

Information Shocks and the Dynamics of the Housing Market*

Jon H. Fiva[†] and Lars J. Kirkebøen[‡]

Abstract

This paper analyzes housing market reactions to the release of previously unpublished information on school quality. Using high quality housing data that precisely bracket the timing of the information shock; we investigate housing price dynamics within school catchment areas. We find a robust short-term housing market reaction to publication of school quality indicators, suggesting that this information was new to the households, and that households are willing to pay for better schools. The publication effect does not seem to be permanent as prices revert to prepublication levels after two to three months.

Keywords: information disclosure; hedonic methods; school quality; housing price dynamics

JEL classification: I21; I28; R21; R23

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[†] Department of Economics, University of Oslo, PB 1095 Blindern, 0317 OSLO, NORWAY. E-mail: j.h.fiva@econ.uio.no

[‡] Statistics Norway, Research Department, PB 8131 Dep., 0033 OSLO, NORWAY. E-mail: kir@ssb.no

I. Introduction

Governments devote considerable resources to developing and publishing comparative information on goods and services of heterogeneous quality. These interventions are applied both to firms operating in the private sector, but increasingly also to producers of public services. An important example is public reporting of school performance, which is becoming increasingly common as part of OECD countries' school accountability systems (OECD, 2007). The main objective of these strategies is to put pressure on service providers to increase quality. However, the value of the publications will depend on the strength of informal mechanisms for learning about quality (Dafny and Dranove, 2008; Chernew et al., 2008). If households already know, or can easily find out about quality differences, then public disclosure of such information is likely to be of little use.

In this paper we take advantage of an exogenous information shock to investigate how households' valuation of schools responds to publication of new information on the quality of schools. While the existing literature on public disclosure of comparative information ('report cards') is dominated by analyses from the United States, we utilize data from the capital of Norway, Oslo. When enrollment in public schools is tied to residential address, as in Oslo, new information is expected to be reflected in housing prices if household value school quality.

We follow Figlio and Lucas (2004) and apply an identification strategy able to identify changes in housing values driven by changes in households' information set. This research design allows us to remove neighborhood unobservables that are likely to bias conventional hedonic price regressions. We thereby add to the growing literature combining quasi-experiments and hedonic price theory to value public services and non-market (dis)amenities, such as school quality (e.g. Black, 1999; Figlio and Lucas, 2004; Bayer et al., 2007; Ries and

Somerville (2009)), air quality (Chay and Greenstone, 2005), airport noise (Pope, 2008a), risk of flooding (Pope, 2008b) and hazardous waste (Greenstone and Gallagher, 2008).

The main contribution of this paper is to offer a detailed investigation of how an exogenous information shock impacts the dynamics of the housing market. With detailed data on housing transactions, including the date of agreement, we can study responses in the housing market on a week to week basis. Previous studies that have dealt with time-varying capitalization effects have mostly studied variation in capitalization effects from one year to the next. Examples include Kiel (1995) and Dale et al. (1999) that analyze housing price dynamics in response to announcements of hazardous waste sites by the US environmental protection agency. Similarly, Figlio and Lucas (2004) show that the housing market in Florida is highly responsive to the release of new information of school quality, but the responses to information are not long-lasting unless the positive information shock is repeatedly reinforced. While we study housing price dynamics in the short to medium run based on one major information shock, earlier studies have looked at changes over time due to the release of additional or updated information. An advantage of using rather short time windows is that secular trends in housing prices are unlikely to be driving any results.

We investigate housing market responses to indicators that capture publication of school quality indicators that isolates schools' contribution from student composition. Our information shock is consequently well suited for an investigation of whether households can identify and how much they value school quality per se. We document that the housing market responds to new information on school quality. The increase in average willingness to pay for school quality suggests that households care about school quality, and that published results provided new information, valued by households. A difference in published results of one standard deviation yields a difference in housing prices of about two percent. Robustness

checks strongly suggest that this is a causal effect of the changing information environment. We analyze price quotes, set prior to publication, and find that the publication effect is not spuriously picking up unobserved housing attributes. To further investigate the validity of our identification strategy, we run ‘placebo regressions’ at dates without any ‘information shock’ and very rarely find results as strong as those obtained at the actual publication date. This is another strong indication that our main results are not driven by systematic unobserved differences among apartments sold before and after publication. Also, the placebo tests demonstrate that school district specific trends in housing prices are very unlikely to be driving our main results. While the impact of publication of school quality on housing prices is very robust, the effect does not seem to be long lasting. There is little trace of the initial response after 12 weeks. Different mechanisms may contribute to the dynamic response to the information shock. We discuss alternative explanations below.

The structure of the paper is as follows: Section II and Section III describe the institutional setting and data. Section IV lays out our empirical strategy, and Section V presents the main results. Section VI discusses the sensitivity of the results, and Section VII discusses housing price dynamics. Section VIII concludes.

II. Institutional Setting

The Norwegian Housing Market

In the current analysis we utilize data from the Norwegian capital Oslo. The size of Oslo is 453 square kilometers, and its 550,000 inhabitants make it the most populous local government in Norway. Commuting by car or public transport is relatively easy to any workplace location within the city; the city can reasonably be thought of as one housing region and one labor market.

The Norwegian housing sector is characterized by a high level of home ownership, with a national rate of 77 percent¹. While the figure for Oslo is lower, just over 70 percent, this is still slightly higher than for the United States², and also higher than for most European Union countries³. Furthermore, renting is mostly temporary; more than 90 percent of couples with school-age children own their homes.

The sale process is initiated by the seller contacting an agent. Seller and agent agree on a price quote, which is not binding, but which indicates their expected price. Homes are advertised in local newspapers and on the Internet, with ads stating the price quote as well as display times, typically at set times for two or three days. Bids are then put forth—and eventually accepted—in the days following the scheduled display.

While home buying is heavily leveraged, most buyers have negotiated a maximum loan with their bank before bidding for a home. Thus, bids are generally unconditional, often valid only for a short time—typically hours or even shorter—and binding for both parties once accepted by the seller. When the home sales studied in this paper took place, the real estate market was still booming, and homes were selling quickly. In our sample the median time to sell, i.e. from the first time a home is advertised to a bid is accepted by the seller, is 11 days, and 75 percent of homes sold within 18 days.

Compulsory Schooling

Compulsory schooling in Norway consists of primary education (grades 1–7) and lower secondary education (grades 8–10). Most students (98 percent) attend public schools operated at the local government level. Private schools are either religious schools or schools that use

¹ http://www.ssb.no/english/subjects/02/01/fobbolig_en/

² <http://www.census.gov/hhes/www/housing/hvs/qtr308/q308tab5.html>

³ <http://www.iut.nu/EU/HousingStatistics2004.pdf>

alternative pedagogical principles. All students are allocated to public primary schools by catchment areas within local governments, and there is limited school choice for a given residence. It is possible to apply for transfer from one school to another, but this school choice is subject to availability. Pupils living within the catchment area are prioritized, providing a clear incentive to live in the catchment area of your preferred school. There is no centralized registration of students attending schools outside their catchment area, but they most likely comprise a small share of all pupils. School catchment borders rarely change, and when they do they tend to affect only a limited number of houses.⁴

Some schools are combined primary and lower secondary schools, thus there is an obvious correspondence between catchment areas (which relate to primary schools) and school results (which only exist for lower secondary schools). In other cases students transfer from their primary school to a specific lower secondary school in the same area. There is typically a unique lower secondary school for any given primary school, but some primary schools pass students on to different lower secondary schools. Transactions in these primary school catchment areas are excluded from the empirical analysis. For the remaining observations there are unique mappings from residential address to lower secondary school. In the following we refer to this mapping when using the term catchment area.

