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Book

More money, better students? : evidence from a funding reform of Danish high schools (preliminary version)

More Money, Better Students? - Evidence from a Funding Reform of Danish High Schools (preliminary version)

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Abstract: This paper investigates the effect of funding in Danish high schools on the educational outcomes of students. To address endogeneity we exploit the standardization of government subsidies in 2008 that lead to rapid convergence in funding pr. student. At the high-school level, more funding lead to more teacher working hours pr. student, but did not increase average teacher wages. We find no significant effects of reform-induced funding changes on a wide range of student outcomes including completion, exam grades and the propensity to continue education. Our IV-strategy yields relatively precise estimates of the effects on student outcomes indicating that our results are not caused by large statistical uncertainty.

1 Introduction

What is the effect of school spending on student educational outcomes? The typical conclusion of correlational studies is that the effect is close to zero, see e.g. the review in Hanushek [2006]. A critique of this literature is that it does not sufficiently take into account the endogeneity of school spending. If more ressources are allocated to schools with many low-achieving students and this reverse causality is left unaccounted for, it would generate a negative bias in the estimate of the effect of spending on educational outcomes. In line with this critique, an emerging literature exploits exogenous variation in funding due to reforms and typically concludes that spending causes higher student success in terms of higher grades, higher completion rates and/or better adult labour market outcomes. (Jackson et al. [2015], Hyman [2017], Lafortune et al. [2018]).

The present paper papers investigates the causal impact of spending on student out-

comes in Danish high schools using a recent reform that provides exogeneous variation in government funding. The paper contributes to the literature in two ways: First, we investigate the effect of funding in high schools whereas existing papers focus on the lower grades of the school system or on the first through twelth grade in its entirety. The effect of funding could be different in high school, since the literature typically suggests that cognitive and non-cognitive abilities are more malleable early on (see e.g. Burgess [2016] and Elango et al. [2015]). Second, to the best of our knowledge, we are the first to study the effect of funding on educational outcomes in a Scandinavian country.

We exploit exogenous variation in funding from a reform that standardized government subsidies from 2008 and onward. Prior to the reform funding per student varied substantially amongst high schools. The standardization implied a relatively rapid convergence eliminating much of this variation. We exploit this by using pre-reform funding levels, i.e. 2007-level, as exogenous variation in post-reform changes in funding, and investigate how these changes affect various outcomes of educational success. This amounts to comparing the development of educational outcomes on high schools with a low initial funding level (reform winners) to those with a high initial funding level (reform losers). In support of the exogeneity of our instrument, we find no differences in pre-reform trends of educational outcomes between reform winners and reform losers. To deal with student selection we control for a number student-level variables reflecting socio-demographic characteristics and grades prior to high school.[1]

The results show that there is no significant effect of funding on a wide range of educational outcomes. First and foremost we investigate the effect on graduation, grades and continuation to post-secondary education. In robustness checks, we document that there is no significant effect on other measures of educational success such as the choice of type of education, the choice of elective courses at A-level, and alternative measures of grade point average. The standard errors on our IV estimates are small and the instrument is strong indicating that our insignificant estimates are not the result of large statistical uncertainty or a weak instrument generating a bias towards zero. We conduct several types of robustness checks to document that our results are robust to changes in the sample,

[1] The evidence of Deming [2014] suggests that controlling for prior test scores is an adequate strategy to account for selection. Moreover, we argue below that any selection bias not accounted for would work against our main conclusion.

outcome variables and estimation strategy.

We proceed by showing that the reforms did not have any significant effect on the number of students. We do however find a small effect on student composition. In particular, more reform-induced funding seems to attract students with a higher prior GPA. A potential explanation for this finding is that reform winners spent the extra funding on factors that high-achievers value particularly high, or that high achievers are better informed about the reform and its consequences. This finding indicates that a possible selection bias would work against the result that the effect of funding is insignificantly different from zero: Since competent students select into schools that receive more money as a consequence of the reform, we should expect a positive bias on the estimated effect of reform-induced funding on educational outcomes.

There are several possible explanations of why our results differ from the newer literature that tends to find positive effect of funding on student outcomes. First, it could be that parents substitute for changes in school quality by helping out with home work or buying supplementary education. In support of this explanation, the literature suggests that school inputs have a particularly large effect on children whose parents have low income or low levels of education. For instance, Jackson et al. [2015] only find a significant effect on children of poor parents. We do not find that the results are different for students whose parents are relatively low paid or less educated. Still, we cannot rule out that our sample of Danish high school students are less disadvantaged than primary school students typically considered in the existing studies from Ango-Saxon countries. Second, it could be that the level of funding is relatively high and, as a consequence, the marginal effect is of additional funding is low. We test this hypothesis by splitting the sample in two: Those that benefitted from the reform, and those that lost. The latter group consists of high schools with a relatively low pre-reform level of funding. We do not find a significant effect for this sample. Still, we cannot rule out that the marginal effect is higher for lower levels of funding than those considered in this analysis. Third, the existing literature focus on lower grades where the effect of spending could be stronger as suggested by the literature on early vs. late education cited above.

A fourth possibility is that the money was spent on factors that did not affect education quality. To investigate this idea we examine the consequences of the reform on the high-

school descisions about spending and employment. We find that more funding implies a higher wage bill and more teacher hours pr. student, while average teacher wages and the high school grades of teachers are unaffected. The literature suggests that teacher quality is a key input in education production, but the evidence on the effect of the quantity of education is mixed.(see the review in Burgess [2016] and Filges et al. [2015]) For instance, the effect of class size on educational outcomes is typically small or non-existent. Thus, a possible explanation for our finding that funding did not affect student outcomes result is that the extra teacher hours was used to lower class size.

The paper proceeds as follows: The next section describes the funding reform. In Section 3, we explain the estimation strategy which falls in two steps: In the first step we present yearly estimates that allow for a graphical inspection of the gradual implementation effects of the reform. The second step employs IV-regressions producing single estimates of the effect of funding on outcomes. In Section 4 we present the results. The final section concludes.

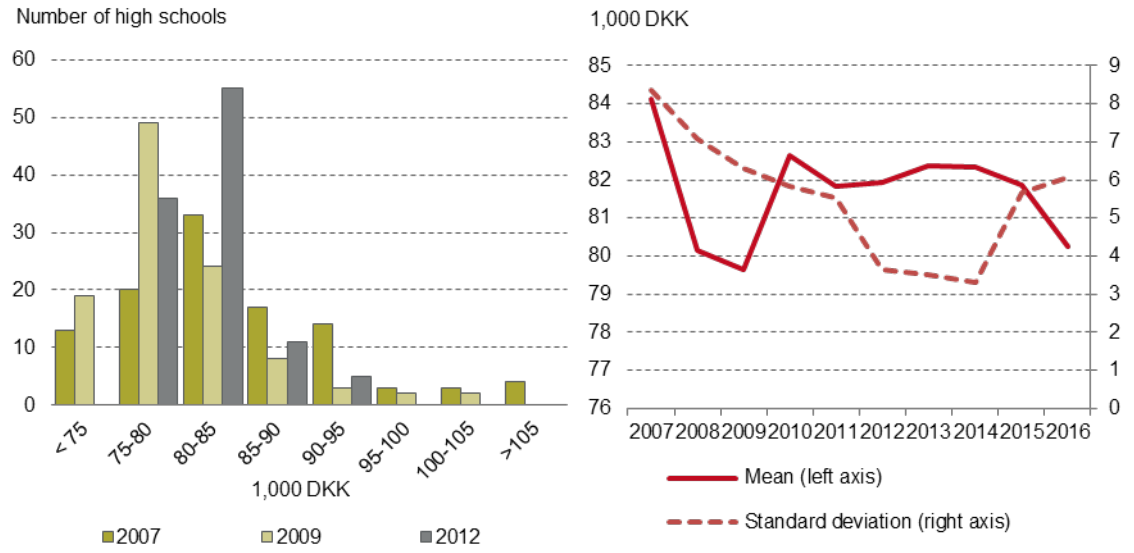
2 The funding reform

The identification strategy relies on exogenous variation in high school spending resulting from a reform of the subsidies to public, government-funded high schools.[2] Prior to 2008, funding did not follow a strict scheme, but was based on individual applications and subject to idiosyncratic decisions related to school-specific factors and differing practices across the county administrations who controlled the schools. Consequently, as shown in figure 1, funding per student varied quite a lot with 13 out of 107 schools receiving less than 75,000 DKK per student and 10 schools receiving more than 95,000 DKK.

In 2007 the high schools became self-governing entities and the year after they started a gradual transition towards a simpler, rule-based funding scheme. Under the new rules, most of the subsidies were administered as a common and constant grant per student.

