institutions or groups of people listed. 0 means that you do not trust the respective institution or group at all, and 10 means that you trust them completely.

1. ... the German Parliament
2. ... the politicians
3. ... the police
4. ... the parties
5. ... the judiciary
6. ... the Federal Government
7. ... the state government
8. ... the state parliament

new page

Question: We will now present you several statements about politics in Germany. Please tell us in each case to what extent you agree or disagree with this statement.
Answer options: Fully agree, tend to agree, partly/partly agree, tend to disagree, fully disagree.

1. Overall, the people agree on what must happen politically.
2. Politicians only care about the interests of the rich and powerful.
3. Parties are necessary to represent the interests of the various social demographics.
4. German democracy gives people like me a say in what the government does.

new page

3.D.4 Past voting behavior

Question: Nowadays, some people do note vote for various reasons. How about you? Did you vote in the previous federal elections in September 2017?

Question: You could to cast two votes in the last federal elections. With the first vote you could vote for a candidate from your constituency (candidate vote) and with the second vote you could vote for a party (list vote). A member of which party was the candidate you voted for?

Question: Which *party* did you vote for?

new page

Question: Did you vote in the previous state elections in Brandenburg in 2014?

Question: Also in the previous state elections, you had two votes. With the first vote you could vote for a candidate from your constituency (candidate vote) and with the second vote you could vote for a party (list vote). Which *candidate* did you vote for?
Question: And which *party* did you vote for ?

new page

Question: Have you ever voted to signal your protest (this includes not voting or casting an invalid ballot)?

new page

Question: What was your protest directed against? You can select multiple answer

options.
Answer options: Government, Politics, Elites, Old parties, Social injustices, Lack of climate policy, Immigration, Refugee policy, Too much interference of the EU in national politics, Other

Question: Looking back, do you still support your protest-related voting decision?

new page

Question: Many people use the terms ″left″ and ″right″ when referring to different political attitudes. Where on the scale would you classify yourself if 0 stands for left and 10 for right?

Question: Parties are also often classified as ″left″ or ″right. How would you classify the following parties on a scale? 0 again stands for left and 10 for right.

- FDP
- CDU/CSU
- SPD
- The Greens
- The Left
- AfD (Alternative for Germany)

new page

Question: When you choose a party in the election, how important are the party's positions on the following issues?
Answer options: very important, rather important, neutral, rather unimportant, not at all important

1. Labor market policy (e.g. unemployment benefits, part-time employment, minimum wage, automation)
2. Tax policy (e.g. solidarity contribution, top tax rates, taxation of international companies)
3. Climate policy (e.g. expansion of renewable energies, reduction of $CO2$ emissions)

4. Pension policy (e.g. securing pension levels, retirement age)
5. Family policy (e.g. child benefits, parental leave, childcare)
6. Health and care (e.g. health insurance contributions, care of relatives)
7. Migration and integration (e.g. immigration policy, language and integration courses)
8. Education and culture (e.g. school system, universities, financial support for cultural institutions)
9. Homeland and customs (e.g. protection of traditions, strengthening of rural areas, religion)
10. Transport and infrastructure policy (e.g. electrical cars, driving bans, local public transport)
11. Digital policy (e.g. expansion of fiber-optic network, digitization of municipal services)

new page

3.D.5 Eliciting first stage for field experiment

Question: State elections will be held in Brandenburg in a few days. During the election campaign, political parties and initiatives try to reach voters with the help of election campaigns, e.g. via online advertising, commercials, posters or events.
Have you seen online advertising, commercials, posters or events by *[name of initiative]* in the last few weeks?

IMPLEMENTATION: SHOW A SERIES OF IMAGES WHERE THE SURVEY PARTICIPANT CAN ANSWER WITH YES OR NO.

new page

Question: Have you talked to other people about these campaigns?
Answer options: Yes, No

new page

Question: Which campaign did you talk about? Multiple answers are possible.______________________________

Question: To which group of people did the person(s) you talked to about these cam-

paigns belong?
Answer options: Friends, Family, Known, Colleagues, Strangers, Other.

3.D.6 Pre-treatment attitudes and beliefs in the domains of climate change, identity, and migration

Question: What percentage of people living in Germany do you think were born outside Germany?
This refers to places of birth outside the current territory of the Federal Republic of Germany.
Please enter a number between 0 and 100.

