Teachers' desired mobility to disadvantaged schools: Do financial incentives matter?

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Abstract

This paper exploits a 2018 reform of teachers' financial incentives to work in some French disadvantaged schools. Based on this quasi-natural experiment, it evaluates the impact of those incentives on teachers' stated preferences to move to such schools. Using data from the internal human resource management of some educational authority, we find that most responsive teachers have less experience and work already in those areas. Counterfactual simulations suggest that the policy has not hurt other disadvantaged schools, but rather induced some teachers not to remain in their current school or to opt less for regular schools.

JEL Codes: I21, I22, J45.

Keywords: Teacher mobility; Financial incentives; Stated preferences; Rank-ordered choices; Disadvantaged schools.

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1 Introduction

To attract and retain good teachers is a challenge that disadvantaged schools have to face in many countries. In France, the allocation of public-tenured teachers results from a centralized mechanism, which helps mitigate the unequal distribution of high-quality teachers (e.g., with high qualification and more experience) over the territory. Nevertheless, teachers working in disadvantaged schools are less qualified, have less experience and are more likely to be contract staff (Benhenda, 2020). In turn, this allocation has adverse consequences on students' outcomes: the link between teacher quality and student achievement has been widely studied, and on the whole, the literature suggests that teachers' experience has a positive impact on pupils' skills (Rockoff, 2004). In contrast, disadvantaged students are often taught by low-quality teachers (Chetty, Friedman, and Rockoff, 2014).

What can the policy maker do in order to temper such inequality? A commonly used public policy instrument consists in offering extra money to teachers working in disadvantaged schools. In practice though, since public teachers are civil servants, their wages can hardly vary within a grade, given a certain level of experience. The Department of Education may provide teachers working in those areas with supplementary financial incentives, namely an extra pecuniary bonus on top of their base salary. The effects of such programs on effective exit mobility have been studied, and the empirical evidence is rather mixed. Clotfelter et al. (2008) found a positive and significant effect of offering teachers an €1,800 annual bonus, in terms of turnover rates at disadvantaged schools in North Carolina. According to Prost (2013), an annual €960 bonus program in France was insufficient to retain teachers in disadvantaged schools. These differences in impact doubtlessly arise from differences in incentive levels.

Our contribution is twofold. First, we rely on a substantial variation of the annual bonus granted to severely disadvantaged schools from 2018 onwards. We take advantage of this nationwide change decided by the policy maker as a quasi-natural experiment, which allows us to exploit the corresponding source of exogenous variation, and thus to infer the causal impact of financial incentives. In 2014, about 730 middle-schools were labelled *Réseau d'Éducation Prioritaire* (REP), and 360 middle-schools, *Réseau d'Éducation Prioritaire renforcé* (REP$^+$). Both types of schools are disadvantaged, but REP$^+$ schools are considered the most challenging in terms of working conditions. From that time onwards, teachers working in REP schools have received a €1,732 annual bonus, this amount remaining unchanged, while in REP$^+$ schools the bonus was upgraded from €2,312 between 2014 and 2017, to €3,479 in 2018 and to €4,646 in 2019. These financial incentives represent nearly 13% of teachers' annual wage, and up to 20% as regards teachers less than 30. Compared with other settings where such pecuniary incentives have been provided, the current framework provides with a high-intensity treatment that is also more salient to the concerned individuals. Second, we investigate whether this gradual but large increase of the bonus at the intensive margin has an impact on desired teacher mobility, rather than on effective mobility, i.e. on actual assignments. This is made possible because we dispose of unique application data issued by the internal human resource management of the French Department of Education in which we observe the rank-ordered lists teachers requested for their school choices. Any teacher asking for a transfer has to fill out such lists indicating her stated preferences, including those who seek a placement in disadvantaged schools. We examine not only occurrences, but also absolute rankings of REP$^+$ schools in those lists as our main outcomes.

Our empirics is therefore based on rank-ordered lists filled out by teachers working in Montpellier's educational authority from 2015 to 2019. First, we conduct a reduced-form analysis that consists in a difference-in-differences approach based on the comparison between REP$^+$ schools, the treatment group, and REP schools viewed as the comparison group. Both types of schools are disadvantaged: their proximity as well as their different exposure to the reform make them natural candidates for such an analysis. We empirically assess the validity of our empirical strategy by performing an event study analysis that supports the absence of any pre-trend before the reform; hence the common trend assumption (CTA) cannot be rejected in our data. Our results point out to some positive, significant impact of the REP$^+$ bonus. The larger the bonus, the higher the share of REP$^+$ schools among top 1 choices: our point estimate of the impact of a supplementary €1,000 per year amounts to +1.4pp on the corresponding share (slightly less than 4% before 2018), resulting in an overall average treatment effect (ATE) of +3.1pp over the period, most of the change having intervened between 2017 and 2018 as confirmed by the event study. As far as absolute rankings are concerned, we consider alternative identification strategies relying on various comparison groups (REP schools, regular schools and both of them), which all point out to significant estimates of the ATE, consistently with a better ranking (about -1 on the list) of REP$^+$ schools consecutive to the reform.

Second, we estimate that an annual bonus of about €3,000 compensates the lower attractiveness of REP$^+$ schools with respect to regular schools. From that viewpoint, the 2018 reform has resulted in some kind of net subsidy targeted toward the former since the current bonus, €4,646, exceeds that amount. We then simulate counterfactual scenarios in which the bonus would not have been increased, or without any bonus at all -another way to measure the causal impact of financial incentives. We adopt here the policy maker's point of view, not teachers' one: such an approach requires therefore to aggregate the data at the school-year level, and hence to take a stand at individual determinants of school choices. Nevertheless, our model performs quite well: when assessing how it fits observed variations in the share of REP$^+$ schools among top 1 choices, simulated changes related to the pecuniary bonus alone explain a large part of these variations, while the remaining, unobserved change (due to residuals) is fairly small, suggesting that confounding factors, if any, would have been neutralized. Counterfactual experiments point out to a slightly convex effect of money on that share: about +2pp for the first €2,300 (i.e. moving from no bonus to a +€2,300 annual bonus) but nearly +3.5pp for the following €2,300 (i.e. moving from a +€2,300 bonus to a + €4,600 annual bonus). The latter result is likely due to the presence of switching costs: it suggests that financial incentives need to be substantial in order to truly induce teachers join disadvantaged schools. Remember that, contrary to other frameworks where incentives are weak, the current programs involve annual bonuses which represent a non-negligible part of teachers' annual wage. As a result, this paper sheds new light on the level of financial incentives which is required for such policies to reach their target. Finally, our counterfactual simulations show that 90% of the rise in REP$^+$ share among top 1 choices is due to their higher relative attractiveness with respect to the outside option (i.e. the option of not requesting any transfer) while the remaining 10% stems from their higher relative attractiveness with respect to regular schools. Importantly, we find that stronger financial incentives have enhanced the attractiveness of REP$^+$ schools without hurting REP schools' one, a yet recurrent critique made against the reform. We interpret this further result as empirical evidence against a possible contamination of the treatment to REP schools, which comforts the identification strategy adopted in the reduced-form approach based on REP schools as a valid comparison group.[1]

Third, we investigate whether teachers respond differently to financial incentives depending on their observed characteristics (qualification, experience, current workplace being a disadvantaged school already, etc.). To this goal, we estimate

[1] To address any concern about the stable unit treatment value assumption (SUTVA) which rules out any (adverse) spillover of the treatment, and on which the previous reduced-form relies, we consider here a discrete-choice model, which takes instead the rivalry of alternatives into account, when performing counterfactual experiments. Indeed, the increased bonus in REP$^+$ schools may indirectly affect the comparison group (REP schools) by making it relatively less attractive: it is therefore likely to decrease its share since school shares sum up, mechanically, to one every year. A discrete-choice model avoids this shortcoming of the reduced-form approach which may overestimate the pure effect of financial incentives.

a rank-ordered Logit at the most disaggregated level, namely individual-year (i.e. list)-alternative (i.e. school), taking advantage of the richness of our data. By definition, such a model is appropriate to infer preferences from ordinal choices. Moreover, the model somehow embeds the previous difference-in-differences approach in that the source of identifying variation of the response to financial incentives relies on the differential change of the annual bonus over time, depending on the type of disadvantaged school.[2] Results point out to an average cost of travel time equal to the opportunity cost of time, namely market hourly wage. In both the current and the previous approach, the ranking of school quality (regular, REP and REP$^+$) can be inferred from the data and we estimate that an annual bonus of +€3,000 would compensate for the relative disadvantage in terms of attractiveness of REP+ *vis-à-vis* regular schools. Last, we find that teachers' reaction to such incentives is in fact very heterogeneous and more pronounced for those at the beginning of their career, and already working in a disadvantaged school. Since our model includes travel time as an explanatory variable, we are able to quantify the effect of the bonus and express it in terms of equivalent commuting time saved daily.

The current article connects to a wide literature devoted to teacher recruitment in disadvantaged schools. Numerous empirical studies have shown that teachers highly value students' characteristics in their decision to remain in a school. For example, Hanushek, Kain, and Rivkin (2004), Falch and Strøm (2005) and Scafidi, Sjoquist, and Stinebrickner (2007), on the basis of panel data on post holders in public schools in the '90s, brought to light the factors which increase teachers' probability of leaving their schools, in Texas, Norway and Georgia respectively. Results are quite similar in these three settings. In Texas, teachers' mobility turns out to be much more related to students' characteristics (race and achievement) rather than to their own wage. In Norway, teachers tend to leave schools with a high proportion of minority students and a high share of students with special needs. In Georgia, teachers are more likely to leave schools with lower test scores, lower family incomes, or a higher proportion of minorities -the latter matters most in their decision. Although the French educational system has some specific features (teachers are civil servants and school assignment is centralized), similar patterns can be found in the data. On the basis of exhaustive mobility data on French teachers from 1987 to 1992, Prost (2013) found that teachers tend to switch schools when they face a high proportion of less able students, students from minority groups or from economically disadvantaged backgrounds.

[2]Once again, this structural, discrete-choice model enables us to address any concern about SUTVA.

In order to enhance the attractiveness of disadvantaged schools, policy tools include offering both pecuniary and non-pecuniary incentives to teachers in order to attract and retain a high-quality staff, so that school heads do not have to fill vacancies with inexperienced or less qualified teachers.[3] This is all the more important that teacher quality, especially experience, has apparently a positive impact on students' achievement (Rockoff, 2004; Rivkin, Hanushek, and Kain, 2005). The effects of these policies are yet not clear-cut and depend on the institutional background, the design and the extent of incentives. For instance, the selection of disadvantaged schools by authorities in such programs could have stigmatising effects, as suggested by Beffy and Davezies (2013). On the basis of a three-year panel from North-Carolina, Clotfelter et al. (2008) found a significant effect of an annual $1,800 bonus on disadvantaged schools' turnover rates. In Norway, from 1992 to 2003, teachers got a premium of about 10% when they worked in schools with a high degree of teacher vacancies; Falch (2011) found that this wage premium reduced the probability of resignation. On the contrary, Prost (2013), studying a program implemented in September 1989, found that the bonus amounts, which increased from €25[4] per month in 1990 to €79 in 1992, were insufficient to retain teachers in these schools. Programs can also aim at improving teacher quality. Cowan and Goldhaber (2018) documented an increase in the share of certified teachers in disadvantaged schools consecutive to a Washington state program. The latter awarded some bonus (15% of the annual salary) to national board certified teachers working in those schools. Last, some programs are not based on financial incentives. A French program implemented in 2005 consists in offering teachers who accept to spend more time in disadvantaged schools higher chances of being assigned next to their preferred area, and of choosing their next school. Benhenda and Grenet (2020) evaluated that this program had a statistically significant and positive impact on the number of consecutive years teachers remain in these schools.

The rest of the paper is organized as follows. Section 2 is devoted to the teacher labour market in France and contains a brief history of disadvantaged schooling programs including REP and REP$^+$ bonuses. The data containing stated preferences is described in section 3. Section 4 presents the reduced-form evidence. Counterfactual simulations which quantify the impact of stronger financial incentives on the share of REP$^+$ schools among top 1 choices are exposed in section 5.

[3]In France where assignment system is highly centralized, Benhenda (2020) estimates that the share of contract-staff among teachers in disadvantaged middle-schools in 2014 is about 12%, against less than 8% in other middle-schools.

[4]This amount corresponds namely to an increase of about 2% for a novice teacher, and only 1% for the most qualified or experienced one.

Section 6 explores the heterogeneity of responses and quantifies the relative importance of teachers' preferences for money and time. Section 7 concludes.

2 Institutional background

2.1 Teacher allocation

In France, public-tenured teacher allocation to middle- and high-schools is fully centralized. The procedure takes place each year. To match teachers' requests and human resources needs, two rounds are based on a system with a priority index called *barème*. At the end of a first round called *mouvement inter* (namely a transfer between educational authorities[5]), beginners and tenured teachers who request a transfer are assigned to some regional educational authority. A second round called *mouvement intra* (namely a transfer within a given educational authority) allocates then teachers into the schools themselves. Three types of teachers participate to that second round: beginners, teachers coming from another region but allocated to this very region at the issue of the first round, and teachers who are already assigned to a position in the region, but who are willing to work in another school. In the latter case, teachers have a supplementary option: it is possible for them to remain in the same position if no match can occur at the issue of the second round. By contrast, beginners and teachers coming from another region who were not assigned to any school after that round are allocated where there is a vacancy. During the second round, teachers submit a rank-ordered list (ROL) that comprises between one and twenty choices. Stated choices may correspond either to a school, a teaching supply area or a geographical area; in that latter case, a teacher's choice is considered as fulfilled whenever she is assigned to any school in the desired area. We assume that this centralized mechanism induces teachers to reveal sincerely their preferences, hence available in the corresponding rank-ordered lists, given that (i) the mechanism includes no application cost, (ii) teachers are aware of injunctions coming by both the Department of Education and unions, (iii) they have a good knowledge of the matching process, but also (iv) they face some uncertainty at the time of application. Further theoretical details about the algorithm in charge of teachers' allocation to schools are provided in Appendix C.1.

[5]There are 30 regional educational authorities, or rectorates, all over the French territory.

2.2 Disadvantaged schools programs

Since 1982, the French Department of Education has developed programs targeted towards disadvantaged schools. It aimed at tempering social inequality in educational outcomes, which the policy maker thus acknowledged. Disadvantaged schools were labeled *Zones d'Éducations Prioritaires (ZEP)* at the time, and selected according to socioeconomic criteria. This compensatory educational policy includes both financial and non-financial components: it allocates more resources to these schools (for instance, the ratio of the number of students over the teaching staff is lower), and supports local pedagogical initiatives. As they tend to avoid schools with underprivileged socioeconomic background pupils, teachers working in disadvantaged schools have received a bonus, the expected objectives of which are, admittedly, to attain a greater stability of pedagogical teams and to attract a higher proportion of tenured teachers. The justification for this "affirmative action", or compensatory scheme, which contrasted at the time with centralized principles that usually prevail in French policy rules, is that an "unequal" investment should help recover equality.

ZEP concern primary schools (1^{st} to 5^{th} grades) and lower secondary schools (6^{th} to 9^{th} grades), as well as upper secondary schools (10^{th} to 12^{th}), but to a lesser extent. The number of ZEP schools has been regularly increasing from about 10% of secondary schools in 1982 to nearly 15% at the end of the '90s.

In the mid-'00s, the efficiency of ZEP policies was put into question (Bénabou, Kramarz, and Prost, 2009). A program was then launched, which included non-financial incentives, and which was targeted towards a short list of middle-schools, in order to encourage teachers to stay longer in the same school (Benhenda and Grenet, 2020).

2.3 The 2018 reform

In 2014, a new list of schools was published by the Department of Education, which was deemed to remain unchanged for several years. These schools received a label that was changed from *ZEP* to *Réseau d'Éducation Prioritaire (REP)*, and divided further into *REP* and *REP*$^{+}$. The word *réseau* (network) puts the emphasize on teamwork within educational staff. Furthermore, schools became systematically networked in the sense that, when a secondary school was labeled as REP or REP^{+}, that label also applied to the primary schools from which their pupils come. In the end, this new program includes fewer schools than the initial ZEP program, but financial resources are now targeted in such a way that public funding increased for each of the concerned schools -especially in REP^{+} schools.