[2]The focus of the analysis is the so-called public common high schools (in Danish, "offentlige almene gymnasier") that teach a broad range of subjects to prepare students for university or college. Other types of secondary schooling, that we disregard, include the vocational high schools (in Danish, "erhvefrrvs-gymnasier") that have a stronger emphasis on natural sciences or business studies than the common high schools. We also disregard the some 20 privately owned high schools, since they follow different funding schemes than public schools. In the remainder of the paper, we use the term high school as a shorthand for public common high school.

Figure 1: The distribution of funding per student, 2007-2016



Notes: Left: The distribution of funding per student in 2007, 2009, and 2012 across the 107 high schools. Right: Average funding per student from 2007 to 2016. In both figures, funding is converted to 2010-prices using a price index for public expenditure.

As a consequence, the differences in funding per student across high school diminished after 2007. In 2012, when the funding reform was fully fased in, there where no schools receiving less than 75,000 DKK or more than 95,000 DKK per student. From 2007 to 2012 the standard deviation of funding per student was more than halved (a fall from 8,400 DKK to 3,600 DKK) while mean funding remained more or less constant. To shield schools from drastic decreases in funding from year to year the new funding scheme was fased in gradually. As a consequence standard deviation declined gradually.

In 2015 the standard deviation of funding increases. This is due to a reform of the funding system that amongst other things redistributed state subsidies towards high schools whose students had low prior test scores. To take into account the potential effects of this redistribution we control for prior test scores interacted with time dummies in the analysis below.

There were two other major reforms of the high school system about the time of implementation of the funding reform. The first reform affected the students that started in high school in 2005. It changed the structure of exams and the students' choice of subjects with the aim of increasing interdisciplinary teaching. The second reform implied that the high schools became self-governing in 2007. Prior to that each school was owned

and controlled by the administration of the county in which it resided. As of 2007 the school was controlled by a board of directors. The board members are appointed by local elected politicians, representatives of the business community, representatives from other educational institutions and the students and teachers at the high school in question.

In the estimation strategy, we remove the common effects of these reforms by controlling for year fixed effects. For the reforms to be a threat to our identification strategy, they would have to affect high schools with higher a funding level in 2007 (our instrument) differently than high schools with a lower funding level in 2007. Moreover, the timing of such effects would be consistent with the gradual implementation of the funding reform. As explained below, the gradual implementation of the funding reform implies that if funding had consequences for student outcomes we would expect the effect to kick in gradually. In particular, we would expect a gradually increasing difference in educational outcomes between reform winners and reform losers starting from cohort 2005 and ending with cohort 2012. There is no reason to expect the 2005 and 2007-reforms to have similar, gradual consequences for student outcomes. Nonetheless, we control for a number of variables at the high school level and student level that could potentially remove confounding variation.

3 Estimation strategy

3.1 Dynamic estimates

The first part of our estimation strategy is to graphically inspect the dynamic evolution of the effect of reform-induced spending on educational outcomes. We construct reform-induced changes in funding by using the predicted values of a regression of the change in funding on the pre-reform level of funding, i.e. funding level in 2007. We then investigate the effect of these reform-induced changes in funding on changes in various measures of educational outcomes over time. To predict changes in funding we estimate convergence equations of the following type:

$$\Delta T_{g,a} = \mu_a + \pi_a T_{g,2007}, \tag{1}$$

where $\Delta T_{g,a}$ is the change in funding per student in high school g from 2007 to year

a. π_a measures the effect of funding level in 2007 ($T_{g,2007}$) on changes in funding from 2007 to year a. We estimate this regression for each of the years from 2008 to 2016. The resulting estimates of π_a track the convergence of funding in the years following 2007. This is inspired by the strategy of Jackson et al. [2015], although they use a somewhat more complicated formula to predict funding changes that also incorporates the type of funding reform. Since all high schools in our sample are affected by the same type of reform, we opt for a more simple model to predict changes in funding. Moreover, there is less need for additional variables to predict funding changes since the initial funding level turns out to be a very strong predictor of subsequent changes in funding on its own.

We then investigate the effect of funding changes on student-level outcomes by estimating regressions of the following type:

$$Y_{i,t,g} = \beta_g + \gamma_t + \alpha_t \Delta \hat{T_{g,2012}} + \rho_t G_{g,2007} + \omega' X_{i,t,g}, \tag{2}$$

where $Y_{i,t,g}$ is the outcome variable for student i who started at high school g in year t. β_g is a high-school fixed effect, γ_t is a year fixed effect. $\Delta \hat{T_{g,2012}}$ is the predicted change in funding per student from 2007 to 2012. We choose 2012 since the results show that by then, the gradual convergence of funding per student has more or less come to an end. The coefficient of interest α_t measure the consequences of a 10,000 DKK increase in expected funding change from 2007 to 2012. Since high-school fixed effects are included, α_t captures the effect of predicted changes on changes in the outcome variable. Furthermore, we let α_t vary over time allowing us to inspect whether the effect of the reform changes over time. This is important since it could take time for the effect of the reform to kick in. Furthermore, the flexible estimation allows us to investigate whether the estimated effects correspond to the gradual implementation of the reform. α_{2004} is set to 0 implying that 2004 is the reference year so α_t measures the effect on the accumulated changes in outcome variables from cohort 2004 to cohort t. Cohort 2004 is the reference cohort, since it is the last cohort to remain unaffected by the reform (the students typically finish in 2007).

Besides year and high school fixed effects we include two control variables at high school level, $G_{g,2007}$, interacted with year fixed effects: The number of students and the high school grade point average. Both variables are measured in 2007. These variables

control for shocks that could affect large schools or schools with high-achieving students differently than other schools. Such shocks could threaten the identification strategy if school size or grade point average is correlated with the instrumental variable (the level of funding per student in 2007) and if the shocks occur around the same time as the reform is implemented. For instance, it could be that the above-mentioned curriculum reform of 2005 or governance reform of 2007 benefited large schools in particular affecting outcome variables in the analysis.

We also include a set of variables at individual level, $X_{i,t,g}$ containing among others gender, origin, parents education level, parents income and dummies for missing parents. In this way we try to avoid selection bias that can be due to for example high schools with higher fundings attracting students with strong social background or higher grade poing average. Table 2 in Appendix 1 gives a full overview of the controls and shows their descriptive statistics.

The controls are included to account for selection. We present two arguments in favor of this strategy. First, the evidence presented in Deming [2014] suggests that controlling for previous student achievement is a viable strategy to eliminate selection concerns. He exploits random student allocation due to lotteries to compute unbiased estimates of school effectiveness measured as test score gains. These estimates are then compared to naive estimates of test score gains that are produced using control variables to account for selection.[3] The results of Deming [2014] reject the hypothesis that these naive estimates of test scores gains are biased even when they are estimated using only prior test scores as a control variable. Second, we argue that the selection bias probably would work against our main result that there is no effect of the reform on student outcomes. The results indicate that reform-induced increases in funding lead to higher prior GPA among first year students. If there is unmeasured ability not controlled for in the main regressions, it would most likely be higher for students with a high prior GPA. This would bias the estimated effect of reform-induced changes in funding on student outcomes in a positive direction working against our main result that the estimate is statistically indistinguishable from

[3]Another strategy to deal with for selection pursued by Jackson et al. [2015] is to assign students to the school district in which the resided resided some years prior to the reform rather than to the actual school they attended. Although this is an effective way of purging the estimated effect of selection bias, the method is not so useful in our case since high school choice is not closely tied to the place of residence.

zero.

Next, we investigate the effect of reform-induced changes in funding on five variables at the high-school level: Total wages paid to employees per student, total number teacher working hours, teachers hourly wage, teacher composition and the total number of students. By investigating these variables, we ask whether reform winners choose to spend the extra funding on wages and teacher working hours (or whether loosers choose to cut down on these), and whether the number of students is affected by funding. We run panel regressions of the following type:

$$O_{g,t} = \pi_g + \theta_t + \beta_t \Delta \hat{T_{g,2012}} + \delta_t G_{g,2007} + \epsilon_{g,t}, \tag{3}$$

where $O_{g,t}$ is the outcome variable, π_g is a high-school fixed effect, θ_t is a cohort fixed effect. $\Delta \hat{T_{g,2012}}$ is the predicted change in funding per student computed based on the regression described by (1) where 2012 is the end year.