At the lower secondary level, there was at the time of publication 48 ordinary government owned schools within Oslo enrolling 14,005 students, 22 of the schools are combined primary/lower secondary schools. Private schools are not included in our analysis.

⁴ This information was obtained through personal communications with municipality officials. We have not been able to find any written sources.

III. Data

School quality

Students are graded by the teachers at the end of lower secondary school, as well as on two external examinations. The average of these grades (grade point average) matter for entrance to upper secondary education. Until recently test scores (or other performance measures) have not been publicly available in Norway. The unadjusted school means of the grade point averages were made publicly available nationally in January 2003. However, this measure largely reflects factors outside the school's control (such as student composition) and is consequently not well suited to increase school accountability, one of the main goals of the initial publication.

Because of this shortcoming an effort was made to develop indicators that isolated the contribution of schools from other factors. These indicators were not made public at the national level, but the adjusted indicators for Oslo schools were published at November 18, 2005. This measure aimed to provide a better indication of each school's contribution to their students' achievements by adjusting mean grade point averages for individual student and parental characteristics,⁵ and the publication received considerable media attention. E.g. *Aftenposten*, the largest regional newspaper (and the second largest newspaper in Norway) presented school rankings on November 19, 2005, the day following publication. Even though the general public was unlikely to fully understand how the school quality indicators were computed, the media presented the new indicators as 'intrinsic school quality'.

⁵ Specifically, these indicators were constructed as the estimated school fixed effect in a student level regression, controlling for a very rich set of family background indicators, as described in Hægeland et al. (2004). As there traditionally have not been any registration of results before the end of lower secondary education, constructing value added indicators has not been possible.

In the empirical analysis we investigate the Oslo housing market response to the 2005 publication of adjusted school quality, which we denote Q_j^A (for school j). This new ‘intrinsic school quality’ is potentially new information to home buyers. To control for the previously available information we use the unadjusted school quality indicator that was published—and got considerable media attention—in 2003, which we denote Q_j^U . An alternative measure of pre-existing information could be the updated unadjusted results, published simultaneously with the new indicators of adjusted quality. However, which of these unadjusted quality measures used does not change our main results appreciably. We elaborate in Section IV.

To simplify interpretation, Q_j^U and Q_j^A are standardized to measure deviations from the national average, in units of (national student level) standard deviations of grade point averages. Thus their scales are comparable in terms of grade points. For the schools in our sample Q_j^U and Q_j^A have means of 0.10 and 0.20, indicating that the Oslo schools score above the national average on both measures. The (school level) standard deviations are 0.24 and 0.09, respectively, reflecting the fact that variability between schools is greatly reduced when controlling for student composition. The adjusted and unadjusted school quality indicators are weakly correlated, with the coefficient of correlation at the school level being only 0.29 between the adjusted indicator published in 2005 and the unadjusted indicator published in 2003.

In spite of Norway being a country known to be fairly egalitarian, there is considerable variation in quality across schools. This is especially true for unadjusted performance of schools in Oslo, as this city is much more segregated than the rest of Norway. However, there is considerable variation also in the adjusted performance measure. For example, moving a student from a school at the 25th percentile in our sample to a school at the 75th percentile is

expected to improve her grade point average with 0.15 student level standard deviations, or about 6 percentiles around the national median (e.g. from the 47th to the 53rd percentile). The magnitude of this difference can be illustrated by its significance when applying for upper secondary schools: The average student would in 2003 have his choice set extended with one school, from seven to eight schools. The total number of upper secondary schools in Oslo is 16. The maximum increase in a student's upper secondary school choice set, by moving her from a lower secondary school at the 25th percentile to the 75th percentile, is five.⁶ The difference between the highest and lowest scoring schools is about 0.39 student standard deviations, or 15 percentiles.

Housing transactions

We utilize a rich data set on housing transactions in Oslo from January 1, 2003 to December 31, 2006. The original data set consists of 79,322 transactions.⁷ All sales records are re-sales of apartments. Each record contains information on size (square meters), year of construction, date the item was listed for sale, price quote, date of sale, price, financial liability, and residential address. The residential address allows us to match transactions to school catchment areas. Excluding observations we cannot unambiguously associate with any school catchment area, observations with missing data, or observations with dates that seem unreasonable, we are left with 38,562 observations.⁸ We have on average about 185

⁶ The calculation of the scores required for admission changed in 2007, and the old scores are no longer publicly available; the calculations presented are based previously downloaded data from the municipality web page. Admission requirements for 2001 and 2002 are roughly similar, with the choice set of the average student being extended with two schools. All results pertain to the academically oriented course in public schools, encompassing about half of all students.

⁷ The source of the transactions data is the database of the Norwegian firm Eiendomsverdi (<http://www.eiendomsverdi.no/>). Eiendomsverdi collects transaction data for home sales in Norway from real estate agents to maintain a database of housing transaction, selling pricing-relevant information back to real estate agents and others.

⁸ This attrition comes in part from incomplete residential addresses, making it impossible to link an address to any particular primary school, partly from ambiguity in information regarding the transfer from primary to lower secondary school. Furthermore, new schools were constructed between the publications and school quality indicators were not published for schools with few pupils or with large variation in the number of

observations per week, but this average masks an increase from 138 in 2003 to 182 in 2004, 201 in 2005 and 221 in 2006.⁹ In the empirical analysis we experiment with different bandwidths bracketing the timing of the information shock to make sure that we do not lack power to test our hypothesis to be elaborated in the next section. The main source of loss of observations is missing variables, especially date of sale and price quote. There is no sign that the loss of observations is systematic and causing a bias. As far as we can observe, the observations excluded are similar to those included.¹⁰ Furthermore, as we estimate valuation within areas using a change in the available information, our results would still be unbiased in the case of a time-invariant difference in loss of data between catchment areas, even if the results may have been less representative for the market as a whole.

Table A1 presents descriptive statistics for key variables. The average apartment in our sample is 66 square meters and sold for NOK 1.81 million (USD 350,000). Although the sizes of the apartments in our sample are fairly homogenous, there is much variation in housing prices across school catchment areas (not reported for brevity). The average price per square meter ranges from NOK 16,400 to NOK 35,900.