Question: How sure are you about your answer to the previous question?
Answer options: Very certain, certain, fairly certain, uncertain, very uncertain.

Question: What percentage of people living in Germany do you think are Muslims?
Please give a percentage between 0 and 100.

Question: What do you think the percentage of people of the Muslim faith living in Germany was before the so-called refugee crisis in the summer of 2015?
Please give a percentage between 0 and 100.

new page

Question: Now we want to hear your opinion regarding whether someone who was born and raised outside of Germany should be allowed to come to Germany and live here. How important should the following things be for this decision - in your opinion?
Answer options: 0 = extremely important, 10 = extremely unimportant

That this person...
...has a good school education and vocational training?
...is willing to accept the way of life in Germany?
...can speak German?
...adheres to the Christian faith?

new page

Question: The following questions have these answer options: strongly connected, fairly connected, little connected, not connected at all

1. How strongly do you feel connected to your community (city) and its citizens?
2. How strongly do you feel connected to your state and its citizens?
3. How strongly do you feel connected to Germany as a whole and its citizens?
4. How strongly do you feel connected to the European Union and its citizens?

new page

Question: There are people who come to Germany and apply for asylum because they are afraid of persecution in their own country. How strongly do you agree or disagree with the following statement?
The state should be generous when considering asylum applications.
Answer options: strongly agree, agree, partly agree, disagree, strongly disagree.

new page

Question: A much-discussed topic in Germany is climate change. How much do you agree or disagree with the following statements?
Answer options: Strongly agree, agree, partly agree, disagree, strongly disagree.

1. Climate change is one of the biggest problems of our time
2. The lignite phase-out is essential for climate protection. It must therefore be implemented quickly, even if it costs jobs.
3. It is the task of politicians to drive climate protection forward with legislation.
4. Climate change is a man-made problem.

new page

Question: Another intensely debated topic is the role of customs and tradition in Germany. How much do you agree or disagree with the following statements?
Answer options: Strongly agree, agree, partly agree, disagree, strongly disagree.

1. Politicians should make an active effort to preserve local and national German customs.
2. Minority customs should be promoted in Germany.
3. It is better for a country if almost everyone has the same customs and traditions.

new page

Question: How much do you agree or disagree with the following statements?

1. The German economy gains from immigration.
2. Germany loses its national identity through immigration.

new page

Question: How do you think the following factors will affect prosperity in Germany in the future?
Answer options: Positive influence, rather positive influence, no influence, rather negative influence, negative influence, no influence.

1. ...intelligent machines replacing human labor.
2. ...outsourcing of jobs abroad.
3. ...immigration of workers to Germany.

new page

3.D.7 Media consumption

Question: The media play an important role in the political context in Germany. In the following, we therefore ask you some questions about media use. For each of the media listed below, please indicate how much or how little you trust them.
Answer options: very high trust, high trust, medium trust, low trust, very low trust.

- Public television and radio
- Private television and radio
- Print media and online portals of print media
- Social media

new page

Question: Which of the following sources do you use most to find out about political events in Germany? Please select a maximum of three options.

- Public television

- Private television
- Public radio
- Private radio
- Print media
- Online portals of print media
- Social media
- Other

new page

Question: What influences you the most in your use of various media?

- Cost
- Convenience
- Political slant in news coverage
- Digital availability
- Comprehensibility
- Other

new page

Question: Do you use Facebook?
Question: Do you use Twitter?

new page

3.D.8 Attention check

Question: The next question is about the following problem. In surveys like this, there are sometimes participants who do not read the questions carefully and just ”click” quickly through the questionnaire. As a result, there are many random answers that distort the results of the study. Therefore, to signal that you read our questions carefully, we ask that you indicate “Very interested” and “Not at all interested” as your answers to the next question.
How interested are you in politics?