Since 2014, REP and REP$^+$ schools as well as the specific resources allocated to those schools have remained remarkably stable. However, an important public policy change occurred from 2018 onwards: the annual bonus granted to each and any teacher working in *REP*$^+$ schools has been gradually increased. Its face value finally doubled in 2019 when compared with 2017. This paper precisely exploits this variation viewed as a quasi-natural experiment, an exogenous shock on financial incentives decided and implemented by the policy maker. In 2015, annual bonuses in REP (resp. REP$^+$) schools were equal to €1,734 (resp. €2,312) and remained unchanged until 2017. During the 2018-2019 period, bonuses did not vary in REP schools while they increased linearly in REP$^+$ schools: €3,479 in 2018 (+50% with respect to baseline year 2017, up to inflation), and €4,646 in 2019 (+100% with respect to baseline year 2017, up to inflation), as shown by Figure 1. [6] Compared with the average annual wage of a teacher aged less than 30,[7] these annual bonuses represent an incremental salary of about 7% in REP, but 9% in REP$^+$ between 2015 and 2017, 14% in 2018 and about 19% in 2019. An advantage of the current setting lies therefore in such a bonus being large and salient to the concerned teachers. Another evidence in favor of the salience of this policy lies in that this increase of the REP$^+$ bonus was announced by candidate Emmanuel Macron during his 2017 presidential campaign. On top of that, there has been no other reform related to priority education in secondary schools which could have acted as a confounding factor during this period. There was a much publicized contemporary reform, though, which consisted in the reduction of class sizes, but this change concerned primary schools only.

3 Data

The application data used here is provided by the human resources department of the Montpellier's educational authority. It contains all assignment preferences stated in the form of rank-ordered lists by teachers in this rectorate between 2015 and 2019. As regards teachers, the data contains information about their gender, their age, their location, the subject they taught, and the school where they currently work (the latter information being available neither for beginners

[6]from 4/3 to 2 and finally to 8/3 times the bonus granted in REP schools.

[7]Thanks to the *Système d'Information sur les Agents du Servive Public (SIASP)*, an administrative database on civil servants' wages, we can compute that wage for teachers under 30 and working in a middle-school of Montpellier's educational authority: about €24,500 on average, including overtime and all types of bonus at the exception of REP- and REP$^+$-specific bonus. For teachers over 40 working in the same schools, a REP-specific bonus constitutes a 5% increase, against 7% for REP$^+$-specific bonus between 2015 and 2017, 10% in 2018 and 14% in 2019.

nor for supply teachers). We also know their qualification, i.e. whether they hold the CAPES or an *Agrégation*,[8] and their seniority. For each school-specific choice, the data includes the school's education level (i.e. whether it is a middle-school or a high-school), its location, its labeling as a REP or a REP^{+}, and the corresponding bonus.

On top of the previous database, we resort to administrative data in order to get an exhaustive set of teachers working within the Montpellier's educational authority between 2015 and 2019. Thanks to that database, we are able to compute annual shares of teachers applying to other schools within the authority (see Table 9). These shares decrease over time: at first sight, the 2018 reform might have encouraged some teachers to join REP+ schools (from nearly 200 choices before to 300 choices after), but had seemingly little impact, at the extensive margin, on the number of teachers requesting any transfer at all -hence on the probability of requesting some transfer since the headcount of teachers in the authority had remained rather stable all over that period. Meanwhile, the average number of choices per rank-ordered list also diminished, which suggests that the reform didn't lead teachers to simply add up REP^{+} schools in their application files. Moreover, the number of choices toward REP schools did not decrease over that period while regular schools were less often elicited on those lists. On the whole, this empirical evidence is consistent with the hypothesis of the reform having an impact at the intensive margin: a same pool of teachers would now favor REP^{+} as opposed to regular schools. However, it could also be that, in the absence of the reform, more teachers would have requested some transfers: in that case, the reform would have induced more teachers to stay in place -perhaps it would have retained teachers in REP^{+} schools, remember Prost (2013) for instance. The model developed in section 5 will enable us to disentangle among the two concurring explanations, namely a crowding-out of choices toward regular schools by REP^{+} schools, as opposed to less transfer requests. Moreover, by breaking down the shares of teachers requiring some transfer according to the type of school where they are currently assigned at the time when they apply, Table 10 actually suggests that the sharpest decrease of teachers willing some transfer is found among REP^{+} schools (about one half). This further empirical evidence is therefore also consistent with the reform retaining teachers in those disadvantaged areas, hence with a higher stability of pedagogical teams. However, the focus of this paper is the causal impact of financial incentives on attracting teachers in disadvantaged schools -not on re-

[8]CAPES accounts for *Certificat d'Aptitude au Professorat dans l'Enseignement Secondaire* and is the main teaching certification level. The *Agrégation* is an advanced qualification obtained at the issue of a more competitive examination.

taining them; on top of that, we lack data on teacher allocation into schools, hence we do not address the latter question.

Moreover, this external dataset enables us to analyze the characteristics of teachers requesting some transfer within the rectorate, i.e. participating to the second round of the process. The SIASP file contains administrative information on each and any civil servant in France, including teachers; it includes usual socio-demographic characteristics (gender, age, residence location, qualification) on top of the annual wage and the current workplace, i.e. school of assignment. Moreover, by comparing variables present both in our database and in SIASP, we are able to proceed to a statistical matching, hence to identify in SIASP the teachers who are requesting a transfer and those who are not. This procedure allows us to characterize the sub-sample of teachers requesting a transfer (mostly the share among all teachers and observed characteristics), which enables us (i) to define the outside option of the model used in section 5 for counterfactual simulations, and (ii) to address possible selection concerns. Appendix Table 12 shows actually that those teachers are more often young (hence they are more likely to be in the early stage of their career), male, hold more frequently the Agrégation,

When they fill out their ROL, teachers have to indicate between 1 and 20 choices. There is substantial dispersion about the number of stated choices, namely the length of these lists: see the corresponding pattern displayed by Figure 2. The capacity constraint of the lists turns out to bind for about 10% of ROLs. In that case, the weakly truth-telling (WTT) assumption (see Appendix C.1) casts doubts, which questions sincere revelation of preferences. On top of cognitive costs among other application costs, the capacity constraint inherent to the mechanism at stake may refrain individuals from truthfully reporting their complete preferences. In a conservative approach, we select therefore these ROLs out of our estimation sample.

Since both teachers' and school locations' zipcodes are available in the data, we are able to compute commuting times thanks to the Metric App' based on centers of corresponding zipcodes.[9] Appendix Table 13 shows an apparently strong correlation between the rank in the list and the commuting time, which suggests that the latter is a key determinant of school choice. It will be therefore necessary to take it into account when modelling teachers' preferences in a structural fashion.

Our working sample is defined as follows. Table 14 in Appendix depicts in details the successive selection steps from the original data provided by the Mont-

[9]When a teacher ranks a school located in her own municipality of residence, the average travel time from this municipality is imputed.

pellier's educational authority. This process leaves us with 5,902 ROLs (about one half of the ROLs included originally) after selecting out:

- ROLs filled out by beginners and teachers coming from another educational authority. Remember from section 2.1 that those teachers are allocated to fill up vacancies when none of their choices can be afforded. This stressful situation is likely to induce strategic behavior here (those ROLs might not reveal preferences);

- ROLs filled out by vocational teachers, vocational schools being unaffected by REP/REP$^+$ programs;

- ROLs where WTT may not hold, mostly lists with 20 stated choices and lists filled out by beginners who can get an extra non-pecuniary bonus, expressed in points on their priority index, as regards their top 1 choice in the list; revealing truthfully one's preferences is then not a dominant strategy.

- ROLs with geographical preferences only. When applying to schools, teachers may opt for geographical choices, which, by definition, correspond to any school within some administrative area -either a municipality, a group of municipalities, a *département* or the whole educational authority. We exclude those choices from our main analysis, but provide with a robustness check in which we split those choices into as many schools as they correspond to, see Appendix C.4 on that topic.

We also select out lists such that a move is suspected, for instance when the minimal commuting time within a list exceeds one hour. We do not consider either specific positions like school counsellors. Last, some specific stated choices (teaching in a jail, for instance) are eliminated, too -but the rest of the list, if any, is not. After this process, we obtain our working sample when we keep remaining school-specific choices only (cf. row 6 of Table 14).

Table 11 in Appendix provides with some descriptive statistics on teachers from the working sample. 60% of them are women. The median age is 42. 85.5% of second-round lists are filled out by holders of the CAPES while the remaining 14.5% correspond to a teacher detaining an *Agrégation*. From year to year, the priority index fluctuates, which is both related to changes in the rules defining its computation and to changes in the composition of teachers asking for a different assignment than their current one. The average expected commuting time is about 28 minutes, but this variable exhibits substantial dispersion; nearly a third of stated choices (32.2%) correspond to schools being located in the same municipality as the one where teachers live.

Figure 3 displays the evolution of the share of REP$^+$ schools among all choices: that share increased from 2.9% in 2017 to 4.9% in 2018 and to 6.9% in 2019. Meanwhile, the share of REP schools hardly changed over that period. The same diagnosis holds more or less as regards that share among top 1 choices: Figure 4 shows that this share moved from 3.7% in 2017 to 6.2% in 2018 and finally to 9.3% in 2019, hence an increase by +2.5pp between 2017 and 2018, and by +5.6pp between 2017 and 2019. Last, much heterogeneity is at stake: Table 15 in Appendix details the corresponding changes in shares depending on teachers' level of education, seniority and qualification.

4 Reduced-form evidence

We propose here a reduced-form evaluation of the impact of stronger financial incentives in REP$^+$ schools from 2018 onwards. To that aim, we consider three outcomes related to teachers' desired mobility towards these schools: (i) the share of REP$^+$ schools among all school choices, (ii) the share of REP$^+$ schools among top 1 choices only, and (iii) the rank of REP$^+$ schools in teachers' rank-ordered lists.

4.1 Share of REP$^+$ schools among all choices

First, we investigate whether the policy change had an impact on the share of REP$^+$ schools among all school choices made explicitly by teachers when filling out their rank-ordered lists. The question we address here is whether observed changes have been truly induced by the policy, and by how much.

Our identification strategy relies on a difference-in-differences approach where REP$^+$ schools constitute the treatment group and REP schools are the comparison group. This method takes advantage of the reform, which we view as a quasi-natural experiment that provides us with exogenous variation in the determinants of teachers' school choices. Our choice of the comparison group is guided (i) by the fact that the annual bonus in REP schools did not vary over the period of study while it increased linearly in REP$^+$ schools from 2018 onwards, and (ii) by the proximity of REP and REP$^+$ schools in terms of observed (and probably unobserved) characteristics as regards the quality of education, teachers and pupils, including their headcount within Montpellier's educational authority (17 REP$^+$ schools and 16 REP schools) and their observed shares (about 3-4%). Based on Figure 3, the share of REP$^+$ schools among all choices increased from 2.9% in 2017 to 4.9% in 2018 and to 6.9% in 2019; meanwhile, the share of REP schools hardly varied from 2017 (3.6%) to 2018 (3.8%), and increased slightly to 4.5% in 2019 (+0.8pp). As a result, the unconditional difference-in-differences estimates of the average treatment effects (ATE) induced by the change in teachers' incentives is $(4.9 - 2.9) - (3.8 - 3.6) = +1.8$pp in 2018, and $(6.9 - 2.9) - (4.5 - 3.6) = +3.1$pp in 2019.

Removing regular schools from the estimation sample here, and denoting by s_{ikt} the share of $k-$type schools ranked by teacher i on year t where $k \in \{\text{REP}, \text{REP}^+\}$, we consider the following estimating equation to implement a difference-in-differences approach with controls:

$$s_{ikt} = a\text{REP}_k^+ + b\text{REP}_k^+ \times \mathbb{1}\{t \geq 2018\} + \nu_t + \mu_{ik} + u_{ikt}, \tag{1}$$

where REP_k^+ is a dummy that equals one when $k = \text{REP}^+$, ν_t are year dummies, μ_{ik} is a fixed-effect accounting for unobserved teachers' taste for $k-$type schools, and u_{ikt} is some idiosyncratic error term, which we assume normally distributed. Standard errors are clustered at the teacher level, allowing for possible autocorrelation of individual preferences over time. The coefficient a refers to the average difference in attractiveness between REP and REP$^+$ schools over the period while b is the main coefficient of interest, i.e. the ATE.

The results are provided by columns 1 and 2 of Table 1. The estimated ATE is significant at usual levels. Column 1 points out to an estimated ATE of nearly +2.4pp, close to the average of unconditional estimates. After controlling for teacher $\times$ $k-$type of school fixed-effects, i.e. for teachers' unobserved taste for either REP schools or REP$^+$ schools, the point estimate falls to +2.1pp (column 2) while remaining statistically significant at usual levels. Figure 5 displays the event study analysis.[10] The corresponding point estimates are fairly close to unconditional estimates in 2018 (a significant +1.7pp) and in 2019 (a significant +2.5pp).

It is possible that previous estimates do not capture the mere effect of financial incentives alone, since non-financial incentives promoting REP$^+$ schools may have also been provided by the policy maker over the period. For instance, the reduction of class size in REP$^+$ primary schools has been implemented from 2017 onwards, through a duplication of first and second grades. Though this public policy did not impact directly REP$^+$ middle-schools nor high-schools, it may have been interpreted as a positive signal towards these disadvantaged areas, and such changes might be viewed as confounding factors in the above estimating equations. To

[10] Replacing b with b_t in equation (1) normalizing point estimates to zero in 2017, i.e. one year before the implementation of the reform.

disentangle the impact of financial from non-financial incentives -in other words, to identify teachers' marginal benefit of income in their elicited preferences for schools-, we now rely on the gradual increase in the pecuniary bonus implemented in REP$^+$ schools from 2018 onwards. Hence we consider the following econometric specification:

$$s_{ikt} = \tilde{a}\text{REP}_k^+ + \tilde{b}\text{BONUS}_{kt} + \tilde{\nu}_t + \tilde{\mu}_{ik} + \tilde{u}_{ikt}, \tag{2}$$

which helps us separate the impact of pecuniary incentives alone (encompassed in the coefficient $\tilde{b}$) from the effect of non-pecuniary incentives (by comparing the corresponding magnitude implied by this estimate $\tilde{b}$ with $\hat{b}$).

The results are displayed by columns (3) and (4) of Table 1. An increase of €1,000 would imply a significant increase of +1.15pp in the share of REP$^+$ schools among all choices. Remembering that the pecuniary bonus increased in REP$^+$ schools by €1,167 both in 2018 and in 2019, this result suggests that financial incentives alone explain most of the effect found above (about +1.3pp every year, to be compared with previous +1.7pp in 2018 and +2.5pp in 2019).

Importantly, the shares of disadvantaged schools have experienced rather similar variations before 2018, which could already be observed based on descriptive statistics, and which the event study analysis confirms. Such empirical evidence claims in favor of the absence of any differential pre-trend that would jeopardize the plausibility of the identification strategy. Put differently, the common trend assumption (CTA), according to which our outcomes would have experienced parallel trends in both comparison and treatment groups, cannot be discarded in the data.

4.2 Share of REP$^+$ schools among top 1 choices

Second, we consider another outcome, replacing previous outcome s, i.e. the share of REP$^+$ schools among all choices, with s_1, i.e. that very share among top 1 choices only, while adopting the same identification strategy as above. At the individual level, s_1 is simply a dummy equal to one when a teacher ranks some REP$^+$ school first on her list.[11] The results are given in Table 2[12] while Figure 6 reports the estimates from the event study. Remember from Figure 4

[11] A binary model could be used here, instead of a linear specification, which would nevertheless lead to similar conclusions; corresponding estimates are available upon request.