In Figure 1 we illustrate the development in housing prices in the Oslo and the neighboring local government, Bærum, over the period 1992 to 2009. In the main period we study there was a substantial growth in housing prices (2004Q4 to 2006Q4, indicated by vertical lines),

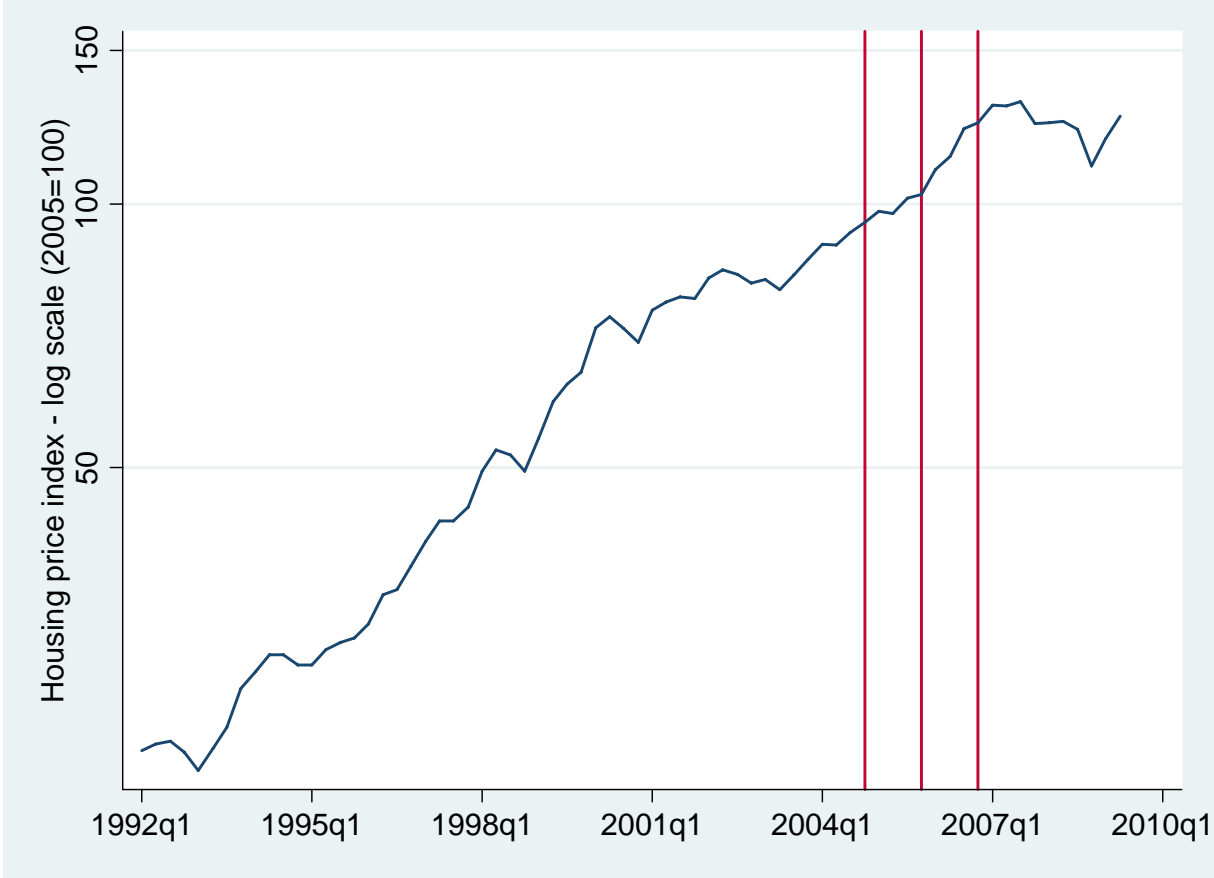
pupils between subsequent years. We also exclude erroneous data where the date of the price quote is reported to be after the date of sale, observations where the time to sell is reported to be very high (above the 99th percentile) and observations where the price quote is below 85 percent or above 135 percent of the actual price. Excluding these observations has a limited impact on the results.

⁹ There are large seasonal variations in the number of transactions. In particular, the number of transactions is very low around Christmas and New Year and also lower in summer and around Easter. Excluding weeks with few observations do not alter our main results to be presented below.

¹⁰ For example, except for the lower secondary schools with no unique mapping from a primary school, the distribution across schools is similar for transactions dropped as for those used in the analysis. The same is the case for size distributions, as long as we exclude the obviously erroneously registered observations with area equal to zero or missing, and similarly for year of construction. The observations missing essential variables have similar averages as our final sample for the variables that we do observe.

but it was not extraordinary in comparison to previous years. In 2009, after the global economic downturn, nominal housing prices are again higher than they were at the end of 2006.

Figure 1 Housing Price Index 1992 to 2009



Note: Housing price index for apartments in Oslo and Bærum from 1992Q1 to 2009Q2. Vertical lines at 2004Q4, 2005Q4 and 2006Q4. Publication of school quality indicators took place in the middle of 2005Q4. Source: Statistics Norway.

IV. Empirical Strategy

In this paper we rely on an exogenous information shock to investigate how new information impacts housing prices. This strategy enables us to say whether the newly published information is indeed previously unknown to the households, and to give some indications of household valuation of school quality.

The major challenge common to all studies investigating the relationship between school quality and housing values is to distinguish between the impact of school quality and other correlated characteristics. Because students with wealthier and more highly educated parents generally score better in school, schools with higher performing students tend to be located in more affluent and expensive neighborhoods. Thus, estimates of the value of schooling are likely to be upward biased if one does not control for neighborhood characteristics (Black, 1999; Bayer et al., 2007). Employing a research design where we compare housing prices immediately before and after the information shock within catchment areas, we net out all effects of school-level variables that do not vary over time.

Our empirical strategy is inspired by Figlio and Lucas' (2004) study of the impact of 'school report cards' in Florida. They find that the housing market responds strongly to first time assignment of school letter grades, but weaker effects for subsequent publications. In a related analysis, Kane et al. (2003) fail to find any effect of publication of test scores on housing prices using data from North Carolina. Pope (2008a, 2008b) applies similar strategies to investigate how changes in US seller disclosure laws impact the implicit price for airport noise and the risk of flooding respectively, and finds substantial housing market responses. Our analysis is also related to the literature that investigate market responses to the release of comparative information in various other settings, e.g. Jin and Leslie (2003) on restaurants, Dafny and Dranove (2008) on health plans, Bundorf et al. (2009) on fertility clinics and in particular, Hastings and Weinstein (2008) on public schooling.

We model housing prices employing a standard form hedonic price model. The hedonic model is a revealed preference approach to valuing attributes of different products (e.g. air quality, school quality, crime) that are not explicitly traded in their own markets (the classical

contribution is Rosen, 1974). Specifically, we relate housing prices to perceived school quality, in our specification the latent variable Q_j^* , by the following semi-log model:¹¹

$$(1) \quad \log P_{ijtw} = \alpha_j + \beta_w + \delta Q_{jt}^* + \mathbf{x}_{ij} \boldsymbol{\eta} + \varepsilon_{ijtw}.$$

In Eq. (1), P_{ijtw} is the price, including financial liability, of item i in school catchment area j , sold at day t in week w . α_j is vector of school catchment fixed effects, which will absorb neighborhood characteristics that do not change over time. β_j is a vector of week fixed effects, capturing general variation in apartment prices over time. Furthermore, we include the vector \mathbf{x}_{ij} which includes apartment size and construction year dummies.¹² α_j , β_j , $\boldsymbol{\eta}$, and δ are parameters to be estimated, and ε_{ijtw} is an error term. Because we observe multiple transactions in each catchment area at various points in time and our key variable of interest is at the school level, we adjust the standard errors for potential correlation in error terms within catchment areas.¹³

The main variable of interest in Eq. (1) is Q_{jt}^* , capturing the general publicly perceived quality of school j at time t . We model these perceptions as a linear function of the available information on school quality, i.e. the adjusted and unadjusted school quality indicators. These indicators are in turn functions of some underlying ('true') school quality, assumed to be time invariant,¹⁴ but the perceived school quality may depend on the available information.

¹¹ Cropper et al. (1988) study the choice of functional form for hedonic price functions. They find that a semi-log specification performs well, both when the model is correctly specified and when the possibility of misspecification exists.

