# Self-Selection and Treatment Assignment in Field Experiments[*]

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**Abstract**

Self-selection and unbalanced treatment assignment are two major concerns in experimental evaluations as they compromise the validity of a study at the external and the internal margin. In this paper we present evidence on the selection of partner institutions into participation and balanced treatment assignment in field experiments. We answer two questions: (1) do stakeholders that choose to participate in a field experiment differ from the population of interest, and (2) does pre-treatment balancedness on observable characteristics translate to lower bias and increased power in a real-world setting, and which method should best be used if that is the case? To this end, we conducted a recruitment experiment, inviting stakeholders to participate in field experiments with their institutions and varying the salience of the research topic and the stakes of participation. We combine this experimental data with a rich set of administrative data on institution and municipality characteristics to identify a possible self-selection bias. Moreover, we compare pure randomization, matching, and re-randomization to a new treatment assignment method—the minimum mean squared error treatment assignment (min MSE). We find no evidence for a self-selection bias on observable characteristics and estabhlish that balancedness as achieved by the minMSE method reduces bias of treatment effect estimation by 33% compared to pure randomization. The minMSE method performs best in increasing pre-treatment balancedness of observable characteristics compared to pure randomization, matching, and simple re-randomization.

**Keywords:** Self-selection bias, field experiments, treatment assignment

**JEL codes:** C93, I20

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# 1 Introduction

Experimental methods are increasingly used in recent years by academic researchers but also public policy-makers to enrich the toolkit for evidence based policy making. *"The most credible and influential research designs use random assignment"* (Angrist and Pischke, 2009), as randomization ensures a causal interpretation of the respective treatment conditions, independently of characteristics of the participating population. The strength of relying on properly conducted randomized controlled trials is that they not only shall allow for causal interpretation within a sample, but they also should provide reliable out of sample predictions for the population of interest. While appropriate treatment assignment is a necessary condition for internal validity of a study, it is not sufficient for achieving external validity and hence the ability to make out of sample predictions (Hotz, G. W. Imbens, and Mortimer, 2005; Allcott, 2015). However, only if the latter condition is met, randomized controlled trials can be considered as the gold standard for drawing inferences about the effect of a policy (Susan Athey and G. W. Imbens, 2017).

Applying randomized field experiments to inform policy makers about the impact of a given program is likely to increase in the future as digitization lowers the cost of implementation and therefore eases its use (S. Athey and G. Imbens, 2017). Nevertheless, in many cases, field experiments remain difficult to implement, for political, or ethical reasons, or because the population of interest is too small (Susan Athey and G. W. Imbens, 2017). A challenging aspect of field experiments is that they often rely on voluntary participation on the side of the subjects of interest or the leaders of the entities at which the experiment is conducted.[1] This self-selection limits the generalizability of the results to the population of interest (Czibor, Jimenez-Gomez, and J. A. List, 2019).[2].

Despite its importance, self-selection into field experiments remains a largely understudied topic.[3] Only a few studies assess the implications of self-selection for field experimental research (e.g., G. Harrison and J. List, 2004; Ludwig, Kling, and Mullainathan, 2011; Belot and James, 2016) and to the best of our knowledge, no study experimentally varies the characteristics of a field experiment in order to assess their relevance for the probability of selection into field experiments. In many situations, alternatives to experimental interventions are available outside the experiment which could lead to both positive and negative self-selection at the same time as the possibility of being part of the control group entails opportunity costs (Belot and James, 2014). For example, potential innovative partners that are willing to test new programs might be more open to participate in the experiment, but at the same time, they are also more likely to already have participated

[1] Ethics committees often insist on exit clauses and voluntary participation. This might be a consequence of review boards often being developed and installed for life-sciences that tend to ask different questions than social-scientists (Humphreys, 2019, accessed October 16, 2019)

[2] According to Czibor, Jimenez-Gomez, and J. A. List, 2019, there are four threats to the generalizability of (field) experimental results: characteristics of experiment, selective noncompliance, non-random selection, and different populations

[3] Increased interest in recent years has led to the development of tools to foster the trust in generalizability of education field experiment. For example the website *The Generalizer* https://www.bethtipton.com/generalizations helps designing sample recruitment plans for school studies in the US.

in other effective programs lowering their willingness to participate in an additional program (Allcott, 2015). As Belot and James (2016) notice, most (field) experimental studies provide little or no information on how participants were recruited and the experimental sample is not compared to the broader population of interest.[4] So far, there is ample research analyzing non-random participation in laboratory (e.g., Slonim et al., 2013; Krawczyk, 2011; Abeler and Nosenzo, 2015; Charness, Gneezy, and Kuhn, 2013; Cleave, Nikiforakis, and Slonim, 2013; Anderson et al., 2013; Falk, Meier, and Zehnder, 2013; Benndorf, Möllers, and Normann, 2017; Lazear, Malmendier, and Weber, 2012) and artefactual field experiments (e.g., G. W. Harrison, Lau, and Rutstr-m, 2009; Frijters, Kong, and Liu, 2015) but whether participants in natural field experiments differ from the general population remains largely unstudied. Moreover, we still know little about site selection bias, this is, whether the probability that a program is adopted or evaluated is correlated with its impacts (Allcott, 2015). Two notable exceptions are the studies by Allcott (2015) and Belot and James (2016). Allcott (2015) tests for site selection bias in the context of the Opower energy conservation programs and Belot and James (2016) analyze the selection of local school authorities into a policy relevant experiment.

The second key question concerns the internal validity of the experiment. Pure randomization, i.e. the allocation of treatment to individuals is left purely to chance, is often perceived as ex-ante fair and hence politically easier to implement (Burtless, 1995). In large samples, pure randomization achieves balanced treatment and control groups, i.e. groups will have, on average, identical characteristics. However, the probability of differences in characteristics is increasing in small-scale experiments or experiments in which treatment assignment is implemented at a superordinate level, e.g. at school-level instead of student-level. Randomization methods trying to avoid imbalance on characteristics in small-scale experiments are e.g., re-randomization, stratification, or pair-wise matching. The question is whether these methods achieve to create sufficient overlap to consistently estimate the average treatment effect, as limited overlap may lead to distorted confidence intervals (Rothe, 2017).

Survey results indicate that even among experts there is no agreement on which method to use for treatment assignment to achieve balance and precision of estimations (Bruhn and McKenzie, 2009). In simulation studies, Bruhn and McKenzie (2009) show that the method of randomization does not seem to matter in large samples (300 units) but that pair-wise matching outperforms stratification and re-randomization (and pure randomization) in achieving balance in covariates in smaller samples. Interestingly, while Bruhn and McKenzie (2009) ask which method for treatment assignment to use, Kasy (2016) argues that any randomization is not optimal for treatment assignment. Instead, he proposes a new assignment method which involves minimizing the expected mean squared error (MSE) of a point estimator. Schneider and Schlather (2017) interpret this method as a re-randomization method, simplify it considerably and extend it to allow for assigning multiple treatment arms. So far, the different treatment assignment methods have been compared in

[4] Belot and James (2016) focus on experiments in the fields of policy evaluation, personnel and development economics in the Top-5 journals and in the American Economic Journal: Applied Economics.

simulation studies and with binary treatments only but have not been compared with real field experimental data and in settings with more than one treatment arm.

This paper contributes to two important questions addressing external and internal validity: (i) can we detect self-selection of institutional stakeholders into field experiments?, and (ii) does the method of treatment assignment—pure randomization, re-randomization, pair-wise matching, min MSE method—matter for balancing covariates and increasing the precision and power of detecting treatment effects? We answer these questions by conducting a recruitment experiment in which we ask institutional stakeholder, i.e. headmasters, about their willingness to participate in a field study. The recruitment experiment was conducted in Germany's most populous state North Rhine-Westphalia (NRW) with more than 3,000 elementary and secondary schools. We sent personalized emails directly to headmasters who could respond to our email by choosing one of three links: "strong interest" (headmasters are willing to participate), "light interest" (research topic is interesting but headmaster wants to be contacted again later), and "opt-out" (headmasters do not want to be contacted by researchers again). Not clicking on any link was recorded as "no response".

John List meint, dass man jedes empirische Papier mit dem Kommentar not external valid zerstören können. Daher geht es darum, dass beste Setting zu finden indem man seine Fragestellung untersuchen möchte. Wir müssen also argumentieren warum NRW das ideale setting ist um self-selection bias zu untersuchen und dass es kein besseres Setting gibt. Hier anführen, dass wir uns self-selection among head-gatekeeper within an entitiy anschauen wollen und nicht die self selection auf einem übergeordneten level. Daher ist NRW ideal.

These response rates allow us to shed light on self-selection into participation by combining them with a rich set of administrative data on school and municipality characteristics. We then estimate whether schools which positively responded (opting-in) differ along these characteristics from schools which did not respond or actively opted out, thereby identifying a potential self-selection bias. We further investigate how to get headmasters' attention. This is—besides the issue of self-selection—a key question when conducting field experiments. We therefore varied the content of the invitation email to headmasters along two dimensions: the main topic of the planned field study, and the extrinsic incentive to participate. Our invitation emails either invited headmasters to participate in a survey on the collaboration between schools and academia, or invited them to participate in a larger field experiment. Invitation emails to participate in the field experiment highlighted three different research topics: (i) e-learning, (ii) parental involvement, or (iii) integration of migrant children. The survey treatment serves as our control group as it measures headmasters' willingness to invest a minimum amount effort to respond to our email. Varying the topic of the planned field experiment and comparing response rates to the control group allows to identify whether we could attract headmasters' attention in this manner. Moreover, we analyze whether financial rewards for participation can attract headmasters' attention by varying whether schools were eligible to receive an extrinsic incentive for participation or not.

To answer the question which treatment assignment increases balance of covariates and precision of estimates, we randomized schools—before contacting them via email—into treatment arms using pure randomization, matching, re-randomization, or the min MSE treatment assignment method by Schneider and Schlather (2017). Pure randomization assigned schools to the control group or one of the treatments by pure chance, matching—due to its nature—was used in a sub-sample for assigning two treatments only, as it tries to find pairs of units that are comparable, and then one is randomly assigned to the treatment and one to the control group. Re-randomization in general means random assignment until a criterion of balancedness, sometimes subjective judgment of the researcher, is met. The basic idea of the min MSE method as interpreted by Schneider and Schlather (2017) is to re-randomize treatment status a given number of times, but each time only for some randomly selected units, and only if it improves the theoretical criteria of balancedness, namely the mean squared error of the treatment effect as a function of covariates, the new assignment is kept and used as basis for the next iteration. Roughly speaking, the criteria maximizes covariate variances in all treatment groups, thereby achieving balancedness.

We find no evidence for non-random selection of schools. Schools which responded positively to our email do not significantly differ with respect to observable characteristics from schools which did not respond or actively opted out. However, we find that the topic of the experiment matters to attract headmasters' attention. Schools which were invited to participate in an experiment on parental involvement or on the integration of migrant children were more likely to positively respond than schools in the control group. Interestingly, inviting schools to participate in an experiment on e-learning and offering financial extrinsic incentives had no positive effect on headmasters' attention. Furthermore, the min MSE treatment assignment method is superior to pure randomization, matching, and re-randomization in increasing (i) pre-treatment balancedness of observable characteristics, (ii) the precision of treatment effect estimates (reduces bias by 33%), and (iii) power in detecting treatment effects (lower p-values).

# 2 External validity and site selection

Our model draws on the models in Allcott (2015) and Belot and James (2014) with the main difference being that we do not assume that the decision maker knows about the experimental treatment and hence decides upon the perceived effectiveness of the treatment, but rather that the decision maker knows about the topic of the study, and assesses its usefulness on the topic. As Belot and James (2014) already recognize, the knowledge of the treatment itself allows for positive selection, i.e. selecting out of potential participants that expect the treatment intervention to be more effectively implemented outside the experiment and without running the risk of being put in the control group. In our setup we avoid this problem by not distributing information on the new technology under study, this also allows for a simplification of the decision problem and focus on a different aspect of selection: the selection based on deontological motives.

We define $T_i \in \{1, 0\}$ as the treatment indicator for individual $i$ with potential outcomes $Y_i(1)$ $(Y_i(0))$ when (not) treated. $i$'s difference in potential outcomes is $\tau_i = Y_i(1) - Y_i(0)$ and $X_i$ is a vector of covariates and $X$ constitutes the support of the covariates. The target population is the population for which we would like to estimate the *Average Treatment Effect* (ATE). The sample population is the population which was exposed to experiment. $D_i \in \{1, 0\}$ indicates if individual $i$ is in the sample. The ATE in the target population can be consistently estimated under the following four assumptions:

Belot and James fangen ihren Theorieteil ähnlich an wie wir und zitieren dort auch Alcott. Sie führen allerdings auch noch eine 5. Assumption auf "No unit selection bias over a large number of units". Wir sollten schauen wie wir uns von Belot in der Theorie unterscheiden und sie ggf hier zitieren, sie werden ja sehr wahrscheinlich unsere Referees werden. Belot und James haben bei ihrer Field experiment ankündigung die treatments schon drinnen. "have a prior belief about the effectiveness of the treatment, which we denote by $tau_i^*$".

**Assumption 1** Unconfoundedness. $T_i \perp (Y_i(1) > Y_i(0))|X_i$

**Assumption 2** Overlap. $0 < Pr(T_i = 1|X_i = x) < 1$

**Assumption 3** External unconfoundedness. $D_i \perp (Y_i(1) - Y_i(0))|X_i$

**Assumption 4** External overlap. $0 < Pr(D_i = 1|X_i = x) < 1$ for all $x \in X$

Suppose the headmaster (decision maker) considers replying to a call and participates in a study $s$, $(D \in 0, 1)$. We assume that she will either apply cost benefit calculation of the value of the study for the school or use deontological reasoning to reject or accept participation. Let this value depend on the school characteristic $x$ and let the cost of participation be fixed at $c$ and some unobserved headmaster type that can either be utilitarian or deontological $w \in \{ut, de\}$. We assume that the value $b(x)$ depends monotonically and continuously increasing on the (uni-dimensional) school characteristic $x$ and the cost of participation. Let this value be given by:

$$V(D, x, y) = (b(x) - c)D \quad \text{for decision maker using cost benefit arguments} \tag{1}$$

A decision maker using cost-benefit analysis will decide to participate as long as the cost is smaller than the benefit. Therefore, there exists a unique (intermediate value theorem) cutoff $\bar{x}$ above which the headmaster will decide to participate. A decision maker using deontological reasoning—*de*—will participate if the study is legitimate according to her world view. Offering a bounty that reduced costs of participation will not be effective in these cases. We can further refine the notion of legitimacy. Legitimacy can either be denied upon a (i) general rejection of school evaluations (ii) a rejection of the objective of the study. While the latter point can be addressed by framing (as in the experiment), the first point will always lead to non-participation of the decision maker.

**Consequences for participation** For the *ut*-types of decision maker, we obtain a clear selection pattern. Schools with characteristics below the cutoff $\bar{x}$ will never participate in the study leading to selection on observables $X$, violating the external overlap Assumption 4. If the characteristics of interest $X$ are statistically independent of the deontological type of the decision maker, poses not further selection issues.

**Relationship of deontological reasoning and cost-benefit analysis** Is this assumption justified?

# 3 Experimental setup and treatment assignment

The recruitment experiment was conducted in North Rhine-Westphalia (henceforth NRW)—Germany's most populous state—from October 2016 to January 2017. We contacted schools that were included in the official school list of the Ministry of Education in NRW as of March 2016. To reduce headmasters' costs of responding to our inquiry, all contact with the schools was electronically by email asking headmasters on participating in a scientific study. Moreover, we learned in previous studies that the responsiveness of schools in NRW does not depend on whether we send a posted letter or email (see Panel B of Table 2). Recruitment emails were sent out October 2, 2016 and for those schools who did not respond—neither positively nor negatively—we sent out two reminder emails.[5] The first reminder email was sent one month after the first contact, on November 2, 2016. The second three weeks after the first. This reminder was already announced in the first invitation email in order to induce schools to give feedback and in order to achieve an active opting-out measure. We added another warning for a reminder that we will contact them again in three weeks if they have not responded by the deadline. The last short reminder was sent three weeks later. This last reminder made the answering option (opt-in, opt-out) showing up prominently at the beginning of the email, again to increase the feedback rate.