3.2 IV regressions

To shed further light on the effect of funding changes on educational outcomes we run a variety of IV-regressions. While the regressions presented above allows for a graphical and intuitive presentation of the result, one concern is that they do not take into account the compound uncertainty that arises when predicted values of funding changes are used in the second step. One of the benefits of using IV-regressions is that they do take into account this compound uncertainty. The second stage of the IV regression is based on the following type of equation at the individual level:

$$O_{i,t,g} = \lambda_g + \chi_t + \omega \overline{F_{g,t}} + \phi_t G_{g,2007} + \psi X_{i,t,g} \tag{4}$$

$O_{i,t,g}$ is the outcome variable for student i who started at high school g in year t. λ_g is a high-school fixed effect, χ_t is a year fixed effect. In all regressions we include controls at high school level represented by $G_{g,2007}$. Controls at individual level, $X_{i,t,g}$ are also included in all regressions except the regression with prior GPA as the dependent variable. Funding enters through the variable $\overline{F_{g,t}}$ defined as the average funding affecting a student who starts at high school g in year t and graduates in year $t+3$. It is computed as the

weighted average of the funding per student received in year t and in each of the subsequent three years.[4] This allows for a more precise estimate of the effect of the actual funding faced by the students, than dynamic estimates described above that are based on predicted changes in funding from 2007 to 2012.

In the first stage of the IV regression we instrument average funding with the pre-reform funding level:

$$\overline{F_{g,t}} = \Lambda_g + \varsigma_t + \mathit{\Pi}_t F_{g,2007} + \Phi_t G_{g,2007} + \Upsilon X_{i,t,g} \tag{5}$$

$F_{g,2007}$ is the funding level in 2007 at high school g, Λ_g is a high-school fixed effect, ς_t is a year fixed effect. We allow the effect of pre-reform funding to vary from year to year to reflect the gradual implementation of the reform. The estimates of $\mathit{\Pi}_t$ will thus reflect the convergence of funding levels starting in 2007.

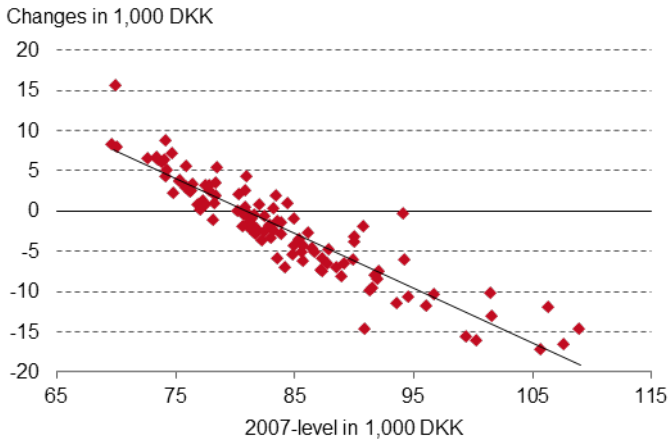
4 Results

4.1 Predicting funding changes

Figure 2 plots the change in funding per student from 2007 to 2012 vs. funding per student in 2007 across high schools, that is, the regression shown in (1). The relationship is strikingly tight ($R^2 = 0.84$) demonstrating that the initial funding level in 2007 is a strong predictor of funding changes. The estimated coefficient is 0.68. In other words, 1 DKK higher funding in 2007 is associated with a 0.68 DKK drop in the change in funding from 2007 to 2012.

[4] $\frac{1}{6}F_{g,t} + \frac{1}{3}F_{g,t+1} + \frac{1}{3}F_{g,t+2} + \frac{1}{6}F_{g,t+3}$. Note that $F_{g,t}$ and $F_{g,t+3}$ weigh half of $F_{g,t+1}$ and $F_{g,t+2}$, since the students begin and end school in the summer and thus, in year only attend classes a single semester in each of the years t and $t+3$.

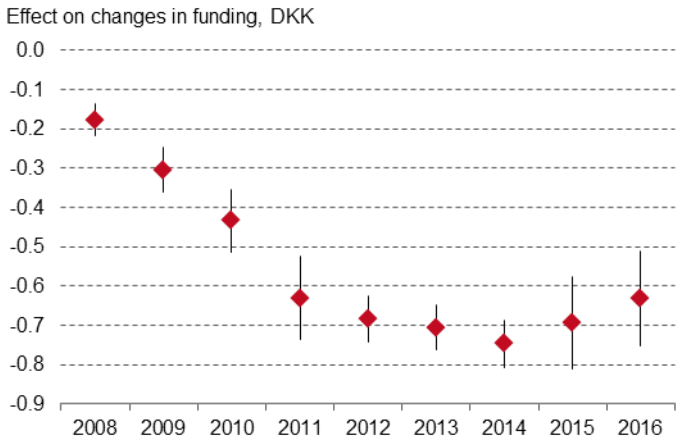
Figure 2: Convergence of funding from 2007-12



Notes: Scatter plot of the change in funding per student from 2007 to 2012 (y-axis) vs. the level of funding per student in 2007 across 107 high schools (x-axis). The fitted line is from a simple regression. All fundings are expressed in 2010-level using price index for public expenditure.

Figure 3 plots the convergence coefficients for each of the cohorts from 2008 to 2016, that is, the estimates of α_{2008} through α_{2016} from (1). This illustrates the gradual convergence of funding from 2007 to 2012 as the implementation of the reform progresses. In 2012 the convergence has more or less come to a halt. There is a slight tendency towards decreasing coefficient estimates in 2015 and 2016. As explained above, a reform in 2015 redistributed funding towards high schools with many students with low test scores in 9th grade. This could account for the slight weakening of the relationship between the change in funding and initial funding. However, the predictive power of the level of funding in 2008 remains very strong indicating that the 2015 reform is no threat to the identification strategy.

Figure 3: Convergence coefficients from 2008-16



Notes: Estimated coefficients from nine separate regressions of the change in funding per student from 2007 to the each of the years from 2008 to 2016 on the level of funding per student in 2007 across 107 high schools. The lines indicate 95 pct. confidence intervals computed using robust standard errors. All fundings are expressed in 2010-level using price index for public expenditure.

4.2 Results at the student level

This section presents the main results on how changes in funding has affected student educational outcomes. We draw on Danish register data to construct most outcome variables and control variables - the appendix describes the data construction and sources in greater detail.[5] We first investigate the effect of changes in funding on educational outcomes measured by three variables: An indicator for completion of high school, the high school GPA and an indicator for further education beyond high school.[6] Next, we examine how changes in funding affects student composition reflected in the pre-high-school GPA of freshmen.

Figure 4 documents the consequences of changes in funding for completion defined as an indicator of whether a student finishes high school within four years of entry. The left figure shows that starting from cohort 2005 and onward the completion ratio increases more for students on high schools that benefit from the funding reform (reform winners). At a first sight, this would seem to support the hypothesis that more funding raises comple-

[5] There are to types of students at the high schools, those studying for the gymnasium exam (in Danish: stx), and those studying for the higher prepatory exam (in Danish: hf). The GPAs of these two groups are not directly comparable, so in the main analysis we only include students in the first category who constitute the vast majority. Appendix 2 shows that the results for completion and continuation to further education are similar when higher preparatory students are included in the analysis.

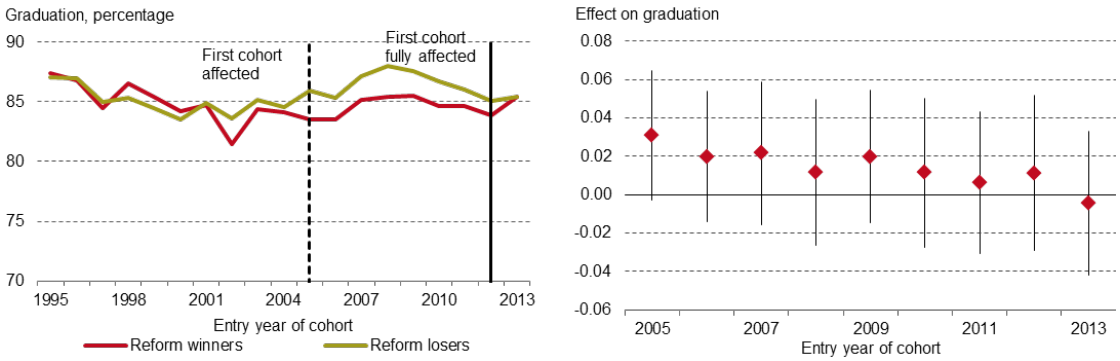
[6] Appendix 2 presents results using a number of alternative outcome variables such as the number of high-level elective courses, and additional measures of GPA. The results support the main conclusion that there is no effect of the reform on educational outcomes.

tion. However, the timing of the increase does not seem to fit the gradual implementation of the reform. In particular, the increase is implausibly large for cohort 2005. Students from cohort 2005 are the first to be affected by the reform and enter high school in 2005 and typically graduate in the summer of 2008. Thus, they are only affected by the reform in half of 2008 - the first year of implementation where convergence of funding had barely begun.The right figure documents that the effect of predicted changes in funding on graduation is insignificant when controls at the student and high-school level are included. The figure shows estimates on the predicted funding change from 2007 to 2012 interacted with year dummies as described by the student-level regression in (2). As discussed above, student selection is taken into account by controlling for a number of control variables at the student level: Parental education and income, gender, age, and prior GPA.[7] The estimates are insignificant throughout the period. Moreover, the largest estimate is in 2009 and estimates tend to decrease for later cohorts. One would expect the opposite if there was an effect on graduation given the gradual implementation of the reform. In total, the results do not support the hypothesis that funding affects completion.