¹² We have experimented with a richer specification, including also a non-linearity in size, number of bedrooms, an indicator of whether the apartment is in a co-operative, and the financial liability's share of the total cost. This has a very limited impact on the main results. Because of this lack of impact, and because the number of bedrooms variable appear to be of lower quality, we only present results from the more parsimonious specification.

¹³ This is done using the cluster option in Stata.

¹⁴ Thus, any systematic time variation in perceived quality will be attributed to the publication of indicators on school quality. Given the short time windows we study, we believe this assumption is not restrictive.

We expect changes in households' information set to lead to a reweighing of the school attributes reflected by the two quality indicators. To estimate these changes we interact adjusted and unadjusted quality with D_t , which is a dichotomous variable equal to one after publication (November 18, 2005). Thus, our empirical specification, to be estimated by ordinary least squares (OLS) is:

$$(2) \quad \log P_{ijtw} = \tilde{\alpha}_j + \beta_w + \delta(aD_tQ_j^A + bD_tQ_j^U) + \mathbf{x}_{ij}\boldsymbol{\eta} + \varepsilon_{ijtw}.$$

In Eq. (2) the effect of the publication of the indicators on housing prices will be captured by the interaction terms $D_tQ_j^U$ and $D_tQ_j^A$. Everything unchanged by the publication, including other neighborhood characteristics and school quality to the extent that this was already known prior to publication, is absorbed by the catchment fixed effects, $\tilde{\alpha}_j$.

As mentioned in Section III, we use the unadjusted quality indicators published in 2003 to control for pre-existing knowledge when estimating the price response. The fact that there was also a 2005 publication of updated unadjusted results in principle does complicate our estimation strategy somewhat. A full specification of the available information could be argued to include both the 2003 and 2005 publications of the unadjusted quality information, as well as the new information on adjusted quality. However, the high persistence in unadjusted quality makes the estimates from such a specification highly imprecise (the correlation coefficient is above 0.8). Furthermore, the results from estimations using the 2003 and the 2005 unadjusted quality produce very similar results. We choose the 2003 figures as the most intuitive measure of known unadjusted quality.

Our main parameter of interest is δ , which we expect to be positive. However, it is clear from Eq. (2) that we can only estimate δa and δb . Thus, we cannot identify δ without making assumptions about a and b . These coefficients measure to what extent households perceptions

of school quality changes when the information environment changes, and how these changes correlates with the school attributes reflected in the quality indicators. Assumptions about the coefficients of Eq. (2) translate into assumptions about what value households place on adjusted and unadjusted quality, before and after publication of the former. We believe some assumptions with testable implications are justified.

Both households with and without children may care about local school quality. Households without children care because the quality of their school will affect the price they can sell their house for. However, it is reasonable to expect that the preferences of parents (or potential parents) play a key role in driving a potential housing market reaction to publication of new information on school quality. Ultimately, parents care about a wide range of outcomes for their children, including but not limited to educational and labor market success, well-being at school, and the friends their children make. This may give rise to a preference for good schools, even in the absence of any preference for school quality. Parents may value competent peers, irrespective of the schools contribution to the students' achievements. This may for example be the case if parents believe peer effects are strong.¹⁵

If households do not value school quality ($\delta = 0$), or at least not school quality as captured by the adjusted indicator there will be no effect of the 'information shock'. Similarly, if the indicators did not provide any new information, weights should not change ($a = 0, b = 0$), and there will not be any effect of the 'information shock'. This leads to our Hypothesis 1:

Hypothesis 1. If households value school quality, and the 2005 school performance indicators provided relevant information to them, the regression coefficient corresponding to δa should be positive.

¹⁵ For further discussion, see Rothstein (2006).

δa does not necessarily capture the full capitalization effect of school quality into housing prices, this will only be the case if households were ignorant about school quality before publication, and then accept the adjusted indicator as a valid and permanent measure of school quality. In less extreme cases, where there is only partial pass-through of the published indicator into perceived quality—for example because the information is already partly known and thus captured by the fixed effects—our estimate of δa will be a downward-biased estimate of household valuation of school quality, δ .¹⁶

Even though only the adjusted quality provides any significant amount of new information, we still want to condition on the pre-existing unadjusted indicators. This is because the knowledge of adjusted quality may lead the households to reduce their emphasis on unadjusted quality. If households did care about intrinsic quality, but were ignorant of this prior to the publication, unadjusted quality may have served as a best guess. When better knowledge of intrinsic quality is obtained, the previously available information may be disregarded. Thus, there are two reasons for including the previously known unadjusted school quality in the regressions: First, studying how new information impacts the valuation of old information is interesting on its own. Second, as adjusted and unadjusted quality are positively correlated, not controlling for changes in valuation of unadjusted quality may bias our results for adjusted measure of quality.

The unadjusted quality information was already public prior to the 2005 publication, and was then supplemented, or possibly supplanted, by an additional measure. Thus, its weight in deciding perceived quality should have stayed the same or gone down, implying that $b \leq 0$. If

¹⁶ On theoretical grounds we cannot rule out that the weighting of the adjusted indicators decreases with publication, leading to a situation where favorable indicators may be negatively correlated with changes in housing prices. This will be the case if parents have an idea of ‘good’ and ‘bad’ schools prior to publication, but systematically overrate ‘good’ schools and underrate ‘bad’ schools.

the households do indeed have any preference for school quality as such, the weights will stay the same only if the indicators provided no new information, otherwise at least some emphasis would be shifted from unadjusted to adjusted quality. This is the basis for our second hypothesis:

Hypothesis 2. If households value school quality, and the 2005 school performance indicators provided relevant information, which in turn made households attach less weight to unadjusted results, then the regression coefficient corresponding to δb should be negative.

The hedonic price regression is estimated using the data set described in Section III. Because unobservables affecting house prices are likely to be systematically correlated with the school performance indicators, we base our inference on within catchment changes in prices in order to remove all effects of variables related to each school area that do not vary over time. Furthermore, we evaluate changes in housing prices tightly bounded around the publication date, such that variables unrelated to the publication can be expected to stay constant. We experiment with different bandwidths around the date of publication, ranging from 2 to 104 weeks (± 1 week to ± 52 weeks). While the existing studies on publication of school quality indicators rely on monthly data and omit months close to publication, we use the exact date of sale (agreement). We consequently offer a more precise test and can offer evidence of housing price dynamics.

As discussed above, we study a time period where the real estate market was booming. For this to bias our results upwards it must be the case that price appreciation is higher in areas with schools of high unadjusted quality. An empirical analysis from Boston, US, suggest that it is the other way around (Case and Mayer, 1996), implying a potential downward bias in our estimates. To investigate this issue we run ‘placebo regressions’ at dates without any ‘information shock’ and compare them to the estimates based on the actual publication date.

V. Price Response

Table 1 displays the results from regressions of log price on different sets of explanatory variables, based on transaction data from 2005. There is a strong positive relationship between the size of the apartment and log price across all specifications. Older apartments tend to sell for a lower price, relative to apartments built after 1999 (our reference category). The impact of construction year is non-monotonic, e.g. apartments built before 1900 receive a higher price than apartments built between 1960 and 1979. Week fixed effects alone explain only about 1 percent of the variation in log price.