We contacted all (elementary and secondary) schools in NRW that fulfilled our basic requirements. Our three exclusion criteria were: (a) schools with a medical focus, (b) schools that mainly teach adults in second-chance-education or evening schools, and (c) schools in the largest cities of NRW. We excluded (a) and (b) schools types as not each research topic is relevant for them, e.g, we should expect no positive responses of schools teaching adults for the research topic "parental involvement". Schools in larger cities (type (c)) were excluded for two reasons: First, schools in metropolitan areas are over-researched and receive many inquiries, e.g., by bachelors and master students. Second, we were concerned about reputation effects and ongoing partnerships in schools in larger cities. We previously conducted several other experiments in schools in larger NRW-cities (Riener and Wagner, 2019; Fischer and Wagner, 2018; Wagner, 2016) which might cause a positive or negative reputation effect for participation in an additional study. Moreover, schools with already existing partners and ongoing programs might or might not be more likely to participate (Allcott, 2015). In total 3305 schools were contacted which represents 66.29% of all schools in NRW.

wir sollen diskutieren, ob wir diesen ersten Punkt behalten wollen. Man könnte auch argumentieren, dass es gerade weil sie over-researched sind es interessant ist sich anzuschauen, wer auf neue Anfragen antwortet. GR: Da wir nicht sagen können wie oft schulen an studien teilgenommen haben könnten wir sagen, dass diese overresearchedness mehr noise in die daten bringt? auch kein top argument, aber ich denke wir brauchen was hier

*The recruitment email:* Headmasters were asked to express their interest participating in a scientific study using a standardized email message. This message introduced our selves and our expertise in conducting scientific studies in schools, mentioned the respective research question, briefly explained the methodology we will use, and outlined the workload for the schools. We kept the information in the email intentionally very short to increase the likelihood of headmasters reading the message. However, headmasters could access more

[5] First wave October 2, 2016. We sent emails in batches of 50 per 2 hour interval on mailing day using the internal LimeSurvey procedure to handle invitations to surveys. In total, we had only about 3% of emails that bounced.

information on the project—scientific foundation for the research question, timeline of the study, exclusion criteria for participation, and information about data protection—by clicking on a link provided in the email (see the online Appendix for a facsimile of the recruitment email). Moreover, to measure schools responsiveness, headmasters could indicate their interest in participating by clicking one of three links displayed at the bottom of the recruitment email. Clicking the first link, headmasters could show strong interest in the project and were told that they will be contacted again with a detailed plan of the experiment. Choosing the second link, a school could indicate that they are generally interested in the topic but at the time see no capacity to participate. The third link was an opt-out link where the schools could opt-out of participating and receiving further reminders. We communicated that if they do not actively opt-out, they will continue to receive reminders in their mailbox. After clicking on one of the three links, schools were forwarded to a questionnaire asking for further details of the school (e.g., what the position of the respondent is within the school).

## 3.1 Treatments

We implemented a 3×2 design, in which both the research topic of the collaborative project (E-learning vs. parental involvement vs. integration of migrant children) and the provision of incentives (no incentive vs. monetary incentive) were varied independently. All collaborative projects were presented in the same way and were equally long. The treatment variation was the first and last paragraph of the email, announcing that we plan to conduct an experiment about the respective research topic within the school. Whether schools could get a monetary incentive for participation was altered in the fourth paragraph of the email. Moreover, we administered an additional treatment—the control treatment—where we simply asked the headmasters to fill out a questionnaire online. The rationale for this treatment is to lower the bar for participation substantially and construct a benchmark of schools that are willing to contribute to scientific studies, but for whatever reason do not want to participate in an experiment.

**Baseline treatment** In the baseline treatment, we asked schools to participate in an online survey. In this survey, we asked about headmasters' point of view regarding the collaboration between academia and schools, this is, how insight gained in academic research can be integrated in schools' daily life. Importantly, answering the survey did not involve participation in any experimental study and it required a minimum of headmasters' time—approx. five minutes. Due to the low stakes of the survey and the time frame, we interpret the responsiveness in the survey schools' baseline responsiveness in dealing with inquiries of academic researchers.

**E-learning** In this treatment we asked about participating in a study on the use of electronic devices in education, in particular which types of electronic testing formats could be implemented in schools and how they perform compared to traditional pen and paper exams. This treatment was motivated by a recent move of the German government to increase spending towards research on digital media in the classroom.[6]

**Parental involvement** Here, we asked schools whether they would like to participate in a study aiming at analyzing the effect of getting parents involved in their children's education (e.g. students in-class behavior and academic performance). This treatment was motivated by recent academic research using electronic devices (e.g. text messaging) reduce information frictions between parents and children (e.g. Bergman and Chan, 2019; Kraft and Rogers, 2015). These studies show that active participation of parents in their children's education can lead to favorable educational and behavioral outcomes.

**Integration of migrant children** In this treatment we asked schools to participate in a study to analyze how students with a migration background and language difficulties could be effectively integrated into classroom education. This topic was inspired by the increasing migration to Germany from 2015/16 which was covered widely in the media and constituted a major challenge for schools to rapidly integrate children without German as their first language into the school environment.

**Monetary incentive** Besides varying the research topic, we also altered whether we offered schools a financial incentive or not. Two schools could win a 700 Euro budget in the case of participating. This money could be used for school internal projects, such as continued education for teachers or study material. The motivation for this treatment was to shed light on whether financial incentives have the power to attract gatekeepers' attention and to increase their willingness to respond to the email. In order to evaluate the size of our financial incentive, we show the share of the incentive in terms of the yearly budget for the training of teachers in Table 9 (schools get a yearly budget of 45 Euros per full-time employed teacher). It turns out that for more than 70 percent of the schools our incentive constitutes a share of 80 to 90 percent of the yearly budget. Clearly in terms of expected value this financial incentive is rather low (1̃1,76 Euros if we consider all schools who responded positively). However, headmasters did not know how many schools we contacted or how many schools responded positively. Hence, they could only form a belief about the expected value of the financial incentives. Based on our experience wit headmasters, we are confident that they did not perform the calculation to derive a belief about the expected value but that the absolute value of 700 Euros was salient to them (if at all they might have compared the financial incentive to their yearly budget). Although, we are confident that headmasters did not think of the financial incentive in expected terms, we cannot proof it.

The design of the treatments are based on an experiment we planned to implement in schools after the summer break in 2017. However, due to programming and funding problems, we were not able to finally conduct the experiment after our initial contact end of 2016.[7] Thus, the recruitment experiment presented in this paper does not involve deception as we initially planned to implement the announced experiment after

[6] http://www.bildung-forschung.digital/

[7] Potential (educational) partners need to be contacted at an early stage of the project as it takes some time to get them on board.

having contacted schools. The goal of the planned experiment in 2017 was to randomly provide schools with a digital class-book and to analyze its impact on students' in-class behavior and educational attainment. Features of the electronic class-book should encompass a communication tool for teachers to send private or public notifications to students and/or parents. Moreover, we planned to incorporate an in-build translator within the communication tool to reduce language barriers and better integrate migrant children and their parents. Another feature we intended to implement within the electronic class-book was an "exercise and testing tool" in which teachers could send students personalized exercises, remind them about assignments, e.g, homework, and to test students online.

## 3.2 An Experiment on Balance, Precision and the Role of Treatment Assignment Methods

We empirically want to assess the degree to which balance in observable characteristics matters for precision of the estimation of treatment effects in a real-world setting.[8] To this end, we added another experimental layer to our research design. We conducted an experiment within the experiment outlined above to empirically study the relation between balance and precision in a real-world setting using real treatment effects.

parallel to anstatt within? finde within ein wenig misleading

Taking advantage of our unique setting, we divided the whole sample of more than 3000 schools into smaller, comparable subsamples, and use different treatment assignment mechanisms to vary the degree of balance (of covariates) in these subsamples. We then assess the precision of estimation in the subsamples and relate it to pre-treatment balance. The design of our experiment allows us to compare commonly used treatment assignment methods as a side product — in particular in settings with one and multiple treatments and multiple (possibly continuous) covariates. We compared a new method, the minMSE method, to re-randomization as popularized by Bruhn and McKenzie (2009), as well as to pure randomization and where possible—in the case of a binary treatment—to pair-wise matching.

**Treatment Assignment Mechanisms and Considered Settings** In all considered settings of this added experimental layer we account for the nature of the actually available pre-treatment information. That is: At least some pre-treatment variables are continuous, there are more than three variables available and all of them might be relevant for the outcome measured. Lastly, no exact split is needed, such as a sharp 1:1 division of, e.g., both females and males in treatment and control group. Therefore, in the overall setting of our experiment, treatment assignment using stratification is either impossible or not needed, thus including it would at best yield blurred results. However, we acknowledge that in simple settings or when an exact split is important, stratification or a combination with the studied treatment assignment mechanisms might be the best solution.

[8] Theoretically, Rothe (2017) shows that limited overlap—a certain notion of imbalancedness—may lead to distorted confidence intervals and Greevy et al. (2004) shows that balance in observable characteristics indeed leads to a higher precision of estimation.

The settings that we consider differ in the amount of treatment groups that are to be formed. The simplest setting considers the case of binary treatment, and we increase the number of groups to up to seven.

Using simulations in the stylized setting of a binary treatment, where several continuous pre-treatment characteristics are to be balanced, Bruhn and McKenzie (2009) show that pair-wise matching outperforms stratification and the re-randomization approaches considered. Thus, in this setting (binary treatment, multiple continuous covariates), we compare the pair-wise matching approach with the new minMSE approach. To mimick a standard use-case for the matching approach to treatment assignment we use a quite small sample that has to be divided into a treatment and a control group.

According to Schneider and Schlather (2017), the minMSE method is particularly appealing in cases where attrition might be a concern. Examples include repeated measurements at schools where due to illness of participants, 10% of the sample can be expected to be absent or when randomization is performed at the cluster level. In those cases, dropping a non-missing observation from the sample because of its missing pair might limit the power of the study below a critical threshold. The minMSE approach, however, does not require this step.

For settings with more than one treatment group, we are not aware of any simulation results. Moreover, as far as we know, to date, there is no readily implementable, theoretically founded standard approach to allocate units to more than two groups using matching or any alternative treatment allocation method. Therefore, we limit ourselves to the described case.

The re-randomization approach is possibly mostly used for moderate group sizes and—depending on the exact approach chosen—one or more treatment groups. The Min-Max-t-stat-method, popularized by Bruhn and McKenzie, 2009, is easily extendable to multiple treatment arms, and we therefore chose this method to compare it to the minMSE method when 7 experimental groups are desired with a group size of 30; yielding a total sample size of 210 units.

The new method, the min MSE method, builds on earlier work by Kasy (2016), in particular the introduced statistic of balancedness. However, compared to the approach by Kasy (2016), it requires less technical parameters by building on a simpler theoretical framework, and it still involves randomization to a certain degree. Thus, it can be interpreted as a re-randomization approach and traditional randomization inference can be applied.

Do we have to be more concrete how we differ here

We therefore compared the matching approach to the min MSE method mimicking this very setting with two desired experimental groups, and a target group size of 15 units each; thus a sample of 30 units has to be allocated to two experimental groups.

**Implementation of treatment assignment approaches** To this end, we draw sub-samples of our total sample consisting of 3575 schools.[9] We randomly draw 12 sub-samples consisting of different numbers of schools, and for those sub-samples we draw—without repetition from the whole remaining sample of

The missing schools were used in the pilot? Check the numbers, above we talk about 3305 schools

[9] We had to drop 20 schools for not having any pupils.

schools—groups of equal sizes that were comparable to the ones randomly drawn. [10] Balance was checked with the omnibus test of equivalence between groups introduced by Hansen and Bowers (2008). The p-value for the null hypothesis of equivalence ranged from xx to xx and was only in one sub-sample smaller than .05.

Get those numbers, doublecheck

In this way, we obtained 24 sub-samples consisting of 12 pairs of pair-wise comparable sub-samples. Of each pair, we randomly allocated one sub-sample to the minMSE approach, and the other sub-sample to a comparison method. Then, within sub-samples, treatment allocation was performed by the assigned method for up to 7 experimental groups, which is the maximum number of groups needed (see Section 3.1 on treatments). This design is illustrated in Table 11.

In order to study how "balancedness" translates into increased precision in a real-world setting with real treatment effects that are unknown at the time of treatment assignment, we compare pure randomization and the minMSE approach, as on average for a limited number of draws, we expect the minMSE approach to perform considerably better in terms of balancing covariates, and in particular producing less cases of extreme imbalance.

Define how we measure balancedness

We compare the "balancedness" of formed experimental groups using the two approaches: pure randomization and the minMSE method, for creating two to seven experimental groups, each group consisting of 20 observations.

After having allocated the schools in 12 subgroups (matching vs. minMSE sub-sample, rerandomization vs. minMSE sub-sample and ten randomization vs. minMSE sub-samples) to experimental groups, around one third of the sample was not yet assigned to an experimental group. Taking into account the treatment assignments already made, using the minMSE method, we allocated those remaining schools to the control and the treatment groups, with the restriction to have the group sizes as equal as possible and the goal to achieve overall balance across treatments in the whole sample. The resulting assignment to experimental groups is balanced as assessed with the omnibus test by Hansen and Bowers, 2008: the p-value when testing the null hypothesis of balanced groups is above 0.66.

**Implementation of the Treatment Assignment Methods** CITE R package Rerandomization: Brun and McKenzie Code

**Measures of Balance and Precision** The measures are...

Balance.

One measure of precision of estimation is the bias of the estimation. This notion of precision we will label the precision of the experiment. In this sense, an estimation is precise if it is close to the true value that would

[10] Comparability of the groups of schools—or balancedness among the covariates or observables of the groups—was achieved with an algorithm using the same decision criteria as the minMSE approach.

be obtained by measuring the effect with the whole population or by repeating the experiment sufficiently often with different sub-samples, thereby averaging out any influence that is not due to the treatment. Taking advantage of the fact that the schools in our experiment actually constitute the whole population of schools in North-Rhine Westphalia, we can compute the bias of every estimation. We first compute the treatment effects for the whole population. Then we estimate the effects for the sub-samples. Finally, we compute the bias for every sub-sample, i.e. the deviation between the estimated effect for the sub-sample and the effect for the whole population of schools.

The second measure of precision of estimation that we apply is linked to statistical significance. [11] Our measure of power consists of higher or lower p-values of the treatment effect estimations. We thus assess the increase in power in our setting that can be attributed to a treatment assignment mechanism — if it comes with an increase in the precision of the experiment; otherwise, we get “more significant”, but less correct results.

# 4 Results

The result section is organized in the following way. We first describe our data and present descriptive statistics. Second, we compare the minMSE treatment assignment method to alternative randomization techniques with respect to pretreatment blance of covariates (overlap condition and Omnibus test of imbalance), precision of the estimator (bias and p-values of treatment effect coefficient), and investigate how balancedness of covariates affects precision. Thereafter, we analyze whether a self-selection bias into participation exists and how to attract gatekeepers attention. Finally, we present survey data on gatekeepers perceived usefulness of academic research.

## 4.1 Data and Descriptive Statistics

We obtained observable characteristics at municipality and school level which stem from official statistics. Municipality level data were publicly available from the German statistical offices and school level data were purchased from the statistical office of NRW. These data include—at the municipality level—inter alia the number of inhabitants, unemployment rate, election results, land prices, composition of the workforce, and the social index of the municipality. Data at school level comprise inter alia the school type, number of students, average age of teachers, compulsory teaching hours of teachers, and migration background information of students and their parents. A complete list and detailed description of the background characteristics can be found in Appendix E.

We first take a look at response rates of the recruitment experiment. Panel A in Table 1 summarizes

[11] Although we believe that the precision of the experiment should always be considered first, because a wrong estimation that is significantly estimated might even be dangerous, we acknowledge that for many researchers, the statistical interpretation is of great importance aswell.

whether schools did not respond, actively opted out, showed "light" interest, this is, clicking on the link indicating to be contacted again later, or responded positively ("strong" interest). We observe that most schools did not respond to our inquiry, ranging from 71.72% in the (pooled) parental involvement treatment to 78.22% in the pooled E-learning treatment. Active opting-out is highest in our baseline treatment (20.61%) and lowest in the E-learning treatment (13.51%). Positive response rates are lowest in the integration of migrant children treatment (3.66%) and highest in the baseline treatment (6.33%), which might be due to the fact that schools simply had to answer a questionnaire without the commitment to participate in an experiment.

We can now compare these response rates to the response rates observed in Riener and Wagner (2019) and Fischer and Wagner (2018), and Wagner (2016)—studies conducted in NRW—to analyze whether the no response rates in this experiment are unusually high or low. In Panel A of Table 2, we observe that the no response rates in secondary schools vary from 67.26% in Riener and Wagner (2019) to 76.92% in Fischer and Wagner (2018) and the no response rate of the recruitment experiment lying in between (72.40%). With respect to elementary schools, we observe that the no response rates are higher in Wagner (2016) (86.12%) compared to this study (76.77%). A Fisher's exact test for differences in response rates yields that the no response rate in Wagner (2016) is significantly higher than the no response rate in this study. A notable difference between this study and the three other experiments is that response rates of the other studies stem from schools in larger cities and stakes of the experiments vary from very low in this study to high in Fischer and Wagner (2018). Given these differences, response rates are in a considerable range. A natural question to ask is whether responsiveness of headmasters depends on the contact channel—email or posted letter. Using data of Riener and Wagner (2019) we can shed light on this issue as the authors varied whether they contacted schools via a posted letter, an email, or a combination of both. In Panel B of Table 2, we observe similar response rates for all three contact types ranging from 64.91% in the *Letter* and 68.42% in the *Email* treatment. A Fisher's exact test for differences in response rates indeed indicates that the small differences in response rates by contact type are not statistically significant.

*Who responded to the recruitment email?* As we sent our inquiry to the schools' official email address, it is interesting to know who responded, this is, what position the respondent has within the school. Schools which clicked on one of the three links in our recruitment email were guided to a questionnaire. In this questionnaire, we asked about the position of the respondent within the school. 840 schools responded to our recruitment email and 188 (∼22%) also answered the respective questionnaire. As can be seen in Panel B of Table 1, headmasters answer inquiries most of the time (73.40%) followed by the dean of students (12.23%).

Tables 4 and 3 present descriptive statistics on background characteristics of schools and municipalities we will later use in our analyzes. Columns (1)-(3) show means of the three treatments (e-learning, parental involvement, and integration of migrant children), column (4) describes our control group (scientific contribution), and column (5) pools columns (1)-(4). Testing for differences between treatments and control group characteristics, the age of teachers in the E-Learning treatment is marginally significantly lower from the age of teachers in the control group (p = 0.064). With respect to municipality characteristics, the election outcome for the christian democratic union (CDU) in the E-Learning and Parental involvement treatment are marginally significantly different from the election outcomes in the control group (p = 0.061 and p = 0.093). Overall, we observe that differences between treatments and the control group are small for school nor municipality characteristics and therefore economically insignificant. Hence, it seems that our randomization procedure was successful.