Pre-reform trends in graduation rates are similar for reform winners and reform losers. This finding supports the identification strategy by suggesting that the results are not driven by underlying differences in graduation trends due to unobserved factors.

[7]Parental education is a set of dummies for the level of education of the mother and a set of dummies for the level of education of the father. Parental income is the sum of both parent's wage income. Prior GPA is measured in the 9th grade, which students typically finish just before or one year before entering high-school. Appendix 1 describes the data definitions and sources in greater detail.

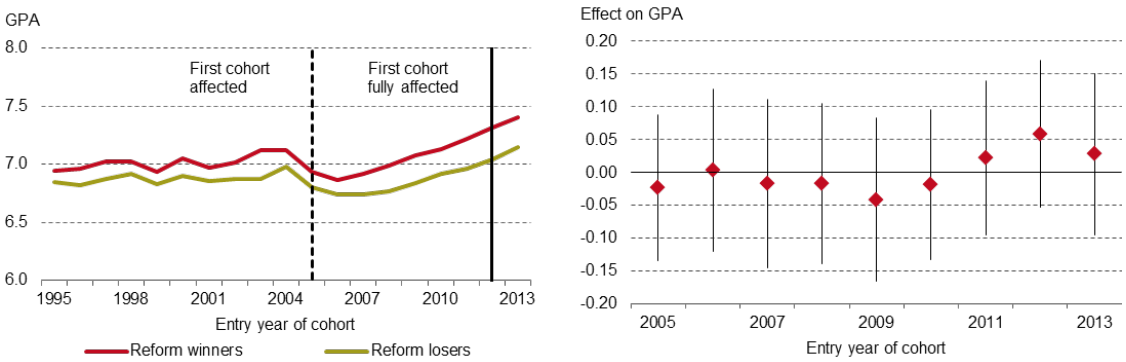
Figure 4: Effect on graduation



Notes: Left: Graduation rate by year the cohort entered high school. Reform winners (losers) are students on high schools where the predicted change in funding per student from 2007 to 2012 is above (below) the average change in funding per student. Predicted change in funding per student is computed as the fitted values from a cross-school regression of the change in funding from 2007 to 2012 on the funding level in 2007. Right: Estimated coefficients on the predicted change in funding per student in 10,000 DKK from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. The dependent variable is a graduation dummy. Regressions are at the student level and include cohort fixed effects, high-school fixed effects, and control variables at the high school and student level. The vertical lines indicate 95 pct. confidence intervals computed using standard errors clustered at the high-school level.

Figure 5 documents the estimated effect on high school GPA. The left figure shows that trends in GPA are roughly similar for reform winners and reform losers prior to the reform. Again, this attests to the validity of the reform in terms of identifying the causal effect of funding changes. After 2008 there is a hike in the GPA of both groups, however, the increase is largest for winners. This is consistent with the hypothesis that the reform-induced changes in funding had a positive impact on educational success. However, it is also possible that the result is driven by the increased selection of students with high prior GPAs into high schools that benefited from the reform. The right figure supports the latter explanation: Once socioeconomic characteristics and prior GPA is controlled for, there is no statistically significant effect of reform-induced changes in funding on high-school GPA.

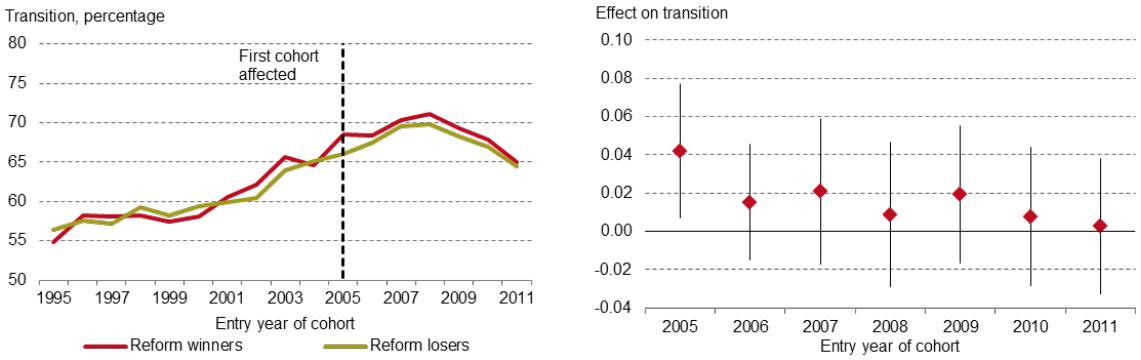
Figure 5: Effect on GPA



Notes: Left: GPA by year the cohort entered high school. Reform winners (losers) are students on high schools where the predicted change in funding per student from 2007 to 2012 is above (below) the average change in funding per student. Predicted change in funding per student is computed as the fitted values from a cross-school regression of the change in funding from 2007 to 2012 on the funding level in 2007. Right: Estimated coefficients on the predicted change in funding per student in 10,000 DKK from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. The dependent variable is high-school GPA. Regressions are at the student level and include cohort fixed effects, high-school fixed effects, and control variables at the high school and student level. The vertical lines indicate 95 pct. confidence intervals computed using standard errors clustered at the high-school level.

Our third measure of educational success reflects student continuation in education beyond high school. We construct an indicator that equals one if the student attends college, university or other types of post-secondary education two years after graduation. Figure 6 documents that, as in the case of the two previous outcomes, pre-reform trends are similar for reform winners and losers. Continuation increases slightly more for reform winners than for reform losers starting with the 2005 cohort - the first cohort potentially affected by the reform. However, the right figure shows that when control variables are introduced there is no significant effect of reform-induced changes on the propensity of attending education beyond high school. In total, the results using the three different measures of educational success support the conclusion that there is no statistically significant effect of funding on educational success.

Figure 6: Effect on continued education



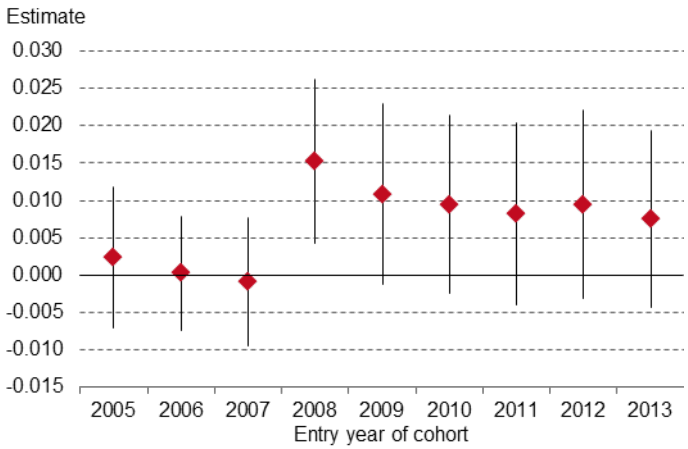
Notes: Left: Fraction of students who attend post-secondary education after high school by year the cohort entered high school. Reform winners (losers) are students on high schools where the predicted change in funding per student from 2007 to 2012 is above (below) the average change in funding per student. Predicted change in funding per student is computed as the fitted values from a cross-school regression of the change in funding from 2007 to 2012 on the funding level in 2007. Right: Estimated coefficients on the predicted change in funding per student in 10,000 DKK from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. The dependent variable is a an indicator for whether the student attend post-secondary education. Regressions are at the student level and include cohort fixed effects, high-school fixed effects, and control variables at the high school and student level. The vertical lines indicate 95 pct. confidence intervals computed using standard errors clustered at the high-school level.

Next, we investigate how changes in funding affects the selection of students. Figure 7 shows the estimates on the predicted change in funding from 2007 to 2012 interacted with cohort dummies as described by the student-level regression in (2). The dependent variable is the 9th grade GPA typically measured just before or one year before entering high school. Since high-school and year fixed effects are included, the estimates capture the effect of predicted change in funding measured in 10,000 DKK on the change in prior GPA from cohort 2007 to the cohort in question. For instance, the estimate of 0.01 for cohort 2010 indicates that increasing the predicted change in funding by 10,000 DKK increases the change in the prior GPA by 0.01 grade points.