In line with the literature, we find that better performing schools tend to be located in neighborhoods with more expensive housings (e.g. Bayer, Ferreira, and McMillan, 2007). The results indicate a price differential of about 5–10 percent for a one (school level) standard deviation difference in school results. As stressed by the literature, this does not necessarily reflect a causal relationship, in the sense that housing prices are higher because of good schools. It seems quite likely that schools of different (observed and perceived) quality, and especially with different peer groups and thus unadjusted results, will be located in neighborhoods with different characteristics. This means that a naïve regression of log prices on school quality, not controlling for neighborhood, yields biased results that partly reflect the correlation between school quality and other neighborhood characteristics.

Table 1: Cross-sectional regression results, 2005. The dependent variable is log price

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Size		0.011 [29.61]**	0.010 [34.65]**	0.010 [29.47]**	0.010 [28.47]**	0.010 [27.25]**	0.010 [26.96]**
<i>Year of construction:</i>							
Before 1900			-0.069 [2.09]*	-0.092 [3.15]**	-0.107 [3.27]**	-0.114 [3.85]**	-0.177 [8.78]**
Between 1900 and 1919			-0.045 [1.51]	-0.107 [3.57]**	-0.080 [2.54]*	-0.123 [4.08]**	-0.178 [7.20]**
Between 1920 and 1939			-0.113 [3.54]**	-0.155 [5.55]**	-0.135 [4.39]**	-0.165 [5.90]**	-0.209 [14.04]**
Between 1940 and 1959			-0.281 [9.20]**	-0.297 [10.65]**	-0.256 [9.71]**	-0.280 [9.29]**	-0.226 [12.08]**
Between 1960 and 1979			-0.428 [12.02]**	-0.423 [11.53]**	-0.406 [10.95]**	-0.409 [10.28]**	-0.243 [13.30]**
Between 1980 and 1989			-0.275 [3.53]**	-0.276 [4.28]**	-0.238 [3.40]**	-0.252 [4.09]**	-0.164 [8.07]**
Between 1990 and 1999			-0.13 [2.79]**	-0.129 [3.07]**	-0.109 [2.59]*	-0.116 [2.94]**	-0.093 [3.94]**
Q_j^U				0.418 [4.68]**		0.378 [3.96]**	
Q_j^A					0.734 [3.05]**	0.469 [2.57]*	
Week fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Catchment area fixed effects	No	No	No	No	No	No	Yes
# obs.	10457	10457	10457	10457	10457	10457	10457
R ²	0.01	0.54	0.69	0.75	0.71	0.76	0.85

Notes: Columns 1 through 7 report coefficients of OLS regressions. Absolute value of t statistics based on catchment area clustered standard errors in brackets, * significant at, or below, 5 percent; ** significant at, or below, 1 percent.

While the estimated coefficients on variables standardized according to grade points are larger for adjusted quality, the more compressed scale of this variable implies that the ‘effect’ of a one school level standard deviation is greater for unadjusted quality.¹⁷ This is the case when the indicators are included separately in the regression (specification 4 and 5), and also when we include both measures (specification 6). In the latter case, the coefficients associated with both unadjusted and adjusted quality are somewhat reduced, but still of nontrivial size.

¹⁷ This follows from the results presented in Table 1 and the fact that the school level standard deviations of Q_j^U and Q_j^A are 0.24 and 0.09 respectively. Thus we find price differentials of about 10 and 5–8 percent for a school level standard deviation of the two quality measures, depending on whether we control for both simultaneously.

Table 2 presents results based on our empirical strategy laid out in Section IV. Our inference is based on within catchment area changes in housing prices. If publication of school performance indicators provides new information to households, and households value this information, then housing prices should respond accordingly. We apply eight different bandwidths, ranging from ± 1 week to ± 52 weeks.

Table 2: Housing market reaction to publication of school quality indicators. The dependent variable is log price

Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	± 1 week	± 2 weeks	± 3 weeks	± 4 weeks	± 8 weeks	± 13 weeks	± 26 weeks	± 52 weeks
Interval	2005.11.11– 2005.11.24	2005.11.4– 2005.12.1	2005.10.28– 2005.12.8	2005.10.21– 2005.12.15	2005.9.23– 2006.1.12	2005.8.19– 2006.2.16	2005.5.20– 2006.5.18	2004.11.19– 2006.11.16
$D_t Q_j^A$	0.181 [1.43]	0.143 [1.39]	0.179 [2.29]*	0.132 [1.73]	0.167 [3.05]**	0.139 [2.64]*	0.068 [1.82]	0.027 [0.81]
$D_t Q_j^U$	0.084 [1.70]	0.021 [0.60]	0.002 [0.07]	-0.019 [0.85]	-0.020 [1.00]	-0.024 [1.12]	-0.021 [1.67]	-0.028 [2.57]*
# obs.	487	911	1281	1631	2804	5271	10968	21753
R ²	0.89	0.88	0.86	0.86	0.86	0.85	0.85	0.86
# obs. pre publ.	230	460	695	920	1845	3084	5675	10413
# obs. post publ.	257	451	586	711	959	2187	5293	11340

Notes: Columns 1 through 8 report coefficients of OLS regressions for different bandwidths. Absolute value of t statistics based on catchment area clustered standard errors in brackets. All estimations control for size, year of construction (dummies), week fixed effects, and catchment area fixed effects. * significant at, or below, 5 percent; ** significant at, or below, 1 percent.

$D_t Q_j^A$ is found to be statistically significant for bandwidths of 6, 16, and 26 weeks, consistent with Hypothesis 1. We fail to obtain a statistically significant effect for the narrowest and widest bandwidths. In every regression, however, we find a positive effect and the estimated magnitude is not very different for the very narrow (± 1 week, ± 2 weeks) to the middle bandwidths (± 13 weeks). For the wider bandwidths, the estimated coefficients are much smaller and not statistically significant at conventional levels, indicating that there may not be a long-term effect. We return to this issue in Section VII.

The short term results suggest that favorable information about the local school is immediately transferred into higher offers by home buyers. The estimates suggest that a one

(school level) standard deviation increase in Q_j^A is associated with about a 1.5 percent increase in housing prices, all else equal. For the average apartment in our sample this corresponds approximately to an increase of NOK 27,000 (USD 5,250), which—as noted in Section IV—because of the possibility of pre-existing knowledge about school quality is likely to be a lower bound on valuation. This estimate is similar to what is reported by earlier papers utilizing quasi-experimental methods to study how school quality is capitalized into housing prices (such as Black (1999) for the US, Gibbons and Machin (2003) for the UK, Fack and Grenet (2007) for France and Ries and Somerville (2009) for Canada).¹⁸ Figlio and Lucas (2004) find a larger impact of the release of new information of school quality in Florida. An initial “A” grade is associated with a 19.5% increase in housing prices the first year of publication.

We do not find strong support in favor of Hypothesis 2. We fail to reject the null hypothesis of no impact of $D_t Q_j^U$ for dates closely bracketed around publication. Thus, it does not seem that the old quality measure is disregarded when new information is introduced, or that what matters is the difference between the two measures. We do, however, find some traces of a statistically significant negative long-term effect, but we are cautious in interpreting this as a causal effect because wider bandwidths may be more vulnerable to omitted variables and housing market trends that vary across school districts.