Answer options: Very much interested, Very interested, A little interested, Almost not interested, Not at all interested.

new page

SUBSEQUENTLY, WE RANDOMIZED PARTICIPANTS IN ONE OF THE THREE EXPERIMENTAL CONDITIONS: "CAMPAIGN ADS – BERLIN", "CAMPAIGN ADS – LOCAL", AND "CONTROL"

Campaign ads – Berlin

Question: Did you know that there are civil society organizations in which citizens join together to oppose right-wing populism?
Answer options: Yes, No

new page

Question: We will now show you examples of an online initiative against right-wing populism by the organization "Kleiner Fünf" from Berlin. Have you ever seen content by "Kleiner Fünf" outside of this survey?
Answer options: Yes, No

new page

Here you can see examples of this content:

1. Turn a blind eye to climate change! Do you want that? Right-wing populists* in Brandenburg claim that climate change is not man-made and want to roll back measures taken so far for climate protection. Yet citizens in Brandenburg are already feeling the dramatic consequences of climate change with droughts, forest fires and storms. Our children will suffer even more from such effects.

2. My customs beat your customs! Is that what you want? Right-wing populists in Brandenburg want to promote traditional state customs, but question the practice of other customs. Yet various customs already enrich our society, such as those of the Sorbian and Wendish minorities. Customs should not be played off against each other.

3. Policies at the expense of the "little people"! Is that what you want? Right-wing populists in Brandenburg are campaigning for the wealth tax to remain abolished. This was not paid by the "little people" but by people with greater wealth. This tax revenue is then missing from the state budget, from which the benefits for the "little people" are financed. In this way, the rich are to be relieved

at the expense of people with fewer assets.

new page

Question: Have you ever seen any of the above content by “Kleiner Fünf” outside of this survey?
Answer options: Yes, No

new page

Question: Please indicate here the channels through which you have already encountered “Kleiner Fünf”. Multiple answers are possible.

- Flyers
- Posters
- Social media (e.g. Facebook)
- Demonstrations
- Campaign events
- Friends or acquaintances
- Family
- Others

new page

Question: Have you been in contact with other organizations of this type?
Answer options: Yes, No

new page

Question: Please indicate the channels through which you have come into contact with such organizations. Multiple answers are possible.

- Flyers
- Posters
- Social media (e.g. Facebook)
- Demonstrations
- Campaign events

- Friends or acquaintances
- Family
- Others

new page

Next up is a guessing question where you can win money. Among the survey participants whose guesses are closest to the correct value, we will give away additional mingle points worth €100. We also asked members of "Kleiner Fünf" where they would rank themselves on a political "left-right" scale if 0 stood for left and 10 for right. Please estimate the average response of these members to one decimal place (i.e., "8.5").
Numerical entry field
Note: it may take up to 4 weeks to determine the winners and pay out the additional mingle points.

new page

Question: Do you identify with "Kleiner Fünf"?
Answer options: 0 = I don't identify at all, 10 = I fully identify

new page

Campaign ads – Local

Question: Did you know that there are civil society organizations in which citizens join together to oppose right-wing populism?
Answer options: Yes, No

new page

We will now show you examples of an online initiative against right-wing populism by the organization "Kleiner Fünf," which many people from Brandenburg support.
Question: Have you ever become aware of content from "Kleiner Fünf" outside of this survey?
Answer options: Yes, No

new page

Here you can see examples of this content:

1. Turn a blind eye to climate change! Do you want that? Right-wing populists* in Brandenburg claim that climate change is not man-made and want to roll

back measures taken so far for climate protection. Yet citizens in Brandenburg are already feeling the dramatic consequences of climate change with droughts, forest fires and storms. Our children will suffer even more from such effects.

2. My customs beat your customs! Is that what you want? Right-wing populists in Brandenburg want to promote traditional state customs, but question the practice of other customs. Yet various customs already enrich our society, such as those of the Sorbian and Wendish minorities. Customs should not be played off against each other.

3. Politics at the expense of the ”little people”! Is that what you want? Right-wing populists in Brandenburg are campaigning for the wealth tax to remain abolished. This was not paid by the ”little people” but by people with greater wealth. This tax revenue is then missing from the state budget, from which the benefits for the ”little people” are financed. In this way, the rich are to be relieved at the expense of people with fewer assets.

Question: Have you ever seen any of the above content by “Kleiner Fünf” content outside of this survey?
Answer options: Yes, No

new page

Question: Please indicate the channels through which you have come into contact with such organizations. Multiple answers are possible.