The estimates are statistically insignificant and close to zero for the 2005 through 2007 cohorts and then becomes positive and significant for the 2008 cohort. The first three cohorts enter high school before 2008 where the reform is implemented. In this light, it is reassuring that there is no effect selection. The fourth cohort enters high school in 2008, the year in which the reform is implemented. Although the reform only affected funding in 2008 slightly, it is quite possible that the expectation of the convergence of funding in the years to come affected students selection in 2008.

There are two possible drivers of the selection effect: It could be that students with higher GPAs seek out schools with increases in funding because they are better informed about the funding changes, or it could be because or because they value well-funded schools more. In either case, the result underlines the importance of accounting for selection by controlling for prior GPA in the following investigation of the effects of funding on student level outcomes.

Figure 7: Selection on prior GPA

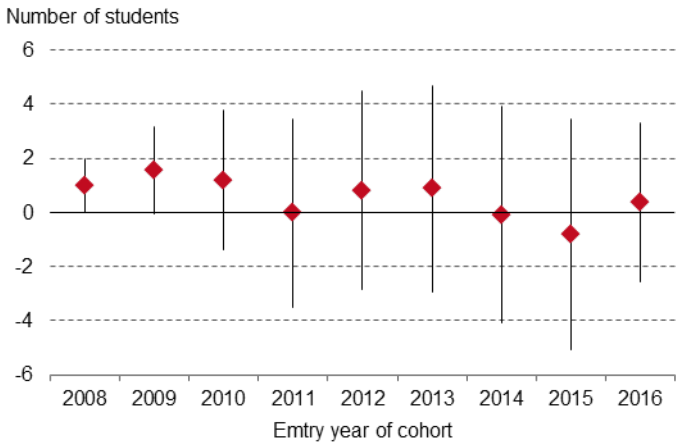


Notes: Estimated coefficients on the predicted change in funding per student from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. Dependent variable is prior GPA. Panel regressions at the high-school level. The predicted change in funding is computed as the fitted values from the simple regression of the change in funding per student from 2007 to 2012 on the level of funding in 2007. The regressions include year and high-school fixed effects and two control variables: The number of students in 2007 and the average GPA in 2007 both of which are interacted with cohort dummies. Balanced panel of 95 high schools from 2007 to 2016. The vertical lines indicate 95 pct. confidence intervals computed using standard errors clustered at the high-school level.

Figure 8 shows that the reform-induced changes in funding did not have a significant effect on the number of high-school students. The estimates of the coefficient on predicted changes in funding is statistically insignificant in all years. There are two possible explanations of this finding: First, it could be that although students are relatively free to choose the school they would like to apply for, the changes in funding do not affect student demand. This would be the case if the extra funding is spend on activities that do not attract more applicants. Second, there could be administrative restrictions on the number of admitted students. Half a year before school start, each high school sets a mandatory cap on the number of admitted students. The school coordinates with nearby high schools and local authorities, but ultimately each school decides on the size of its own cap. The local authorities may, however, subsequently recommend the enforcement of an alternative cap to the Ministry of Education. Although this procedure is rarely used, it is possible that the threat of recommendation affects the decisions of the high schools on the admitted number of students. A motive for evening out the distribution of students could be to prevent closure of small schools. It is also entirely possible, however, that some high

schools do not wish to increase their number of students because of limitations on capacity that could prove costly to overcome. For instance, it can be costly for schools in urban areas to acquire the space needed to accommodate more students.

Figure 8: Effect on the number of students



Notes: Estimated coefficients on the predicted change in funding per student from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. The dependent variable is the total number of students. Panel regressions at the high-school level. The predicted change in funding is computed as the fitted values from the simple regression of the change in funding per student from 2007 to 2012 on the level of funding in 2007. The regressions include year and high-school fixed effects and two control variables: The number of students in 2007 and the average GPA in 2007 both of which are interacted with cohort dummies. Balanced panel of 95 high schools from 2007 to 2016. The lines indicate 95 pct. confidence intervals computed using robust standard errors..

Appendix 2 shows that the results are robust to changes in the outcome variable and sample. First, we investigate whether including other measures of educational success as the dependent variable changes the conclusion. At the high-school level, we use the share of high preparatory school students as the dependent variable. At student level, we run regressions with a number of alternative dependent variables: The number of elective courses at the highest level (level A), the number of natural-science elective courses at the highest level and several alternative measures of GPA. We do not find any statistically significant effects of the predicted change in funding on any of these measures.

Second, we expand our sample including students at higher preparatory schools to check whether our conclusion is sensitive to sample composition. Students at higher preparatory schools are not included in the main analysis since they are not directly comparable with high school students (stx). Running the regression including students at higher preparatory schools does not change the conlusion about insignificant results found in the main analysis. We also run separate regressions for disadvantaged students with low parental education or income. While the literature suggests that the effect of funding is particularly strong

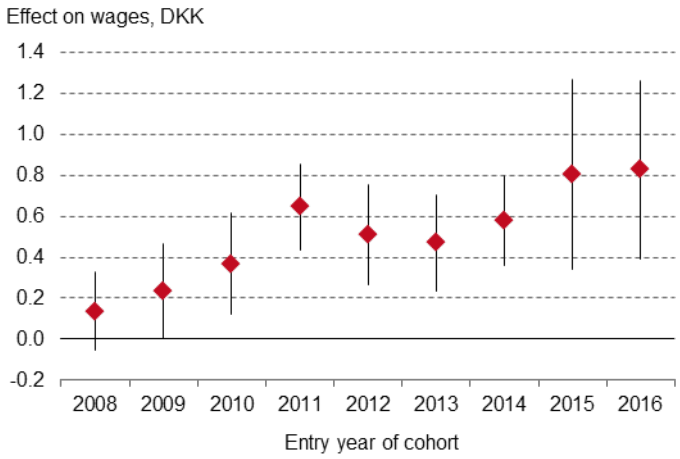
on these types of students, we find no statistically significant effect.

At last, we split high schools in two groups: one group with funding level below the median of funding per student in 2007, and another group with funding level above the median of funding per student in 2007. We run separate regressions for those two groups. The effect of changes in funding is insignificant for both groups of high schools.

4.3 The effect on teacher wages, working hours and GPA

To shed light on the results of the previous section, we investigate the consequenes of the budgetary changes for inputs. In particular, we examine the effect on four variables at the high-school level: Employee wages, teacher working hours, the composition of teachers and the total number of students. Figure 9 shows that reform winners has spent most of the extra funding on employee wages. The figure shows the estimated coefficients of β_t from regression in (3) where the total wage bill per student is the dependent variable. Recall that the dependent variable is the predicted change in funding per student from 2007 to 2012. In 2012 the estimate on predicted change per student is 0.58 indicating that for each 10,000 DKK increase in funding, wages increase by 5,800 DKK.

Figure 9: Effect on wages per student



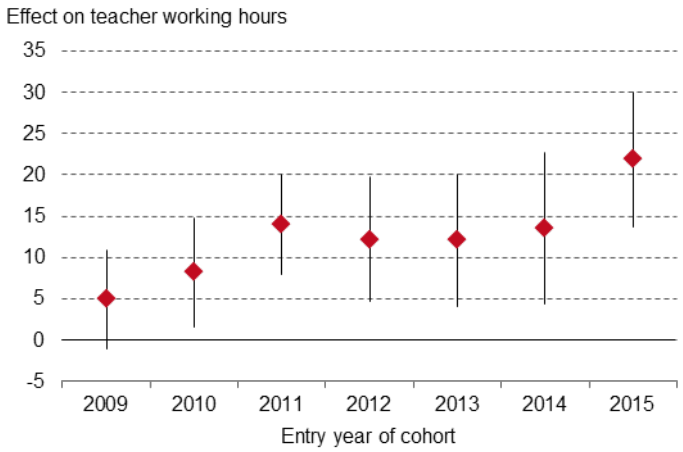
Notes: Estimated coefficients on the predicted change in funding per student from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. The dependent variable is wages per student. Panel regressions at the high-school level. The predicted change in funding is computed as the fitted values from the simple regression of the change in funding per student from 2007 to 2012 on the level of funding in 2007. The regressions include year and high-school fixed effects and two control variables: The number of students in 2007 and the average GPA in 2007 both of which are interacted with cohort dummies. Balanced panel of 95 high schools from 2007 to 2016. The lines indicate 95 pct. confidence intervals computed using robust standard errors.

To investigate whether the increase in wages is reflected in increased the total number of teacher working hours we run a regression with the number of working hours per student as the dependent variable. Working hours could increase as a consequence of new hirings or via increased hours of existing staff. As figure 10 shows, the reform winners spent the extra funding, in part, to increase the teacher working hours pr. student. The effect is significant in all years except the first. year.[8] An increase in the funding by 10,000 DKK from 2008 to 2012 will result in 12 hours per student from 2008 to 2012.