## 4.2 From Balance to Precision and the Role of Treatment Assignment Methods

In this section, we first discuss the results on pre-treatment balancedness of covariates with respect to two criteria, overlap and a test to detect imbalance due to Hansen and Bowers (2008). Then, we assess the differences in precision of estimation of treatment effects due to the different considered treatment mechanisms. As measures of precision, we use the bias of an estimation and the p-value of the coefficient of the treatment effect resulting from an estimation of the latter. Lastly, we present results on the link between balance and precision on the internal margin of balance, relating the degree of balance with the degree of precision.

### 4.2.1 Balance

**Overlap** Figure 1 shows the comparison of purely random treatment assignment and minMSE treatment assignment with respect to balance as measured by overlap (see Assumptions 2 and 4 in Section 2). For this measure, we consider five of the variables used for treatment assignment: all categorical information about schools (type of school, authority type, gender of the headmaster, municipality ID), plus a discretized version of the number of pupils using three equally populated bins. The remaining data are constant across municipalities, and the municipality ID is already included in the variables considered. Therefore, those data are excluded as they would distort the result. Yet, this means that the balance described here is an upper limit of what we would see if we included all variables considered for treatment assignment, and the difference in balance might be interpreted as a lower limit.

We consider the overlap condition as fulfilled for a level of a variable (say "female" of the variable "gender"), if this characteristic is represented in all possible groups. In Draw three, there are seven groups to be formed, whereas in Draw 12, the characteristic is to be distributed and thus to be found in only two groups (see Table 11 for details). In some cases, there are more groups to be formed than a certain characteristic is represented in the respective sample. In these cases, we consider the overlap condition as fulfilled if the characteristic is found in the maximal possible number of groups.

Figure 1a compares how often the overlap condition is fulfilled in the samples where treatment assignment was performed either completely at random or with the minMSE method. Considering all draws, variables and characteristics, the overlap condition is fulfilled in 60% of all cases when assigning treatment purely random, and in 71% of the cases when relying on the minMSE method. The difference in fulfilling the overlap condition between the two treatment assignment methods is significant (Chi-squared test, p-value $< 0.001$).

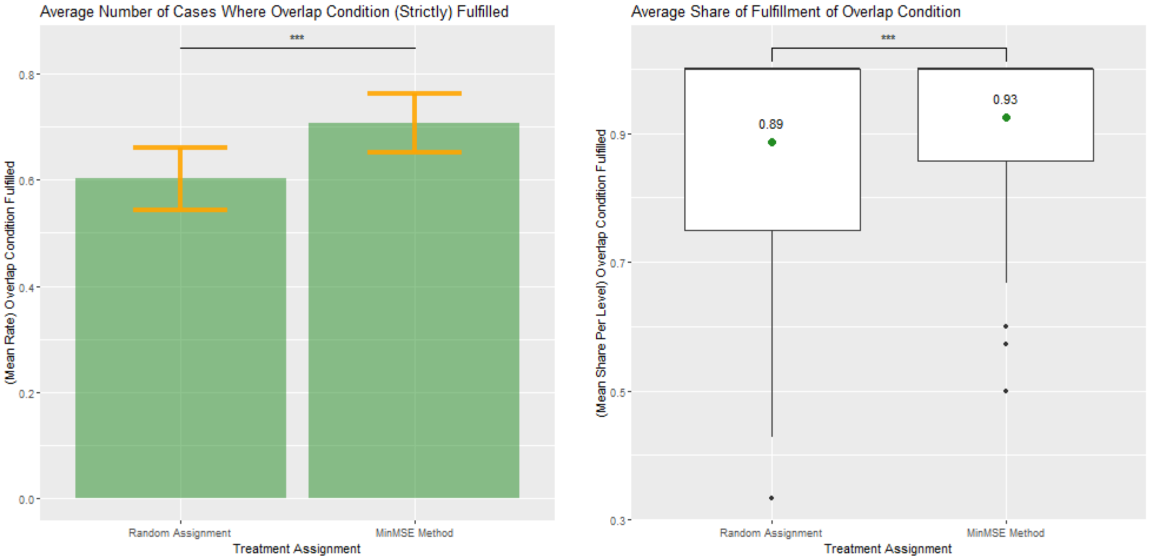
Figure 1b compares the average share of fulfillment of the overlap condition. Consider the case of the variable gender. If males were assigned to three of the six groups, but not to the three others, although in the total sample more than six males were present, the share of fulfillment of the overlap condition would be .5 for this variable and this characteristic. For every combination of draw, variable and characteristic of a variable, we obtain one share of fulfillment.

Figure 1b shows that, on average, both treatment assignment approaches perform relatively similar. However, given that the maximum share is 1, the difference is bigger than it seems. Yet, the more striking result is the difference in variance of this share. The 25% quantile (minimum) of the distribution of the share of fulfillment of the overlap condition resulting from the minMSE method is .86 (.5) compared to .75 (.33) when relying on purely random assignment. In that sense, proper treatment assignment may be understood as an "insurance" against adverse "draws" in which balance is really not that good, as indicated by the minimum shares of two methods.

Figure 1: Comparison of Pretreatment Balance: Overlap Condition

(a) Average Number of Cases Where Overlap Condition Fulfilled

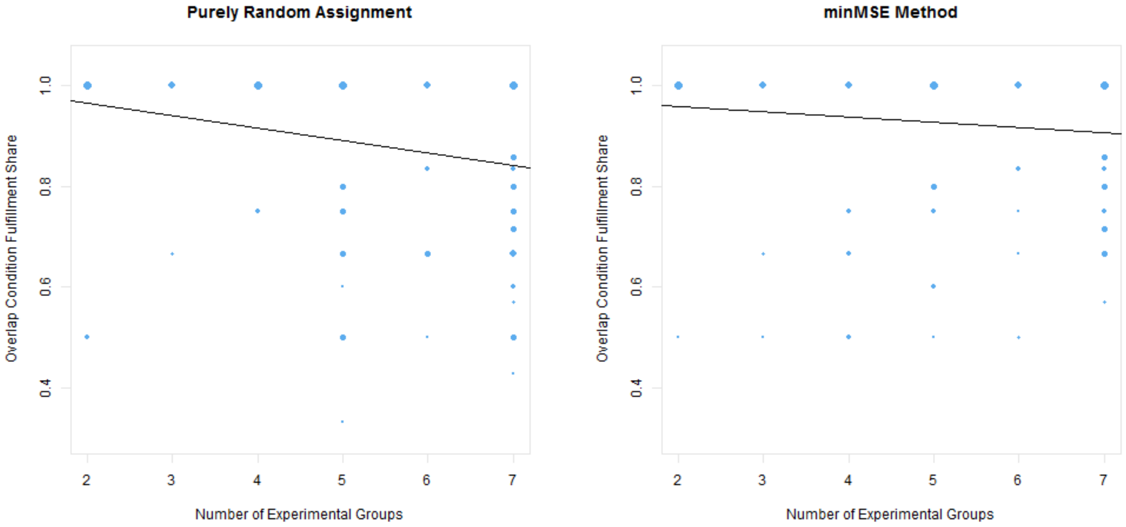
(b) Average Share of Fulfillment of Overlap Condition



*Note: The first graph (Figure 1a) compares the average number of cases, in which the overlap condition is fulfilled. Generally, the overlap condition is considered as fulfilled, if a characteristic of a variable is found in all treatment groups. *** denotes significance of a chi-squared test at the 1% level. The second graph (Figure 1b) compares the average ratio of treatment groups, in which a characteristic is found, to the total number of treatment groups to be assigned in a draw (in the general case). *** denotes significance of a rank sum test at the 1% level.*

As expected, the success rate is higher for the draws with less groups to be assigned. However, there is a significant difference (as indicated by an interaction term, p-value $< 0.001$) between the treatment assignment methods in the decay of the success rate for the overlap condition with increasing number of groups to be formed. The decay in balance as measured by the overlap condition is 1% per additional treatment group when using the minMSE method, and nearly 2.5 times as much for assignment of groups purely at random; see Figure 2.

Figure 2: Comparison of Pretreatment Balance: Average Share of Fulfillment of Overlap Condition with Increasing Number of Experimental Groups



*Note: These graphs present the decay of balance of pre-treatment characteristics as the number of treatment groups to be formed increases for the two treatment assignment approaches considered. Here, balance is measured by the overlap condition (see Assumptions 2 and 4 in Section 2). The difference in slopes (decay) (about -2.5 for purely random assignment vs. -1 for the minMSE Method) is significant at the 1% level.*

**Omnibus Test of Imbalance (Hansen and Bowers, 2008)** Based on the omnibus test of imbalance due to Hansen and Bowers (2008), our second measure of pretreatment balance considers all variables used for treatment assignment. It bases on a statistic that accounts for correlation between the specified variables, thus “corrects” for comparison of multiple variables accross control and treatment group and summarizes all differences in one single statistic that approximately follows a chi-squared distribution.

We run the test for every combination of treatment and control group possible in a draw. Table 12 summarizes these results by reporting the minimal p-value of any comparison between control and treatment group in a draw. Note that low p-values imply low balance, whereas higher p-values indicate better balance. In none of the groups, the null hypothesis of balance is rejected at conventional significance levels. A Wilcoxon rank-sum test statistically confirms the obvious observation that minimal p-values are smaller for the minMSE method – even when including pair-wise matching and rerandomization as comparison methods. The null hypothesis of equality of balance (as measured by p-values) is rejected with a p-value $< 0.01$.[12]

[12] This finding is robust at least at the same significance level to not aggregating the p-values over the treatment groups of a

The relation between balance based on the omnibus test by Hansen and Bowers (2008) and the number of groups to be formed is the same as when measuring balance with the overlap condition: Balance decreases with increasing number of groups to be assigned. The pearson correlation coefficient between number of groups and the minimal p-value is $\rho = -.91$ (p-value $< 0.001$) when pooling pure randomization, matching and rerandomization, and it is $\rho = -.49$ and non significant for the minMSE method.

**Result 1.** ***Balance:*** *Pretreatment balance between control and treatment groups is significantly higher when groups are assigned using the minMSE method compared to alternatives: purely random treatment assignment, pair-wise matching and min-max t-statisic rerandomization. The degree of balance significantly decreases with the number of experimental groups that are to be assigned when using purely random treatment assignment but not when using the minMSE method. The minMSE method allows to assign about 2.5 more groups with the same decrease in balance as compared to pure randomization.*

#### 4.2.2 Precision

One way to assess precision is the bias, i.e. the difference between the true value and the estimated value. We use the bias as one measure to compare the performance of the different treatment assignment mechanisms with respect to precision. To this end, we consider the three main outcomes in this paper: Response (yes/no), positive response (yes/no) and whether the questionnaire was filled (yes/no) and pool the results. Following Bruhn and McKenzie (2009), we also compare precision as indicated by the p-values of the estimations of the treatment effect.

**Precision: Bias** For every treatment (i.e. every topic with or without incentivization), we can estimate its treatment effect on our main outcomes using the full sample. Assuming this estimation corresponds to the true value, the absolute bias of an estimated treatment effect is simply the absolute difference between this estimated value and an estimation that only uses the observations in a given subsample. We consider all treatment effects resulting from an estimation using the full sample with a statistically significant estimated coefficient at least at the 10% level. Of the estimations using subsamples, we include all estimations, where the p-value of the treatment effect estimation indicates more precision than pure randomness, i.e. where it is below 0.5.

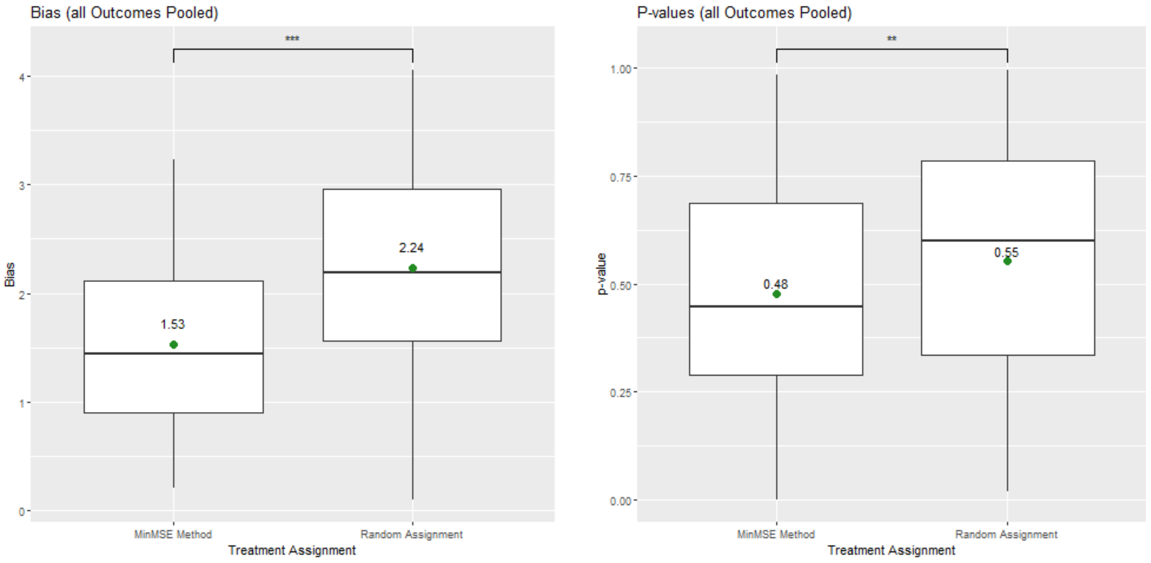
Figure 3a summarizes the result. For comparison, the absolute bias is expressed in standard deviations of the respective treatment effect estimations, i.e. the standard deviations of all estimates of the treatment effect of the respective topic with or without incentivization (e.g. e-learning with incentivization). The (absolute) bias differs significantly between the subsamples in which treatment was assigned purely at random and those in which the minMSE method was used for treatment assignment (Wilcoxon rank sum test, $p < 0.007$). On average, the bias is nearly 1.5 times as large when assigning treatments purely at random compared to when

draw by using the minimal value and to aggregating by taking the mean p-value of the ombnibus test of imbalance instead of the minimal p-value of any comparison between treatment group and control.

using the min MSE method. Moreover, the median bias of estimations in the "purely random assignment" samples (2.2) is larger than the 75% quantile in the "minMSE assignment" samples (2.1; median 1.4).

**Precision: P-Value of Treatment Effect Coefficient** We can measure precision also by the p-value of an estimation. Again, we consider all the estimations using subsamples that estimate treatment effects that were estimated with a statistically significant coefficient at least at the 10% level with the full sample. Figure 3b summarizes the result. The estimations in the samples, where treatment was assigned using the minMSE method, differ significantly in their p-values when estimating significant treatment effects from the estimations in the samples, where treatment groups were assigned purely at random (Wilcoxon rank sum test, $p < 0.031$). The mean (median) p-value of estimations in the "purely random assignment" samples is .55 (.60), where it is .48 (.45) in the "minMSE assignment" samples.

Figure 3: Comparison of Precision: (Absolute) Bias and P-Values of Estimations



(a) Bias in Estimation

(b) P-Values of Estimations

*Note: This graph shows precision in estimation of treatment effects considering three outcomes (response, positive response and whether a survey was filled in) when assigning treatments purely at random compared to using the minMSE method. Figure 3a presents the distribution of (absolute) bias in estimating significant treatment effects. Estimated treatment effects using subsamples are subtracted from the treatment effect estimated using the full population of schools; the absolute value of the difference is the bias shown here, given that the p-value of the estimation in the subsample is below .5 (i.e. more precise than purely random). Figure 3b presents the distribution of p-values of the estimations of the treatment effects using the different subsamples. Stars indicate resuls from a Wilcoxon rank sum test, where ***/** denotes significance at the 1%/5% level.*

**Result 2.** ***Precision:*** *Precision is higher when using the minMSE method. Assigning treatment with the minMSE method reduces the bias in estimation of treatment effects by 33% compared to purely random treatment assignment.*

### 4.2.3 The Relation Between Balance and Precision and the Role of Treatment Assignment

In Section 4.2.1 we have shown that balance is higher when assigning units to treatment groups using the minMSE method as compared to purely random treatment assignment – independently of the measure of balance that we have used. In Section 4.2.2, using two measures of precision, we have shown that also precision of treatment effect estimations is higher when using subsamples in which treatment was assigned with the minMSE method as compared to those estimations based on subsamples where treatment was assigned purely at random. As we have kept everything else as constant as possible between the subgroups in a draw except for the treatment assignment mechanism, which we have shown to affect the balance, this already serves as evidence that balance increases precision considerably in our real world setting – by reducing the bias by 33% on average with the same effect on the median.[13]

This finding is supported by a correlation analysis on the same data: The Pearson correlation coefficient between pre-treatment balance as expressed by p-values corresponding to the omnibus test of (im)balance by Hansen and Bowers (2008) (the higher, the better) and (absolute) bias in treatment effect estimations (the lower, the better) is $\rho = -.42$ (p-value $< 0.001$).