VI. Robustness

A potential problem with our identification strategy is that systematic unobserved differences among apartments sold before and after publication (correlated with the published school quality indicators) may exist. To investigate the validity of our identification strategy we

¹⁸ Bayer et al. (2007) find a smaller response than these studies.

conduct two key robustness checks: (i) we consider apartments which have price quotes set and have been advertised before publication, but are sold after publication; and (ii) we conduct falsification tests where we estimate models for a wide range of (non-publication) dates. We have also rerun our main regressions, excluding outliers with a very low or very high square meter price. This does not have a large impact on our estimated coefficients, or on the level of statistical significance, and are therefore not reported. Similarly, we have repeated the analysis for other definitions of outliers, as well as with more flexible time trends allowing for separate week effects for different geographical areas (based on zip codes), without our results changing much.

Price Quote

Table 3 shows results from regressions where the dependent variable is the log of the sale price to price quote ratio. The range of bandwidths is the same as in Table 2, but as most apartments are sold shortly after the price quote is set the narrower bandwidths are the most interesting. In the first week after November 18, 2005, 97 percent of the apartments had a price quote set before November 18. However this number drops to 40 percent in week 2, and 20 percent in week 3 and week 4. As can be seen from the number of observations, extending the bandwidth adds few observations sold after the publication, when we restrict the analysis to those transactions where the price quote is set before November 18. Still, for all bandwidths we do find a statistically significant effect of adjusted school quality. Thus, apartments in the catchment areas of high-scoring schools tend to sell at a higher price, relative to sellers expected price when the apartment was first advertised before publication, than apartments in the catchment areas of lower-scoring schools. This is a strong indication that systematic unobservable differences between apartments sold before and after publication do not bias our results. If there were indeed such systematic differences, these should then be

unobservable both to the researcher, as well as to the seller and agent, in which case it seems unlikely that they influence the price.¹⁹

There may however be an endogeneity problem associated with the use of price quotes. If price quotes are set high, the time to sell is likely to increase, and an apartment with a high price quote set before the publication becomes more likely to be sold post-publication. However, for this to explain our findings in this section, this must be correlated with the school quality indicators, and there is no a priori reason for this to be the case. Furthermore, most sales we study, and certainly most when we limit the sample to the two weeks surrounding publication, are sold shortly after price quotes are set. For these apartments, time to sell probably to a large degree results from day of the week variations (many apartments are displayed on the weekend and sold on Monday or Tuesday), and we are less likely to have a problem with endogeneity. Still, we also repeated the analyses presented in Table 3, while controlling for time to sell. This hardly had any impact on the results presented above, and we thus conclude that endogeneity in time to sell is unlikely to influence our results.²⁰

¹⁹ We have also separately studied the price quotes set before the publication date, as done for price in Table 3, finding no significant correlation between adjusted school quality and log price quote.

²⁰ These results are not reported, but are available upon request.

Table 3: Sale price relative to price quote, dependent variable is log of sales price to price quote ratio. Date is date of sale, and the sample is restricted to apartments with price quote set before November 18, 2005

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bandwidth	±1 week	±2 weeks	±3 weeks	±4 weeks	±8 weeks	±13 weeks	±26 weeks	±52 weeks
Interval	2005.11.11– 2005.11.24	2005.11.4– 2005.12.1	2005.10.28– 2005.12.8	2005.10.21– 2005.12.15	2005.9.23– 2006.1.12	2005.8.19– 2006.2.16	2005.5.20– 2006.5.18	2004.11.19– 2006.11.16
$D_t Q_j^A$	0.203 [3.40]**	0.123 [2.05]*	0.099 [2.05]*	0.114 [2.74]**	0.113 [3.16]**	0.088 [2.88]**	0.097 [3.23]**	0.090 [2.85]**
$D_t Q_j^U$	-0.012 [0.55]	-0.006 [0.25]	-0.014 [0.67]	-0.014 [0.77]	-0.02 [1.28]	-0.027 [1.79]	-0.031 [2.34]*	-0.029 [2.20]*
# obs.	480	793	1056	1306	2266	3581	6195	10936
R ²	0.21	0.20	0.15	0.14	0.12	0.10	0.10	0.10
# obs. pre publ.	230	460	695	920	1845	3084	5675	10413
# obs. post publ.	250	333	361	386	421	497	520	523

Notes: Columns 1 through 8 report coefficients of OLS regressions for different bandwidths. Absolute value of t statistics based on catchment area clustered standard errors in brackets. All estimations control for size, year of construction (dummies), week fixed effects, and catchment area fixed effects. * significant at, or below, 5 percent; ** significant at, or below, 1 percent.

Placebo Publication Effects

The week fixed effects that we include in our regressions capture general time trends common to all school districts in Oslo. There may however be seasonal or other time effects that differ across school districts that potentially bias our results. To address this issue we estimate artificial publication effects, at dates without any information shock, and compare these with the estimated effect at November 18, 2005. If we found similar types of results in regressions based on artificial publications, it would suggest that omitted (local) trends, seasonal effects, other omitted variables or wrong functional form led to the results presented above.

Table 4 displays results similar to those in Table 2, from regressions based on transactions one year before the actual publication of the indicators.²¹ We note that all estimated correlations between adjusted quality and log housing prices are smaller in absolute values

²¹ Note that the placebo publication date chosen here is November 19, 2004. Housing sales follow a distinct day of week pattern, and for this reason we want the day of the week of the 2004 placebo publication to be the same as the day of week of the actual 2005 publication. The results would not change noticeably if we rather used November 18, 2004.

than those we found using the actual 2005 publication. Furthermore, the estimated correlations are mostly negative, and never statistically significant. Thus, the implied difference in difference estimates are mostly larger than the estimates reported in Table 2, and it seems unlikely that our results are driven by seasonal effects.²²

Table 4: Placebo publication effect, one year before actual publication. The dependent variable is log price

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bandwidth	±1 week	±2 weeks	±3 weeks	±4 weeks	±8 weeks	±13 weeks	±26 weeks	±52 weeks
Interval	2004.11.12- 2004.11.25	2004.11.5- 2004.12.2	2004.10.29- 2004.12.9	2004.10.22- 2004.12.16	2004.9.24- 2005.1.13	2004.8.20- 2005.2.17	2004.5.21- 2005.5.19	2003.11.21- 2005.11.17
$D_t Q_j^A$	-0.087 [0.65]	-0.076 [0.92]	-0.005 [0.06]	-0.025 [0.35]	-0.014 [0.21]	0.006 [0.14]	-0.012 [0.34]	-0.006 [0.21]
$D_t Q_j^U$	-0.076 [1.12]	-0.043 [1.18]	-0.083 [2.43]*	-0.036 [1.18]	-0.039 [1.80]	-0.011 [0.62]	0.015 [0.95]	-0.005 [0.44]
# obs.	465	879	1246	1614	2865	5230	10055	19732
R ²	0.85	0.86	0.86	0.84	0.85	0.85	0.85	0.84
# obs. pre publ.	238	462	697	966	1985	3251	5317	9319
# obs. post publ.	227	417	549	648	880	1979	4738	10413

Notes: Columns 1 through 8 report coefficients of OLS regressions for different bandwidths. Absolute value of t statistics based on catchment area clustered standard errors in brackets. All estimations control for size, year of construction (dummies), week fixed effects, and catchment area fixed effects. * significant at, or below, 5 percent; ** significant at, or below, 1 percent.