Extra working hours could be spent on more lessons pr. student, smaller class sizes or other teacher tasks such as preparation or administrative duties. Andersen et al. [2016] conduct a randomized trial showing that more lessons has a significant effect on the reading skills of second grade students in Danish primary schools. Meanwhile, the review of Filges et al. [2015] suggests that the effect of class size on test scores is small and close to zero. A possible explanation for our result is that the changes in teacher hours primarily entailed changes in class size or other factors with limited effect on student outcomes.

[8]We use 2008 as the reference year since it was not possible to obtain data for working hours of a satisfying quality prior to 2008.

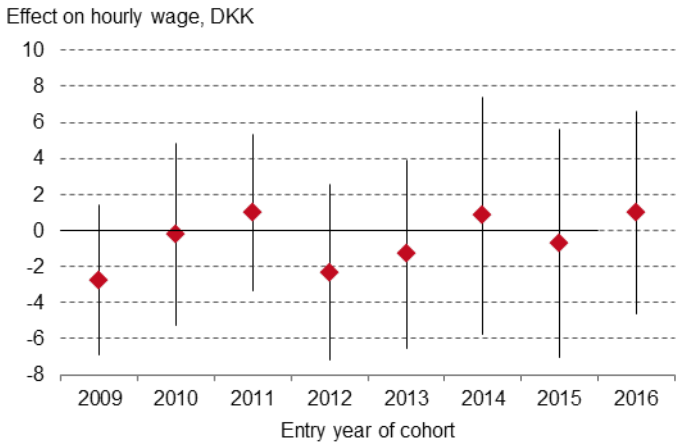
Figure 10: Effect on teacher working hours per student



Notes: Estimated coefficients on the predicted change in funding per student from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. The dependent variable is the number of teacher working hours pr. student. Panel regressions at the high-school level. The predicted change in funding is computed as the fitted values from the simple regression of the change in funding per student from 2007 to 2012 on the level of funding in 2007. The regressions include year and high-school fixed effects and two control variables: The number of students in 2007 and the average GPA in 2007 both of which are interacted with cohort dummies. Balanced panel of 95 high schools from 2007 to 2016. The lines indicate 95 pct. confidence intervals computed using robust standard errors.

The increased reform-induced changes in fundings are not reflected in higher hourly wage for teachers as showed in figure 11. This indicates that reform winners did not use the extra funding to increase hourly teacher wages with the aim to, e.g., attract more qualified teachers.

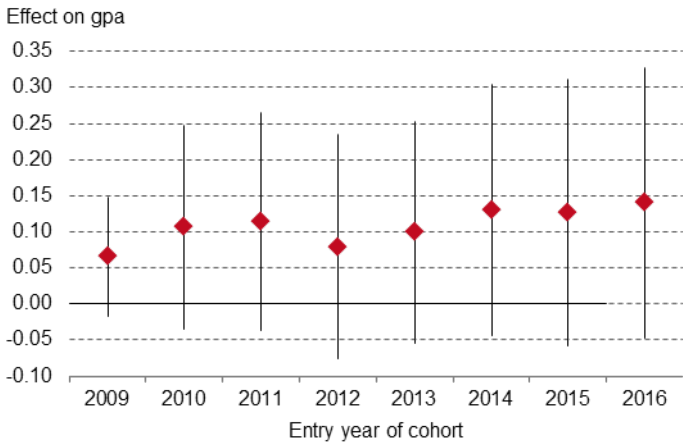
Figure 11: Effect on hourly wage



Notes: Estimated coefficients on the predicted change in funding per student from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. The dependent variable is the hourly teacher wage. Panel regressions at the high-school level. The predicted change in funding is computed as the fitted values from the simple regression of the change in funding per student from 2007 to 2012 on the level of funding in 2007. The regressions include year and high-school fixed effects and two control variables: The number of students in 2007 and the average GPA in 2007 both of which are interacted with cohort dummies. Balanced panel of 95 high schools from 2007 to 2016. The lines indicate 95 pct. confidence intervals computed using robust standard errors.

One way to measure teachers qualifications is by using their obtained high school GPA. Teachers high school GPA is an indicator and of course not a fully adequate measure of teacher quality. As figure 12 shows, reform winners have not attracted teachers with higher high school GPA - although there is a small and positive effect, it is insignificantly different from zero in all years. This finding is interesting given that the literature suggests that teacher quality is important to student outcomes (see e.g. Burgess [2016]). Thus, another (and perhaps additional) potential explanation for our result is that the changes in funding did not instigate changes in teacher quality.

Figure 12: Effect on teachers qualifications



Notes: Estimated coefficients on the predicted change in funding per student from 2007 to 2012 interacted with cohort dummies for each of the years from 2008 to 2016. The dependent variable is the average teacher GPA from high school. Panel regressions at the high-school level. The predicted change in funding is computed as the fitted values from the simple regression of the change in funding per student from 2007 to 2012 on the level of funding in 2007. The regressions include year and high-school fixed effects and two control variables: The number of students in 2007 and the average GPA in 2007 both of which are interacted with cohort dummies. Balanced panel of 95 high schools from 2007 to 2016. The lines indicate 95 pct. confidence intervals computed using robust standard errors.

4.4 IV results

This section supplements the above results with IV-estimates. The IV-regressions have several benefits compared to the panel regressions of the previous section: First, they take into account the compound effect on standard errors of having a first stage that predicts reform-induced changes in funding. Second, they are based on a more precise computation of the funding level that is relevant to the students of each cohort. In particular, we compute the average level of funding throughout the expected school period of the student in question. This takes into account that the gradual implementation implies that older cohorts face smaller changes in funding than the younger cohorts. Third, the IV-estimates show the average effect of funding across all cohorts.

Table 1 shows the results from IV-regressions. The results are very much in line with those of the previous section: First, there is a positive and significant effect on the prior GPA of first-year students confirming that there is selection of high-achieving students into schools that benefit from the reform. Second, there is no statistically significant effect on

any of the measures of educational success. In all four regressions, the Kleibergen-Paab statistics indicate that the instrument is a strong predictor of changes in funding. This is reassuring, since a weak instrument would bias the second-stage estimate towards zero. Moreover, the IV-estimates are statistically precise suggesting that the conclusion that the effect is statistically insignificant is not the result of statistical uncertainty.

Table 1: IV results

	GPA primary	Completion	Further education	GPA high school
Average funding	0.008**	-0.001	-0.001	0.003
per student	(0.004)	(0.001)	(0.001)	(0.003)
Individual level controls	No	Yes	Yes	Yes
Number of observations	232136	229985	175465	196512
Kleibergen-Paap F statistic	76.52	76.50	81.43	78.73

Notes: Estimated coefficients of increasing the reform-induced changes in fundings with 10,000 DKK per student from the year they enter the high school to the year they finish. Average funding per student is an average of funding the student receive during the three years of high school. Estimates are from IV-regressions as described in section 3.2. The lines indicate 95 pct. confidence intervals computed using robust standard errors. We include controls at high school level in the regressions with prior GPA as the dependent variable. Controls at student level are included in the remaining three regressions.

5 Conclusion

In 2008 Danish high schools started a gradual transition towards a simpler, rule-based funding scheme resulting in dimishing differences in funding per student across high schools. Using this reform which provided exogenous variation in government funding, we investigate the causal effect of funding on several education outcomes.

First, we construct reform-induced changes in funding by using the predicted values of a regression of the change in funding on the pre-reform level of funding. We use the predicted funding changes to identify the effect of funding changes on student outcomes. In particular, we study the effect on three variables: graduation, grades and continuation to further education. The analysis shows that there is no significant effect of funding on neither of the three educational outcomes. We perform a range of robustness checks, all supporting the conclusion of no statistically significant effect on student outcomes.

To shed light on this conclusion we examine the consequences of funding changes at the high school level employing five outcome variables: 1) total wages paid to teachers per student, 2) number of teacher working hours per student, 3) the hourly teacher wages,

4) teachers qualifications measured by their high school GPA and 5) total number of students. The results show that reform winners tend to spend more money on the total wage which is reflected in more teacher working hours per student. We find no significant effect for the remaining three outcomes. In total, this suggests that the funding changes had consequences for the quantity of teaching, but not the quality. A potential explanation for our main result regarding student outcomes is that changes in teacher working hours was spent on factors that had limited impact on student outcomes. Another potential explanation is that the spending levels in the pre-reform years were sufficiently high that the marginal benefit of increased ressources was small. A third potential explanation is that students substituted for changes in school inputs by adjusting their own effort or their parents' nullifying any potential effects on learning outcomes.