An increase of .1 in the p-value associated with imbalance due to the test by Hansen and Bowers (2008) results in an increase in bias of .25 standard deviations (a ninth of the average bias in the “purely random” samples, or a sixth in the “minMSE” samples). Using aggregated values of the bias at the draw/treatment assignment level,[14] the average share of fulfillment of the overlap condition significantly predicts bias: We find that a 10 percentage point higher fulfillment share of the overlap condition is associated with a more than .4 standard deviations smaller bias.

**Result 3.** ***The Relation between Balance and Precision:*** *The degree of balance increases the degree of precision. The exact relationship depends on the measures used for balance and for precision. A 33% lower bias has been attained due to better balance by using a proper treatment assignment method, the minMSE method, in our experiment. Given that the treatment effects in our setting are independent of the covariates used, this result may likely be a lower limit of what can be expected in different settings.*

## 4.3 Self-selection of gatekeepers into participation?

Table 5 presents our first main results for the selection of schools into participation. We present regressions—coefficients present marginal effects from probit regressions—where the dependent variable is having responded to our request, this is, clicking on one of the three links in the recruitment email indicating interest in participating or actively opting out. Our explanatory variables are presented in two groups: (a) school level variables and (b) municipality level variables. We control for multiple testing using Romano Wolf with 100 repetitions. Pooling all treatments, we do not find school or municipality characteristics which determine

[13] Note that this is likely to be a lower limit, since there is no (significant) interaction between covariates and the treatments; see Section 4.4

[14] We use aggregate values on this level, as the fulfillment share can only be meaningfully measured on the draw level; this is the measure used in Section 4.2.1.

whether a school is more or less likely to respond to our inquiry. When doing a sample split at the treatment, we find that in the e-learning treatment schools that have a higher student's migration background are less likely to participate and for the study on general scientific interest, schools with a higher share of full time employed teachers show a slightly higher responsiveness. These are the only two significant results of the 33 × 5 estimated coefficients. However, although statistically significant, these effect sizes are economically not meaningful.

Table 6 examines school and municipality characteristics that determine positive response of schools. We find a similar picture, this is, while in the pooled regressions we do not find characteristics that determine differences in positively responding, we observe that average compulsory teaching hours in the school are slightly positively related with a positive respond in the e-learning treatment, and for the scientific contribution treatment that schools in municipalities in which smaller parties received a higher vote share are more likely to respond positively. Again, we find no evidence for self-selection into participation—only two results of the 33 × 5 estimated coefficients are significant. One downside of using administrative available school and municipalities characteristics is that they do include measures on headmasters' (unobserved) personal characteristics e.g. headmaster quality or open mindedness which might determine headmasters' reaction to our inquiry. However, headmasters' unobserved characteristics should likely be correlated with some of our observed school and municipality characteristics such as social index of the municipality, school size, land prizes or share of migrant pupils. As we do not observe self-selection on these covariates indicates that there is no self-selection which is determined by headmasters' quality.

Make the grouping a bit better in the tables: For example group together migtration vars, election vars and create visually separate blocks. VW:visually separate blocks could be difficult as tables might become too large

**Result 1–Self-selection:** *We do not find evidence of selection into participation taking place.*

sollen wir die Diskussion zu unobserved headmaster characteristics überhaupt mit einbringen?

## 4.4 Attracting gatekeepers' attention?

We now analyze how to attract headmasters' attention, this is, whether their willingness to respond depends (i) on the proposed research topic the scientific study wants to answer, and (ii) the opportunity to receive an extrinsic financial incentive.

Table 7 presents the effect of highlighting and varying the research topic of the proposed study in the initial email on the headmasters' willingness to positively respond, to give any respond, and to fill out the survey. Columns (1)-(3) report on the pooled effect of receiving any research topic compared to receiving no research topic. Columns (4)-(6) then differentiate by research topic.[15] We find that explicitly stating a research topic in the initial email significantly increases headmasters' willingness to respond positively compared to headmasters who were only asked to participate in a survey. Moreover, we do not find a

[15] Tables 15 and 16 show that results hold up if we calculate bootstrapped standard errors or apply randomization inference.

significant on whether headmasters responded at all (positive responses and negative responses). A positive treatment effect for the number of positive responses and no change in overall responsiveness implies that active opting out decreased. Further, we do not find an effect on the willingness to to fill out the survey. Turning to the proposed research topics, we find that it indeed matters for headmasters' responsiveness which kind of research question the study wants to answer. We find that headmasters increase their willingness to positively respond to our email if we proposed a research topic on getting parents involved or how to better integrate migrant children but no statistical significant effect for the topic e-learning. On the contrary, we find for the e-learning treatment that overall responsiveness significantly decreased and that headmasters were less likely to fill out the survey.

**Result 2–Research topic:** *The topic of the proposed research topic matters to attract headmasters' attention.*

sollen wir hier jetzt noch darüber spekulieren warum e-learning nicht zieht? Politisch gewollt vom Bildungsministerium, eventuell daher schon viele Studien und Anfragen zu diesem Thema; Schulen fühlen sich mit Hinblibk auf geschultes Personal und IT-Ausstattung nicht für eine solche Studie in der Lage...

In the incentive treatment, we intended to attract headmasters' attention by offering schools the possibility to win 700 Euros which could, e.g., spent on teachers' training or teaching material. Table 8 shows whether schools being eligible to receive the financial reward change their responsiveness with respect to positive responses, any response, and filling out the survey. Panel A shows the results pooling all research topics and Panels B, C, and D present results for the respective research topic treatment. Overall, we find no significant effect of the extrinsic incentive on any of the outcome variables neither for the pooled sample nor for each of the respective research topics. As the share of the financial incentives on schools' yearly budget for teacher training varies by school (see Table 9), we analyze in Table 10 whether we heterogeneous responses by the share of the incentive exists. We find that the size of the incentive share does not matter to increases headmasters' responsiveness for none of the outcome variables.

**Result 3–Monetary incentives:** *Extrinsic financial incentives do not attract headmasters' attention.*

### 4.5 Survey

In the recruitment email, gatekeepers could access more information on the proposed research question by clicking on the respective link. Moreover, each headmaster clicking on one of the three links indicating their willingness to participate (opt-out, light interest, opt-in) was guided to a short questionnaire asking about headmasters' perceived usefulness of academic research (see the online appendix for the additional information given and the survey).

Figure 4 shows that only a small fraction (approx. 4%) of schools was actively seeking for additional and more detailed information on the proposed research topic. Moreover, there is no significant difference between the research topics. If at all, schools in the e-learning treatment tend to ask more often for further information than schools in the parental involvement and school in the integration of migrant children treatment. Putting this into perspective with the finding that schools in the e-learning treatment were less likely to respond (positively) compared to schools in the two other treatments, it seems that was not an uninformed choice.

Turning to our survey, we find that—not surprisingly—schools who responded positive to our inquiry were also significantly more likely (significant at the 1%-level, Wilcoxon ranksum test) to fill out our questionnaire than schools who showed light interest in our study and schools who actively opted out (see figure 5). Overall, roughly 32% of schools who responded in any way also (partly) filled out the questionnaire. Here we asked inter alia whether schools think that academic research could be integrated in schools' daily life, whether they think academic research is informative for educational policy makers, whether headmasters themselves are generally interested in the results of academic research, whether they find the proposed reseach topic interesting, whether their school has no personnel capacity to participate in the study, and whether the school think that schools receive too many inquiries by researchers. Headmasters were asked to indicate whether they agree or disagree with these statements on a 1 (disagree) to 10 (agree) scale. The answers are shown in figures 6 and 7. In line with the regression results in section 4.4, it seems that headmasters perceive their research topic as interesting. However, they also agree with the statement that there are too many inquiries by researchers to participate in a study and that they do not have enough personnel resources for participation. Moreover, while headmasters seem to be generally interested in the results of academic reseach and they think that academic research is useful to inform educational policy makers, they do not belief that research results can be integrated in daily school life.

check that

# 5 Discussion

is there anything we have to discuss?

# 6 Conclusion

Conclusion

# References

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# A Tables

Check whether Tables are in correct order (the order they appear in the main text) and whether we refer to all tables also those in the online appendicies

Table 1: Response rates and position of respondent

**Panel A: Response rates by treatment**

| *Treatment* | *No response* | *Opted out* | *Light interest* | *Strong interest* |
|---|---|---|---|---|
| E-learning (N=955) | 78.22 | 13.51 | 4.50 | 3.77 |
| | (747) | (129) | (43) | (36) |
| Parental involvement (N=930) | 71.72 | 17.31 | 5.70 | 5.27 |
| | (667) | (161) | (53) | (49) |
| Integration migration (N=930) | 74.52 | 15.27 | 6.56 | 3.66 |
| | (693) | (142) | (61) | (34) |
| Scientific cooperation (N=490) | 73.06 | 20.61 | 0.00 | 6.33 |
| | (358) | (101) | (0) | (31) |

**Panel B: Position of respondent**

| *Position (German)* | *Position (English)* | *Absolute* | *Share* | *Cumulative* |
|---|---|---|---|---|
| Oberstudiendirektor | “Headmaster” | 138 | 73.40 | 73.40 |
| Studiendirektor | “Dean of Students’ ’ | 23 | 12.23 | 85.63 |
| Oberstudienrat | “Senior Teacher” | 5 | 2.66 | 88.29 |
| Studienrat | “Junior Teacher” | 2 | 1.06 | 89.35 |
| Referendar | “Trainee Teacher” | 8 | 4.26 | 93.61 |
| Sekretariat | “Office Staff” | 5 | 2.66 | 96.27 |
| Other | | 7 | 3.73 | 100.00 |

*Note:* Panel A summarizes the responses (in %) of schools depending on the treatment topic. Cell entries represent percentages and parentheses present the absolute numbers of responses. Panel B contains information on the respondent, i.e. the person who filled out the questionnaire. Column (1) is the German description of the respondents position.

Table 2: Descriptive Statistics - Comparison of response rates

**Panel A: Response rates**

| | Secondary Schools | | | Elementary Schools | |
|---|---|---|---|---|---|
| | This study | Riener and Wagner, 2019 | Fischer and Wagner, 2018 | This study | Wagner, 2016 |
| No Response | 72.40 | 67.06 | 76.92 | 76.77 | 83.13 |
| | (1196) | (114) | (110) | (1269) | (207) |
| Responded | 27.60 | 32.94 | 23.08 | 23.23 | 16.87 |
| | (456) | (56) | (33) | (384) | (42) |
| Stakes in Study | very low | low | high | very low | low |
| Fisher's exact test for difference in response rates | | p=0.152 | p=0.281 | | p=0.027 |

**Panel B: Contact type** (Riener and Wagner, 2019)

| | *Letter* | *Email* | *Letter+Email* |
|---|---|---|---|
| No Response | 66.07 | 68.42 | 67.27 |
| | (37) | (39) | (37) |
| Responded | 33.93 | 31.58 | 32.73 |
| | (19) | (18) | (18) |
| Fisher's exact test for difference in response rates | | p=0.978 | |

*Note:* This table summarizes . Write how the stakes in each experiment were (this study = super low, Riener and Wagner, 2019 and Wagner, 2016 = low stakes, Fischer and Wagner, 2018 = high stakes). Cell entries represent percentages, the number of observations in parentheses. Two-sided Fisher's exact test for difference in response rates. * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

Table 3: Descriptive Statistics - School-level data

| | (1)<br>E-Learning | (2)<br>Parental<br>Involvement | (3)<br>Integration<br>Migration | (4)<br>Scientific<br>Contribution | (5)<br>Overall |
|---|---|---|---|---|---|
| Gender of headmaster | 0.679 | 0.616 | 0.634 | 0.635 | 0.642 |
| | (0.017) | (0.018) | (0.018) | (0.025) | (0.009) |
| Av. comp. teaching hours | 21.213 | 21.054 | 21.108 | 21.154 | 21.130 |
| | (0.085) | (0.091) | (0.084) | (0.121) | (0.046) |
| Students in day care | 95.412 | 95.001 | 94.769 | 94.636 | 95.000 |
| | (0.389) | (0.417) | (0.420) | (0.586) | (0.219) |
| Age of teachers (full time empl.) | 39.858 | 39.951 | 40.004 | 40.555 | 40.028 |
| | (0.215) | (0.227) | (0.224) | (0.315) | (0.119) |
| Students migration background | 30.373 | 30.404 | 28.857 | 28.719 | 29.710 |
| | (0.631) | (0.705) | (0.647) | (0.859) | (0.349) |
| Students migrated | 6.385 | 6.375 | 6.163 | 6.604 | 6.352 |
| | (0.253) | (0.257) | (0.268) | (0.347) | (0.137) |
| Parents migrated | 28.468 | 28.515 | 27.349 | 26.800 | 27.919 |
| | (0.593) | (0.662) | (0.625) | (0.807) | (0.331) |
| Number of students | 329.547 | 323.894 | 331.552 | 332.293 | 328.928 |
| | (9.050) | (8.992) | (8.960) | (13.138) | (4.835) |
| Female students | 46.817 | 47.008 | 49.558 | 48.814 | 47.938 |
| | (0.309) | (0.276) | (2.447) | (1.877) | (0.752) |
| Non-German students | 7.195 | 7.230 | 7.194 | 7.368 | 7.230 |
| | (0.265) | (0.274) | (0.269) | (0.326) | (0.141) |
| Non-German female students | 3.400 | 3.412 | 3.305 | 3.404 | 3.377 |
| | (0.131) | (0.133) | (0.121) | (0.152) | (0.067) |
| Share fullt time empl. teachers | 55.915 | 56.000 | 55.315 | 55.675 | 55.735 |
| | (0.548) | (0.578) | (0.524) | (0.820) | (0.297) |
| Students speak no German at home | 15.987 | 16.461 | 15.266 | 15.070 | 15.782 |
| | (0.502) | (0.555) | (0.500) | (0.663) | (0.274) |
| Number of classes | 12.497 | 11.988 | 12.247 | 12.120 | 12.227 |
| | (0.229) | (0.215) | (0.213) | (0.321) | (0.118) |
| *N* | 955 | 930 | 930 | 490 | 3305 |
| Proportion | 0.289 | 0.281 | 0.281 | 0.148 | 1.000 |

*Note:* This table summarizes.. For none of the differences in group means, the p-value associated with the corresponding t-statistic would imply significance at the 5% level – without correcting for multiple comparisons only the age of teachers in the E-Learning treatment is marginally significantly different from the age of teachers in the Scientific Contribution Treatment at the 10% level (p = 0.064).

Table 4: Descriptive Statistics - Municipality-level data

| | (1)<br>E-Learning | (2)<br>Parental<br>Involvement | (3)<br>Integration<br>Migration | (4)<br>Scientific<br>Contribution | (5)<br>Overall |
|---|---|---|---|---|---|
| Inhabitants | 371.964 | 372.022 | 369.553 | 368.521 | 370.791 |
| | (3.854) | (3.893) | (3.873) | (5.471) | (2.069) |
| Status married | 48.457 | 48.538 | 48.501 | 48.479 | 48.495 |
| | (0.045) | (0.042) | (0.042) | (0.060) | (0.023) |
| Unemployment rate | 2.352 | 2.347 | 2.348 | 2.358 | 2.350 |
| | (0.025) | (0.026) | (0.025) | (0.035) | (0.013) |
| Voter turnout 2013 | 73.238 | 73.218 | 73.408 | 73.205 | 73.275 |
| | (0.111) | (0.116) | (0.113) | (0.157) | (0.061) |
| Elections party: CDU | 42.911 | 42.964 | 43.258 | 43.587 | 43.124 |
| | (0.207) | (0.217) | (0.219) | (0.301) | (0.115) |
| Elections party: SPD | 30.091 | 30.082 | 29.826 | 29.646 | 29.948 |
| | (0.176) | (0.185) | (0.183) | (0.248) | (0.096) |
| Elections party: FDP | 5.189 | 5.202 | 5.255 | 5.174 | 5.209 |
| | (0.038) | (0.040) | (0.040) | (0.054) | (0.021) |
| Elections party: Grune | 6.932 | 6.899 | 6.907 | 6.854 | 6.904 |
| | (0.055) | (0.055) | (0.057) | (0.076) | (0.030) |
| Elections party: DieLinke | 5.309 | 5.275 | 5.240 | 5.240 | 5.270 |
| | (0.034) | (0.035) | (0.035) | (0.050) | (0.019) |
| Elections party: Other | 8.425 | 8.444 | 8.379 | 8.345 | 8.405 |
| | (0.041) | (0.042) | (0.042) | (0.055) | (0.022) |
| German citizenship | 93.145 | 93.137 | 93.121 | 93.027 | 93.118 |
| | (0.057) | (0.057) | (0.057) | (0.081) | (0.031) |
| Education: Uni access | 17.489 | 17.227 | 17.252 | 17.311 | 17.322 |
| | (0.132) | (0.130) | (0.131) | (0.181) | (0.070) |
| Education: High School | 27.099 | 27.051 | 27.063 | 27.042 | 27.067 |
| | (0.095) | (0.096) | (0.097) | (0.129) | (0.051) |
| Land prices in 2014 | 134.365 | 134.081 | 133.941 | 133.947 | 134.104 |
| | (1.259) | (1.279) | (1.267) | (1.762) | (0.676) |
| Share people aged 64 or older | 20.511 | 20.534 | 20.528 | 20.512 | 20.522 |
| | (0.026) | (0.027) | (0.027) | (0.036) | (0.014) |
| Religion: Other | 27.400 | 27.167 | 27.003 | 27.218 | 27.196 |
| | (0.200) | (0.202) | (0.202) | (0.284) | (0.108) |
| Religion: Protestant | 27.951 | 27.413 | 27.125 | 27.545 | 27.507 |
| | (0.426) | (0.410) | (0.410) | (0.594) | (0.223) |
| Male Workers | 51.595 | 51.635 | 51.645 | 51.632 | 51.626 |
| | (0.030) | (0.031) | (0.030) | (0.043) | (0.016) |
| Social index of municipality | 30.033 | 29.750 | 29.391 | 30.005 | 29.769 |
| | (0.508) | (0.517) | (0.517) | (0.723) | (0.274) |
| *N* | 955 | 930 | 930 | 490 | 3305 |
| Proportion | 0.289 | 0.281 | 0.281 | 0.148 | 1.000 |

*Note:* This table summarizes.. For none of the differences in group means, the p-value associated with the corresponding t-statistic would imply significance at the 5% level – without correcting for multiple comparisons only the the election outcome for CDU in the E-Learning treatment and Parental involvement are marginally significantly different from the Scientific Contribution Treatment at the 10% level (p = 0.061 and p = 0.093).