To further test the likelihood of area-specific trends driving our results, we have also for a range of bandwidths estimated placebo publication effects for all dates from January 1, 2003 and onwards, to the last time window not containing any actual post-publication dates. Table 5 reports the t-values from the regression centered on November 18, 2005 and the share of estimated t-values from placebo regressions that are larger than or equal to this. For completeness we also report the share of estimated t-values that are larger than or equal to this in absolute value. As is evident from the table, it is a very rare occurrence to find results as strong as those we get for the actual publication date. For 12, 16, and 20 weeks bandwidths

²² We have also looked in detail at placebo regressions for November 18 in 2003 and 2006. With one exception, we never find statistically significant effects of adjusted performance in regressions bracketed around November 18 for these years. The exception is the widest bandwidth in 2003 (± 52 weeks). Results are not reported, but available upon request.

we find well below one percent of the placebo t-values to be larger than the actual t-value at November 18, 2005.

Table 5: Estimated “placebo publication effects” relative to the actual publication effect. The dependent variable is log price

Bandwidth (weeks)	Coeff., Nov 18, 2005	t-value, Nov 18, 2005	$ t_{\text{placebo}} \geq t_{18\text{Nov}2005}$ (percent)	$t_{\text{placebo}} \geq t_{18\text{Nov}2005}$ (percent)
±1	0.181	1.43	20.89	8.85
±2	0.143	1.39	22.24	10.63
±3	0.179	2.29	6.23	3.36
±4	0.132	1.73	10.43	5.02
±6	0.154	2.54	1.34	0.62
±8	0.167	3.05	1.17	0.21
±10	0.203	3.93	0.66	0.00
±13	0.139	2.64	7.35	5.97

Notes: Placebo publication effects are based on estimations from all dates from January 1, 2003 onwards, until the last date that does not include November 18, 2005. All estimations control for size, year of construction (dummies), week fixed effects, and catchment area fixed effects.

As a final robustness check, we also run placebo regressions for the price quote specification. Once again, the publication effect stands out compared with placebo publication effects. For eight-week bandwidths and longer we never find as high t-values in placebo regressions as we do at the actual publication date. Furthermore, for the narrowest bandwidth, which in this case may be the most relevant one, less than one percent of the placebo effects are positive and as statistically significant as the actual estimated publication effect. These results are omitted for brevity.

VII. Housing Price Dynamics

As noted in Section V, the lack of an effect for wider bandwidths may indicate that there is no permanent impact of publication on housing prices. In this Section we investigate this in detail. In Figure 2, we plot estimated coefficients and t-values associated with all daily four-week periods, from six months before the publication date until six months after, comparing with two different pre-publication periods. Looking at the evolution of the post-publication

results, there seems to be a short-term effect lasting up to around two months, after which the effect disappears.

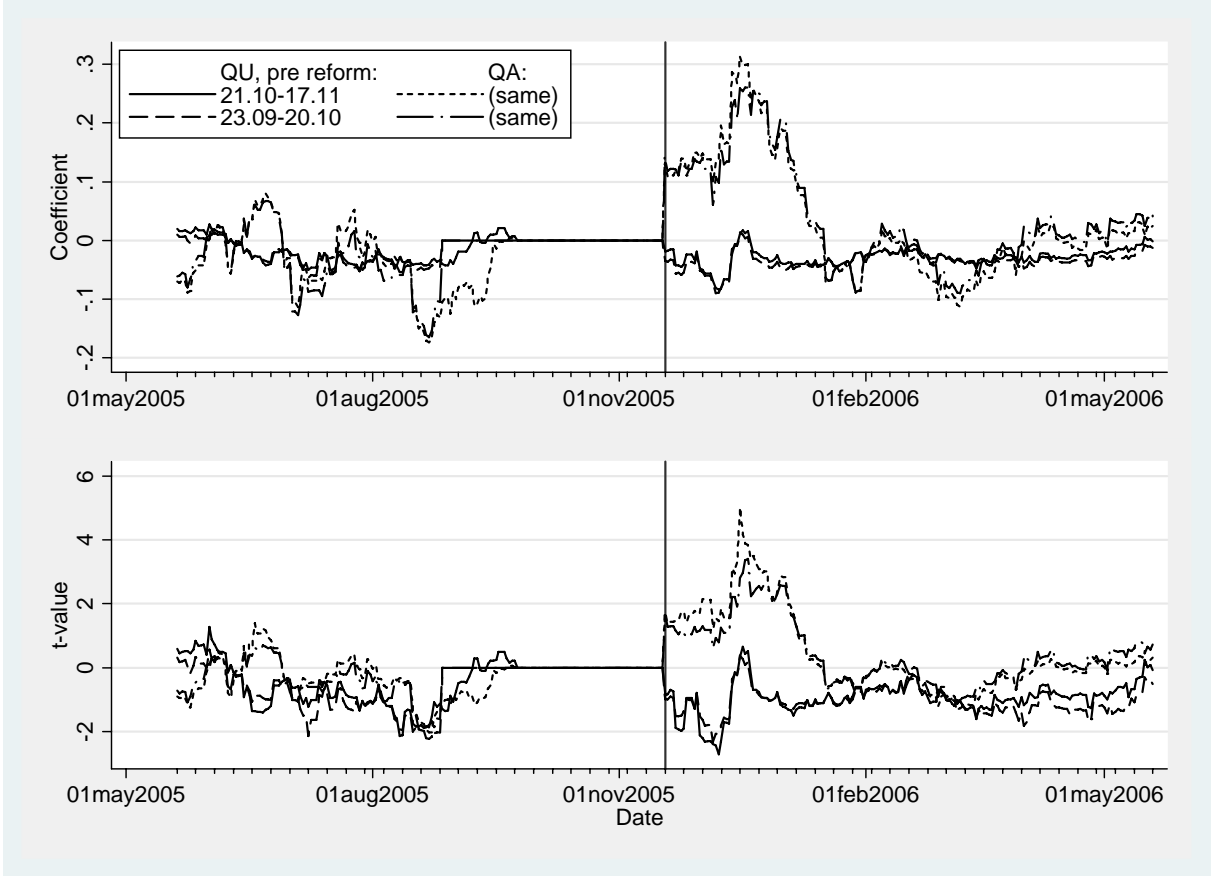
The estimated effect is strongest for the period around 4 to 8 weeks after publication, and is for most of this period of a larger magnitude than the estimates reported in Table 2, yielding an effect on housing prices of about three percent for a one standard deviation difference in school quality. The t-values are above two for almost all the four week windows from the one starting on December 13 (and ending on January 9), to the one starting on January 2 (and ending on January 29), and afterwards always stay below two. This implies that the main results do not depend on the limited number of transactions around Christmas and New Year.

Comparing the post publication transactions with an alternative transactions preceding publication, September 23 to October 20, rather than October 21 to November 17, the results are similar. Thus, our results do not seem to be an artifact of the pre-publication period used to make the comparisons. We find a statistically significant publication effect using any of the four possible permutations of the two non-overlapping pre-reform time windows and two arbitrarily chosen non-overlapping post-reform time windows, as long as the latter are close to the reform. We believe this provides yet another strong robustness check for our main findings.

Furthermore, looking at the leftmost part of the figures, comparing the pre-publication reference periods to other pre-publications intervals, we estimate no strong or systematic effects, supporting the conclusion that our main findings are not spurious. Figure 2 also provides some evidence of a negative short-term effect of unadjusted test scores. This result is much weaker than our result for adjusted quality, but for some time windows the negative effect is statistically significant at the five percent level (irrespective of pre-publication

period), and the estimated coefficients for almost the entire six months after publication are below zero.