6 Appendix 1: Data description

This appendix describes the data sources used in the analysis and includes tables with full estimation results. The analysis is based on register data from Statistics Denmark and accounting data from The Ministry of Education and Hvidmand and Sievertsen (2015). These are described in the following section.

6.1 Accounting data

To account for the level of funding before and after the reform we use accounting data downloaded February 2018 from the database of The Ministry of Education (http://regnskabsportal.uvm.dk/). Data before 2011 are not accessible from this webpage. For this period we use accounting data from Hvidmand and Sievertsen (2015) who downloaded the data at an earlier point in time. [9]

In order to compare accounting data across different years we deflate it using a price index of public consumption.

6.2 Register data

The main data source is the student register (KOTRE) containing a large and detailed data set that follows every student through the layers of the education system. For each student we observe the type of education, the start and end date and the school ID of the institution in question. We use school ID to link the individual data with the accounting data.

The student outcomes are measured at the individual level in three different ways:

1. *Completion*: An indicator that equals one if the student graduates no later than four years after entry, and equals zero otherwise.

2. *Further education*: An indicator that equals one if the student tertiary education no more than two years after graduating high school and 0 otherwise.

[9] Hvidmand and Sievertsen (2015) corrected the data on a number of occasions by comparing it with the original accounting data, checking for outliers, and checking for the 1,000-numbers discrepancies. The ministry has afterwards been informed about these data issues, and there is no sign of similar issues in the data from 2011-2016.

3. *High school grade point average*: The average of all grades achieved during the high school period.

We include a range of control variables in our analysis. The variables along with their descriptive statistics are listed in the table below. We compute descriptive statistics for cohorts, namely 2005 and 2012. The cohort entering the high school in 2005 is only marginally affected by the reform, since students in this group typically finish high school in the summer of 2008. I.e. they only affected half a year. The cohort entering in 2012 is fully affected by the reform, since the funding levels have fully converged by 2012. We include to controls for prior GPA: First we calculate the total 9th grade GPA, i.e. across all subjects and test types (written and oral tests). Second, we compute the GPA only for written tests in mathematics and Danish. Parental education is a set of dummies for the level of education of the mother and a set of dummies for the level of education of the father. Parental income is deflated to 2016-levels using a wage index for an industry worker.

Table 2: Descriptive statistics

	2005	2012
Female	61%	59%
Age	16,8	16.9
GPA	7.2	7.5
GPA, Danish	7.3	7.3
GPA, Math	6.9	7.3
Origin		
Danish	91%	90%
Western immigrants	1%	1%
Non-western immigrants	4%	3%
Western descendant	0%	0%
Non-western descendant	3%	6%
Missing parents		
Father missing	3%	2%
Mother missing	1%	1%
Income in DKK		
Fathers income, DKK	324,943	457,158
Mothers income, DKK	220,945	317,053
Parents income, DKK	529,085	754,822
Labour market status		
Employed, mother	84%	84%
Unemployed, mother	6%	6%
Outside the labour force, mother	5%	6%
Other status, mother	3%	3%
Employed, father	83%	83%
Unemployed, father	4%	4%
Outside the labour force, father	5%	6%
Other status, father	2%	2%
Education		
Primary school, mother	15%	12%
High school, mother	5%	6%
Vocational, mother	29%	34%
Short-cycle higher education, mother	5%	5%
Medium-cycle higher education, mother	33%	30%
Long cycle higher education, mother	9%	10%
Unknown, mother	4%	3%
Primary school, father	14%	14%
High school, father	5%	6%
Vocational, father	35%	37%
Short-cycle higher education, father	5%	7%
Medium-cycle higher education, father	18%	16%
Long-cycle higher educatio, father	15%	13%
Unknown, father	8%	7%
Number of observations	22,405	32,313

7 Appendix 2: Robustness checks

We present a range of robustness checks in this section. First, we look at alternative measures of outcome variables both at student at high school level. Next, we change the specification of student sample and high school sample. At last, we estimate the results with an alternative method.

7.1 Alternative measures of educational outcome

Our main analysis focuses on three educational outcomes: Graduation, GPA and further education. To check whether the results are sensitive to the choice of outcome variable we use a range of alternative measures of educational outcomes. We first construct a dependent variabel measuring the number of high-level (A-level) subjects chosen by the student. We both compute the total number of high-level subjects and the number of high-level natural science subjects. As shown in table 3 we find no statistically significant effect of reform-induced changes in funding.

Table 3: Number of A-level subjects as dependent variable, student level

	A-level subjects, all	A-level subjects, natural science
Predicted	-0.005	-0.003
changes in funding x 2005	(0.004)	(0.003)
Predicted	-0.001	0.001
changes in funding x 2006	(0.003)	(0.003)
Predicted	0.001	-0.002
changes in funding x 2007	(0.003)	(0.004)
Predicted	-0.001	0.002
changes in funding x 2008	(0.003)	(0.003)
Predicted	-0.002	-0.001
changes in funding x 2009	(0.003)	(0.003)
Predicted	-0.002	-0.001
changes in funding x 2010	(0.003)	(0.004)
Predicted	-0.003	-0.001
changes in funding x 2011	(0.003)	(0.004)
Predicted	-0.002	0.003
changes in funding x 2012	(0.003)	(0.003)
Predicted	-0.001	0.000
changes in funding x 2013	(0.003)	(0.003)
Observations	198665	78282
R^2	0.341	0.090

Notes: Year 2004 is the reference year. Standard errors are clustered at the high-school level. See main text for specification and data sources.

The main analysis includes high school GPA as dependent variable calculated across all subjects and exam types. In order to check the robustness we include three alternative measures of GPA. First, we only look at written exams shown in column 1. In column 2 we only focus on Danish, while Math is in focus in column 3. The effects is insignificant for all three variables indicating that reform-induced changes in funding does not impact high school GPA regardless the definition of the dependent variable.

Table 4: Different measures of high school GPA, student level

	GPA, written exams	GPA, Danish	GPA, Math
Predicted	-0.008	-0.001	-0.027
changes in funding x 2005	(0.008)	(0.012)	(0.019)
Predicted	-0.009	-0.003	-0.022
changes in funding x 2006	(0.009)	(0.011)	(0.021)
Predicted	-0.004	-0.011	-0.009
changes in funding x 2007	(0.009)	(0.011)	(0.022)
Predicted	0.002	0.006	0.000
changes in funding x 2008	(0.009)	(0.011)	(0.020)
Predicted	-0.002	0.006	-0.024
changes in funding x 2009	(0.008)	(0.010)	(0.020)
Predicted	-0.012	-0.006	-0.036*
changes in funding x 2010	(0.008)	(0.010)	(0.021)
Predicted	0.001	0.020*	-0.024
changes in funding x 2011	(0.008)	(0.012)	(0.019)
Predicted	0.006	0.007	-0.010
changes in funding x 2012	(0.009)	(0.010)	(0.020)
Predicted	0.003	-0.000	-0.001
changes in funding x 2013	(0.008)	(0.011)	(0.022)
Observations	214760	206399	150501
R^2	0.482	0.288	0.371

Notes: Year 2004 is the reference year. Standard errors are clustered at the high-school level. See main text for specification and data sources.

The changes in funding are only affecting students at high schools (stx) and not student at higher preparatory schools (hf). We next investigate whether the fraction of higher preparatory students changed as a consequence of the funding changes. We find no statistically significant effect indicating that number of students at higher preparatory schools is not affected by the reform-induced changes in funding.

Table 5: The share of students at higher preparatory school as dependent variable, high school level

	Share of students at higher preparatory schools
Predicted	-0.001
changes in funding x 2008	(0.002)
Predicted	-0.001
changes in funding x 2009	(0.002)
Predicted	-0.001
changes in funding x 2010	(0.001)
Predicted	-0.002
changes in funding x 2011	(0.002)
Predicted	0.000
changes in funding x 2012	(0.002)
Predicted	-0.002
changes in funding x 2013	(0.002)
Predicted	-0.001
changes in funding x 2014	(0.002)
Predicted	-0.000
changes in funding x 2015	(0.002)
Predicted	-0.001
changes in funding x 2016	(0.002)
Observations	2354
R^2	0.847

Notes: Year 2007 is the reference year. See main text for specification and data

7.2 Alternative specification of student sample

As explained in section 4.3, we exclude students at higher preparatory schools in the main analysis since they are not directly comparable with students at high schools (stx). Table 6 shows that including this group of students does not change our conclusion (note that the table does not include GPA since grades are not comparable between the two types of students.