Table 5: Results - Dep. Var: Responded

| | (1) | | (2) | | (3) | | (4) | | (5) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Pooled | | E-Learning | | Parental Involvement | | Migration | | Scientific Contribution | |
| *School-level contr.* | | | | | | | | | | |
| Vocational (Gesamtsch.) | 0.108 | (0.051) | 0.065 | (0.097) | 0.021 | (0.090) | 0.184 | (0.106) | 0.132 | (0.099) |
| High School | 0.210 | (0.094) | 0.177 | (0.199) | 0.186 | (0.164) | 0.267 | (0.161) | 0.183 | (0.227) |
| Vocational (Hauptsch.) | 0.050 | (0.040) | 0.009 | (0.071) | 0.052 | (0.053) | 0.160 | (0.068) | -0.071 | (0.100) |
| Vocational (Realsch.) | -0.010 | (0.032) | 0.019 | (0.051) | -0.021 | (0.063) | 0.034 | (0.063) | -0.171 | (0.074) |
| Other school types | 0.054 | (0.034) | 0.118 | (0.068) | 0.045 | (0.043) | 0.035 | (0.078) | -0.031 | (0.057) |
| Gender of headmaster | -0.013 | (0.020) | -0.031 | (0.025) | 0.015 | (0.028) | -0.021 | (0.029) | -0.020 | (0.036) |
| Av. comp. teaching hours | 0.007 | (0.004) | 0.007 | (0.004) | 0.008 | (0.006) | 0.012 | (0.008) | -0.003 | (0.009) |
| Students in day care | 0.005 | (0.002) | 0.004 | (0.006) | 0.003 | (0.004) | 0.006 | (0.005) | 0.000 | (0.006) |
| Age of teachers (full time empl.) | -0.000 | (0.001) | 0.001 | (0.002) | -0.002 | (0.002) | -0.003 | (0.002) | 0.005 | (0.002) |
| Students migration background | -0.000 | (0.002) | -0.012* | (0.004) | -0.001 | (0.003) | 0.006 | (0.004) | 0.006 | (0.004) |
| Students migrated | -0.000 | (0.001) | 0.005 | (0.002) | -0.002 | (0.003) | -0.002 | (0.003) | -0.003 | (0.003) |
| Parents migrated | 0.001 | (0.001) | 0.012 | (0.004) | 0.002 | (0.003) | -0.004 | (0.004) | -0.002 | (0.003) |
| Number of students | 0.000 | (0.000) | 0.000 | (0.000) | -0.000 | (0.000) | 0.000 | (0.000) | -0.000 | (0.000) |
| Female students | -0.001 | (0.001) | -0.001 | (0.002) | -0.001 | (0.002) | -0.000 | (0.000) | -0.001 | (0.000) |
| Non-German students | 0.000 | (0.003) | -0.007 | (0.005) | 0.006 | (0.005) | -0.002 | (0.006) | 0.000 | (0.008) |
| Non-German female students | -0.004 | (0.005) | 0.005 | (0.011) | -0.011 | (0.011) | 0.002 | (0.011) | -0.013 | (0.018) |
| Share fullt time empl. teachers | -0.000 | (0.001) | -0.000 | (0.001) | -0.001 | (0.001) | -0.002 | (0.002) | 0.006*** | (0.002) |
| Students speak no German at home | -0.000 | (0.001) | 0.002 | (0.002) | -0.001 | (0.002) | -0.001 | (0.002) | -0.002 | (0.002) |
| Number of classes | 0.001 | (0.003) | -0.007 | (0.006) | 0.012 | (0.006) | -0.001 | (0.009) | 0.014 | (0.008) |
| *Munic.-level contr.* | | | | | | | | | | |
| Inhabitants | 0.000 | (0.000) | 0.000 | (0.000) | 0.000 | (0.000) | -0.000 | (0.000) | 0.000 | (0.000) |
| Status married | 0.029 | (0.012) | 0.026 | (0.016) | 0.037 | (0.030) | 0.035 | (0.014) | 0.024 | (0.026) |
| Unemployment rate | 0.003 | (0.022) | 0.030 | (0.029) | 0.047 | (0.049) | -0.057 | (0.030) | -0.027 | (0.050) |
| Voter turnout 2013 | -0.003 | (0.004) | -0.005 | (0.006) | -0.012 | (0.008) | 0.004 | (0.006) | 0.013 | (0.007) |
| Elections party: SPD | -0.002 | (0.003) | -0.001 | (0.004) | -0.007 | (0.004) | 0.005 | (0.005) | -0.001 | (0.007) |
| Elections party: FDP | -0.025 | (0.013) | -0.022 | (0.018) | -0.033 | (0.019) | -0.028 | (0.018) | -0.020 | (0.030) |
| Elections party: Grune | 0.012 | (0.010) | 0.007 | (0.013) | 0.010 | (0.014) | 0.001 | (0.015) | 0.023 | (0.024) |
| Elections party: DieLinke | -0.034 | (0.017) | -0.025 | (0.029) | -0.038 | (0.032) | -0.028 | (0.028) | -0.036 | (0.023) |
| Elections party: Other | 0.010 | (0.009) | -0.011 | (0.016) | 0.013 | (0.021) | 0.014 | (0.021) | 0.020 | (0.035) |
| German citizenship | 0.006 | (0.009) | 0.008 | (0.007) | 0.012 | (0.017) | 0.007 | (0.012) | -0.009 | (0.018) |
| Education: Uni access | 0.006 | (0.007) | 0.006 | (0.009) | 0.015 | (0.012) | 0.013 | (0.008) | -0.008 | (0.014) |
| Education: High School | 0.001 | (0.005) | 0.003 | (0.007) | -0.001 | (0.011) | -0.001 | (0.007) | -0.001 | (0.012) |
| Land prices in 2014 | 0.000 | (0.001) | 0.002 | (0.001) | 0.001 | (0.001) | -0.002 | (0.001) | -0.001 | (0.001) |
| Share people aged 64 or older | -0.014 | (0.022) | -0.008 | (0.029) | 0.004 | (0.042) | -0.015 | (0.037) | -0.060 | (0.040) |
| Religion: Other | -0.006 | (0.006) | -0.010 | (0.005) | -0.014 | (0.009) | 0.004 | (0.006) | -0.004 | (0.012) |
| Religion: Protestant | 0.001 | (0.002) | 0.003 | (0.002) | 0.003 | (0.003) | -0.003 | (0.002) | 0.004 | (0.004) |
| Male Workers | 0.014 | (0.020) | 0.067 | (0.029) | 0.014 | (0.042) | 0.014 | (0.031) | -0.074 | (0.040) |
| Social index of municipality | 0.001 | (0.001) | 0.003 | (0.001) | 0.003 | (0.002) | -0.001 | (0.002) | -0.004 | (0.002) |
| *N* | 3305 | | 955 | | 930 | | 930 | | 490 | |

*Note:* This table summarizes the determinants of any response to the recruitment email that was sent to schools. The coefficients are the marginal effects from a probit regression, standard errors in parentheses. p-vlaues are adjusted for MHT using RomanoWolf with 100 reps. * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

Table 6: Results - Dep. Var: Positive Response

| | (1) | | (2) | | (3) | | (4) | | (5) | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Pooled | | E-Learning | | Parental Involvement | | Migration | | Scientific Contribution | |
| *School-level contr.* | | | | | | | | | | |
| Vocational (Gesamtsch.) | 0.054 | (0.036) | 0.077 | (0.060) | 0.020 | (0.069) | 0.070 | (0.081) | 0.004 | (0.069) |
| High School | 0.148 | (0.061) | 0.206 | (0.108) | 0.124 | (0.109) | 0.122 | (0.139) | 0.113 | (0.125) |
| Vocational (Hauptsch.) | -0.057 | (0.027) | -0.046 | (0.049) | -0.094 | (0.050) | -0.013 | (0.055) | -0.062 | (0.057) |
| Vocational (Realsch.) | -0.020 | (0.023) | 0.019 | (0.037) | -0.021 | (0.040) | -0.043 | (0.045) | -0.063 | (0.045) |
| Other school types | 0.009 | (0.020) | 0.047 | (0.032) | 0.033 | (0.034) | -0.032 | (0.056) | -0.042 | (0.051) |
| Gender of headmaster | -0.003 | (0.012) | -0.008 | (0.018) | 0.013 | (0.022) | 0.004 | (0.019) | -0.021 | (0.022) |
| Av. comp. teaching hours | 0.005 | (0.002) | 0.008* | (0.003) | 0.001 | (0.004) | 0.009 | (0.005) | 0.007 | (0.004) |
| Students in day care | 0.004 | (0.002) | 0.005 | (0.003) | 0.004 | (0.003) | 0.003 | (0.004) | 0.003 | (0.004) |
| Age of teachers (full time empl.) | -0.000 | (0.001) | 0.000 | (0.001) | -0.003 | (0.001) | -0.000 | (0.002) | 0.003 | (0.002) |
| Students migration background | 0.001 | (0.001) | -0.004 | (0.003) | -0.001 | (0.002) | 0.004 | (0.003) | 0.003 | (0.002) |
| Students migrated | -0.000 | (0.001) | 0.003 | (0.001) | -0.003 | (0.002) | -0.003 | (0.001) | 0.001 | (0.002) |
| Parents migrated | -0.000 | (0.001) | 0.005 | (0.003) | 0.001 | (0.002) | -0.002 | (0.003) | -0.002 | (0.002) |
| Number of students | 0.000 | (0.000) | 0.000 | (0.000) | 0.000 | (0.000) | -0.000 | (0.000) | 0.000 | (0.000) |
| Female students | -0.001 | (0.001) | -0.000 | (0.001) | -0.002 | (0.001) | -0.001 | (0.002) | -0.002 | (0.002) |
| Non-German students | -0.001 | (0.002) | -0.006 | (0.003) | 0.006 | (0.004) | -0.006 | (0.005) | -0.003 | (0.005) |
| Non-German female students | 0.003 | (0.004) | 0.004 | (0.007) | -0.004 | (0.009) | 0.015 | (0.009) | -0.003 | (0.011) |
| Share fullt time empl. teachers | -0.000 | (0.000) | 0.000 | (0.001) | -0.001 | (0.001) | -0.001 | (0.001) | 0.001 | (0.001) |
| Students speak no German at home | -0.001 | (0.001) | -0.001 | (0.001) | -0.000 | (0.001) | -0.002 | (0.001) | -0.000 | (0.001) |
| Number of classes | 0.001 | (0.002) | -0.002 | (0.003) | 0.003 | (0.004) | 0.006 | (0.007) | 0.003 | (0.005) |
| *Munic.-level contr.* | | | | | | | | | | |
| Inhabitants | 0.000 | (0.000) | -0.000 | (0.000) | 0.000 | (0.000) | -0.000 | (0.000) | -0.000 | (0.000) |
| Status married | 0.008 | (0.005) | 0.007 | (0.010) | 0.022 | (0.016) | 0.009 | (0.012) | 0.022 | (0.012) |
| Unemployment rate | -0.002 | (0.010) | 0.018 | (0.015) | 0.052 | (0.023) | -0.039 | (0.029) | -0.069 | (0.028) |
| Voter turnout 2013 | 0.001 | (0.002) | 0.007 | (0.004) | -0.003 | (0.004) | 0.003 | (0.004) | -0.004 | (0.004) |
| Elections party: SPD | -0.001 | (0.001) | 0.001 | (0.002) | -0.004 | (0.003) | -0.002 | (0.003) | -0.002 | (0.003) |
| Elections party: FDP | -0.005 | (0.005) | 0.010 | (0.010) | -0.008 | (0.011) | -0.030 | (0.011) | 0.008 | (0.013) |
| Elections party: Grune | 0.003 | (0.005) | -0.008 | (0.009) | -0.001 | (0.010) | 0.015 | (0.009) | 0.013 | (0.008) |
| Elections party: DieLinke | -0.006 | (0.011) | 0.019 | (0.021) | -0.006 | (0.019) | -0.023 | (0.022) | -0.004 | (0.015) |
| Elections party: Other | 0.015 | (0.007) | 0.010 | (0.013) | -0.010 | (0.013) | 0.036* | (0.014) | 0.013 | (0.015) |
| German citizenship | -0.001 | (0.003) | 0.003 | (0.005) | 0.004 | (0.006) | -0.015 | (0.009) | -0.005 | (0.007) |
| Education: Uni access | -0.001 | (0.002) | 0.002 | (0.005) | 0.008 | (0.007) | -0.010 | (0.005) | 0.000 | (0.007) |
| Education: High School | 0.003 | (0.002) | -0.006 | (0.004) | -0.003 | (0.006) | 0.018* | (0.005) | 0.001 | (0.005) |
| Land prices in 2014 | 0.000 | (0.000) | 0.001 | (0.000) | 0.001 | (0.001) | -0.001 | (0.001) | -0.000 | (0.001) |
| Share people aged 64 or older | -0.012 | (0.010) | -0.038 | (0.020) | 0.009 | (0.025) | -0.034 | (0.018) | 0.029 | (0.020) |
| Religion: Other | -0.006 | (0.003) | -0.006 | (0.003) | -0.006 | (0.006) | -0.006 | (0.005) | -0.006 | (0.006) |
| Religion: Protestant | 0.002 | (0.001) | 0.002 | (0.001) | 0.003 | (0.002) | 0.001 | (0.002) | 0.000 | (0.002) |
| Male Workers | 0.001 | (0.009) | 0.011 | (0.012) | 0.048 | (0.023) | -0.044 | (0.019) | -0.029 | (0.021) |
| Social index of municipality | 0.001 | (0.000) | 0.000 | (0.001) | 0.002 | (0.001) | -0.000 | (0.001) | 0.001 | (0.001) |
| *N* | 3305 | | 955 | | 930 | | 930 | | 490 | |

*Note:* This table summarizes the determinants of a positive response to the recruitment email that was sent to schools. The coefficients are the marginal effects from a probit regression, standard errors in parentheses. p-vlaues are adjusted for MHT using RomanoWolf with 100 reps. * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

Table 7: Results - Role of Treatment Topic

| | (1)<br>Positive Response | (2)<br>Responded | (3)<br>Filled Out Survey | (4)<br>Positive Response | (5)<br>Responded | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| Treated | 0.036** | -0.029 | -0.027 | | | |
| | (0.012) | (0.020) | (0.015) | | | |
| Incentive | 0.005 | 0.021 | 0.010 | 0.005 | 0.021 | 0.009 |
| | (0.011) | (0.020) | (0.008) | (0.011) | (0.020) | (0.008) |
| E-Learning | | | | 0.019 | -0.064* | -0.052** |
| | | | | (0.016) | (0.025) | (0.018) |
| Parental Inv. | | | | 0.047*** | 0.001 | -0.012 |
| | | | | (0.013) | (0.025) | (0.017) |
| Integration Migr. | | | | 0.040** | -0.024 | -0.019 |
| | | | | (0.014) | (0.019) | (0.016) |
| School-level contr. | Yes | Yes | Yes | Yes | Yes | Yes |
| County-level contr. | Yes | Yes | Yes | Yes | Yes | Yes |
| $N$ | 3305 | 3305 | 3305 | 3305 | 3305 | 3305 |

*Note:* This table summarizes . p-vlaues are adjusted for MHT using RomanoWolf with 100 reps * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

Table 8: Incentive Treatment

| | (1)<br>Pos. Response | (2)<br>Pos. Response | (3)<br>Responded | (4)<br>Responded | (5)<br>Filled Out Survey | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| **Panel A: Pooled (N=3305)** | | | | | | |
| Incentive | 0.013 | 0.013 | 0.012 | 0.014 | 0.001 | 0.002 |
| | (0.011) | (0.011) | (0.019) | (0.019) | (0.008) | (0.008) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel B: E-Learning (N=955)** | | | | | | |
| Incentive | -0.002 | 0.001 | 0.007 | 0.009 | 0.005 | 0.009 |
| | (0.020) | (0.019) | (0.032) | (0.031) | (0.014) | (0.013) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel C: Parental Involvement (N=930)** | | | | | | |
| Incentive | 0.030 | 0.036 | 0.041 | 0.042 | 0.009 | 0.014 |
| | (0.025) | (0.023) | (0.028) | (0.028) | (0.017) | (0.016) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel D: Integration Migration (N=930)** | | | | | | |
| Incentive | -0.015 | -0.016 | 0.006 | 0.002 | 0.006 | 0.005 |
| | (0.017) | (0.016) | (0.027) | (0.027) | (0.018) | (0.016) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |

*Note:* This table summarizes .Probit marginal coeff *=0.05

Table 9: Share of incentive on school's yearly budget for training of teachers

| (1)<br>Share of Incentive | (2)<br>Absolute | (3)<br>Percent | (4)<br>Cumulative |
|---|---|---|---|
| $80< \text{x} \leq 90\%$ | 1,003 | 71.90 | 71.90 |
| $70< \text{x} \leq 80\%$ | 56 | 4.01 | 75.91 |
| $60< \text{x} \leq 70\%$ | 50 | 3.58 | 79.49 |
| $50< \text{x} \leq 60\%$ | 73 | 5.23 | 84.72 |
| $40< \text{x} \leq 50\%$ | 68 | 4.87 | 89.59 |
| $30< \text{x} \leq 40\%$ | 87 | 6.24 | 95.83 |
| $20< \text{x} \leq 30\%$ | 51 | 3.66 | 99.49 |
| $10< \text{x} \leq 20\%$ | 7 | 0.50 | 99.99 |
| Total | $1,395$ | 100.00 | |

*Note:* This table summarizes the share of the financial incentive for participating in the study (700 Euros) on the school's yearly budget. Column (3) shows for how many schools in our sample the extrinsic incentives resembles the respective share in column (1), for example, for 70.1% of the schools 700 Euros would be 80-90% of their yearly budget. $\mathit{Share\ of\ Incentive} = \frac{700\ \mathit{Euro}}{\mathit{School's\ yearly\ budget}}$

Table 10: Incentive Treatment - Share Budget

| | (1)<br>Pos. Response | (2)<br>Pos. Response | (3)<br>Responded | (4)<br>Responded | (5)<br>Filled Out Survey | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| **Panel A: Pooled (N=3305)** | | | | | | |
| Share Budget | 0.005 | 0.013 | -0.000 | 0.013 | -0.004 | 0.004 |
| | (0.013) | (0.014) | (0.024) | (0.026) | (0.011) | (0.012) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel B: E-Learning (N=955)** | | | | | | |
| Share Budget | -0.024 | -0.014 | -0.017 | -0.008 | 0.007 | 0.015 |
| | (0.023) | (0.023) | (0.040) | (0.040) | (0.018) | (0.017) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel C: Parental Involvement (N=930)** | | | | | | |
| Share Budget | 0.013 | 0.027 | 0.020 | 0.037 | -0.009 | 0.014 |
| | (0.030) | (0.030) | (0.036) | (0.040) | (0.020) | (0.021) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel D: Integration Migration (N=930)** | | | | | | |
| Share Budget | -0.010 | -0.009 | 0.012 | 0.007 | 0.009 | 0.007 |
| | (0.021) | (0.019) | (0.035) | (0.036) | (0.023) | (0.020) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |

*Note:* This table summarizes .Probit marginal coeff *=0.05

Table 11: An Experiment on Balance and Precision: Design

| Sub- | Method | Control | | Treatment Group | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| sample | | 0 | 1 | 2 | 3 | 4 | 5 | 6 | $\sum$ |
| 1 | Matching | 15 | 15 | | | | | | 30 |
| | MinMSE | 15 | 15 | | | | | | 30 |
| 2 | ReRandomization | 30 | 30 | 30 | 30 | 30 | 30 | 30 | 210 |
| | MinMSE | 30 | 30 | 30 | 30 | 30 | 30 | 30 | 210 |
| 3 | Randomization | 20 | 20 | 20 | 20 | 20 | 20 | 20 | 140 |
| | MinMSE | 20 | 20 | 20 | 20 | 20 | 20 | 20 | 140 |
| 4 | Randomization | 20 | 20 | 20 | 20 | 20 | 20 | 20 | 140 |
| | MinMSE | 20 | 20 | 20 | 20 | 20 | 20 | 20 | 140 |
| 5 | Randomization | 20 | 20 | 20 | 20 | 20 | 20 | | 120 |
| | MinMSE | 20 | 20 | 20 | 20 | 20 | 20 | | 120 |
| 6 | Randomization | 20 | 20 | 20 | 20 | 20 | 20 | | 120 |
| | MinMSE | 20 | 20 | 20 | 20 | 20 | 20 | | 120 |
| 7 | Randomization | 20 | 20 | 20 | 20 | 20 | | | 100 |
| | MinMSE | 20 | 20 | 20 | 20 | 20 | | | 100 |
| 8 | Randomization | 20 | 20 | 20 | 20 | 20 | | | 100 |
| | MinMSE | 20 | 20 | 20 | 20 | 20 | | | 100 |
| 9 | Randomization | 20 | 20 | 20 | 20 | | | | 80 |
| | MinMSE | 20 | 20 | 20 | 20 | | | | 80 |
| 10 | Randomization | 20 | 20 | 20 | 20 | | | | 80 |
| | MinMSE | 20 | 20 | 20 | 20 | | | | 80 |
| 11 | Randomization | 20 | 20 | 20 | | | | | 60 |
| | MinMSE | 20 | 20 | 20 | | | | | 60 |
| 12 | Randomization | 20 | 20 | | | | | | 40 |
| | MinMSE | 20 | 20 | | | | | | 40 |
| | | 490 | 490 | 420 | 380 | 300 | 220 | 140 | 2440 |

*Note:*

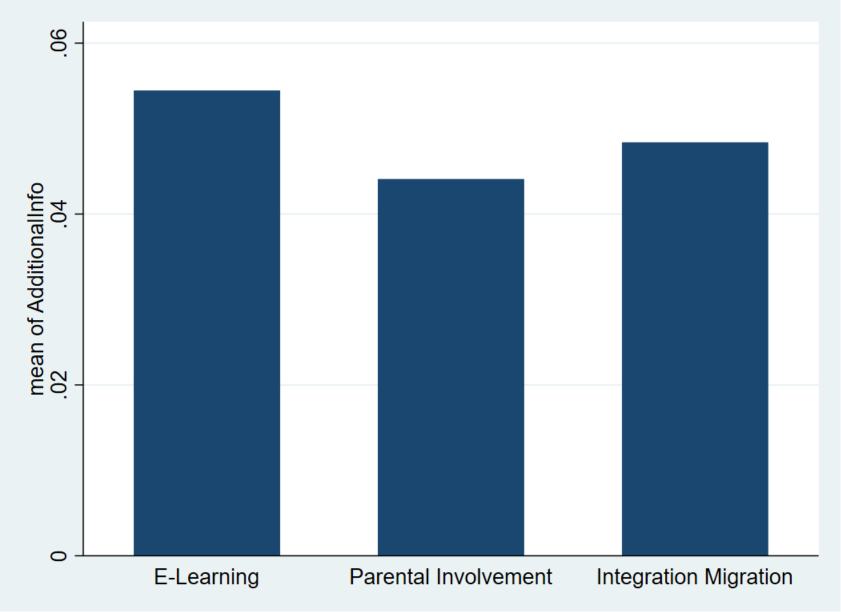
Table 12: Results - Treatment Assignment: Balance

| Comparison Method | Draw | Groups | p(minMSE) | p(ComparisonMethod) |
|---|---|---|---|---|
| Matching | 1 | 1 | 0.55 | 0.47 |
| Rerandomization | 2 | 6 | 0.40 | 0.25 |
| Randomization | 3 | 6 | 0.38 | 0.22 |
| Randomization | 4 | 6 | 0.45 | 0.22 |
| Randomization | 5 | 5 | 0.57 | 0.30 |
| Randomization | 6 | 5 | 0.53 | 0.35 |
| Randomization | 7 | 4 | 0.57 | 0.31 |
| Randomization | 8 | 4 | 0.38 | 0.31 |
| Randomization | 9 | 3 | 0.52 | 0.28 |
| Randomization | 10 | 3 | 0.48 | 0.39 |
| Randomization | 11 | 2 | 0.38 | 0.42 |
| Randomization | 12 | 1 | 0.72 | 0.50 |

*Note:*

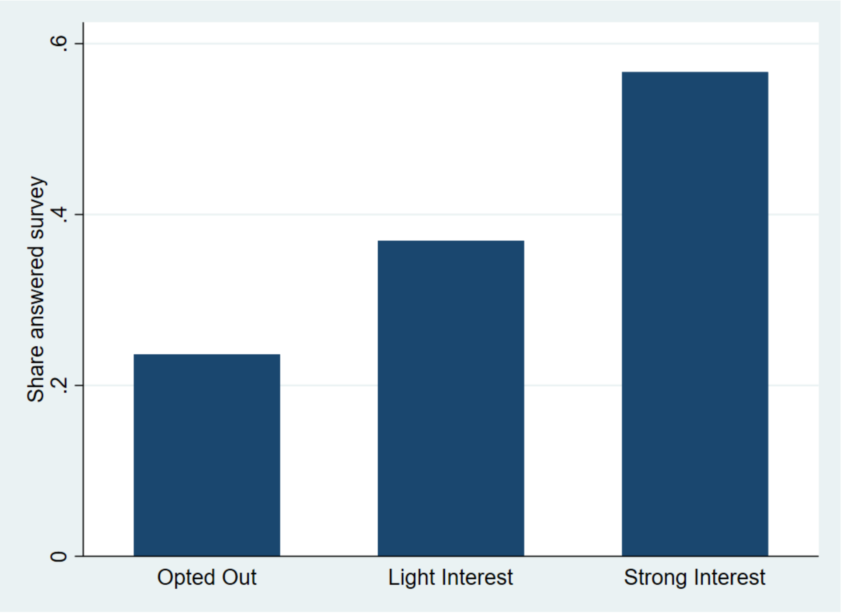
# B Graphs

Figure 4: Additional information accessed by treatment



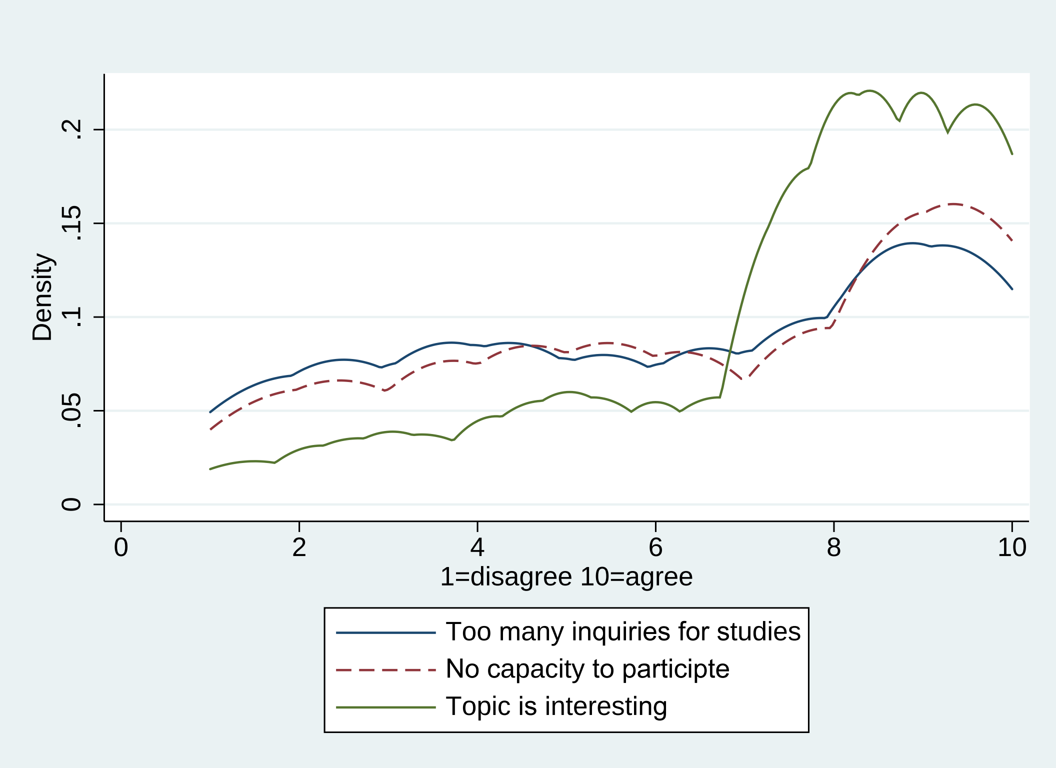
*Note: This graph presents.*

Figure 5: Share of schools answering survey by response



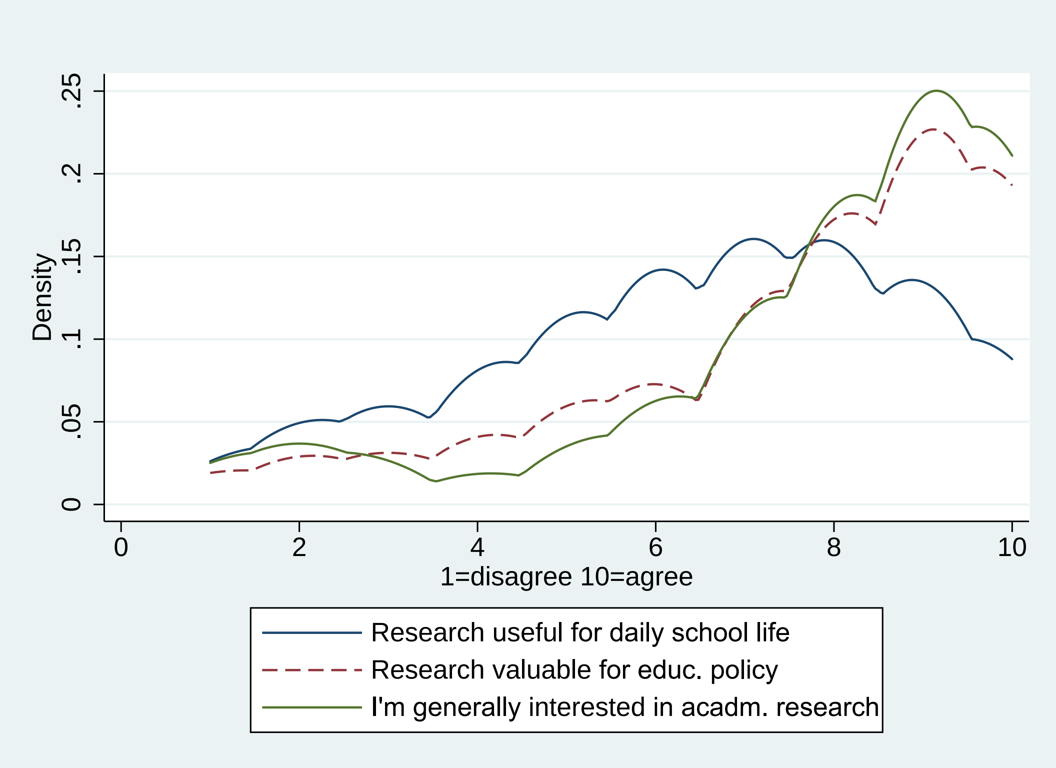
*Note: This graph presents.*

Figure 6: Questionnaire: Capacity for Academic Research



*Note: This graph presents. scale from 1 (disagree) to 10 agree*

Figure 7: Questionnaire: Interest in Academic Research



*Note: This graph presents. scale from 1 (disagree) to 10 agree*

# C Online Appendix - Additional Tables

## C.1 Randomization Check With Bootstrapped Standard Errors

Table 13: Randomization - School-level data

|  | (1)<br>E-Learning | (2)<br>Parental Involvement | (3)<br>Integration Migration | (4)<br>Scientific Contribution | (5)<br>Overall |
|---|---|---|---|---|---|
| Gender of headmaster | 0.679 | 0.616 | 0.634 | 0.635 | 0.642 |
|  | (0.017) | (0.017) | (0.016) | (0.026) | (0.009) |
| Av. comp. teaching hours | 21.213 | 21.054 | 21.108 | 21.154 | 21.130 |
|  | (0.080) | (0.084) | (0.084) | (0.114) | (0.049) |
| Students in day care | 95.412 | 95.001 | 94.769 | 94.636 | 95.000 |
|  | (0.401) | (0.392) | (0.418) | (0.548) | (0.202) |
| Age of teachers (full time empl.) | 39.858 | 39.951 | 40.004 | 40.555 | 40.028 |
|  | (0.197) | (0.232) | (0.206) | (0.295) | (0.124) |
| Students migration background | 30.373 | 30.404 | 28.857 | 28.719 | 29.710 |
|  | (0.618) | (0.684) | (0.671) | (0.838) | (0.355) |
| Students migrated | 6.385 | 6.375 | 6.163 | 6.604 | 6.352 |
|  | (0.247) | (0.274) | (0.280) | (0.364) | (0.132) |
| Parents migrated | 28.468 | 28.515 | 27.349 | 26.800 | 27.919 |
|  | (0.576) | (0.650) | (0.591) | (0.771) | (0.352) |
| Number of students | 329.547 | 323.894 | 331.552 | 332.293 | 328.928 |
|  | (9.169) | (9.251) | (8.708) | (13.286) | (5.232) |
| Female students | 46.817 | 47.008 | 49.558 | 48.814 | 47.938 |
|  | (0.313) | (0.261) | (2.291) | (1.830) | (0.685) |
| Non-German students | 7.195 | 7.230 | 7.194 | 7.368 | 7.230 |
|  | (0.269) | (0.279) | (0.280) | (0.348) | (0.136) |
| Non-German female students | 3.400 | 3.412 | 3.305 | 3.404 | 3.377 |
|  | (0.129) | (0.138) | (0.129) | (0.151) | (0.067) |
| Share fullt time empl. teachers | 55.915 | 56.000 | 55.315 | 55.675 | 55.735 |
|  | (0.583) | (0.591) | (0.512) | (0.846) | (0.273) |
| Students speak no German at home | 15.987 | 16.461 | 15.266 | 15.070 | 15.782 |
|  | (0.474) | (0.532) | (0.483) | (0.657) | (0.277) |
| Number of classes | 12.497 | 11.988 | 12.247 | 12.120 | 12.227 |
|  | (0.228) | (0.209) | (0.210) | (0.305) | (0.115) |
| *N* | 955 | 930 | 930 | 490 | 3305 |
| Proportion | 0.289 | 0.281 | 0.281 | 0.148 | 1.000 |