[12] The number of observations differs slightly from the ones in Table 1 because our estimation sample contains some rank-ordered lists from which the top 1 choice has been removed, see Appendix C.4 for details.

that unconditional estimates were equal to $(6.2 - 3.7) - (3.0 - 3.9) = +3.4$pp in 2018, and to $(9.3 - 3.7) - (5.5 - 3.9) = +4.0$pp in 2019. The event study estimates turn out to be close (+3.6pp and +3.0pp respectively) -if any, slightly lower in 2019. Finally, the estimated ATE is +3.2pp, and still +3.1pp after controlling for teacher $\times$ $k-$type of school fixed effects. All those point estimates are significant at usual levels, which suggests that the attractiveness of REP$^+$ schools has increased consecutive to the reform with respect to REP schools' one.

Columns 3 and 4 of Table 2 focus on the impact of financial incentives alone. A €1,000 bonus would induce a supplementary increase of +1.4pp of the share of REP$^+$ schools among top 1 choices with respect to the corresponding share of REP schools, after controlling for teachers' unobserved taste for REP and REP$^+$ schools. Hence the sole €1,167 increase would have been responsible for +1.6pp each year, i.e. +1.6pp in 2018 and +3.2pp in 2019. Non-financial incentives would then matter: financial incentives alone would not fully explain both observed and estimated differentials in shares in 2018, immediately consecutive to the reform. A concurring explanation would lie in teachers anticipating future increase as soon as 2018.

4.3 Rank of REP$^+$ schools in teachers' lists

Last, we exploit the richness of our dataset that contains the exact ranking of teachers' school choices. Instead of aggregating the data at the rank-ordered list-type of school (regular, REP or REP$^+$) level, we now base our analysis on the raw data at the rank-ordered list-school level. We seek to understand the impact of the policy change on that ranking, which leads us to consider the following estimating equation:

$$\text{RANK}_{ijt} = e\text{REP}_j^+ \times \mathbb{1}\{t \geq 2018\} + X'_{ijt}f + r_j + \zeta_t + v_{ijt}, \tag{3}$$

where RANK_{ijt} designates the order of school j in the rank-ordered list filled out by teacher i on year t, r_j is a school fixed-effect, ζ_t account for annual dummies, and X_{ijt} includes control variables, especially commuting time by car between home and school denoted by d_{ij}, measured in minutes during rush hours. The error term v_{ijt} is normally distributed, and standard errors are clustered at the teacher level in order to take autocorrelation of residuals into account.

We adopt once again a difference-in-differences approach based on a treatment group, still composed of REP$^+$ schools, and a comparison group. However, there is no reason why regular schools should be disregarded as a valid comparison group here: contrary to shares, rank do not sum up mechanically to one, and the only necessary condition for the identification strategy to be valid is whether the CTA holds. Figure 7 shows actually that there is much imprecision as regards the average rank (about 5 before 2018) of both REP and REP$^+$ schools in rank-ordered lists, due to the scarcity of such schools in the educational authority.[13] As a result, it is not really possible to validate nor to invalidate our empirical strategy based on such eyeball evidence. Note also that the average rank of regular schools, about 4, remains fairly constant over time, and that there is much less imprecision on that rank due to the higher number of such schools, 233. We decide therefore to adopt three different identification strategies by considering three different comparison groups: (i) REP schools, (ii) regular schools, and (iii) both REP and regular schools. Remember from Figure 1 that in both REP and regular schools, financial incentives did not vary over the period, though the bonus amounts to €1,734 in REP schools while there is none in regular schools. Also, the aggregation (iii)is made possible by the inclusion of school fixed-effects r_j in equation (3) which capture this time-invariant pecuniary amount, among other.

It is in fact crucial to include school fixed-effects in equation (3). An interpretation of those unobserved school-specific factors has to do with "attractiveness" or "school quality" as perceived by teachers, but not by the econometrician. In the absence of unobserved school-specific effects, the coefficient e would certainly capture a selection effect due to the differential attractiveness of REP$^+$ schools with respect to schools in the comparison group -this selection effect being probably less pronounced in the first identification strategy since the intuition suggests that the gap with REP schools is smaller than the one with regular schools. However, conditional on those fixed effects, hence on the classification into $k-$types of schools which has remained mostly unchanged over the period,[14] the coefficient e measures the desired ATE.

As before, when willing to disentangle financial from non-financial incentives, we may consider the alternative specification:

$$\text{RANK}_{ijt} = \tilde{e}\text{BONUS}_{jt} + X'_{ijt}\tilde{f} + \tilde{r}_j + \tilde{\zeta}_t + \tilde{v}_{ijt}, \tag{4}$$

[13] This is the reason why we wish to replicate the current analysis on a larger dataset including possibly other educational authorities with more disadvantaged schools like Créteil or Versailles, or even on data at the nationwide level.

[14] A single REP$^+$ school has closed, a REP school was turned into a REP$^+$ school, and a regular school became a REP school all over the period considered here.

The estimation of the latter model relies on the following exclusion restriction:

$$\mathbb{E}[\text{BONUS}_{jt}, d_{ij} | \tilde{r}_j, \tilde{\zeta}_t, X_{ijt}, \tilde{v}_{ijt}] = 0. \tag{5}$$

Though not testable, the plausibility of this identification assumption hinges on the exogeneity of the policy change. The absence of measurement error in travel time, the exogeneity of location choices as well as the linearity in travel costs are nevertheless more tricky assumptions, and we provide below with some robustness checks, especially by considering alternative measures of travel time.

The estimation results are given in Table 3. On the one hand, the first identification strategy suffers from a small sample size, in relation with the scarcity of disadvantaged schools in the educational authority. We find an effect of -0.7, not significant at usual levels, on the average rank of REP$^+$ schools in lists. On the other hand, according to both second and third identification strategies, REP$^+$ schools were ranked better consecutive to the reform, the estimated change being -1.2, and significant at usual levels. According to the event study displayed by Figure 8, the effect is mostly driven by year 2018.

We proceed to some falsification test by considering a fake treatment group composed of all schools in a given *département* of Montpellier's educational authority: we do as if those schools were all REP$^+$, hence dropping REP$^+$ schools from the estimation sample for the sake of this exercise only. The results are displayed by Table 4. Reassuringly, in each of the five *départements*, the point estimate is not significant at usual levels .

To assess the robustness of our results, we investigate whether they depend on possible measurement error as regards a key determinant of school choice, namely travel time used as a covariate above. We consider alternative concepts of travel time (measured during off-peak hours instead of during rush hours, Euclidean distance and effective distance, the last two ones being expressed in kilometers instead of in minutes), as well as a quadratic specification of travel costs instead of a linear one. Results remain very close to those found above, see Table 16 on this topic. This robustness check may be viewed as some kind of sensitivity analysis with respect to the exclusion restriction (5).

4.4 Other robustness checks

4.4.1 Censoring

As already explained, one may worry that WTT is less likely when ROLs have exactly 20 choices, that is, when the length constraint is binding, due to possible censoring issues. This is the reason why we have excluded those lists from the estimation, up to now. Including them as a robustness exercise would not dramatically alter our results. Corresponding estimates are available upon request.

4.4.2 Nonlinearity of the effect

Moving from rank 20 to rank 19 is not equivalent to moving from rank 2 to rank 1. More generally, one may suspect heterogeneous, nonlinear effects with respect to the rank of the choice itself. Restricting our attention to choices ranked 10 or less would yet not change much previous diagnosis. Corresponding estimates are available upon request.

4.4.3 Other outcomes

Figures 10 and 11 in Appendix show two supplementary event studies realized on different outcomes: the share of REP$^+$ schools among top 2 choices and that same share among top 3 choices, respectively. From both qualitative and quantitative viewpoints, our previous results remain unchanged.

4.5 Discussion and limits

Overall, our estimations suggest that the policy was efficient at enhancing the attractiveness of REP$^+$ schools. Both descriptive evidence and causal analysis based on difference-in-differences and event study approaches point out to shares of REP$^+$ schools among all (or top 1) choices being multiplied by about two within only two years. Raw statistics show that those shares rose from 2.9 to 6.9% and from 3.7 to 9.3%, respectively; unconditional DinD estimates amount to nearly +3.1pp and up to +4.0pp; event study estimates yield +2.5pp and +3.0pp. The effect of financial incentives alone would be +1.15pp per extra €1,000 as regards the share among all choices, and +1.4pp per extra €1,000 as regards the share among top 1 choices: in other words, those incentives would be the source of almost the whole effect (+2.7pp and +3.3pp), given the observed variation in bonus (+€2,334) induced by the public policy at stake.

Another interesting conclusion that can be drawn from previous reduced-form analysis has to do with the plausibility of the WTT assumption in our setting. The proximity of estimated ATEs relative to shares among all choices, on the one hand, and to shares among top 1 choices, on the other hand, suggests that restricting our attention to the top of the list can be done without bias, compared with considering the whole list -as should be the case under the WTT assumption.

Indeed, according to the latter, teachers rank their most-preferred schools by their true preference order, but not necessarily rank all schools: remember that the median observed length of rank-ordered lists is slightly above 4, which means that teachers rank about 4 schools out of 266 only. Overall, this empirical evidence supports the WTT assumption in the current context, and in what follows, we restrict our attention to top choices only.

That being said, at least two factors require some caution when interpreting the above results.

First, the source of identifying variation lies in only 17 REP$^+$ schools contributing to the estimation of the ATE. Disposing of supplementary data, in particular of more REP$^+$ schools located in other educational authorities, would enlarge our source of identifying variation, on top of conferring higher external validity.

Second, the identification strategy relies both on the CTA and on the Stable Unit Treatment Value Assumption (SUTVA), the latter being likely violated if the comparison group (typically REP schools) is indirectly affected by the treatment. Such spillovers are likely with outcomes based on shares among choices because shares of REP, REP$^+$ and regular schools sum up to one every year, due to rivalry of mutually exclusive alternatives. Hence, when the share of REP$^+$ increases, it follows mechanically that either the share of REP schools or the share of regular schools decreases -perhaps both of them. Previous estimates might then suffer from a contamination bias which might lead to overestimate the true ATE: the treatment is likely to decrease the share of the comparison group, which would result in a larger differential with the share of the treatment group. The possible violation of SUTVA concerns also the rank outcome: if the treatment leads to a higher relative attractiveness of REP$^+$ schools, their rank in lists should decrease, which mechanically increases the rank of other schools. To overcome this limitation of the reduced-form approach, we believe that a structural model relaxing SUTVA is needed in order to empirically assess the causal impact of incentives on teachers' desired mobility, which we will be doing in next section.

5 Counterfactual analysis

We seek here to simulate counterfactual 2018 and 2019 years such that bonuses would be kept unchanged at their 2017 level in REP$^+$ schools. By comparison with observed outcomes on those years, these simulations yield an evaluation of the causal impact of the change in financial incentives decided by the policy maker. Other thought experiments can be simulated, including a series of counterfactual years 2015 to 2019 in which bonuses in either REP or REP$^+$ schools would be removed, for instance. Those exercises allow us to disentangle finely the role played by each and any variation in corresponding pecuniary amounts, and help us quantify the possibly nonlinear effect of income. All over that section, we aggregate the data at the school-year level and our outcome of interest is schools' share among top 1 choices.

5.1 Econometric specification

We adapt a competition model introduced by Berry (1994). In his model, consumers face a number of differentiated products and purchase the one that maximizes their utility; they may also decide not to purchase at all, i.e. choose the outside option. This model can straightforwardly be applied to our setting by letting teachers play the role of consumers and schools the role of products. Such a discrete-choice model is more appropriate to deal with shares that sum up to one: the rivalry of mutually exclusive alternatives needs to be taken into account, especially in order to address any concern about SUTVA. Each year, we consider a single market, namely the whole Montpellier's educational authority, with a fairly large number of schools[15] The outside option corresponds to teachers in the educational authority who do not request any transfer at all. Every year t from 2015 to 2019, the share of a given school j among top 1 choices, i.e. the analogue of the product market share s_{jt}, divided by s_{0t} the share of the outside option, i.e. the fraction of teachers whose top 1 choice is their current school (they do not fill out any ROL in that case), depends only on characteristics of that school:

$$\log s_{jt} - \log s_{0t} = \delta_{jt} \equiv \beta \text{BONUS}_{jt} + \alpha_j + \gamma_t + \xi_{jt}, \tag{6}$$

where δ_{jt} is the mean utility level, specified as a linear index of (i) attractiveness of school j, encompassed here by the fixed-effect α_j, (ii) financial incentives BONUS_{jt}, (iii) aggregate shocks common to all schools in the educational authority, captured by year dummies γ_t as well as (iv) unobserved local demand shocks ξ_{jt} that account for any residual change in the reputation of school j on year t. This formula is derived from the theoretical share in a multinomial Logit:

$$s_{jt} = \frac{e^{\delta_{jt}}}{\sum_{k=0}^{J} e^{\delta_{kt}}}, \tag{7}$$

[15] 266, remember Table 14 for instance. In Berry's model, the asymptotics is in the number of products/schools.

where $j = 1, \ldots, J$ indexes schools. As usual, $\delta_{0t} = 0$ is the normalization of the mean utility level provided by the outside option.[16] This approach assumes that teachers' preferences are uncorrelated with their current school. Put differently, we allow for that current school to affect the probability of filling out a ROL, i.e. of requesting some transfer, but not to shape preferences over schools. This exclusion restriction enables us to identify preferences[17] under WTT since elicited preferences are rank-ordered at the top of the lists by assumption. The current model does not include any taste heterogeneity, it can rather be interpreted as a model of pure vertical differentiation whereby teachers all agree on school quality. By contrast, next section will relax this assumption by allowing more heterogeneity at the individual level. We view those approaches as complement: the current one adopts the centralized viewpoint of the educational authority, hence at an aggregate level, while the model developed in next section departs from this approach and focuses on determinants of school choice at the teacher level.

The estimation procedure exploits estimating equation (6) and consists in inverting the empirical counterparts of theoretical shares $\hat{s}_{jt}$ in order to recover the mean utility levels $\hat{\delta}_{jt}$ which rationalize those observed shares. This equation is particularly convenient since it is linear in the parameters of interest. Under this approach, unobserved local demand shocks ξ_{jt} are the residuals of a regression where the dependent variable, a logarithmic transform of shares, is explained by a set of covariates that comprises the bonus on top of year- and school- fixed effects. Standard errors are clustered at the school level. In order to perform counterfactual experiments described below, though, this method requires to dispose of schools that are always present, i.e. chosen, in the sample, hence to deal with the problem of zeroes. This standard problem in empirical IO, and more generally the selection problem in applied econometrics, turns out to have no straightforward solution, especially in this setting, see for instance D'Haultfœuille, Durrmeyer, and Février (2019) for an attempt of solving that issue. We follow here the approach developed by Gandhi, Lu, and Shi (2021) and replace observed shares by their Laplace transform $s^L_{jt} = \frac{N_t s_{jt}+1}{N_t+J+1}$,[18] which allows us to handle the case where $s_{jt} = 0$.

The point estimate $\hat{\beta}$ is 0.239 with a standard error of 0.076 (Table 5): it is positive and significant at usual levels, suggesting once again that financial incentives did increase schools' perceived attractiveness. The inclusion of school

[16] Such a normalization is required for identification purposes since $(\beta, \alpha_j, \gamma_t, \delta_{0t}, \xi_{jt})$ and $(\beta, \alpha_j, \gamma_t - \delta_{0t}, 0, \xi_{jt})$ are observationally equivalent.

[17] for schools and for money (among other amenities) as well.

[18] N_t designates the total number of teachers in the educational authority, requesting a transfer or not on year t.

fixed effects in that equation is nevertheless crucial to obtain that positive sign, as was already the case in the reduced-form analysis: otherwise, the bonus captures merely the relative distaste for disadvantaged schools, and the sign of $\hat{\beta}$ is negative.