- Flyers
- Posters
- Social media (e.g. Facebook)
- Demonstrations
- Campaign events
- Friends or acquaintances
- Family
- Others

new page

Next up is a guessing question where you can win money. Among the survey participants whose estimate comes closest to the value, we will give away additional mingle

points worth €100.
We also asked members of "Kleiner Fünf" the question where they would place themselves on a political "left-right" scale if 0 stood for left and 10 for right.
Please estimate the average answer of these members to one decimal place (i.e., "8.5").
Numerical entry field
Note: it may take up to 4 weeks to determine the winners and pay out the additional mingle points.

new page

Question: Do you identify with "Kleiner Fünf"?
Answer options: 0 = no identification at all, 10 = full identification

new page

Control

Question: Did you know that there are civil society organizations in which citizens join together to oppose right-wing populism?
Answer options: Yes, No

new page

Question: Have you ever come into contact with organizations of this type?
Answer options: Yes, No

new page

Question: Please indicate here the channels through which you have come into contact with such organizations. Multiple answers are possible.

- Flyers
- Posters
- Social media (e.g. Facebook)
- Demonstrations
- Campaign events
- Friends or acquaintances
- Family
- Others

new page

FOR THE REMAINDER OF THE SURVEY, WE USED THE SAME SURVEY ITEMS FOR ALL PARTICIPANTS, YET REPLACED "KLEINER FÜNF" WITH "CIVIL-SOCIETY ORGANIZATIONS CONTENDING AGAINST RIGHT-WING POPULISM" IN THE CONTROL GROUP.

3.D.9 First stage of identity treatment

Next up is another guessing question in which you can win extra money. Among the survey participants whose estimates are closest to the correct value, we will draw additional mingle points worth €100.

Question: Estimate how other survey participants from Brandenburg answered the question about the identification with ["Kleiner Fünf" / civil society organizations contending against right-wing populism]. Please estimate the average answer to one decimal place (i.e. "8.5", for example).
The question was: Do you identify with '["Kleiner Fünf" / civil society organizations contending against right-wing populism]? The value 0 represents "no identification at all", the value 10 represents full identification.
Numerical entry field
Note: it can take up to 4 weeks until the winners are determined and the additional mingle points are paid out.

new page

Question: Do you agree with ["Kleiner Fünf" / civil society organizations contending against right-wing populism] and [its/their] goals, as far as you can assess them?
Answer options: 0 = no identification at all, 10 = full identification

Question: Do you consider ["Kleiner Fünf" / civil society organizations contending against right-wing populism] to be competent when it comes to correctly assessing the political, social and economic problems and needs in Brandenburg?
Answer options: 0 = no identification at all, 10 = full identification

new page

Question: According to what you know so far: Do many people from Brandenburg support ["Kleiner Fünf" / civil society organizations contending against right-wing populism]?

Answer options: Yes, No, I don't know

new page

3.D.10 Intended voting behavior in the 2019 state elections

Question: State elections will be held in [*federal estate*] on September 1. Are you planning to vote in the upcoming state elections?
Answer options: Yes, rather yes, rather no, no

new page

Question: In the upcoming election you can again cast two votes: With your candidate vote you can vote for a candidate from your constituency and with your list vote you can vote for a party. A member of which party is the candidate you intend to vote for?
Answer options: FDP, The Left, CDU/CSU, Other, AfD, The Greens, SPD.

Question: And which party will you vote for?
Answer options: FDP, The Left, CDU/CSU, Other, AfD, The Greens, SPD

new page

Question: There are several political parties in Germany. Each of them would like to get your vote in elections. For each of the following parties, please indicate how likely it is that you will ever vote for that party. Please use the scale below each party.
Answer options: 0 = very unlikely, 10 = very likely

- FDP
- The Left Party
- CDU/CSU
- AfD
- The Greens
- SPD

new page

Question: How would you rate the CDU's expertise in the following policy domains?
Answer options: 0 = no expertise at all, 10 = very good expertise
THIS SET OF QUESTIONS IS REPEATED FOR THE AFD.