*Note:* This table summarizes. BOOTSTRAPP. SE in parantheses

## Table 14: Randomization - Municipality-level data

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | E-Learning | Parental | Integration | Scientific | Overall |
| | | Involvement | Migration | Contribution | |
| Inhabitants | 371.964 | 372.022 | 369.553 | 368.521 | 370.791 |
| | (3.827) | (3.617) | (3.609) | (5.314) | (2.053) |
| Status married | 48.457 | 48.538 | 48.501 | 48.479 | 48.495 |
| | (0.049) | (0.043) | (0.042) | (0.061) | (0.023) |
| Unemployment rate | 2.352 | 2.347 | 2.348 | 2.358 | 2.350 |
| | (0.024) | (0.027) | (0.025) | (0.035) | (0.013) |
| Voter turnout 2013 | 73.238 | 73.218 | 73.408 | 73.205 | 73.275 |
| | (0.113) | (0.115) | (0.117) | (0.157) | (0.058) |
| Elections party: CDU | 42.911 | 42.964 | 43.258 | 43.587 | 43.124 |
| | (0.224) | (0.220) | (0.228) | (0.316) | (0.119) |
| Elections party: SPD | 30.091 | 30.082 | 29.826 | 29.646 | 29.948 |
| | (0.180) | (0.190) | (0.172) | (0.257) | (0.089) |
| Elections party: FDP | 5.189 | 5.202 | 5.255 | 5.174 | 5.209 |
| | (0.036) | (0.037) | (0.038) | (0.050) | (0.020) |
| Elections party: Grune | 6.932 | 6.899 | 6.907 | 6.854 | 6.904 |
| | (0.055) | (0.054) | (0.061) | (0.078) | (0.029) |
| Elections party: DieLinke | 5.309 | 5.275 | 5.240 | 5.240 | 5.270 |
| | (0.036) | (0.037) | (0.032) | (0.050) | (0.020) |
| Elections party: Other | 8.425 | 8.444 | 8.379 | 8.345 | 8.405 |
| | (0.042) | (0.041) | (0.041) | (0.059) | (0.022) |
| German citizenship | 93.145 | 93.137 | 93.121 | 93.027 | 93.118 |
| | (0.060) | (0.060) | (0.058) | (0.079) | (0.030) |
| Education: Uni access | 17.489 | 17.227 | 17.252 | 17.311 | 17.322 |
| | (0.119) | (0.143) | (0.116) | (0.181) | (0.067) |
| Education: High School | 27.099 | 27.051 | 27.063 | 27.042 | 27.067 |
| | (0.089) | (0.099) | (0.095) | (0.136) | (0.048) |
| Land prices in 2014 | 134.365 | 134.081 | 133.941 | 133.947 | 134.104 |
| | (1.292) | (1.149) | (1.252) | (1.858) | (0.738) |
| Share people aged 64 or older | 27.400 | 27.167 | 27.003 | 27.218 | 27.196 |
| | (0.195) | (0.190) | (0.213) | (0.292) | (0.103) |
| Religion: Other | 27.400 | 27.167 | 27.003 | 27.218 | 27.196 |
| | (0.200) | (0.202) | (0.202) | (0.284) | (0.108) |
| Religion: Protestant | 27.951 | 27.413 | 27.125 | 27.545 | 27.507 |
| | (0.429) | (0.400) | (0.413) | (0.615) | (0.209) |
| Male Workers | 51.595 | 51.635 | 51.645 | 51.632 | 51.626 |
| | (0.028) | (0.031) | (0.033) | (0.045) | (0.017) |
| Social index of municipality | 30.033 | 29.750 | 29.391 | 30.005 | 29.769 |
| | (0.536) | (0.564) | (0.531) | (0.702) | (0.288) |
| *N* | 955 | 930 | 930 | 490 | 3305 |
| Proportion | 0.289 | 0.281 | 0.281 | 0.148 | 1.000 |

*Note:* This table summarizes. BOOTSTRAPPED. SE in parantheses

## C.2 Treatment Effects With Randomization Inference and Bootstrapped Standard Errors

Table 15: Results - Role of Treatment Topic

| | (1)<br>Positive Response | (2)<br>Responded | (3)<br>Filled Out Survey | (4)<br>Positive Response | (5)<br>Responded | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| Treated | 0.036* | -0.029 | -0.027 | | | |
| | (0.015) | (0.174) | (0.139) | | | |
| Incentive | 0.005 | 0.021 | 0.010 | 0.005 | 0.021 | 0.009 |
| | (0.629) | (0.265) | (0.230) | (0.647) | (0.323) | (0.217) |
| E-Learning | | | | 0.019 | -0.064* | -0.052* |
| | | | | (0.279) | (0.012) | (0.016) |
| Parental Inv. | | | | 0.047** | 0.001 | -0.012 |
| | | | | (0.005) | (0.958) | (0.501) |
| Integration Migr. | | | | 0.040* | -0.024 | -0.019 |
| | | | | (0.016) | (0.220) | (0.281) |
| School-level contr. | Yes | Yes | Yes | Yes | Yes | Yes |
| Munic.-level contr. | Yes | Yes | Yes | Yes | Yes | Yes |
| *N* | 3305 | 3305 | 3305 | 3305 | 3305 | 3305 |

*Note:* This table summarizes . BOOTSTRAPPED STD ERR WITH 200 REP. P values * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

Table 16: Results - Role of Treatment Topic

| | (1)<br>Positive Response | (2)<br>Responded | (3)<br>Filled Out Survey | (4)<br>Positive Response | (5)<br>Responded | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| Treated | 0.036* | -0.029 | -0.027* | | | |
| | (0.020) | (0.180) | (0.030) | | | |
| Incentive | 0.005 | 0.021 | 0.010 | 0.005 | 0.021 | 0.009 |
| | (0.640) | (0.160) | (0.270) | (0.640) | (0.160) | (0.270) |
| E-Learning | | | | 0.019 | -0.064*** | -0.052*** |
| | | | | (0.060) | (0.000) | (0.000) |
| Parental Inv. | | | | 0.047*** | 0.001 | -0.012 |
| | | | | (0.000) | (0.940) | (0.310) |
| Integration Migr. | | | | 0.040*** | -0.024 | -0.019 |
| | | | | (0.000) | (0.190) | (0.080) |
| School-level contr. | Yes | Yes | Yes | Yes | Yes | Yes |
| County-level contr. | Yes | Yes | Yes | Yes | Yes | Yes |

*Note:* This table summarizes . RANDOMIZATION INFERENCE WITH 100 REP. P-values. * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

## C.3 The Role of Monetary Incentives

Table 17: Incentive Treatment

| | (1)<br>Pos. Response | (2)<br>Pos. Response | (3)<br>Responded | (4)<br>Responded | (5)<br>Filled Out Survey | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| **Panel A: Pooled (N=3305)** | | | | | | |
| Incentive | 0.013 | 0.013 | 0.012 | 0.014 | 0.001 | 0.002 |
| | (0.214) | (0.219) | (0.505) | (0.491) | (0.943) | (0.764) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel B: E-Learning (N=955)** | | | | | | |
| Incentive | -0.002 | 0.001 | 0.007 | 0.009 | 0.005 | 0.009 |
| | (0.923) | (0.968) | (0.814) | (0.794) | (0.738) | (0.597) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel C: Parental Involvement (N=930)** | | | | | | |
| Incentive | 0.030 | 0.036 | 0.041 | 0.042 | 0.009 | 0.014 |
| | (0.254) | (0.192) | (0.149) | (0.136) | (0.622) | (0.443) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel D: Integration Migration (N=930)** | | | | | | |
| Incentive | -0.015 | -0.016 | 0.006 | 0.002 | 0.006 | 0.005 |
| | (0.381) | (0.357) | (0.811) | (0.937) | (0.744) | (0.776) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |

*Note:* This table summarizes .Probit marginal coeff *=0.05. BOOTSTRAPPED STD ERR WITH 200 REP. P values

Table 18: Incentive Treatment

| | (1)<br>Pos. Response | (2)<br>Pos. Response | (3)<br>Responded | (4)<br>Responded | (5)<br>Filled Out Survey | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| **Panel A: Pooled (N=3305)** | | | | | | |
| Incentive | 0.013 | 0.013 | 0.012 | 0.014 | 0.001 | 0.002 |
| | (0.360) | (0.330) | (0.480) | (0.370) | (1.000) | (0.670) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel B: E-Learning (N=955)** | | | | | | |
| Incentive | -0.002 | 0.001 | 0.007 | 0.009 | 0.005 | 0.009 |
| | (0.910) | (0.980) | (0.760) | (0.760) | (0.770) | (0.590) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel C: Parental Involvement (N=930)** | | | | | | |
| Incentive | 0.030 | 0.036 | 0.041 | 0.042 | 0.009 | 0.014 |
| | (0.210) | (0.150) | (0.150) | (0.130) | (0.620) | (0.430) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel D: Integration Migration (N=930)** | | | | | | |
| Incentive | -0.015 | -0.016 | 0.006 | 0.002 | 0.006 | 0.005 |
| | (0.470) | (0.440) | (0.870) | (0.930) | (0.640) | (0.750) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |

*Note:* This table summarizes .Probit marginal coeff *=0.05. RANDOMIZATION INFERENCE WITH 100 REP

Table 19: Incentive Treatment - Share Budget

| | (1)<br>Pos. Response | (2)<br>Pos. Response | (3)<br>Responded | (4)<br>Responded | (5)<br>Filled Out Survey | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| **Panel A: Pooled (N=3305)** | | | | | | |
| Share Budget | 0.005 | 0.013 | -0.000 | 0.013 | -0.004 | 0.004 |
| | (0.734) | (0.396) | (0.991) | (0.619) | (0.727) | (0.724) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel B: E-Learning (N=955)** | | | | | | |
| Share Budget | -0.024 | -0.014 | -0.017 | -0.008 | 0.007 | 0.015 |
| | (0.318) | (0.597) | (0.628) | (0.845) | (0.699) | (0.515) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel C: Parental Involvement (N=930)** | | | | | | |
| Share Budget | 0.013 | 0.027 | 0.020 | 0.037 | -0.009 | 0.014 |
| | (0.641) | (0.421) | (0.610) | (0.417) | (0.676) | (0.517) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel D: Integration Migration (N=930)** | | | | | | |
| Share Budget | -0.010 | -0.009 | 0.012 | 0.007 | 0.009 | 0.007 |
| | (0.602) | (0.661) | (0.757) | (0.859) | (0.709) | (0.761) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |

*Note:* This table summarizes .Probit marginal coeff *=0.05. BOOTSTRAPPED STD ERR WITH 200 REP. P values

Table 20: Incentive Treatment - Share Budget

| | (1)<br>Pos. Response | (2)<br>Pos. Response | (3)<br>Responded | (4)<br>Responded | (5)<br>Filled Out Survey | (6)<br>Filled Out Survey |
|---|---|---|---|---|---|---|
| **Panel A: Pooled (N=3305)** | | | | | | |
| Share Budget | 0.005 | 0.013 | -0.000 | 0.013 | -0.004 | 0.004 |
| | (0.710) | (0.570) | (0.330) | (0.520) | (0.290) | (0.700) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel B: E-Learning (N=955)** | | | | | | |
| Share Budget | -0.024 | -0.014 | -0.017 | -0.008 | 0.007 | 0.015 |
| | (0.080) | (0.210) | (0.240) | (0.420) | (0.690) | (0.290) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel C: Parental Involvement (N=930)** | | | | | | |
| Share Budget | 0.013 | 0.027 | 0.020 | 0.037 | -0.009 | 0.014 |
| | (0.810) | (0.520) | (0.860) | (0.330) | (0.150) | (0.700) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |
| **Panel D: Integration Migration (N=930)** | | | | | | |
| Share Budget | -0.010 | -0.009 | 0.012 | 0.007 | 0.009 | 0.007 |
| | (0.860) | (0.940) | (0.560) | (0.610) | (0.520) | (0.640) |
| School-level contr. | No | Yes | No | Yes | No | Yes |
| Munic.-level contr. | No | Yes | No | Yes | No | Yes |

*Note:* This table summarizes .Probit marginal coeff *=0.05. Randomization with 100 REP

# D Online Appendix - Evidence From School Experiments in the Largest Cities in North-Rhine-Westphalia

Table 21: Descriptive Statistics: NRW-Data vs Experiments

| | (1)<br>NRW Secondary Schools | (2)<br>Riener and Wagner, 2019 | (3)<br>Fischer and Wagner, 2018 | (4)<br>NRW Elementary Schools | (5)<br>Wagner, 2016 | (6)<br>(1) vs. (2), p-value | (7)<br>(1) vs. (3), p-value | (8)<br>(4) vs. (5), p-value |
|---|---|---|---|---|---|---|---|---|
| Av. comp. teaching hours | 21.100 | 21.619 | 21.156 | 21.160 | 22.106 | 0.031 | 0.892 | 0.000 |
| | (0.074) | (0.186) | (0.181) | (0.055) | (0.126) | | | |
| Students in day care | 90.033 | 81.945 | 77.628 | 100.000 | 100.000 | 0.000 | 0.000 | |
| | (0.401) | (1.430) | (1.528) | (0.000) | (0.000) | | | |
| Age of teachers | 41.125 | 40.400 | 39.934 | 38.925 | 37.627 | 0.112 | 0.015 | 0.000 |
| | (0.141) | (0.315) | (0.312) | (0.187) | (0.321) | | | |
| Students migration background | 27.797 | 47.290 | 43.649 | 31.635 | 46.398 | 0.000 | 0.000 | 0.000 |
| | (0.491) | (1.776) | (1.943) | (0.492) | (1.555) | | | |
| Students migrated | 6.816 | 12.959 | 9.723 | 5.885 | 9.387 | 0.000 | 0.000 | 0.000 |
| | (0.218) | (0.989) | (0.850) | (0.167) | (0.538) | | | |
| Parents migrated | 25.997 | 39.756 | 37.064 | 29.854 | 42.890 | 0.000 | 0.000 | 0.000 |
| | (0.466) | (1.629) | (1.788) | (0.465) | (1.464) | | | |
| Number of students | 447.099 | 662.125 | 758.510 | 209.967 | 242.249 | 0.000 | 0.000 | 0.000 |
| | (8.503) | (24.713) | (25.601) | (1.933) | (5.065) | | | |
| Female students | 46.851 | 48.838 | 50.053 | 49.033 | 49.040 | 0.000 | 0.000 | 0.000 |
| | (1.495) | (0.831) | (0.940) | (0.101) | (0.231) | | | |
| Non-German students | 7.853 | 16.697 | 12.002 | 6.604 | 11.372 | 0.000 | 0.000 | 0.000 |
| | (0.219) | (1.047) | (0.767) | (0.175) | (0.555) | | | |
| Non-German female students | 3.535 | 50.834 | 52.382 | 3.218 | 50.542 | 0.000 | 0.000 | 0.000 |
| | (0.100) | (1.004) | (1.133) | (0.089) | (0.944) | | | |
| Share full time empl. teachers | 62.881 | 58.162 | 56.081 | 48.541 | 55.538 | 0.000 | 0.000 | 0.000 |
| | (0.357) | (0.975) | (0.981) | (0.403) | (0.947) | | | |
| Students speak no German at home | 14.114 | 28.530 | 24.534 | 17.460 | 30.693 | 0.000 | 0.000 | 0.000 |
| | (0.359) | (1.547) | (1.559) | (0.409) | (1.493) | | | |
| Number of classes | 15.310 | 18.881 | 20.182 | 9.124 | 10.131 | 0.000 | 0.000 | 0.000 |
| | (0.194) | (0.537) | (0.602) | (0.079) | (0.201) | | | |

*Note:* This table summarizes . Standard Deviation in parentheses.