From previous estimates, it can already be assessed whether the program has reached its target from an "equality of opportunity" viewpoint. Comparing the estimated attractiveness in regular schools and in disadvantaged schools gives a proxy of the comparative advantage of the former over the latter, the goal of the bonus being to compensate for such a differential. Disposing of estimated schools' attractiveness, net of any pecuniary bonus, enables us to determine whether the policy has attained its primary objective. More precisely, we compute $\overline{\hat{\alpha}_j}^{\text{regular}}$, on the one hand, and $\overline{\hat{\alpha}_j}^{\text{REP}^+} + \hat{\beta}\text{BONUS}_{jt}$, on the other hand, both in 2018 and 2019. First, the average difference $\overline{\hat{\alpha}_j}^{\text{REP}^+} - \overline{\hat{\alpha}_j}^{\text{regular}}$ is negative, consistently with the current segmentation of schools.[19] To some extant, our model is therefore able to infer unobserved school quality from the data[20]. Second, the magnitude of the difference in attractiveness between REP$^+$ schools and regular schools is about -0.71, which means that teachers should be compensated for by about €3,000 annually in order to be indifferent between a regular school and a REP$^+$ school. Remembering that the annual bonus was €2,312 in 2017, €3,479 in 2018 and €4,646 in 2019, the policy has more than compensated the attractiveness differential from 2018 onwards. In this regard, such empirical evidence could be interpreted as "affirmative action" in favor of REP$^+$ schools. In 2018, the compensation is almost perfect: their attractiveness, proxied by the average share among top 1 choices, 6.2%, is roughly equal to the overall fraction of REP$^+$ schools ($17/266 \approx 6.4\%$) within the educational authority. In 2019, some overcompensation is at stake: our estimates suggest that, on average, REP$^+$ schools became more attractive than regular schools, with a positive differential of +.4, consistently with a higher observed share among top 1 choices (9.3%). In that sense, our model performs quite well when replicating shares at the aggregate level, and enables us to infer preferences for both schools and money accurately.[21]

[19] The same holds as regards $\overline{\hat{\alpha}_j}^{\text{REP}} - \overline{\hat{\alpha}_j}^{\text{regular}}$.

[20] The rank-ordered Logit in next section will provide with similar results, from a qualitative viewpoint.

[21] In the same vein, the observed share of REP schools is always below $16/266 \approx 6\%$ between 2015 and 2019, consistently with our estimates of net attractiveness being less than regular schools' average.

5.2 Breaking down the evolution of shares

We implement next a series of thought experiments that allow us to break down the observed changes in school shares from 2017 to 2019 along two channels: (a) financial incentives, and (b) demand (or attractiveness) shocks. For the sake of readability, we focus on aggregate shares (the shares of REP, REP$^+$ and regular schools) that are conditional on requesting some transfer: by definition, these shares sum up to one annually. We start with the environment that prevailed in 2018 and 2019 (same demand conditions and same bonus in disadvantaged schools) and successively replace certain parameters with their values in 2017. Specifically, we simulate the following counterfactual situations:

(a) We change the bonus, that is, we replace its current value in 2018 or 2019 with its value in 2017. We compute the shares that prevail after the change, thus assessing the pure effect of financial incentives by computing the shares in the absence of the 2017 reform. The counterfactual mean utility levels are:

$$\tilde{\delta}^a_{jt} = \hat{\delta}_{jt} - \hat{\beta}\left(\text{BONUS}_{jt} - \text{BONUS}_{j2017}\right) \quad \forall t = 2018, 2019, \tag{8}$$

which neutralizes the reform implemented by the policy maker from 2018 onwards. The counterfactual shares are:

$$\tilde{s}^a_{jt} = \frac{e^{\tilde{\delta}^a_{jt}}}{\sum_{k=1}^{J} e^{\tilde{\delta}^a_{kt}}}, \tag{9}$$

paying attention to the fact that the denominator starts at $k = 1$ (instead of $k = 0$) since the shares are conditional on not choosing the outside option.

(b) We change the demand parameters, namely the time-varying, local attractiveness shocks $\hat{\xi}_{jt}$ and replace them in 2018 and 2019 with their value in 2017. Note that changing aggregate demand shocks $\hat{\gamma}_t$ on year t would not change anything at all since such terms cancel out from conditional shares (9) which are, in fact, independent from such common shocks to all schools. To isolate the sole role of local demand shocks, we compute conditional shares $\tilde{s}^b_{jt}$ issued from counterfactual mean utility levels $\tilde{\delta}^b_{jt}$ as before:

$$\tilde{\delta}^b_{jt} = \hat{\delta}_{jt} - \left(\hat{\xi}_{jt} - \hat{\xi}_{j2017}\right) \quad \forall t = 2018, 2019. \tag{10}$$

By definition, combining (a) and (b) yields exactly the same observed shares as in 2017 since corresponding mean utility levels are identical. The fit of the model is perfect, by definition, once it has been fed up with the correct residuals, because these residuals are designed to match observed conditional shares. As a result, in order to break down observed changes in school shares among top 1 choices, and given the specification (6) adopted here, it is both necessary and sufficient to disentangle financial incentives from unobserved local demand shocks only.

Table 6 displays the results of this decomposition exercise.[22] It confirms that financial incentives play a role in explaining the sharp increase in the demand for REP$^+$ schools after 2017 (+2.0pp in 2018 and +4.4pp in 2019) in a context that would have been favorable to these schools even in the absence of the reform. Indeed, both 2018 and 2019 local demand shocks turned out to enhance REP$^+$ schools' attractiveness when compared with 2017 ones: neutralizing the change in local demand shocks yields lower shares (5.9 and 7.4%) than the observed ones (6.2 and 8.6%) on this balanced sample. The reform has thus acted as a supplementary force on top of good reputation shocks in 2018 and 2019. This empirical evidence looks all the more plausible that the role played by unobserved local demand shocks, i.e. residuals in equation (6), is limited.[23] Besides, this decomposition reveals that the share of REP schools would have not been much affected by the reform: in the absence of the reform, their counterfactual share in 2019 amounts to 5.8% whereas their observed share is 5.6%. Next section will confirm that the diversion of demand has in fact occurred rather at the expense of regular schools, and even more at the expense of the outside option, than at the expense of REP schools -hereby validating somehow the reduced-form, difference-in-differences approach, and alleviating concerns raised by a possible violation of SUTVA.

5.3 Counterfactual experiments: the role of financial incentives

To explore further the role played by the sole financial incentives, one can imagine many other counterfactual experiments corresponding to various amounts of financial incentives $\widetilde{\text{BONUS}}_{jt}$, compute counterfactual mean utility levels $\tilde{\delta}_{jt} = \hat{\delta}_{jt} - \hat{\beta}(\text{BONUS}_{jt} - \widetilde{\text{BONUS}}_{jt})$ and simulate conditional schools' shares among top

[22] Minor differences between Figure 4 and Table 6 ("Observed" rows) are due to the Laplace transform.

[23] By contrast, omitting school fixed-effects in equation (6) would lead to diametrically opposite results: a minor role played by financial incentives, and unobserved demand shocks acting as confounding factors, i.e. explaining the vast majority of the seemingly higher attractiveness of REP$^+$ schools -a scenario which is harder to believe in. More precisely, corresponding counterfactual shares would be 6.2% in 2018 and 8.4% in 2019 when neutralizing the reform, i.e. quite close to observed shares (6.2% and 8.6%). Holding unobserved local demand shocks at their 2017 level would yield shares of 4.4% in 2018 and 4.5% in 2019.

1 choices. For instance, remembering Figure 1, and in order to gradually decompose the impact of each and any change in financial incentives, one may consider four distinct $\widetilde{\text{BONUS}}_{jt}$:

(0) $\widetilde{\text{BONUS}}_{jt} = \text{BONUS}_{jt}$, i.e. the observed bonus, which yields $\tilde{\delta}^0_{jt} = \hat{\delta}_{jt}$; this experiment corresponds to the observed situation.

(1) $\widetilde{\text{BONUS}}_{jt} = \text{BONUS}_{j2017}$, i.e. the amount prevailing in 2017 in each type of schools (€1,734 in REP schools and €2,312 in REP$^+$ schools), which yields $\tilde{\delta}^1_{jt}$; this experiment neutralizes the reform and corresponds exactly to previous experiment (a), penalizing REP$^+$ schools only from 2018 onwards;

(2) $\widetilde{\text{BONUS}}_{jt} = \underline{\text{BONUS}}_{jt}$, i.e. the (minimum) amount prevailing in REP schools over the period (€1,734), regardless of the REP/REP$^+$ status, which yields $\tilde{\delta}^2_{jt}$; this experiment penalizes REP$^+$ *vis-à-vis* REP schools;

(3) $\widetilde{\text{BONUS}}_{jt} = 0$: we remove any bonus everywhere, which yields $\tilde{\delta}^3_{jt}$ -this experiment penalizes unambiguously disadvantaged schools (both REP and REP$^+$) *vis-à-vis* regular schools.

All those experiments are such that $\widetilde{\text{BONUS}}_{jt} \leq \text{BONUS}_{jt}$, $\forall j, t$: as a result, given formula (7) and since $\hat{\beta} > 0$, (i) each counterfactual share of REP$^+$ schools will be unambiguously smaller than (or equal to) its observed value, and (ii) each counterfactual share of regular schools will be unambiguously higher than (or equal to) its observed value. Equivalently, the counterfactual share of disadvantaged schools, i.e. the sum of the shares of REP and REP$^+$ schools, will be unambiguously lower. The magnitudes of such changes are, however, to be quantified by the simulations. Furthermore, the relative position of the counterfactual share of REP schools with respect to its observed value is ambiguous: it empirically depends on the magnitude of $\hat{\beta}$. In practice, we will see that it hardly changes at all, even in experiment (1).

Figure 9 displays the contribution of each experiment to the differential between observed data (experiment 0) and complete removal of incentives (experiment 3). An eyeball analysis is sufficient to convince oneself that those effects are credibly in line with Figure 1. Interestingly, our simulations conclude to a nonlinear impact of money. Moving from €2,312 to €4,646 in 2019, a difference of €2,334, would increase the share of REP$^+$ schools by 3.5pp from 5.1% to 8.6%. Yet moving from €0 to €2,312 would increase it by 2pp only from 3.1% to 5.1%. As an intermediate experiment, increasing that bonus from €1,734 to €2,312, a change of €578, would cause the corresponding share to rise by 0.6pp from 4.5% to 5.1%. Our results are thus consistent with a convex effect of the bonus, which may be explained by the presence of switching costs: substantial amounts of money should then be offered to compensate for non-pecuniary costs (e.g., moving costs, social costs, administrative costs, among others) associated with moving from one workplace to another.

Last, Table 7 focuses on experiment (1) -the same as experiment (a) above- and displays the corresponding changes in unconditional shares, i.e. taking the outside option into account. This exercise enables us to answer a concern raised by teachers' unions: the reform would benefit to REP$^+$ schools but hurt REP schools, which would therefore not be Pareto-improving. According to such an hypothesis, the share of REP$^+$ schools may well increase, but the share of REP schools would decrease and the total share of disadvantaged schools might even be reduced. However, based on the current sample and the assumptions of our model, we find no empirical evidence in favor of such an hypothesis. First, Table 10 confirms that teachers working in REP schools do not make more transfer requests consecutive to the reform. Second, the share of REP$^+$ schools would have increased consecutive to stronger financial incentives, but 91% of this change would be due to business stealing from the outside option (teachers not requesting any transfer in the absence of the reform would change their mind consecutive to supplementary incentives), while remaining 8% would be explained by business stealing from regular schools (teachers otherwise willing to join some regular school would opt for a REP$^+$ school instead). The fraction of teachers who trade off a REP school against a REP$^+$ school in their rank-order list due to these supplementary incentives would be negligible (less than 1%).

On top of tempering this public policy concern, the fact that REP schools have not suffered -even indirectly- from the reform alleviates other methodological concerns regarding a possible violation of SUTVA, which would endanger our reduced-form approach. Previous results suggest that REP schools constitute an appropriate comparison group,[24] since adverse spillovers of the treatment can apparently be neglected. Interestingly, the previous difference-in-differences has concluded to a +1.4pp impact of supplementary €1,000 on the share of REP$^+$ schools among top 1 choices, a figure that is completely in line with the current +3.5pp estimate obtained from the counterfactual simulations, and corresponding to a €2,334 change. All those results look pretty consistent with one another, though obtained thanks to different methods, which comforts us in our evaluation of the reform at stake. They are also weakly suggestive of the reform having enhanced the stability of the pedagogical team in REP$^+$ schools, through both higher incoming and lower outgoing mobility, though further data on teacher as-

[24]This would not be the case of regular schools, which we consider as such when looking at the rank in lists, though.

signment (on top of a longer period of observation, probably) would be required to empirically assess the latter statement.

6 Heterogeneous responses to financial incentives

Why does heterogeneity in teachers' response to the policy change matter? An important policy concern with such compensatory programs has to do with selection, i.e. the risk of attracting teachers who are already working in disadvantaged schools, or younger teachers, with less experience, etc. By contrast, enhancing teaching quality might require to recruit older teachers, with more seniority and some experience in regular schools. Unfortunately, disadvantaged schools are often composed of young teachers seeking to escape as soon as possible, i.e. as soon as their priority index enables them to leave, and we will show that those teachers who actually respond to such incentives belong rather to that category. As a result, it is to fear that this reform has not achieved at improving teaching quality; at the very least, it has not changed notably the composition of teaching staff in REP^+ schools. Another important outcome to look at would be the stability of the pedagogical team, as mentioned above.[25]

To deal with that heterogeneity, we make best use of our micro-data available at a very detailed level, namely teachers' rank-ordered lists including multiple school choices viewed as alternatives. This comparative advantage of our data helps us learn more about teachers' preferences and about their heterogeneous attitudes towards money and time. Based on elicited preferences, we resort to a structural model of school choice, which enables us to infer teachers' travel costs but also their marginal utility of income. To identify the latter, we still use the reform as a source of exogenous variation in financial incentives, i.e. in the taste for money: we encompass previous difference-in-differences approach within a rank-ordered discrete-choice model. We are then able to quantify the opportunity cost of travel time, or equivalently to express the marginal benefit of income in terms of saved daily commuting time. We estimate a model that is designed to handle rank-ordered lists, namely a rank-ordered Logit model (see, e.g., Abdulkadiroğlu et al., 2020; Fack, Grenet, and He, 2019). Relying on both cross-sectional and longitudinal variation, the estimation will lead to document a substantial heterogeneity in teachers' responses to financial incentives.

In what follows, we assume that teacher i working in current school s opting for

[25]The ultimate outcome would be students' achievement as proxied by test scores -a valuable information which, unfortunately, we do not have.

school j among the J_t schools in Montpellier's educational authority on year t[26] derives the following net utility, denoting by B_{ist} the benefit enjoyed when he works in his current school and by C_{ist} the mobility cost incurred when he decides to leave that school:

$$U_{ijst} = V_{ijst} + B_{ist} - C_{ist} + \varepsilon_{ijst} = \pi_j + \eta_i \text{BONUS}_{jt} - \lambda_i d_{ij} + B_{ist} - C_{ist} + \varepsilon_{ijt}. \quad (11)$$

By contrast, if this teacher does not request any transfer, he gets his reserve utility, his outside option, and does not incur any mobility cost. It is however useless to specify any outside option in the current setting since it won't play any role in teachers' ranking among alternatives/schools j. Indeed, we adopt a conditional approach by focusing on schools ranked in ROLs only, as opposed to ROLs that would be augmented by the outside option, namely his current school s, which would lie somewhere at the bottom of his ROL -but the researcher would not know exactly where. We focus rather on the top of those lists, i.e. on schools that are actually observed in our application data. This can be done without bias based on WTT, some empirical support in favor of which has already been provided by previous reduced-form evidence. Doing so guarantees that the outside option won't play any role in our estimation procedure. On top of that, teachers' choice among alternatives j is independent from $B_{ist} - C_{ist}$ due to this term canceling out from the corresponding discrete choice. This is conditional on the fact that the worst elicited alternative yields a higher utility than the outside good -or equivalently, that those schools are ranked according to true preference ordering: this must yet be the case since under WTT those alternatives are revealed preferred to the outside good.

The above specification states that teachers make a trade-off between school attractiveness π_j, unobserved to the econometrician, and distance d_{ij}. For the sake of simplicity, we assume linear travel costs, but we perform a robustness check by considering a quadratic specification (see Table 16 in Appendix). Moreover, when school-specific bonuses are available, as is the case in disadvantaged schools, the previous trade-off is mitigated by financial incentives, increasing the global attractiveness to the level $\pi_j + \eta_i \text{BONUS}_{jt}$ if η_i designates teacher's marginal utility of income.