1. Labor market policy (e.g. unemployment benefits, part-time employment, minimum wage, automation)
2. Tax policy (e.g. solidarity contribution, top tax rates, taxation of international companies)
3. Climate policy (e.g. expansion of renewable energies, reduction of CO2 emissions)
4. Pension policy (e.g. securing pension levels, retirement age)
5. Family policy (e.g. child benefits, parental leave, childcare)
6. Health and care (e.g. health insurance contributions, care of relatives)
7. Migration and integration (e.g. immigration policy, language and integration courses)
8. Education and culture (e.g. school system, universities, financial support for cultural institutions)
9. Homeland and customs (e.g. protection of traditions, strengthening of rural areas, religion)
10. Transport and infrastructure policy (e.g. electromobility, driving bans, local public transport)
11. Digital policy (e.g. expansion of fiber-optic network, digitization of municipal services)

new page

3.D.11 Posterior attitudes in the domains of climate change, identity, and migration

Question: Now think of immigrants who come to Germany and belong to a different ethnic group than most Germans: How much would it bother you if such person...
Answer options: 0 = would not bother me at all, 10 = would bother me a lot

- ... was your neighbor?
- ... got married to a person closely related to you?

Question: What do you think: How are immigrants who have recently arrived in Germany treated by the government compared to people who were born in Germany?
Answer options: much better, a little better, the same, a little worse, much worse

Question: Are Germany's crime-related problems increasing or decreasing due to immigrants?
Answer options: 0 = problems with crime increase, 10 = problems with crime decrease

new page

Question: Please indicate to what extent you agree or disagree with the following statements.
Answer options: strongly agree, agree, partly agree, disagree strongly disagree

- Those who protect polluting industries as they are afraid of job losses underestimate the threat that climate change poses to our society.
- Policymakers should raise taxes on flights to protect the environment, even if it makes traveling more expensive.

new page

Question: What do you think about...

- ... introducing Islamic religious education in German schools?
- ... renaming Christmas markets to winter markets?
- ... mosques being built in German cities?
- Immigration enriches German culture. What do you think of this statement?
- Current politics in Germany endanger German customs. What do you think of this statement?
- The national identity of the Germans should be promoted more by policy makers. What do you think of this statement?

new page

Question: What do you think about the following question: Do immigrants who come here generally take away jobs from workers in Germany or do they generally help to create new jobs?

Answer options: 0 = take away jobs, 10 = create new jobs

new page

3.D.12 Sign petition

You now have the opportunity to sign the following petition (collection of signatures): *"Time for a commissioner against Islamophobia and Muslimophobia."* This petition calls for the establishment of a department in the Federal Ministry of the Interior for a commissioner against Islam and Muslim hostility.

Question: Would you like to sign this petition?
Answer options: Yes, No

new page

You have indicated that you would like to sign the petition. To do so, simply click on the link below, which will open in a new window. Please then return to this survey immediately. You can sign the petition after this survey.
Petition: Time for a Commissioner against Islamophobia and Muslimophobia

new page

3.D.13 Donation task

Ten participants of this study will be randomly selected and can receive additional mingle points worth €10 each. The selected participants must decide how much of these €10 they would like to keep for themselves and how much they would like to donate to the following initiative:
Initiative Offene Gesellschaft e.V.
The "Initiative Offene Gesellschaft" campaigns for freedom of opinion, freedom of belief and equal rights, e.g. with debates and art actions in various cities and communities throughout Germany.
If you would like to learn more about the "Initiative Offene Gesellschaft", click on the following link: Initiative Offene Gesellschaft e.V.

Question: If you are selected, how much of the €10 would you like to donate to the "open society"?
Numeric entry field [0-10€]
Note: We will keep the indicated amount and donate this amount directly to the "Ini-

tiative Offene Gesellschaft". It may take up to 4 weeks for the additional mingle points you wish to keep to be paid out.

new page

3.D.14 Share content by "Kleiner Fünf"

In this section of our survey, you have the opportunity to share the content of the organization "Kleiner Fünf", which contends against right-wing populism. Here you can see examples of this content again:

1. Turn a blind eye to climate change! Do you want that? Right-wing populists* in Brandenburg claim that climate change is not man-made and want to roll back measures taken so far for climate protection. Yet citizens in Brandenburg are already feeling the dramatic consequences of climate change with droughts, forest fires and storms. Our children will suffer even more from such effects.