Table 6: Robustness check, including students at higher preparatory schools

	Completion	Further education
Predicted	0.001	0.003**
changes in funding x 2005	(0.002)	(0.002)
Predicted	-0.000	0.001
changes in funding x 2006	(0.002)	(0.001)
Predicted	0.000	0.002
changes in funding x 2007	(0.002)	(0.002)
Predicted	-0.001	0.000
changes in funding x 2008	(0.002)	(0.002)
Predicted	-0.000	0.001
changes in funding x 2009	(0.002)	(0.002)
Predicted	-0.001	0.001
changes in funding x 2010	(0.002)	(0.002)
Predicted	-0.001	0.000
changes in funding x 2011	(0.002)	(0.002)
Predicted	-0.001	0.000
changes in funding x 2012	(0.002)	(.)
Predicted	-0.002	0.000
changes in funding x 2013	(0.002)	(.)
Observations	264864	201766
R^2	0.091	0.225

Notes: Year 2004 is the reference year. Standard errors are clustered at the high-school level. See main text for specification and data.

There is some evidence in the existing litterature that students from low-income families benefit more from increased fundings than other students. To test this hypothesis we rerun the main regressions separately for students from low income families. We define low income families as families with income belonging to the 25 percentage lowest income tail. We try also with a more strict definition where we focus on the 10 percentage lowest income. However, the effect is statistically insignificant in both cases as shown in table 7.

Table 7: Results for students from low income families

	Low income, 25 pct.			Low income, 10 pct.		
	Completion	Further education	GPA	Completion	Further education	GPA
Predicted changes in funding x 2005	0.002 (0.002)	0.005* (0.003)	-0.001 (0.008)	0.007 (0.006)	0.003 (0.008)	0.001 (0.037)
Predicted changes in funding x 2006	0.003 (0.002)	0.002 (0.002)	0.002 (0.009)	0.007 (0.005)	-0.001 (0.007)	-0.003 (0.032)
Predicted changes in funding x 2007	0.004* (0.002)	0.004 (0.003)	0.009 (0.009)	0.002 (0.006)	-0.004 (0.008)	0.030 (0.030)
Predicted changes in funding x 2008	0.001 (0.002)	0.003 (0.002)	0.006 (0.008)	0.001 (0.005)	0.007 (0.006)	0.060** (0.025)
Predicted changes in funding x 2009	0.005** (0.002)	0.005** (0.002)	-0.010 (0.008)	0.012* (0.006)	0.003 (0.007)	-0.008 (0.030)
Predicted changes in funding x 2010	0.002 (0.002)	0.004 (0.003)	-0.002 (0.009)	0.003 (0.005)	-0.001 (0.007)	0.023 (0.022)
Predicted changes in funding x 2011	0.000 (0.002)	0.003 (0.002)	0.011 (0.008)	0.000 (0.004)	-0.003 (0.006)	0.019 (0.027)
Predicted changes in funding x 2012	0.002 (0.002)		0.011 (0.007)	-0.001 (0.005)		-0.006 (0.029)
Predicted changes in funding x 2013	-0.001 (0.002)		0.009 (0.008)	0.001 (0.005)		0.047 (0.030)
Observations	57133	43573	44776	7939	5924	5842
R^2	0.105	0.118	0.473	0.147	0.159	0.452

Notes: Year 2004 is the reference year. Standard errors are clustered at the high-school level. See main text for specification and data.

7.3 Alternative specification of high school sample

Results presented in section 4.3 include both reform winners and losers. It could be that these two groups differ in their reaction to the reform, for instance, as a consequence of decreasing marginal effects. To investigate this idea we estimate separate regressions for reform winners and reform losers. The results are shown in the table 8.

The effect of changes in funding on the completion rate are mostly insignificant for both reform winners and losers. There is some evidence of positive and significant effects for reform winners when the GPA is the dependent variable. However, in the IV-regressions where the average effect of funding is estimated for the entire period (table 10) we find no significant results.

Table 8: Separate regression for reform winners versus losers

	Reform losers			Reform winners		
	Completion	Further education	GPA	Completion	Further education	GPA
Predicted changes in funding x 2005	-0.001	-0.002	-0.019**	0.009	0.009***	0.032**
	(0.002)	(0.002)	(0.007)	(0.006)	(0.003)	(0.014)
Predicted changes in funding x 2006	-0.001	-0.003*	-0.009	0.008	0.004	0.022
	(0.002)	(0.002)	(0.009)	(0.007)	(0.003)	(0.018)
Predicted changes in funding x 2007	-0.001	-0.003	0.002	0.008	0.005	0.021
	(0.002)	(0.002)	(0.011)	(0.007)	(0.003)	(0.016)
Predicted changes in funding x 2008	-0.001	-0.003	-0.008	0.007	0.005	0.028*
	(0.002)	(0.002)	(0.012)	(0.008)	(0.003)	(0.016)
Predicted changes in funding x 2009	0.001	-0.001	-0.017	0.008	0.005*	0.031*
	(0.002)	(0.002)	(0.012)	(0.007)	(0.003)	(0.016)
Predicted changes in funding x 2010	-0.003	-0.003	-0.006	0.012	0.005	0.014
	(0.002)	(0.002)	(0.011)	(0.007)	(0.003)	(0.016)
Predicted changes in funding x 2011	-0.002	-0.004**	-0.014	0.008	0.005	0.024
	(0.002)	(0.002)	(0.010)	(0.007)	(0.003)	(0.016)
Predicted changes in funding x 2012	-0.001		-0.015	0.009		0.042***
	(0.002)		(0.010)	(0.008)		(0.013)
Predicted changes in funding x 2013	-0.001		-0.007	0.006		0.024*
	(0.002)		(0.011)	(0.008)		(0.014)
Observations	96747	73680	81911	133238	133238	114601
R^2	0.103	0.120	0.506	0.091	0.249	0.520

Notes: Reform winners (losers) are students on high schools where the predicted change in funding per student from 2007 to 2012 is above (below) the average change in funding per student. Year 2004 is the reference year. Standard errors are clustered at the high-school level. See main text for specification and data

7.4 Alternative estimation method

Another and more simple estimation approach is to estimate equation (2) at the high school level by using so-called long differences. We first compute, at the individual level, the residual from the regression of the outcome-variable in question on prior GPA and the other student-level background variables included in the main analysis. This residual measures the difference between the outcome of the student and the expected outcome given the socio-economic characteristics and prior GPA of the student. We then average these residuals at the high-school level for each cohort. The averaged residuals measure the average value added of attending the high school in question in that particular cohort. We then compute the difference between the averaged residuals of the end-year cohort and the averaged residuals of the 2004-cohort. We regress these differences on the change in

funding pr. student instrumented by the level of funding pr. student in 2012.

Table 9: Alternative method with long differences

	Completion	GPA	Further education
Change in	0.004	-0.001	
funding per student, 2013	(0.003)	(0.009)	
Change in			0.005
funding per student, 2011			(0.006)
Number of observations	107	107	107
Kleibergen-Paap F statistic	25.29	25.29	6.65

Notes: See the main text for a description of the construction of the outcome variables. Regressions at the high-school level. Change in funding instrumented by the funding level in 2007.

7.5 Robustness for IV-regressions: Reform winners vs. losers

Table 10: IV-regressions reform winners versus losers

	Reform winners			Reform losers		
	Completion	Further education	GPA	Completion	Further education	GPA
Average funding	0.002	0.000	0.007	-0.001	-0.002	-0.005
per student	(0.002)	(0.002)	(0.008)	(0.002)	(0.002)	(0.009)
Number of observations	133238	101785	114601	96747	73680	81911
Kleibergen-Paap F statistic	169.91	155.11	148.09	20.12	14.03	20.18

Notes: Average funding per student is an average of funding the student receive during the three years of high school. Reform winners (losers) are students on high schools where the predicted change in funding per student from 2007 to 2012 is above (below) the average change in funding per student. Predicted change in funding per student is computed as the fitted values from a cross- school regression of the change in funding from 2007 to 2012 on the funding level in 2007. Year 2004 is the reference year.

7.6 Robustness for IV-regressions: Delimitation of the analysis period

In the main IV-regressions, the funding levels of pre-2007 cohorts are imputed. To check whether this changes the results we rerun the IV-regressions using only data from cohort 2007 and onwards. This does not change the main conclusion that estimated effects of funding on student outcomes are insignificant.

Table 11: IV-regressions with analysis period from 2007

	Completion	Further education	GPA, high school
Average funding	-0.002	-0.002	0.008
per student	(0.001)	(0.001)	(0.005)
Number of observations	172519	117999	147881
Kleibergen-Paap F statistic	100.22	114.15	101.16

Notes: Average funding per student is an average of funding the student receive during the three years of high school. Year 2007 is the reference year.

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