Without loss of generality, i.e. since $B_{ist} - C_{ist}$ cannot be identified from the sole choice among alternatives j, we denote by

$$\overline{V}_{ijt} \equiv \pi_j + \eta_i \text{BONUS}_{jt} - \lambda_i d_{ij} \quad (12)$$

[26] We drop hereafter the index t and denote J_t by J only.

the mean utility level obtained when choosing school j. The idiosyncratic error terms ε_{ist} are supposed to follow some extreme-value (type I) distribution. From previous assumptions, the conditional probability of observing the rank-ordered list $r_{it} = (r_{i1t}, \ldots, r_{iJt})$, given observed and unobserved school- and teacher- characteristics, writes:

$$\mathbb{P}(U_{ir_{i1t}st} > \ldots > U_{ir_{iJt}st} | U_{ir_{iJ}st} \geq U_{i0st}, \text{BONUS}_{jt}, d_{ij}; \theta_{ij}) = \prod_{j=1}^{J-1} \frac{\exp(\overline{V}_{ir_{ijt}t})}{\sum_{k=j}^{J} \exp(\overline{V}_{ir_{ikt}t})} \tag{13}$$

where the right-hand side of equation (13) accounts for the standard closed-form used in the rank-ordered Logit literature -a model being sometimes called the "exploded Logit".

Structural parameters of the model $\theta_{ij} = (\eta_i, \lambda_i, \pi_j)$ include the aversion to travel time λ_i, the marginal benefit of income η_i, two parameters which possibly vary across individuals, and school fixed-effects π_j. The identification of the model, i.e. of the vector θ, stems from (i) the exogeneity of distance at the time of teachers' reporting their preferences, as far as λ_i is concerned; (ii) the exogenous variation in financial incentives provided by the 2018 reform, this source of identifying variation allowing us to recover η_i; and (iii) parametric assumptions (the additive separability of the utility into the mean utility level and the error term, the distribution of errors, and the specification of the mean utility level itself) leading to the monotonicity of formula (13) with respect to $\overline{V}_{ijt}$ and thus to θ_{ij}. Those parameters can be estimated consistently by a conditional likelihood maximization procedure: from Beggs, Cardell, and Hausman (1981), equation (13) provides with a concave log-likelihood, which guarantees the existence and uniqueness of the maximizer. Importantly, we do not face any incidental parameter problem here: should have $B_{ist} - C_{ist}$ not been normalized to zero, would this term have disappeared from the individual likelihood, which renders that normalization unimportant. As regards inference, standard errors are clustered at the teacher level, which accounts therefore for possible autocorrelation of residuals over time in teachers' reporting their preferences.

Our econometric specification is motivated by the desire to account for heterogeneity in teachers' responses, which may be relevant from a public policy perspective. First, travel costs may be higher for older individuals and females, as both the literature and Table 12 in Appendix suggest.[27] As a result, we posit

[27] Teachers with high travel costs, including older individuals and women, according to that Table, may less be prone to request some transfer. It is then fair to assume that, when requesting a transfer, those individuals still incur high travel costs.

the following observed heterogeneity on the coefficient λ_i:

$$\lambda_i \equiv \lambda_0 + \lambda_a f(\text{Age}_i) + \lambda_f \text{Female}_i, \tag{14}$$

where $f(.)$ is some flexible functional form. Second, teachers may be more or less responsive to financial incentives, depending on their qualification, their professional experience, and on whether they are already working in a disadvantaged school:

$$\eta_i \equiv \eta_0 + \eta_n \text{Novice}_i + \eta_b \text{Beginner}_i + \eta_r \text{REP}_i + \eta_+ \text{REP}_i^+ + \eta_a \textit{Agrégation}_i + \eta_c \textit{Classe normale}_i \tag{15}$$

where Novice_i (resp. Beginner_i) is a dummy equal to 1 when teacher i' seniority is less than 3 years (resp. comprised between 4 and 8 years), and REP_i (resp. REP_i^+) is another dummy equal to 1 when the teacher i is already working in a REP (resp. REP^+) school.[28]

In alternative specifications, one could allow for further heterogeneity, for instance in perceived school attractiveness which may itself differ according to teachers' characteristics (i.e. π_{ij}): Abdulkadiroğlu et al. (2020) estimate a similar model separately for each cell $c(i)$ such that all teachers within a cell share common observed characteristics like age, gender, seniority, etc. This approach would yet face here the problem of estimating the $\pi_{c(i)j}$ parameters consistently, a difficult task when the cell size becomes small.

Table 8 confirms that teachers dislike commuting time, i.e. that travel costs matter in their school choice. Yet no significant heterogeneity is found as regards age, probably because of composition effects -oldest individuals being scarce in our data because they are less likely to request any transfer. On the whole, financial incentives matter in the sense that the point estimate $\hat{\eta} \approx 0.260$ is significantly positive (column 1) in the homogeneous model where $\eta_i = \eta$ and $\lambda_i = \lambda$, $\forall i$. Provided that teachers have on average 10 trips per week and work 30 weeks a year, this figure points out to an opportunity cost of time of nearly 13.8€ per hour -about €1,932 per month, which compares well with teachers' net monthly wage. On average, the €2,334 increase between 2017 and 2019 would have represented an equivalent gain of almost 68 minutes saved daily for the concerned individuals.

However, those average numbers mask substantial heterogeneity in teachers' responses to financial incentives, see column 2. For instance, novice teachers,

[28]This variable is measured with error: for instance, teachers who replace temporarily absent teachers may opt for the school where they are currently working in. However, this has a marginal impact: removing those teachers from our estimation does not affect our results.

i.e. teachers with at most 3 years of seniority, are almost twice more sensitive to the bonus: for them, an extra €1,000 is associated with an extra and highly significant point estimate of 0.219, which means that it is worth saving up to 8 more minutes per commuting trip (16 minutes per day). The same diagnosis prevails for teachers already working in a REP$^+$ school: those teachers may be averse to lose their current bonus when opting for a school placed outside of the REP$^+$ network, due to (asymmetric) nominal wage rigidity or to loss aversion, for instance.

It is also worth noting that our estimates of the average gross[29] unobserved taste π_j for REP$^+$ schools lies behind the one prevailing in REP schools, which is itself dominated by regular schools' one. This inferred ranking suggests that our model is able to properly account for the implicit hierarchy in school quality. Equipped with such estimates $\hat{\pi}_j$, we can go further and quantify the equivalent surplus brought by an extra €1,000 to, say, some novice teacher who is already working in a REP$^+$ school: this surplus amounts to slightly less than one half of a standard deviation in attractiveness. Put differently, the 2018 reform had a substantial effect for those individuals: for them, everything happens as if the perceived attractiveness of those schools had been shifted by 1 s.d. Moreover, it is possible to evaluate whether this reform has been successful in filling the gap between disadvantaged and regular schools. Before 2018, in REP$^+$ schools that estimated net attractiveness amounted to about $-0.16 + 0.26 \times 2.312 \approx 0.45$ while in REP schools it was close to $-0.09 + 0.26 \times 1.734 \approx 0.39$, to be compared with a point estimate of 0.53 in regular schools. In 2019, the compensatory scheme implemented in REP$^+$ schools looks like "affirmative action" in the sense that the perceived attractiveness of those schools is virtually increased by the policy maker at a level equal to $-0.16 + 0.26 \times 4.646 \approx 1.05$, which now exceeds the one prevailing in all other schools -a diagnosis which is largely reminiscent of the results found in previous section.

Last, the fit of this structural model, defined at the individual level, cannot be perfect, due to the ordered nature of the outcome (among others; see footnote 31 below).[30] This is the reason why we chose to aggregate the data at the school-year level in section 5 and to consider schools' shares among top 1 choices -a continuous variable- as our preferred outcome. This procedure, combined with the discrete-

[29] i.e. in the absence of any pecuniary bonus, as opposed to the net (perceived) attractiveness $\pi_j + \eta_i \text{BONUS}_{jt}$, see below.

[30] This remark includes a partial outcome out of the whole rank-ordered list, namely the dummy equal to one when teacher i ranks school j first on her list at time t.

choice model developed in section 5, has enabled us to achieve a perfect fit[31] and therefore to simulate desired counterfactuals.

7 Conclusion

This article has evaluated the impact of stronger financial incentives on desired teachers' mobility towards disadvantaged schools, relying on a substantial increase of a pecuniary bonus offered in French REP$^+$ schools from 2018 onwards. First, the empirical evidence based on a difference-in-differences approach suggests that the public policy change has induced a significant +3.1pp (+4pp) effect on schools' shares among top 1 choices after (before) controlling for observed heterogeneity; financial incentives would explain almost all of it, with an equivalent +1.4pp for each supplementary €1,000 in the annual bonus granted to those schools. Second, we show thanks to counterfactual simulations that teachers reporting a preference for a REP$^+$ school consecutive to the reform do not come from REP schools but rather from the outside option (i.e. they would not have requested any transfer in the absence of the public policy shock), and, to a lesser extent, from regular schools. In our view, this result alleviates possible concerns raised by a violation of SUTVA since the approach adopted here takes the rivalry of teachers' choices into account; reassuringly, this model leads us to conclude to a +3.5pp overall impact of the reform, which compares well with previous reduced-form estimates. Third, a structural model reveals that teachers had in fact reacted very differently to pecuniary incentives, and that those who actually respond most are novice and already work in disadvantaged schools.

The most salient limit of the current study is external validity since we rely on data from a single educational authority; on top of that, disadvantaged schools are not numerous, which restrains the identifying variation and might explain why some results are not significant -on top of the heterogeneity in responses, though. One would benefit most of supplementary data issued from other educational authorities, or even at the country level.

Further research on this topic should include a cost-benefit analysis: if the cost of these extra pecuniary incentives is easy to compute (equal to the product of the bonus and of the number of teachers who actually get that bonus), its benefit is much harder to quantify. This brings us to the fact that the ultimate outcome of any educational policy is students' achievement. However, even if there exists

[31] Once unobserved residual components ξ_{jt} have been recovered and taken into account. By contrast, the conditional likelihood estimation does not permit to recover the idiosyncratic shocks ε_{ijt} at the individual level.

some causal chain from the program to students' achievement, the econometrician ignores the policy maker's willingness to pay for one student passing her exams (extensive margin) or obtaining better grades (intensive margin). Besides, such a causal chain would likely imply the stability of the teaching staff: pedagogical teams remaining longer in a given school should help improve teaching quality, and thus students' outcomes. Focusing therefore on the duration of a teaching position within a school, as another outcome, would also be of interest in this respect.

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A Figures

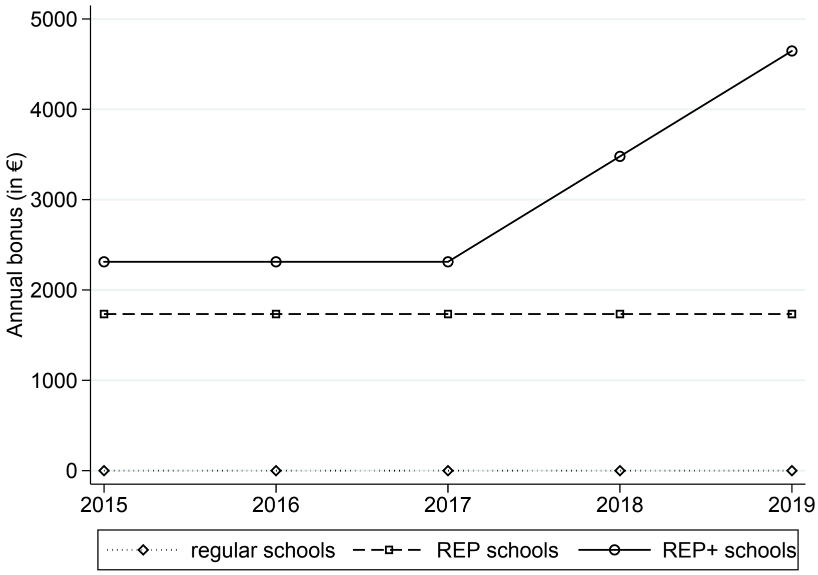

Figure 1: The change in financial incentives in REP$^+$ schools from 2018 onwards

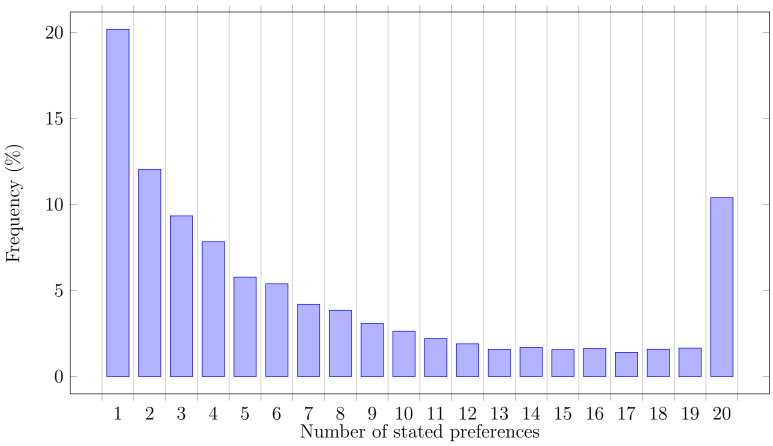

Figure 2: Length of rank-ordered lists

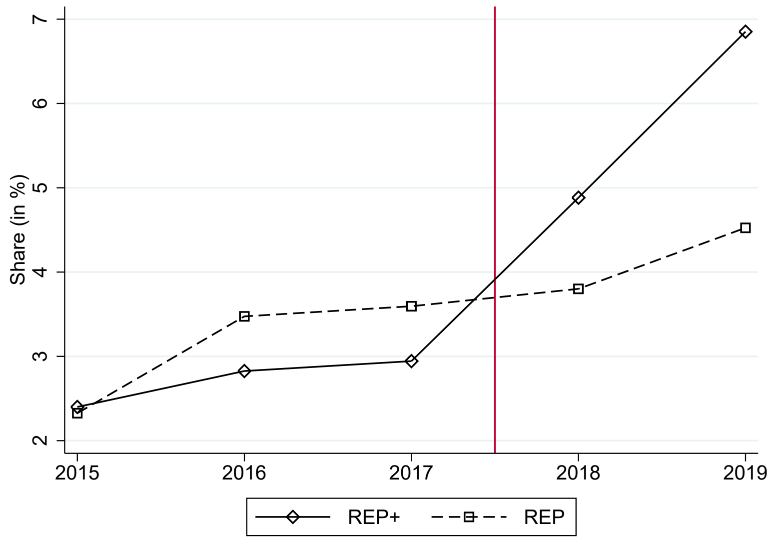

Figure 3: Share of REP/REP+ schools among all choices (%)

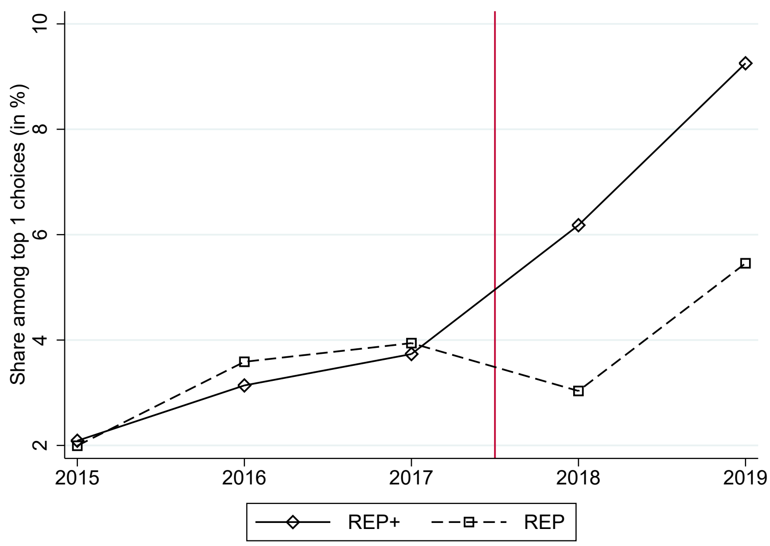

Figure 4: Share of REP/REP+ schools among top 1 choices (%)