2. My customs beat your customs! Is that what you want? Right-wing populists in Brandenburg want to promote traditional state customs, but question the practice of other customs. Yet various customs already enrich our society, such as those of the Sorbian and Wendish minorities. Customs should not be played off against each other.

3. Politics at the expense of the "little people"! Is that what you want? Right-wing populists in Brandenburg are campaigning for the wealth tax to remain abolished. This was not paid by the "little people" but by people with greater wealth. This tax revenue is then missing from the state budget, from which the benefits for the "little people" are financed. In this way, the rich are to be relieved at the expense of people with fewer assets.

You can find more content from "Kleiner Fünf" here: Kleiner Fünf
Question: Would you like to share the above link to more content from "Kleiner Fünf" with other people?
Answer options: Yes, No

new page

Question: With which group of people would you like to share the link to the content of "Kleiner Fünf"? Multiple selection is possible.
Answer options: Family, friends, acquaintances, others

new page

You have indicated that you would like to share the link to the other content of "Kleiner Fünf". To do this, simply click on the link below. This will open your email program and you can enter the email addresses of the people you want to send the "Kleiner Fünf" content to. We do not record your email addresses or the email addresses you enter.
Click to share: Email

new page

3.D.15 Final questions and wrap up

Question: I have filled out this questionnaire very carefully and paid lots of attention throughout.
Answer options: completely, to a large extent, partially, not at all
Question: What do you think was the purpose of this survey?

Once we have collected all the data, we will determine the winners of the estimation questions. It can take up to 4 weeks before the winners are determined and the mingle points are paid out. We may wish to survey you again in a few weeks. If you answer the second survey completely, you will receive a special payment of 50 cents in addition to the usual payment.

Question: I have read the notice about the repeat survey with special compensation.
Answer options: Yes, No
If you have any comments about this survey, please note them here.

Close survey.

END OF SURVEY

3.E Survey Instrument Post-Election Survey

3.E.1 Basic demographic information

1. **Question:** In which state do you live?

2. **Question:** Which educational degree did you obtain? Please report only your highest degree.
   Answer options: Still a student, School finished without graduation, Elementary school / secondary modern school or polytechnic secondary school with 8th or 9th grade certificate, Intermediate school certificate, Realschule certificate or polytechnic secondary school with 10th grade certificate, Advanced technical college certificate (certificate from a technical secondary school, etc.), Abitur certificate or extended secondary school with 12th grade certificate (higher education entrance qualification), Other school certificate If you indicated ”other school-leaving qualification” in the previous question, please note its designation.

3. **Question:** Which vocational training degree did you obtain? Note: You can also select more than one answer.
   Answer options: Vocational-in-company training period with certificate of completion (but no apprenticeship), Partial skilled worker’s certificate, Completed industrial or agricultural apprenticeship, Completed commercial apprenticeship, Vocational internship / traineeship, Vocational school certificate, Technical school certificate, Master craftsman’s certificate, Technician’s certificate or equivalent technical school certificate, Technical college certificate (also certificate from an engineering school), University degree, No vocational training certificate, Other training certificate, namely (please enter in the following field).

new page

3.E.2 Self-reported voting behavior in the 2019 state elections

State elections were held in Brandenburg on September 1, 2019. We would like to ask you a few questions about this.
Question: Did you vote in the state election in Brandenburg on September 1, 2019?
Answer options: Yes, No.

new page

ONLY DISPLAYED FOR PARTICIPANTS WHO REPLIED ”YES” TO THE PREVIOUS QUESTION

We would like to understand why you voted in the state elections in Brandenburg on September 1, 2019.
Question: To what extent do the following statements apply to you?
Answer options: fully applies, tends to apply, partly applies, tends not to apply, does not apply at all

1. I voted because I expected a close election outcome.
2. I voted because I always vote.

ONLY DISPLAYED FOR PARTICIPANTS WHO REPLIED "NO" TO THE PREVIOUS QUESTION

We would like to understand why you did not vote in the state election in Saxony on September 1, 2019.
Question: To what extent do the following statements apply to you?
Answer options: fully applies, tends to apply, partly applies, tends not to apply, does not apply at all