Table 22: Descriptive Statistics: NRW-Data - Municipalities vs Cities

| | (1)<br>Municipality | (2)<br>Independent City | (3)<br>Overall | (4)<br>(1) vs. (2),<br>p-value |
|---|---|---|---|---|
| Av. comp. teaching hours | 22.968 | 23.718 | 23.216 | 0.000 |
| | (0.049) | (0.060) | (0.038) | |
| Teachers fullt time empl. teachers | 17.367 | 20.287 | 18.332 | 0.000 |
| | (0.260) | (0.420) | (0.223) | |
| Teachers part time empl. teachers | 9.814 | 11.056 | 10.224 | 0.000 |
| | (0.127) | (0.208) | (0.110) | |
| Age of teachers | 46.613 | 45.709 | 46.314 | 0.000 |
| | (0.067) | (0.084) | (0.053) | |
| Number of classes | 12.304 | 13.873 | 12.823 | 0.000 |
| | (0.114) | (0.178) | (0.097) | |
| Number of students | 332.277 | 379.267 | 347.797 | 0.000 |
| | (4.667) | (7.300) | (3.959) | |
| Female students | 0.469 | 0.472 | 0.470 | 0.262 |
| | (0.001) | (0.002) | (0.001) | |
| Non-German students | 0.074 | 0.138 | 0.095 | 0.000 |
| | (0.001) | (0.003) | (0.001) | |
| Non-German female students | 0.035 | 0.065 | 0.045 | 0.000 |
| | (0.001) | (0.001) | (0.001) | |
| Students migration background | 0.301 | 0.436 | 0.345 | 0.000 |
| | (0.003) | (0.005) | (0.003) | |
| Students migrated | 0.063 | 0.096 | 0.074 | 0.000 |
| | (0.001) | (0.003) | (0.001) | |
| Parents migrated | 0.283 | 0.394 | 0.320 | 0.000 |
| | (0.003) | (0.005) | (0.003) | |
| Students speak no German at home | 0.162 | 0.288 | 0.204 | 0.000 |
| | (0.003) | (0.005) | (0.003) | |
| Students in day care | 0.951 | 0.940 | 0.947 | 0.004 |
| | (0.002) | (0.003) | (0.002) | |
| Female students in day care | 0.443 | 0.440 | 0.442 | 0.343 |
| | (0.002) | (0.002) | (0.001) | |
| *N* | 3670 | 1810 | 5480 | |
| Proportion | 0.670 | 0.330 | 1.000 | |

*Note:* This table summarizes . Standard Deviation in parentheses.

Table 23: Descriptive Statistics: NRW-Data Sec. Schools - Rural Areas vs Cities

| | (1)<br>Municipality | (2)<br>Independent City | (3)<br>Overall | (4)<br>(1) vs. (2),<br>p-value |
|---|---|---|---|---|
| Av. comp. teaching hours | 23.006 | 23.661 | 23.211 | 0.000 |
| | (0.077) | (0.098) | (0.061) | |
| Teachers fullt time empl. teachers | 27.124 | 32.955 | 28.952 | 0.000 |
| | (0.407) | (0.680) | (0.355) | |
| Teachers part time empl. teachers | 12.608 | 16.096 | 13.701 | 0.000 |
| | (0.224) | (0.363) | (0.194) | |
| Age of teachers | 47.731 | 47.162 | 47.552 | 0.001 |
| | (0.098) | (0.114) | (0.077) | |
| Number of classes | 15.554 | 18.719 | 16.546 | 0.000 |
| | (0.191) | (0.294) | (0.163) | |
| Number of students | 457.361 | 555.575 | 488.148 | 0.000 |
| | (8.313) | (13.360) | (7.133) | |
| Female students | 0.447 | 0.448 | 0.448 | 0.894 |
| | (0.003) | (0.004) | (0.002) | |
| Non-German students | 0.081 | 0.148 | 0.102 | 0.000 |
| | (0.002) | (0.005) | (0.002) | |
| Non-German female students | 0.037 | 0.067 | 0.046 | 0.000 |
| | (0.001) | (0.002) | (0.001) | |
| Students migration background | 0.278 | 0.405 | 0.318 | 0.000 |
| | (0.004) | (0.008) | (0.004) | |
| Students migrated | 0.067 | 0.099 | 0.077 | 0.000 |
| | (0.002) | (0.004) | (0.002) | |
| Parents migrated | 0.260 | 0.363 | 0.293 | 0.000 |
| | (0.004) | (0.007) | (0.004) | |
| Students speak no German at home | 0.143 | 0.251 | 0.177 | 0.000 |
| | (0.003) | (0.007) | (0.003) | |
| Students in day care | 0.900 | 0.869 | 0.890 | 0.000 |
| | (0.004) | (0.006) | (0.003) | |
| Female students in day care | 0.394 | 0.379 | 0.389 | 0.000 |
| | (0.002) | (0.003) | (0.002) | |
| *N* | 1809 | 826 | 2635 | |
| Proportion | 0.687 | 0.313 | 1.000 | |

*Note:* This table summarizes . Standard Deviation in parentheses.

Table 24: Descriptive Statistics: NRW-Data Elemen. Schools - Rural Areas vs Cities

| | (1)<br>Municipality | (2)<br>Independent City | (3)<br>Overall | (4)<br>(1) vs. (2),<br>p-value |
|---|---|---|---|---|
| Av. comp. teaching hours | 22.931 | 23.766 | 23.220 | 0.000 |
| | (0.060) | (0.075) | (0.048) | |
| Teachers fullt time empl. teachers | 7.882 | 9.653 | 8.495 | 0.000 |
| | (0.090) | (0.139) | (0.078) | |
| Teachers part time empl. teachers | 7.098 | 6.826 | 7.004 | 0.058 |
| | (0.085) | (0.115) | (0.068) | |
| Age of teachers | 45.525 | 44.490 | 45.167 | 0.000 |
| | (0.085) | (0.108) | (0.067) | |
| Number of classes | 9.145 | 9.806 | 9.374 | 0.000 |
| | (0.075) | (0.099) | (0.060) | |
| Number of students | 210.688 | 231.269 | 217.806 | 0.000 |
| | (1.820) | (2.417) | (1.466) | |
| Female students | 0.490 | 0.492 | 0.491 | 0.308 |
| | (0.001) | (0.001) | (0.001) | |
| Non-German students | 0.068 | 0.129 | 0.089 | 0.000 |
| | (0.002) | (0.004) | (0.002) | |
| Non-German female students | 0.033 | 0.064 | 0.044 | 0.000 |
| | (0.001) | (0.002) | (0.001) | |
| Students migration background | 0.323 | 0.461 | 0.371 | 0.000 |
| | (0.005) | (0.008) | (0.004) | |
| Students migrated | 0.059 | 0.094 | 0.071 | 0.000 |
| | (0.002) | (0.003) | (0.002) | |
| Parents migrated | 0.305 | 0.420 | 0.345 | 0.000 |
| | (0.004) | (0.007) | (0.004) | |
| Students speak no German at home | 0.181 | 0.319 | 0.229 | 0.000 |
| | (0.004) | (0.007) | (0.004) | |
| Students in day care | 1.000 | 1.000 | 1.000 | |
| | (0.000) | (0.000) | (0.000) | |
| Female students in day care | 0.490 | 0.492 | 0.491 | 0.308 |
| | (0.001) | (0.001) | (0.001) | |
| *N* | 1861 | 984 | 2845 | |
| Proportion | 0.654 | 0.346 | 1.000 | |

*Note:* This table summarizes . Standard Deviation in parentheses.

Table 25: Results - Other Experiments: Responded

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | WagnerRiener2015 | | Wagner2016 | | FischerWagner2018 | |
| Av. comp. teaching hours | 0.018 | (0.039) | 0.028 | (0.026) | 0.037 | (0.033) |
| Age of teachers (full time empl.) | -0.003 | (0.005) | 0.008** | (0.003) | 0.011** | (0.003) |
| Students migration background | -0.637 | (0.826) | -0.083 | (0.671) | 0.633 | (0.613) |
| Students migrated | 0.882*** | (0.024) | -0.216 | (0.228) | 0.290 | (0.533) |
| Parents migrated | 0.732* | (0.369) | 0.412 | (0.694) | -0.203 | (0.266) |
| Number of students | -0.000 | (0.000) | 0.000 | (0.001) | 0.001* | (0.000) |
| Female students | -0.858* | (0.420) | 0.118 | (0.313) | -0.732 | (0.619) |
| Non-German students | -0.956 | (0.522) | 0.388 | (0.483) | -0.513 | (0.473) |
| Non-German female students | 0.378 | (0.233) | 0.104 | (0.086) | 0.422 | (0.533) |
| Share fullt time empl. teachers | -0.439 | (0.787) | -0.281 | (0.394) | -0.969 | (0.490) |
| Students speak no German at home | -0.226 | (0.821) | -0.471 | (0.190) | -0.243 | (0.500) |
| Number of classes | -0.003 | (0.007) | 0.004 | (0.032) | -0.022 | (0.007) |
| Students in day care | -0.337 | (0.521) | | | 1.075 | (0.325) |
| Vocational (Gesamtsch.) | 0.128 | (0.234) | | | | |
| Vocational (Hauptsch.) | 0.209 | (0.180) | | | | |
| Vocational (Realsch.) | 0.068 | (0.212) | | | -0.350* | (0.162) |
| *N* | 166 | | 243 | | 141 | |

*Note:* This table summarizes . p-vlaues are adjusted for MHT using RomanoWolf with 100 reps. * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

Table 26: Results - Other Experiments: Responded

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | WagnerRiener2015 | | Wagner2016 | | FischerWagner2018 | |
| Av. comp. teaching hours | 0.018 | (0.982) | 0.028 | (0.504) | 0.037 | (0.954) |
| Age of teachers (full time empl.) | -0.003 | (0.791) | 0.008** | (0.005) | 0.011 | (0.961) |
| Students migration background | -0.637 | (0.817) | -0.083 | (0.976) | 0.633 | (0.969) |
| Students migrated | 0.882 | (0.710) | -0.216 | (0.614) | 0.290 | (0.995) |
| Parents migrated | 0.732 | (0.613) | 0.412 | (0.871) | -0.203 | (0.963) |
| Number of students | -0.000 | (0.975) | 0.000 | (0.958) | 0.001 | (0.971) |
| Female students | -0.858 | (0.882) | 0.118 | (0.796) | -0.732 | (0.899) |
| Non-German students | -0.956 | (0.767) | 0.388 | (0.768) | -0.513 | (0.901) |
| Non-German female students | 0.378 | (0.194) | 0.104 | (0.717) | 0.422 | (0.671) |
| Share fullt time empl. teachers | -0.439 | (0.967) | -0.281 | (0.598) | -0.969 | (0.910) |
| Students speak no German at home | -0.226 | (0.918) | -0.471 | (0.507) | -0.243 | (0.987) |
| Number of classes | -0.003 | (0.956) | 0.004 | (0.950) | -0.022 | (0.981) |
| Students in day care | -0.337 | (0.964) | | | 1.075 | (0.897) |
| Vocational (Gesamtsch.) | 0.128 | (0.901) | | | | |
| Vocational (Hauptsch.) | 0.209 | (0.957) | | | | |
| Vocational (Realsch.) | 0.068 | (0.989) | | | -0.350 | (0.835) |
| *N* | 166 | | 243 | | 141 | |

*Note:* This table summarizes .BOOTSTRAPPED. P values * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

Table 27: Results - Other Experiments: Pos. Resp.

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | Riener and Wagner, 2019 | | Wagner, 2016 | | Fischer and Wagner, 2018 | |
| Av. comp. teaching hours | 0.000 | (0.016) | 0.033 | (0.015) | -0.015 | (0.019) |
| Age of teachers (full time empl.) | 0.005 | (0.009) | 0.006 | (0.003) | -0.002 | (0.002) |
| Students migration background | -0.156 | (0.168) | 0.100 | (0.357) | -0.441 | (0.187) |
| Students migrated | 0.625** | (0.052) | 0.032 | (0.147) | -0.021 | (0.192) |
| Parents migrated | 0.127 | (0.056) | 0.059 | (0.392) | 0.201 | (0.109) |
| Number of students | -0.000 | (0.000) | 0.001 | (0.001) | -0.000 | (0.000) |
| Female students | -0.754 | (0.500) | 0.286 | (0.551) | -0.335 | (0.221) |
| Non-German students | -1.280 | (0.500) | 0.602 | (0.417) | 0.175 | (0.060) |
| Non-German female students | 0.112 | (0.350) | 0.173*** | (0.064) | 0.273 | (0.261) |
| Share fullt time empl. teachers | -0.561 | (0.233) | -0.272 | (0.170) | -0.105 | (0.234) |
| Students speak no German at home | 0.344 | (0.296) | -0.426** | (0.141) | 0.212 | (0.126) |
| Number of classes | 0.015 | (0.008) | -0.006 | (0.020) | 0.006 | (0.003) |
| Students in day care | -0.488 | (0.360) | | | 0.027 | (0.136) |
| Vocational (Realsch.) | 0.132 | (0.177) | | | 0.048 | (0.071) |
| Vocational (Gesamtsch.) | 0.069 | (0.109) | | | | |
| Vocational (Hauptsch.) | 0.367** | (0.170) | | | | |
| *N* | 166 | | 243 | | 141 | |

*Note:* This table summarizes . p-vlaues are adjusted for MHT using RomanoWolf with 100 reps. * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

Table 28: Results - Other Experiments: Pos. Resp.

| | (1) | | (2) | | (3) | |
|---|---|---|---|---|---|---|
| | WagnerRiener2015 | | Wagner2016 | | FischerWagner2018 | |
| Av. comp. teaching hours | 0.000 | (1.000) | 0.033 | (0.971) | -0.015 | (0.995) |
| Age of teachers (full time empl.) | 0.005 | (0.996) | 0.006 | (0.961) | -0.002 | (0.994) |
| Students migration background | -0.156 | (0.998) | 0.100 | (0.992) | -0.441 | (0.987) |
| Students migrated | 0.625 | (0.985) | 0.032 | (0.995) | -0.021 | (0.999) |
| Parents migrated | 0.127 | (0.998) | 0.059 | (0.996) | 0.201 | (0.991) |
| Number of students | -0.000 | (0.994) | 0.001 | (0.982) | -0.000 | (0.997) |
| Female students | -0.754 | (0.992) | 0.286 | (0.982) | -0.335 | (0.972) |
| Non-German students | -1.280 | (0.974) | 0.602 | (0.969) | 0.175 | (0.992) |
| Non-German female students | 0.112 | (0.992) | 0.173 | (0.964) | 0.273 | (0.988) |
| Share fullt time empl. teachers | -0.561 | (0.997) | -0.272 | (0.967) | -0.105 | (0.996) |
| Students speak no German at home | 0.344 | (0.991) | -0.426 | (0.971) | 0.212 | (0.994) |
| Number of classes | 0.015 | (0.908) | -0.006 | (0.993) | 0.006 | (0.995) |
| Students in day care | -0.488 | (0.996) | | | 0.027 | (0.999) |
| Vocational (Gesamtsch.) | 0.069 | (0.998) | | | | |
| Vocational (Hauptsch.) | 0.367 | (0.995) | | | | |
| Vocational (Realsch.) | 0.132 | (0.999) | | | 0.048 | (0.998) |
| *N* | 166 | | 243 | | 141 | |

*Note:* This table summarizes . BOOTSTRAPPED. P values * smaller 0.05, ** smaller 0.01, *** smaller 0.001.

# E Online Appendix - Description of background data, Recruitment Email, and Survey

### Description of Variables - School level

*Type of school:* There are 12 different school types in NRW. Among them elementary school, high school and three types of vocational school (Hauptschule, Realschule, Gesamtschule) are the most prominent school types representing appox. 82% of all schools. The remaining 7 school types are subsummed as "Other school types". Elementary school in Germany runs from age 6 to 10 and thereafter students are tracked into secondary exucation. *Hauptschule* (grades 5 to 9 or 10) provides pupils with a basic general education that prepares them for a vocational job, *Realschule* (grades 5 to 10) also prepares students for a vocational job, but also offers the possibility to attend the advanced level of the high school if grades are good enough, *Gesamtschule* (grades 5 to 10 or 12) offers a longer period of common learning and the possibility to obtain all degrees of secondary education, and *High School* - *Gymnasium* (grades 5 to 12) is the most academic school type preparing students to apply to university.

*Gender of headmaster:* Gender of the headmaster was obtained from schools' websites.

*Average compulsory teaching hours:* For each school, we know the sum of how many compulsory hours teachers have to teach. The average compulsory teaching hour is the sum of compulsory teaching hours divided by the sum of all teachers (full time employed, part time employed, trainee teachers).

*Age of teachers (full time employed):* Average age of all full time employed teachers within a school.

*Share full time employed teachers:* Share of teachers who are full time employed.

*Students in day care:* Share of students attending afternoon childcare.

*Students migrated:* Share of students not born in Germany (migrated to Germany with or without family manmbers).

*Parents migrated:* Share of students with at least one parent not born in Germany (includes students born in Germany if at least one parent not born in Germany).

*Students migration background:* Share of students with some migration background in the family (one parent or both parents not born in Germany and/or child not born in Germany). Note that this variable is not the sum of students migrated and parents migrated. The sum of students migrated and parents migrated would double count students that migrated together with their parents.

*Number of students:* The total number of students attending the school.

*Female students:* Number of female students attending the school.

check whether we use the absolute number or the share

*Non-German students:* Share of students not having a German passport

check share

*Non-German female students:* Share of female students not having a German passport.

*Students speak no German at home:* Share of students who do not speak German with their parents.

*Number of classes:* Total number of classes (all grade levels) within a school.

*Municipality level data*

*Inhabitants:* Number of inhabitants of the municipality.

*Status married:* Share of inhabitants being married.

*Unemployment rate:* Statistic for the current period on the share of unemployed workers.

*Voter turnout:* Share of eligible voters who have voted.

*Elections party [name of party]:* Share of votes for the respective political party.

*German citizenship:* Share of people with German citizenship.

*Education: High School:* Share of people with high school degree.

*Education: Uni access:* Share of people with university degree.

check whether this and High School is correct

*Land prices in 2014:* Land prices in corresponding cities in 2014.

*Share people aged 64 or older:* Share of people aged 64 years or older.

*Religion Protestant:* Share of protestant people.

*Religion Other:* Share of people not being protestant or catholic.

*Male Workers:* Share of workers being male.

*Social index of municipality:* Index incorporating information on the unemployment rate, social assistance rate, migrant quota and quota of apartments in single-family homes.

# F Online Appendix - Email communication

## F.1 Initial contact email

## F.2 Reminder email

# G Online Appendix - Initial questionnaire