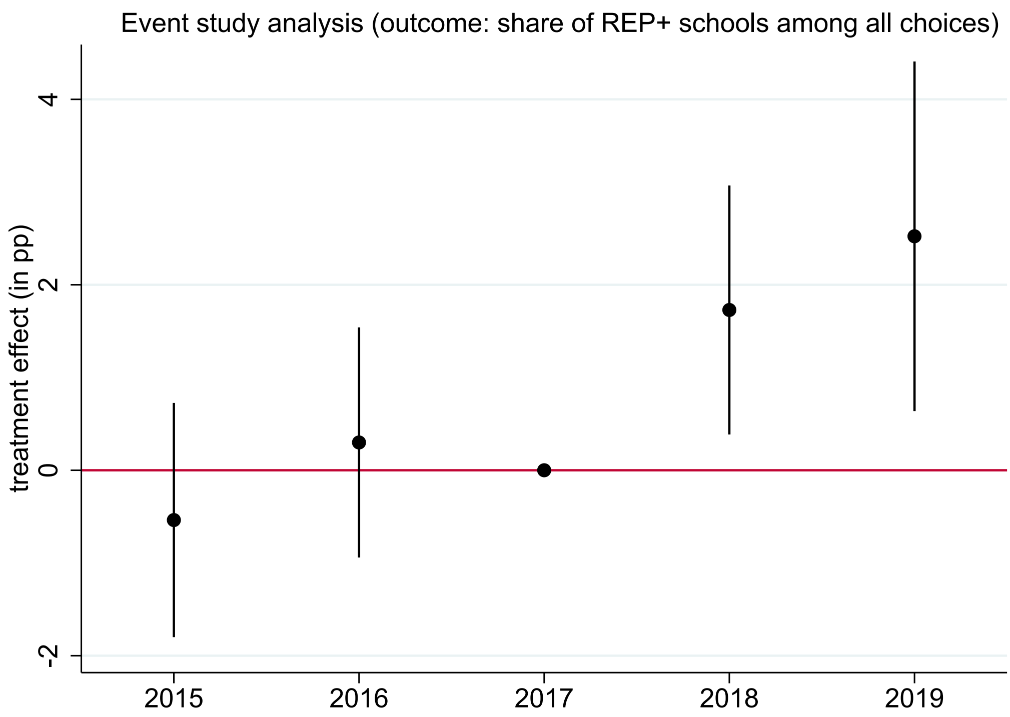

Figure 5: Event study approach (share of REP+ schools among all choices)

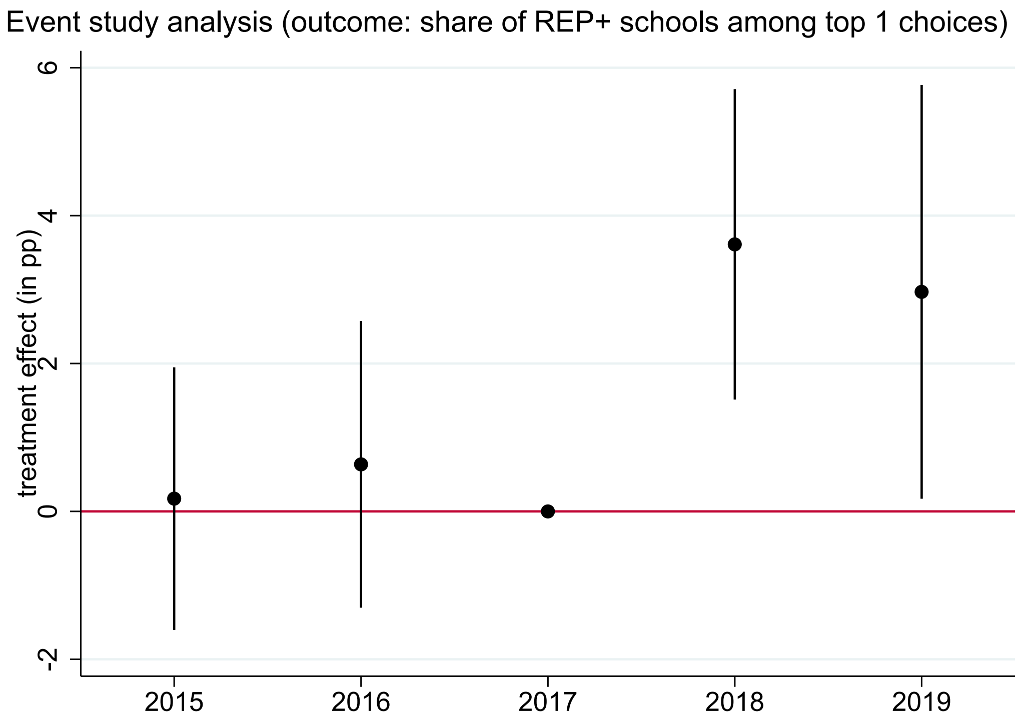

Figure 6: Event study approach (share of REP+ schools among top 1 choices)

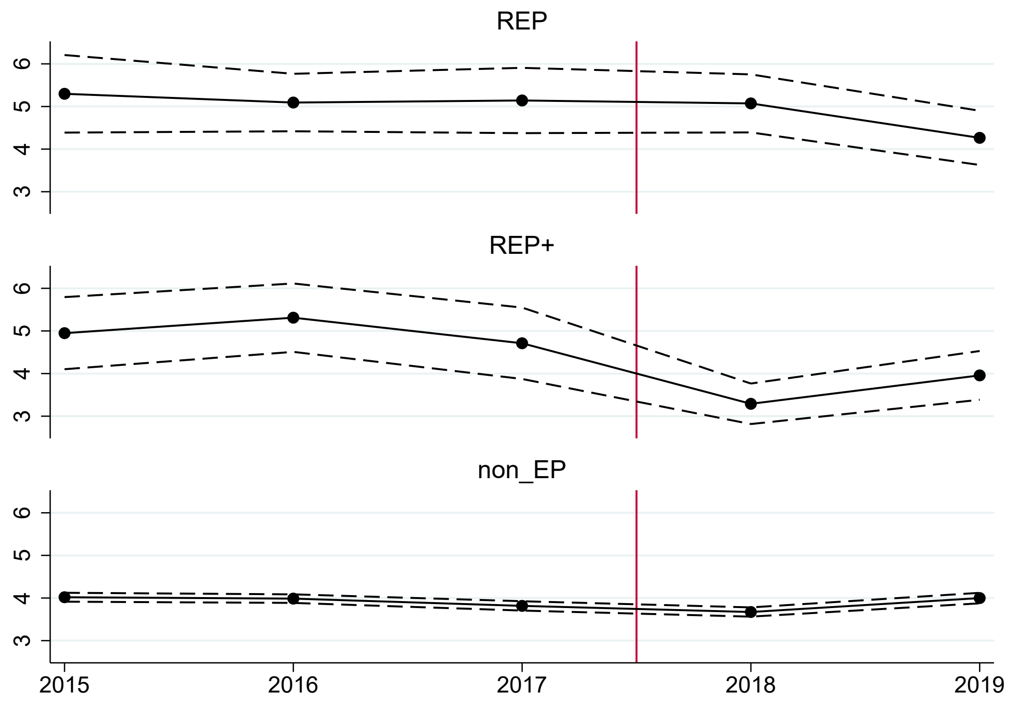

Figure 7: Average rank of schools in lists

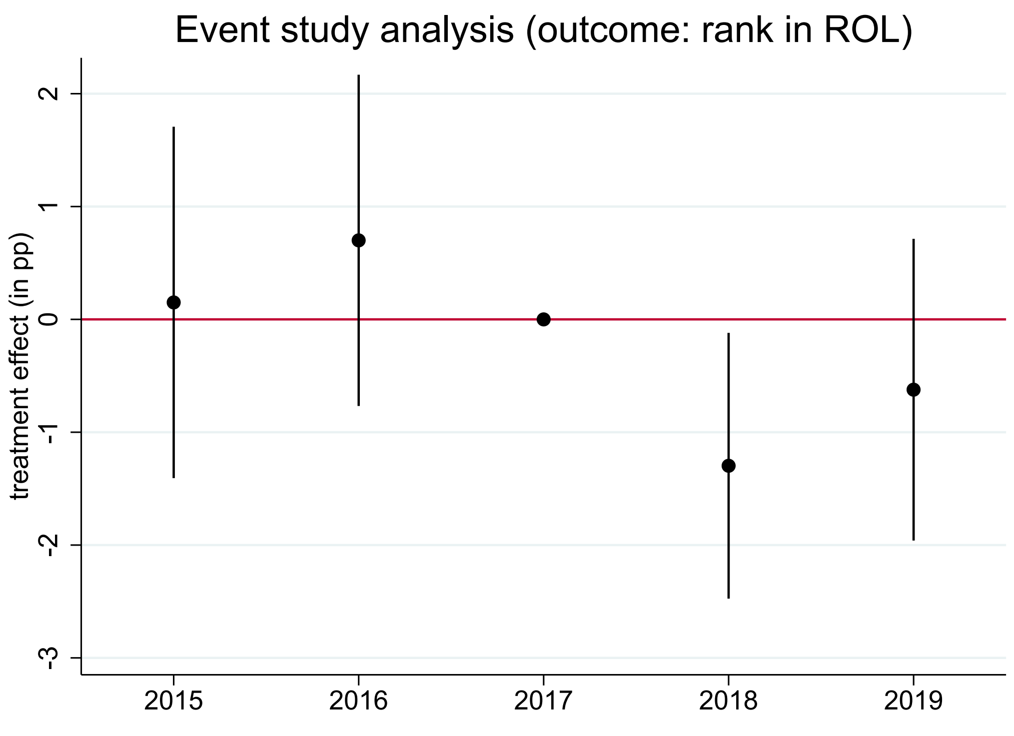

Figure 8: Event study approach (outcome: average rank of REP^{+} schools in lists)

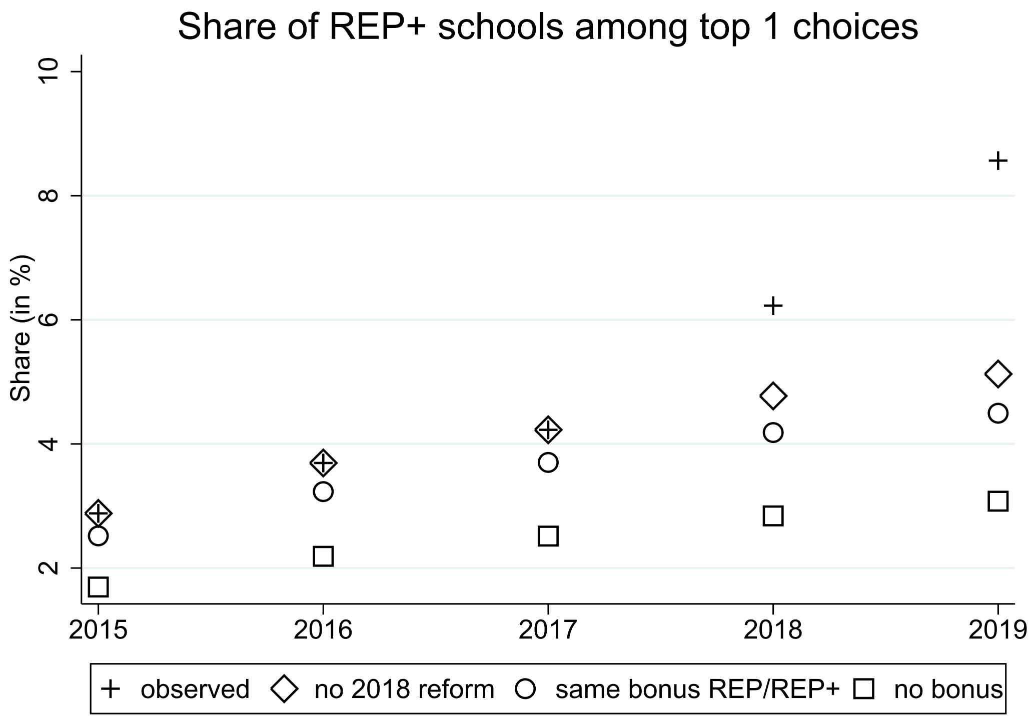

Figure 9: Impact of financial incentives on shares of REP^{+} among top 1 choices

B Tables

Table 1: DinD estimates (outcome: share of REP$^+$ among all choices)

	(1)	(2)	(3)	(4)
REP$^+$	-0.526*		-1.258***	
	(0.307)		(0.424)	
REP$^+$ $\times$ $\mathbb{1}\{t \geq 2018\}$	2.356***	2.083***		
	(0.585)	(0.587)		
Bonus (in €1,000)			1.317***	1.151***
			(0.338)	(0.355)
Year FE	Yes	Yes	Yes	Yes
Teacher $\times$ REP$^+$ FE	No	Yes	No	Yes
Observations	11,804	11,804	11,804	11,804
R^2	0.004	0.005	0.008	0.005

Note. Comparison group: REP schools. Shares: in %.
Robust standard errors clustered at the teacher level.

Table 2: DinD estimates (outcome: share of REP$^+$ among top 1 choices)

	(1)	(2)	(3)	(4)
REP$^+$	-0.201		-0.986*	
	(0.407)		(0.564)	
REP$^+$ $\times$ $\mathbb{1}\{t \geq 2018\}$	3.164***	3.096***		
	(0.819)	(0.862)		
Bonus (in €1,000)			1.591***	1.395***
			(0.468)	(0.503)
Year FE	Yes	Yes	Yes	Yes
Teacher $\times$ REP$^+$ FE	No	Yes	No	Yes
Observations	11,631	11,631	11,631	11,631
R^2	0.005	0.006	0.009	0.004

Note. Comparison group: REP schools. Shares: in %.
Robust standard errors clustered at the teacher level.

Table 3: DinD estimates (outcome: rank of REP$^+$ schools in teachers' lists)

Comparison group	REP schools		regular schools		REP & regular schools	
REP$^+$ $\times$ $\mathbb{1}\{t \geq 2018\}$	-0.720		-1.238***		-1.196***	
	(0.499)		(0.447)		(0.432)	
Bonus (in €1,000)		-0.094		-0.453**		-0.447**
		(0.243)		(0.228)		(0.215)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,327	1,327	17,588	17,588	18,222	18,222
R^2	0.106	0.104	0.144	0.144	0.144	0.143

Note. Controls: travel time (in minutes).
Robust standard errors clustered at the teacher level.

Table 4: Fake treatment groups (*départements*)

	(1)	(2)	(3)	(4)	(5)
Département	Aude	Gard	Hérault	Lozère	Pyrénées-Orientales
Département $\times$ $\mathbb{1}\{t \geq 2018\}$	-0.733	1.604*	-0.912	-0.354	0.906
	(0.901)	(0.876)	(0.726)	(2.818)	(2.017)
Controls	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes
Observations	17,529	17,529	17,529	17,529	17,529
R^2	0.109	0.110	0.109	0.108	0.109

Note. Controls: travel time (in minutes).
Robust standard errors clustered at the teacher level.

Table 5: Estimates from the structural model

Bonus (in €1,000)	0.239***
	(0.076)
Year FE	Yes
School FE	Yes
average among 233 regular schools	0.07
average among 16 REP schools	-0.43
average among 17 REP$^+$ schools	-0.64
Observations	1,330
R^2	0.779

Note. Sample at the school-year level: 266 schools, 2015-2019.
Robust standard errors clustered at the teacher level.

Table 6: Breaking down variations in shares of top 1 choices

	Year	2017	2018	2019
REP$^+$ schools	Observed	4.2	6.2	8.6
	(a) 2017 financial incentives	4.2	4.8	5.1
	(b) 2017 local demand shocks	4.2	5.9	7.4
REP schools	Observed	4.4	3.7	5.6
	(a) 2017 financial incentives	4.4	3.8	5.8
	(b) 2017 local demand shocks	4.4	4.4	4.3
Regular schools	Observed	91.4	90.1	85.8
	(a) 2017 financial incentives	91.4	91.4	89.1
	(b) 2017 local demand shocks	91.4	89.7	88.3

Note. Shares of schools ranked first: in % of rank-ordered lists.