1. I did not vote because I do not feel represented by any of the parties running.
2. I didn't vote because I don't think it matters whether I vote or not.
3. I voted because I consider it my duty as a citizen.
4. I voted because people around me also voted.
5. I did not vote because I wanted to set a sign of protest.
6. What was your protest against that made you not vote? Note: You can select multiple answer options here.
   Answer options: The government, politics, the elites, the old parties, social injustice, lack of climate policy, immigration, refugee policy, too much interference of the EU in national politics, other reason, namely (please enter in the following field).

new page

ONLY DISPLAYED FOR PARTICIPANTS WHO REPLIED "YES" TO THE PREVIOUS QUESTION
In the state election on September 1, 2019 in Brandenburg, you could cast two votes: With the first vote you could vote for a candidate from your constituency and with the second vote for a party.
Answer options: AfD, SPD, Die Linke, CDU, FDP, Die Grünen, Other.

1. **Question** 1: A member of which party is the candidate you voted for?
2. **Question** 2: Which party did you vote for?

new page

Question: To what extent do the following statements regarding your decision which party to vote for apply?
Answer options: fully applies, tends to apply, partly applies, tends not to apply, does not apply at all

1. I voted for the FDP on the basis of its election or party platform.
2. I voted for the FDP because I always vote for this party.
3. I voted for the FDP because of its election campaign.
4. I voted for the FDP because of the election campaign of another party.
5. I voted for the FDP because of conversations with people from my personal environment (e.g. family, friends or colleagues).
6. I voted for the FDP to show support for one or more parties.
7. I voted for the FDP to set a sign of protest.

new page

Question: The election campaign of which other party was partly responsible for you to vote FDP?
Answer options: CDU, The Greens, SPD, AfD, The Left, FDP, Other (please enter in the following field).
Question: Which people in your personal environment influenced your decision to vote FDP in conversations? Note: You can select multiple answer options here.
Answer options: Family, Close friends, Acquaintances, Colleagues, Other people, namely (please enter in the following field).
Question: What were you protesting against that made you vote for FDP? Note: You may select more than one answer choice.
Answer options: The government, politics, the elites, the old parties, social injustice, lack of climate policy, immigration, refugee policy, too much interference of the EU in national politics, other reason, namely (please enter in the following field).

new page

There are quite a few political parties in Germany. Each of them would like to get your vote in elections.

Question: For each of the following parties, how likely is it that you will ever vote for that party? Please use the scale shown below each party. Scale value 0 means this is ″very unlikely″ for you, scale value 10 means this is ″very likely″ for you. You can use the options in between to grade your judgment.
Answer options: 0 = very unlikely, 10 = very likely

- CDU
- The Greens
- SPD
- AfD
- The Left
- FDP

new page

Many people use the terms ″left″ and ″right″ when referring to different political attitudes.

1. **Question:** Where on the scale do you see yourself if 0 stood for ″left″ and 10 for ″right″? You can use the values between 0 and 10 to grade your political orientation. **Answer options:** 0-10

2. Now imagine a person whose political views are typical of your family and close friends. **Answer options:** 0-10
Question: What do you think this person's political orientation on the political left-right scale would be?

3. Now imagine a person whose political views are typical of your community or city. **Answer options:** 0-10
Question: What do you think this person's political orientation on the political left-right scale would be?

4. Now imagine a person whose political views are typical for Brandenburg. **Answer options:** 0-10
Question: What do you think this person's political orientation on the political left-right scale would be?

5. Now imagine a person whose political views are typical for Germany.
   Question: What do you think the political orientation of this person on the political left-right scale would be? **Answer options:** 0-10

new page

Now think of immigrants who come to Germany and belong to a different ethnic group than most Germans. Please use this scale to answer the following questions. The scale value 0 means that something would "not bother you at all", the scale value 10 means that something would "bother you a lot". You can use the values in between to grade your judgment.

1. **Question:** How much would it bother you if such an immigrant was your neighbor?

2. **Question:** How much would it bother you if such an immigrant married someone closely related to you?

new page

3.E.3 Posterior attitudes in the domains of climate change, identity, and migration

Please continue to think about immigrants who come to Germany and belong to a different ethnic group than most Germans.