Figure 2: Dynamics of the estimated publication effect. The dependent variable is log price



Notes: Figure shows the rolling time window estimates of coefficients (top panel) and t-values, from regressions of the publication effect on the valuation of the two quality measures. For each date publication effects are estimated from regressions comparing one of the two fixed pre-publication periods (indicated in the legend) with the four weeks from the date specified and onwards. Effects are also estimated at times before the actual publication (indicated with a vertical line). For post-publication time windows that overlap with the pre-publication periods, or that do contain dates both before and after the actual publication of the indicators on November 18 (i.e., post-publication periods starting dates August, 27 to November, 17 for the earliest pre-publication period and September, 24 to November, 17 for the latest), coefficients and t-values are set equal to zero.

The price reversal that we have documented is consistent with several possible interpretations.

We believe four—to some extent related—explanations may have some merit.

First, while potential buyers in late November 2005 are likely to have taken notice of the published school performance indicators, announced in several media, new buyers entering

the housing market are less and less likely to have taken notice of the publication. The cost of information acquisition will be increasing over time and may contribute to the price reversal.

Second, agents in the housing market may tend to focus on salient housing characteristics when deciding their willingness to pay. This interpretation is consistent with earlier studies documenting that investors may react more to news that is more salient (e.g. Klibanoff et al., 1998; Huberman and Regev, 2001).²³ In a related study to ours, Hastings and Weinstein (2008) document that providing direct information on (unadjusted) test scores to parents, originally published only at the web, leads to an increase in the probability that parents choose a high performing school.

Third, school quality indicators are noisy measures of school performance.²⁴ It may be that the public's perception of the precision of the new school performance indicator were high immediately after publication, maybe as a consequence of media presenting the indicators as 'intrinsic school quality' without reporting uncertainty related to the indicators. But that over time the public learned about the uncertainty related to the published indicators. In fact teacher union leaders and some politicians got considerable media attention for attempts to discredit the published indicators. Related to this, it is interesting to note that Figlio and Lucas (2004) find that the housing market responds strongly to first time assignment of school letter grades in Florida, but weaker effects for subsequent publications.²⁵

Fourth, and related to the two previous points, it may also be the case that the media directly contributed to a 'hype' concerning the new school performance measure. In this case the short

²³ Several empirical studies find that salient news tend to carry greater weight in market prices. See Daniel et al. (2002) for a review of this literature.

²⁴ For a discussion see Kane and Staiger (2002).

²⁵ Figlio and Lucas (2004) match information released in May or June to transactions in July to April the subsequent year. If we take a similar approach, i.e. omitting November and December, and comparing prices in January to October, 2005 with January to October, 2006, we get results very similar to our specification (8) in Table 2. These indicate no impact of adjusted quality, but some decapitalization of the previously published unadjusted quality.

term response should not be interpreted as capturing households willingness to pay for school quality per se (Qa), but rather an effect of the ‘hype’ itself. When the media focuses its’ attention elsewhere and the ‘hype’ dies down, households may have reverted to their old valuation of school areas. This interpretation is consistent with studies from the US finding school expenditures and unadjusted test scores to be associated with with higher housing prices in cross sectional hedonic price regressions, while value added measures are not (Downes and Zabel (2002), Brasington and Haurin (2006)).²⁶

If the housing price dynamics that we have documented should be interpreted as some sort of ‘hype’ one may suspect that real estate agents may have played a role (in addition to the media). By studying price quotes set (by sellers together with their real estate agents) shortly before and after publication, we can investigate this conjecture. In Table 6 we present analyses similar to those in Table 2, but using price quotes as the dependent variable. We find weaker housing market reactions to the publication than we did when studying prices. For the narrowest window, the effect is essentially zero, and for all intervals narrower than ± 26 weeks we find smaller coefficients than for prices. Strong conclusions cannot be drawn based on these results, but they indicate that home buyers are primarily driving the housing market response: Prices—resulting from the interaction between seller and buyer—respond quickly, while price quotes—set by the seller and his agent without any influence from the buyer—respond more sluggishly, possible as a response to an observed change in prices.

²⁶ The value added measures applied in the hedonic models in these two studies are not publicly available (while unadjusted test scores are).

Table 6: Housing market reaction to publication of school quality indicators. The dependent variable is log price quote. Dates are price quote dates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bandwidth	±1 week	±2 weeks	±3 weeks	±4 weeks	±8 weeks	±13 weeks	±26 weeks	±52 weeks
Interval	2005.11.11– 2005.11.24	2005.11.4– 2005.12.1	2005.10.28– 2005.12.8	2005.10.21– 2005.12.15	2005.9.23– 2006.1.12	2005.8.19– 2006.2.16	2005.5.20– 2006.5.18	2004.11.19– 2006.11.16
$D_i Q_j^A$	–0.004 [0.03]	0.080 [0.70]	0.124 [1.59]	0.074 [1.03]	0.099 [1.72]	0.116 [2.50]*	0.068 [2.26]*	0.040 [1.47]
$D_i Q_j^U$	0.105 [1.98]	0.038 [1.03]	0.019 [0.60]	–0.003 [0.16]	–0.007 [0.38]	–0.01 [0.58]	0.004 [0.30]	–0.007 [0.54]
# obs.	487	911	1281	1631	2804	5271	10968	21753
R ²	0.90	0.88	0.87	0.87	0.87	0.86	0.86	0.87
# obs. pre publ.	230	460	695	920	1845	3084	5675	10413
# obs. post publ.	257	451	586	711	959	2187	5293	11340

Notes: Columns 1 through 8 report coefficients of OLS regressions for different bandwidths. Absolute value of t statistics based on catchment area clustered standard errors in brackets. All estimations control for size, year of construction (dummies), week fixed effects, and catchment area fixed effects. * significant at, or below, 5 percent; ** significant at, or below, 1 percent.

VIII. Concluding Remarks

This paper analyzes how an exogenous information shock, namely the first time release of information on ‘intrinsic’ school quality, impacts household valuation of school quality. To pin down changes in households’ willingness to pay, we use data on school catchment areas and housing transactions that precisely bracket the timing of the information shock.

We find a robust short-term response to the release of information on school quality. Housing prices appreciate in catchment areas of well performing schools already the week following publication. This suggests that households are willing to pay for improvements in school quality, and that they were not initially fully informed on quality differences. We document however, that housing prices revert to pre-publication levels after two to three months.

We cannot offer sharp tests of competing hypothesis that may have contributed to the housing price reversal, but we point to the cost of information acquisition, the role of the media and the visibility of the information in the information environment as potential important

explanations. Future research should aim to separate these competing explanations from each other.

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Table A1: Summary statistics

	Mean	Std. dev.
Price (1000 NOK, including financial liability)	1810	837
Price per sq. meter (1000 NOK, including financial liability)	28.04	8.64
Log(Price) (including financial liability)	14.33	0.38
Log(Price quote) (including financial liability)	14.27	0.39
Log(Price/price quote)	0.05	0.07
Q_j^U	0.10	0.24
Q_j^A	0.20	0.09
Size (sq. meter)	66.49	25.65
Year of construction:		
Before 1900	0.13	
Between 1900 and 1919	0.06	
Between 1920 and 1939	0.19	
Between 1940 and 1959	0.22	
Between 1960 and 1979	0.24	
Between 1980 and 1989	0.08	
Between 1990 and 1999	0.04	
After 2000	0.04	