Table 7: Counterfactual experiment: neutralizing the 2018 reform

Year		2017	2018	2019
No transfer request (outside option)	Observed	89.32	90.09	90.53
	Simulated	89.32	90.22	90.84
	Difference	0.00	0.14	0.31
Regular school ranked first	Observed	9.76	8.93	8.13
	Simulated	9.76	8.94	8.16
	Difference	0.00	0.01	0.03
REP school ranked first	Observed	0.47	0.37	0.53
	Simulated	0.47	0.37	0.53
	Difference	0.00	0.00	0.00
REP^+ school ranked first	Observed	0.45	0.62	0.81
	Simulated	0.45	0.47	0.47
	Difference	0.00	-0.15	-0.34
Part of difference coming from REP schools (%)		.	0.37	0.53
Part of difference coming from regular schools (%)		.	8.98	8.19
Part of difference coming from outside option (%)		.	90.64	91.27

Top panels: Unconditional shares (in %).

Bottom panel: Baseline = Difference in REP^+ schools (in absolute).

Table 8: Teachers' preferences

Dependent variable	rank-ordered list (ROL)	
Travel time (in minutes)	-0.018***	-0.027*
	(0.002)	(0.015)
Travel time × female		0.000
		(0.004)
Travel time × aged 40-		0.012
		(0.015)
Travel time × aged 41-50		0.005
		(0.016)
Travel time × aged 51-60		0.002
		(0.016)
Bonus (in €1,000)	0.260**	0.274
	(0.108)	(0.210)
Bonus × ($0 \leq$ Seniority ≤ 3)		0.219***
		(0.085)
Bonus × ($4 \leq$ Seniority ≤ 8)		0.127
		(0.097)
Bonus × currently in REP school		0.139
		(0.093)
Bonus × currently in REP^{+} school		0.245***
		(0.093)
Bonus × *Agrégation*		0.096
		(0.159)
Bonus × *Classe normale*		-0.200
		(0.188)
# of observations (N)	18,222	18,222
$\log(L)/N$	-0.835	-0.834
# of ROLs	5,902	5,902
# of teachers	3,251	3,251
# of school FE (π_j)	266	266
$\bar{\pi}_j$ (REP^{+} schools)	-0.160	-0.156
$\bar{\pi}_j$ (REP schools)	-0.090	-0.019
$\bar{\pi}_j$ (regular schools)	0.532	0.561
1 estimated s.d. of π_j	1.852	1.850

Note. Rank-ordered Logit model (ML estimation).
Robust standard errors clustered at the teacher level.

C Supplementary material

C.1 Institutional background details

From a theoretical viewpoint, the underlying algorithm is a slight modification of a deferred-acceptance (DA) mechanism and proceeds in two steps. First, every teacher already present in a school is ranked above any other teacher external to that school. Second, the matching process results from a DA-algorithm *à la* Gale and Shapley (1962). Importantly, this two-step procedure preserves the strategy-proofness property of the DA mechanism. Fack, Grenet, and He (2019) prove the existence of a pure strategy bayesian Nash equilibrium in such a setting, yet they emphasize the role played by application costs which may refer to cognitive burden and institutional restrictions on ROLs' length. In the absence of any application cost, the mechanism induces a truthful revelation of preferences, but multiple equilibria may arise. In contrast, with positive application costs, and as long as teachers play undominated strategies, they need only submit some partial order of their true preferences. Weakly truth-telling (WTT) is a strategy that consists in ranking her most preferred schools only; it may not be dominant. The plausibility of truthful implementation is an empirical issue: when studying the first round of teacher allocation in France, Combe, Tercieux, and Terrier (2021) argue that the WTT assumption is all the more likely when the mechanism includes no application cost, when teachers are aware of injunctions coming from both the Department of Education and unions, when they have a good knowledge of the allocation process (it depends more generally on teachers' information set), and when they face some uncertainty. *De facto*, at the time of application, teachers know their own priority index for each school, but ignore their colleagues' choices, and can hardly predict school-specific cutoffs. Indeed, teachers are ranked by schools according to their single-dimensional priority index, the so-called *barème*; this framework corresponds to a strict priority environment. The *barème* depends on experience, seniority, individual- and school- characteristics. Uniform rules are applied, which is common knowledge; in particular, schools directors are not involved in this centralized matching procedure. Every teacher can be allocated to a school whenever her priority index exceeds its school-specific cutoff. Those cutoffs cannot easily be predicted: they are not disclosed publicly: in particular, they are not available on unions websites (only the cutoffs related to the first round can be found on those websites).

C.2 Supplementary descriptive statistics

Table 9: Extensive margin

	2015	2016	2017	2018	2019
Share of teachers requesting some transfer (%)	15.3	15.1	13.5	12.6	11.8
Among beginners (age$\leq$ 30)	*27.8*	*30.7*	*31.1*	*28.3*	*26.1*
Average number of choices per list	7.3	6.8	6.6	6.6	6.6
Among beginners (age $\leq$ 30)	*11.4*	*11.1*	*9.9*	*9.6*	*10.7*
Number of choices to:					
REP+ schools	224	206	195	307	329
Among beginners (age $\leq$ 30)	*57*	*66*	*63*	*84*	*79*
REP schools	202	242	200	215	213
Among beginners (age $\leq$ 30)	*40*	*50*	*53*	*45*	*31*
Regular schools	4,990	5,025	4,234	3,722	3,314
Among beginners (age $\leq$ 30)	*415*	*456*	*420*	*283*	*308*

Sample: Teachers from Montpellier educational authority already assigned to a school.

Table 10: Shares of teachers requesting some transfer (depending on current school assignment, %)

	2015	2016	2017	2018	2019
REP$^+$ schools	21.9	22.0	17.4	15.1	10.5
REP schools	23.0	22.3	17.5	13.9	13.2
Regular schools	14.6	14.4	13.1	12.4	11.8

Sample: Teachers from Montpellier educational authority, already assigned to a school.
Lecture: In 2015, 21.9% of tenured teachers assigned to a REP$^+$ school asked for a transfer.

Table 11: Teachers' characteristics

	mean	sd	min	D1	Q1	med	Q3	D9	max	*# of obs.*
Age	42.4	8.4	23	31	36	42	48	55	67	*5,902*
Seniority	13.2	7.9	1	2	4	10	15	20	41	*5,900*
Commuting time	27.9	20.6	1	8	15	25	36	50	208	*5,902*
Priority index (2015)	477	543	21	73	185	331	504	785	3,213	*1,300*
Priority index (2016)	578	839	21	68	186	368	570	931	7,894	*1,340*
Priority index (2017)	602	1,165	21	62	131	350	589	849	8,593	*1,155*
Priority index (2018)	644	1,310	21	66	114	346	538	856	8,540	*1,097*
Priority index (2019)	744	1,231	14	95	215	476	734	1,062	8,444	*1,010*

Sample: Restricted sample, i.e. teachers (non-vocational education) from Montpellier educational authority, already assigned to a school, requesting a transfer, and whose rank-ordered list includes at least a school-specific choice. Full-filled lists and teachers suspected to move are removed.
Note Commuting time is measured during rush hours between teacher residence location and school choice. The average commuting time per ROL is reported here.

Table 12: Probability of requesting some transfer

Dependent variable	Dummy for transfer request
Male	1
Female	0.852*** (0.024)
CAPES	1
Agrégation	1.112*** (0.042)
2015	1
2016	1.047*** (0.044)
2017	0.910*** (0.040)
2018	0.866*** (0.038)
2019	0.810*** (0.037)
Age dummies	Yes
Observations	57,426
$\log(L)/N$	-0.316

Note. Odds ratios from a Logit model. Robust standard errors.

Table 13: Rank in list and commuting time

	Fraction reporting	Commuting time
Rank	(1)	(2)
1	100.00	22
2	79.51	23
3	67.29	24
4	57.87	26
5	49.86	28
6	44.22	28
7	38.83	29
8	34.65	31
9	30.94	31
10	27.93	33
11	25.20	35
12	22.88	36
13	20.93	34
14	19.36	36
15	17.79	37
16	16.28	37
17	14.77	39
18	13.43	40
19	11.86	49

Sample: Rank-ordered lists of school choices filled out by teachers from Montpellier educational authority already assigned to a school. 18,222 choices in 5,902 lists.
Notes : (1) Fractions of teachers applications listing each choice. (2) Average commuting time between a teacher's home and each choice, measured in minutes during rush hours.

Table 14: Definition of working samples: selection steps

	# of teachers	**# of lists**	**# of choices**	# of school-specific choices	# of municipality choices	# of other geographical choices	# of other choices	**# of schools**
Raw data	7,532	14,482	115,386	38,321	42,092	25,349	9,624	405
Deletion of lists from vocational teachers and specific staff (1)	5,426	10,676	89,751	30,191	33,882	18,778	6,900	364
Deletion of lists from teachers of another educational authority	4,360	8,913	60,387	23,618	24,773	8,919	3,077	356
Deletion of lists when WTT assumption is questionable (2)	3,980	7,918	42,548	19,713	16,808	4,449	1,578	351
Deletion of choices for specific schools and teacher positions	3,952	7,848	40,434	19,177	16,808	4,449	0	266
Deletion of lists when teacher is suspected to move	3,825	7,537	38,644	18,222	16,200	4,222	0	266
Deletion of lists with no school-specific choice (**Working sample**)	3,251	5,902	31,164	18,222	10,270	2,672	0	266
Exploding geographical choices into schools	3,825	7,537	117,066	117,066	0	0	0	269
Adding up unranked schools located less than 60 minutes from teacher home (**Exploded sample**)	3,825	7,537	610,520	610,520	0	0	0	269

Notes. (1) Vocational schools are not concerned by REP/REP$^+$ programs.
School librarians, school counselors and school psychologists are not in the scope of the current analysis.
(2) 932 lists are filled with 20 choices, i.e. the maximal length authorized.
106 lists are detected as benefiting from a priority index point bonus on their first choice.

Table 15: Share of REP/REP$^+$ among school choices (%)

		Share among all choices		At least once among first three choices		Share among first choices		
		REP	REP+	REP	REP+	REP	REP+	*N*
Middle-school choices	**2015**	6.3	6.5	9.4	7.5	7.1	7.5	*1,486*
	2016	9.2	7.5	14.6	10.4	12.7	11.1	*1,631*
	2017	9.2	7.5	13.0	9.3	13.0	12.3	*1,380*
	2018	8.8	11.3	10.4	21.3	8.4	17.2	*1,402*
	2019	9.8	14.8	16.3	22.3	13.1	22.3	*1,429*
Novices seniority $\leq$ 3 y.	**2015**	5.4	6.9	13.0	10.5	5.2	12.5	*533*
	2016	7.8	6.7	13.2	12.6	6.4	8.0	*628*
	2017	6.3	7.1	6.5	12.0	5.3	11.5	*617*
	2018	5.9	8.1	4.9	13.3	3.6	15.3	*596*
	2019	8.4	12.4	16.9	22.4	9.5	16.1	*622*
Beginners. seniority 4-8 y.	**2015**	2.8	2.4	2.1	2.5	2.8	2.2	*850*
	2016	4.7	5.4	10.5	10.0	5.7	8.3	*763*
	2017	4.9	4.9	6.7	7.4	6.2	7.3	*699*
	2018	6.0	8.8	6.4	17.4	7.1	10	*719*
	2019	5.4	9.2	7.1	15.9	8.7	12.0	*793*
Teachers. seniority >8 y.	**2015**	1.5	1.5	3.0	2.2	1.4	0.8	*2,659*
	2016	2.2	1.3	4.4	2.0	2.8	1.4	*2,926*
	2017	2.4	1.2	5.2	1.1	3.0	1.2	*2,217*
	2018	2.3	2.4	4.8	5.2	1.7	2.9	*1,921*
	2019	2.7	3.7	6.0	6.8	3.3	6.5	*1,679*
CAPES	**2015**	2.5	2.7	4.5	3.9	2.0	2.5	*3,499*
	2016	3.9	3.2	7.6	5.7	4.1	3.7	*3,608*
	2017	4.0	3.0	6.9	4.3	4.6	4.0	*2,893*
	2018	4.4	6.2	6.0	10.9	3.4	8.9	*2,663*
	2019	5.4	7.9	9.9	14.1	6.4	10.1	*2,498*
Agrégation	**2015**	1.1	0.2	1.8	0.0	1.7	0.0	*543*
	2016	1.4	1.0	2.5	1.3	1.5	0.5	*709*
	2017	1.7	2.8	0.3	4.1	1.1	2.7	*640*
	2018	0.9	1.9	1.4	2.6	1.3	1.9	*573*
	2019	1.0	2.5	2.7	4.7	1.3	5.7	*596*

Sample: Restricted sample of school-specific choices by teachers from Montpellier educational authority already assigned to a school.

C.3 Supplementary robustness checks

Table 16: Estimated ATEs under various specifications of travel costs (outcome: rank of REP^+ schools in lists)

	(1)	(2)	(3)
Comparison group	REP schools	regular schools	REP & regular schools
Travel time (baseline)	-0.720*	-1.238***	-1.196***
	(0.499)	(0.447)	(0.432)
Travel time + travel time squared	-0.768	-1.266***	-1.228***
	(0.493)	(0.440)	(0.425)
Travel time - off-peak hours	-0.716	-1.239***	-1.197***
	(0.500)	(0.449)	(0.434)
Euclidean distance	-0.161	-1.338*	-1.204
	(0.811)	(0.782)	(0.762)
Effective distance	-0.709	-1.246***	-1.195***
	(0.502)	(0.453)	(0.438)
Time FE	Yes	Yes	Yes
School FE	Yes	Yes	Yes

Robust standard errors clustered at the teacher level.

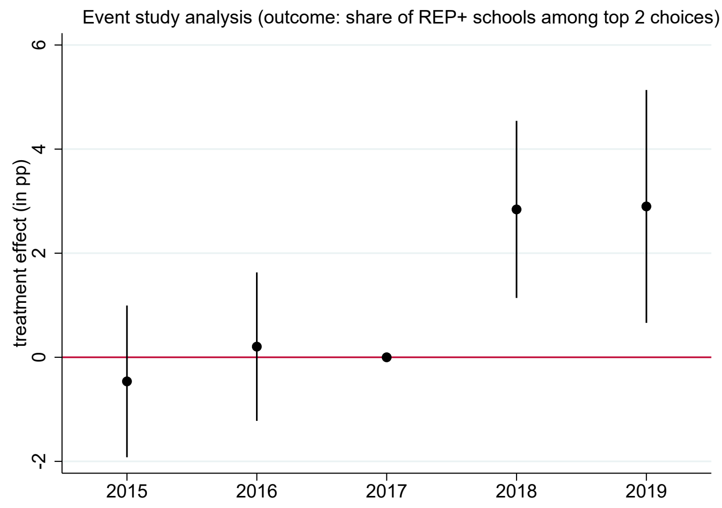

Figure 10: Share of REP$^+$ schools (in % of top 2 choices)

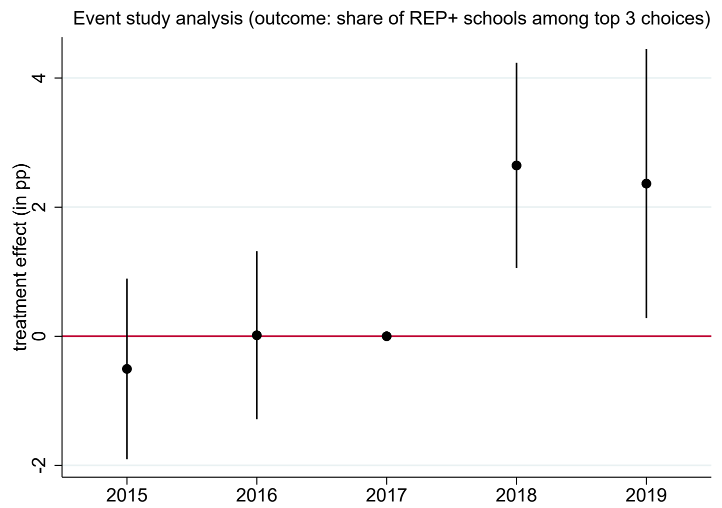

Figure 11: Share of REP$^+$ schools (in % of top 3 choices)

C.4 Considering geographical choices

Exploding geographical choices: an example In this example, we consider a teacher who ranks 4 choices: two schools (M1 and A), then a municipality (M) with three schools (the already ranked M1, but also M2 and M3) and a school called B. We assume further that this teacher is located less than one hour from five other unranked schools, called U1 to U5.