1. **Question:** How are such immigrants, who have come to Germany only recently, treated by the government and the state compared to people who were born in Germany?
   Answer options: much better, a little better, the same, a little worse, much worse

2. **Question:** Do such immigrants increase or decrease Germany's problems with crime?
   Answer options: 0 = problems with crime decrease, 10 = problems with crime increase

new page

Question: To what extent do you agree or disagree with the following statements/measures?
Answer options: strongly agree, tend to agree, partly agree, tend to disagree, strongly disagree.

1. Those who protect environmentally harmful industries out of fear of job losses underestimate the danger that climate change poses to our society.

2. Politicians should increase taxes on flights to protect the environment, even if it makes traveling more expensive.
3. Immigration enriches German culture.
4. Current politics in Germany endanger German customs.
5. The national identity of Germans should be promoted more strongly by politics.
6. Measure: Introduce Islamic religious instruction in German schools.
7. Measure: Rename Christmas markets to winter markets.
8. Measure: Build mosques in German cities.

new page

Question: What would you say, do immigrants coming here generally take away jobs from workers in Germany OR do they generally help create new jobs?
Answer options: 0 = take away jobs, 10 = create new jobs

new page

3.E.4 Re-elicit familiarity with K5's content

Question: Are you familiar with the civil society organization "Kleiner Fünf"?
Answer options: Yes, No

new page

You indicated that you have seen the previous content by "Kleiner Fünf".
Question: Where did you first see it?
Answer options: Social media (Facebook, Instagram, or Twitter), Campaign events or demonstrations, Inside another poll, Elsewhere, namely (please fill in the box below).

new page

Question: What do you think, do many people from Brandenburg support "Kleiner Fünf"?
Answer options: Yes, No, I don't know

new page

3.E.5 Perceived polarization

Question: To what extent do you agree or disagree with the following statements?
Answer options: strongly agree, tend to agree, partly agree, tend to disagree, strongly disagree.

1. The conflicts between the various interest groups in our society have become more extreme in recent years.
2. In recent years, it has become increasingly difficult to find compromises on important political issues.
3. Political discussions should take scientific findings into account more often.
4. The disputes between the various interest groups in our society and their demands on the government are detrimental to the common good.
5. The people agree in principle on what needs to happen politically.

——— *new page* ———

3.E.6 Personality traits

Question: To what extent do the following characteristics apply to you?
Answer options: strongly agree, tend to agree, partly agree, tend to disagree, strongly disagree.

1. Trait: I am rather restrained, reserved.
2. Trait: I trust others easily, believe in the good in people.
3. Trait: I am comfortable, tend to be lazy.
4. Trait: I am relaxed, do not let stress upset me.
5. Trait: I have little artistic interest.
6. Trait: I am outgoing, am sociable.
7. Trait: I tend to criticize others.
8. Trait: I complete tasks thoroughly.
9. Trait: I get nervous and insecure easily.
10. Trait: I have an active imagination.

new page

PARTICIPANTS ENTER THE **GLOBAL PREFERENCES SURVEY** (GPS) MODULE BY FALK ET AL. (2018). WE EMPLOYED THE FULL GPS QUESTIONNAIRE IN GERMAN AVAILABLE AT HTTPS://WWW.BRIQ-INSTITUTE.ORG/GLOBAL-PREFERENCES/DOWNLOADS

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3.E.7 Final questions and wrap-up

To conclude our survey, we would like to ask you a few general questions about our survey.
Question: How much attention did you pay and how carefully did you complete this questionnaire?
Answer options: completely, to a large extent, partially, not at all
Question 1: What do you think was the purpose of this survey?
Question 2: How did you perceive the political outset of this survey?
Answer options: left-wing, rather left-wing, neutral, rather right-wing, right-wing

Please briefly mention here the parts of the survey that you perceived as politically left/right leaning.

In the first round of this survey, which took place a few weeks ago, you answered a series of estimation questions. answered. Those guessing questions have been tied to the opportunity to win an additional incentive. Once we have compiled all the data, we will determine the winners of the guessing questions. This can take up to 4 weeks. Subsequently, the winners will be credited with their additional incentive. The type of credit is subject to the individual regulations of respondi AG and its partners. If you have any comments about the survey, you can note them here. Comments on the survey:

Close survey.

END OF SURVEY

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München, 14. September 2021

JOHANNES WIMMER