As regards the sample restricted to school-specific choices, the municipality choice is simply not considered, while in the exploded sample that choice is replaced with schools M2 and M3. More precisely, it would have been replaced with schools M1, M2 and M3 if M1 had not been ranked above the latter two. Since teachers are assumed to be indifferent between schools within the same geographical area, both M2 and M3 are ranked third. The fourth choice, namely school B, becomes fifth. Finally, we add up schools U1 to U5 at the bottom of the ROL, which is in fact unimportant, with a rank equal to 0.

Initial ROL	**Restricted sample**	**Exploded sample**
1. School M1	**1.** School M1	**1.** School M1
2. School A	**2.** School A	**2.** School A
3. Municipality M		**3.** School M2
4. School B	**4.** School B	**3.** School M3
		5. School B
		0. School U1
		0. School U2
		0. School U3
		0. School U4
		0. School U5

Exploding geographical choices: the general case We assume that when opting for a geographical choice a teacher is indifferent among all schools within the corresponding area. Hence we explode such a choice in each and any school located in that area, with the same rank: as a result, this procedure leads to numerous ties. If a school appears more than once consecutive to this imputation, we keep its first occurrence only (in terms of rank ordering). Also, the ranks of stated choices following a geographical choice are shifted appropriately as explained above.

We assume further that any unranked school located less than one hour away from teachers' residence is revealed less preferred than those in the list. A convenient feature of the rank-ordered Logit model, which we use to estimate preferences in section 6, is precisely to deal with such unranked alternatives in an ordinal fashion: without loss of generality, these schools may receive a rank equal to 0.

To sum up, a second "exploded sample" is obtained as follows:

1. geographical choices are exploded into schools located in the corresponding area; when a school appears more than once in a ROL after this procedure, we select out all occurrences but the top-ranked one;

2. ranks are shifted accordingly: if a geographical choice has rank n in a ROL and is turned into k schools, we impute a rank equal to n to each and any of these schools, and $n + k + 1$ to the next choice; the latter rule is however adjusted to take schools which may have already been ranked into account;

3. we add all schools not included in the list, provided that they are located in an one hour radius from the teacher's residence; such schools are ranked at the bottom of each ROL, and receive a rank equal to 0.[32]

While the restricted sample had 18,222 observations at the teacher-year-school level, the size of the exploded sample has now 610,520 observations at the very same level.

Based on that exploded sample, Figure 12 suggests a positive but smaller effect of the 2018 reform, i.e. of stronger financial incentives provided to teachers working in REP$^+$ schools, on the share of those schools among all schools. From 2017 to 2019, the proportion of REP$^+$ schools among all (explicit or implicit) choices increased by 0.4pp on that sample (from 5.9% to 6.3%).

[32]Ranking these alternatives at the bottom is neither necessary nor important; it is however essential to attribute them a rank (here, 0) that is common to all unranked alternatives, including those coming from different lists.

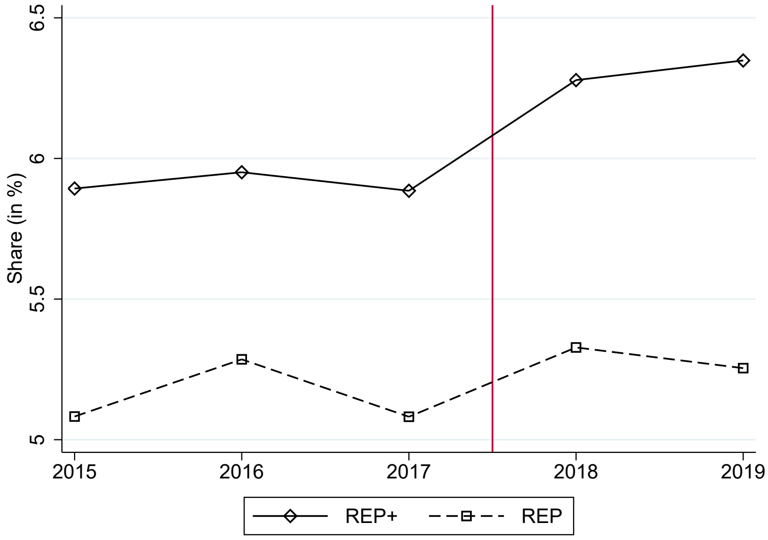

Figure 12: Share of REP/REP^{+} schools in teachers' lists (in % of all schools)

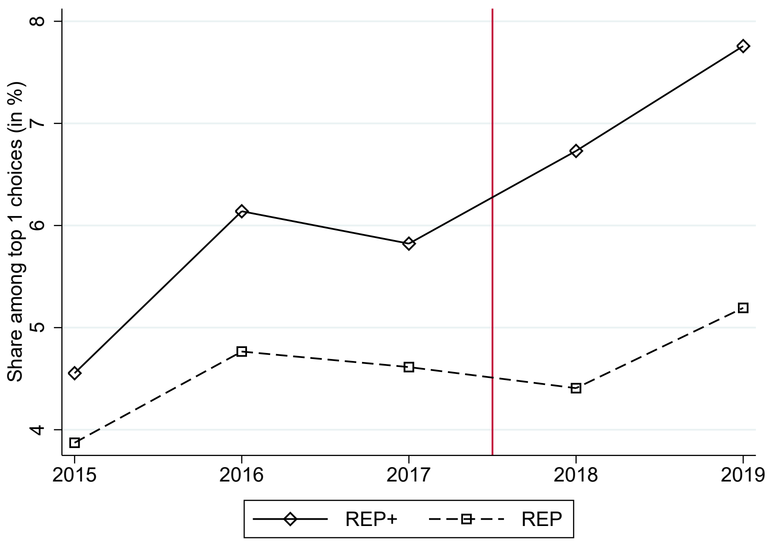

Figure 13: Share of REP/REP^{+} schools (in % of top 1 choices)

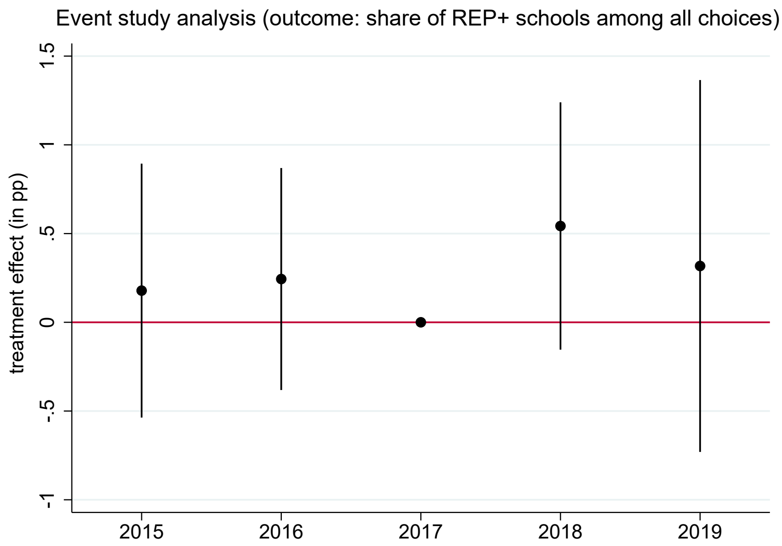

Figure 14: Event study approach (share of REP/REP$^+$ schools)

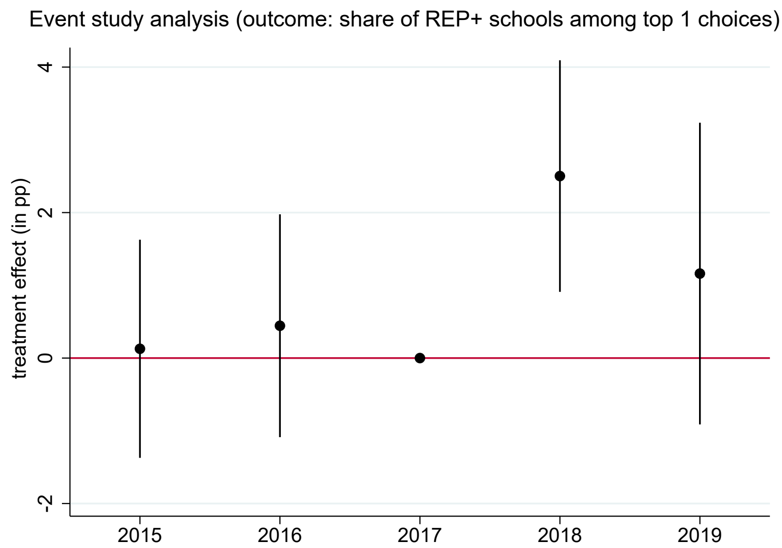

Figure 15: Event study approach (share of REP/REP$^+$ schools among top 1 choices)

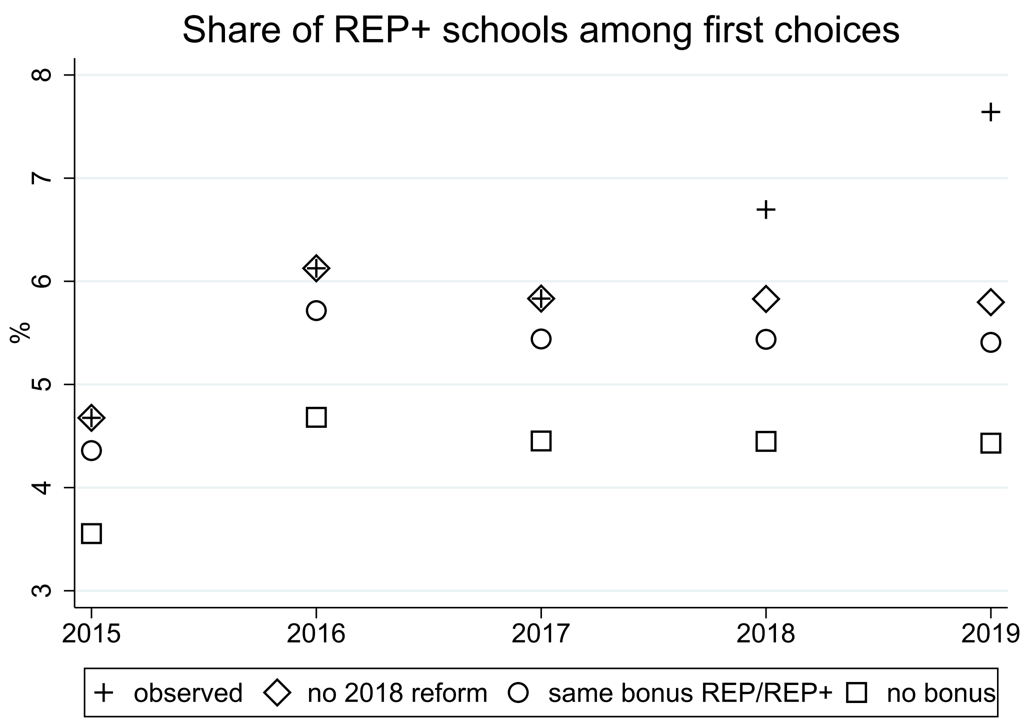

Figure 16: Impact of financial incentives on shares of REP^+ schools among top 1 choices

Table 17: DinD estimates (outcome: share of REP$^+$ schools among all choices)

	(1)	(2)	(3)	(4)
REP$^+$	-0.057		-0.167	
	(0.165)		(0.223)	
REP$^+$ $\times$ $\mathbb{1}\{t \geq 2018\}$	0.453	0.323		
	(0.292)	(0.335)		
Bonus (in €1,000)			0.228	0.116
			(0.175)	(0.206)
Year FE	Yes	Yes	Yes	Yes
Teacher $\times$ REP$^+$ FE	No	Yes	No	Yes
Observations	15,074	15,074	15,074	15,074
R^2	0.001	0.001	0.004	0.001

Note. Comparison group: REP schools. Shares: in %.
Robust standard errors clustered at the teacher level.

Table 18: DinD estimates (outcome: share of REP$^+$ schools among top 1 choices)

	(1)	(2)	(3)	(4)
REP$^+$	-0.312		-0.890*	
	(0.332)		(0.454)	
REP$^+$ $\times$ $\mathbb{1}\{t \geq 2018\}$	2.382***	1.748***		
	(0.641)	(0.672)		
Bonus (in €1,000)			1.191***	0.609
			(0.360)	(0.385)
Year FE	Yes	Yes	Yes	Yes
Teacher $\times$ REP$^+$ FE	No	Yes	No	Yes
Observations	14,864	14,864	14,864	14,864
R^2	0.003	0.003	0.006	0.002

Note. Comparison group: REP schools. Shares: in %.
Robust standard errors clustered at the teacher level.

Table 19: Estimates from the structural model

Bonus (in €1,000)	0.127***
	(0.047)
Year FE	Yes
School FE	Yes
average among 236 regular schools	0.05
average among 16 REP schools	-0.4
average among 17 REP$^+$ schools	-0.38
Observations	1,345
R^2	0.868

Note. Sample at the school-year level: 266 schools, 2015-2019.
Robust standard errors clustered at the teacher level.

Table 20: Breaking down variations in shares of top 1 choices

	Year	2017	2018	2019
REP$^+$ schools	Observed	5.8	6.7	7.6
	(a) 2017 financial incentives	5.8	5.8	5.8
	(b) 2017 local demand shocks	5.8	6.8	7.8
REP schools	Observed	4.7	4.5	5.3
	(a) 2017 financial incentives	4.7	4.6	5.4
	(b) 2017 local demand shocks	4.7	4.7	4.7
Regular schools	Observed	89.4	88.8	87.1
	(a) 2017 financial incentives	89.4	89.6	88.8
	(b) 2017 local demand shocks	89.4	88.5	87.5

Note. Shares of schools ranked first: in % of rank-ordered lists.

Table 21: Counterfactual experiment: neutralizing the 2018 reform

Year		2017	2018	2019
No transfer request (outside option)	Observed	75.09	76.67	75.99
	Simulated	75.09	76.84	76.35
	Difference	0.00	0.17	0.36
Regular school ranked first	Observed	22.29	20.71	20.91
	Simulated	22.29	20.71	21.01
	Difference	0.00	0.04	0.10
REP school ranked first	Observed	1.18	1.06	1.26
	Simulated	1.18	1.06	1.27
	Difference	0.00	0.00	0.01
REP$^+$ school ranked first	Observed	1.45	1.56	1.83
	Simulated	1.45	1.35	1.37
	Difference	0.00	-0.21	-0.46
Part of difference coming from REP schools (%)		.	1.08	1.28
Part of difference coming from regular schools (%)		.	21.03	21.30
Part of difference coming from outside option (%)		.	77.89	77.41

Top panels: Unconditional shares (in %).

Bottom panel: Baseline = Difference in REP$^+$ schools (in absolute).

Table 22: Teachers' preferences

Dependent variable	rank-ordered list (ROL)	
Travel time (in minutes)	-0.038***	-0.029***
	(0.002)	(0.005)
Travel time × female		-0.003
		(0.003)
Travel time × aged 40-		-0.004
		(0.005)
Travel time × aged 41-50		-0.016***
		(0.006)
Travel time × aged 51-60		-0.005
		(0.006)
Bonus (in €1,000)	0.048***	-0.004
	(0.018)	(0.029)
Bonus × novice		0.163***
		(0.016)
Bonus × beginner		0.125***
		(0.018)
Bonus × currently in REP school		0.055*
		(0.028)
Bonus × currently in REP$^+$ school		0.131***
		(0.027)
Bonus × *Agrégation*		-0.206***
		(0.030)
Bonus × *Classe normale*		-0.038
		(0.027)
# of observations (N)	610,520	610,520
$\log(L)/N$	-0.725	-0.724
# of ROLs	7,537	7,537
# of teachers	3,825	3,825
# of school FE (π_j)	269	269
$\bar{\pi}_j$ (REP$^+$ schools)	0.221	0.220
$\bar{\pi}_j$ (REP schools)	0.236	0.241
$\bar{\pi}_j$ (regular schools)	0.329	0.338
1 estimated s.d. of π_j	0.682	0.673

Note. Rank-ordered Logit model (ML estimation).
Robust standard errors clustered at the teacher level.