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Does the Housing Market React to New Information on School Quality?

Abstract:
This paper analyzes housing market reactions to the release of previously unpublished information on school quality. Using the sharp discontinuity in the information environment allows us to study price changes within school catchment areas, thus controlling for neighborhood unobservables. We find a substantial housing market reaction to publication of school quality indicators, suggesting that households care about school quality, and may be willing to pay for better schools. The publication effect is robust to a number of sensitivity checks, but does not seem to be permanent as prices revert to prepublication levels after two to three months. We discuss this reversion in relation to the literature on behavioral finance and the concept of limited attention.

Keywords: valuation of school quality, hedonic methods, price reversion

JEL classification: I21, I28, R21, R23

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1. Introduction

Public reporting of school performances is a feature of school accountability systems that are increasingly common across OECD countries (OECD, 2007). School accountability systems are primarily designed to create incentives for school leaders and teachers to provide good schooling. Proper incentives may be generated through financial rewards or sanctions based on some performance measure, but potentially also from increased monitoring by parents and taxpayers as a consequence of public reporting. Accountability systems may consequently be more effective if households have strong preferences for school quality per se. It is not clear whether this is the case. A large literature has investigated household valuation of school quality by utilizing housing market data. However, correlations between school quality and other neighborhood characteristics make identification of causal effects difficult. Moreover, even though many studies find average test scores to be capitalized into housing prices (e.g. Black, 1999), it is not clear that parents value school quality in itself (Rothstein, 2006).

In this paper we exploit publication of school quality indicators to investigate households' preferences for school quality. Based on the literature on the capitalization mechanism, initiated by Oates (1969), we expect new information on school quality to be reflected in housing prices if school quality is valued by households. The housing market in Oslo is well suited to study the capitalization mechanism because enrollment in public schools, which almost all students attend, is tied to residential address. Another advantage is high quality housing data that precisely bracket the timing of the 'information shock', allowing us to net out all effects of variables related to each school area that do not vary over time. Finally, the school quality indicator that we use isolates schools' contribution from student composition, and is consequently well suited to investigate household valuation of school quality per se.

To estimate the full capitalization effect of school quality into housing prices is beyond the scope of this paper. We do however 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 nonmarket amenities and public services, such as school quality (Black, 1999; Bayer, Ferreira, and McMillan, 2007), air quality (Chay and Greenstone, 2005), airport noise (Pope, 2008), and hazardous waste (Greenstone and Gallagher, 2008).

Utilizing discontinuity design in combination with hedonic price regressions, we find a substantial short-term response in housing prices. The increase in average willingness to pay for school quality suggests that (i) households care about school quality, and (ii) publication of adjusted school results provided new information, valued by households. A differential in published results of one standard deviation yields a difference in housing prices of about two percent, ceteris paribus. Robustness checks strongly suggest that this is a causal effect of the changing information environment. To 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. We also analyze price quotes, set prior to publication, and find that the publication effect is not spuriously picking up unobserved housing attributes.

While the impact of publication of adjusted school results on housing prices is very robust, the effect does not seem to be long lasting. There is no trace of the initial response after 12 weeks. Our interpretation is that agents in the housing market, rather than being proper Bayesian decision makers, tend to focus on salient housing characteristics when deciding their willingness to pay. Being unable to keep in mind and evaluate all relevant information, they tend to focus on school quality when this is easily visible in the information environment. While we are not able to verify whether this conjecture is indeed true in our case, it is consistent with earlier studies documenting that investors may react more to news that is more salient (e.g. Klibanoff, Lamont, and Wizman, 1998; Huberman and Regev, 2001).

The structure of the paper is as follows: In Section 2 and Section 3 we describe the institutional setting and data, respectively. Our empirical strategy is laid out in Section 4, and in Section 5 we present the main results. Additional evidence and sensitivity analyses are presented in Section 6. Section 7 concludes.

2. Institutional Setting

Compulsory schooling in Norway consists of primary education (grades 1–7) and lower secondary education (grades 8–10). Most students (98%) attend public schools operated at the local government level. Private schools are either religious schools or schools that use alternative pedagogical principles. In addition to compulsory schooling, local governments in Norway are also responsible for child care, care for the elderly, and some other services, such as infrastructure. Local governments are financed to a large extent through block grants and regulated income tax sharing.

All students are allocated to public primary schools by catchment areas within local governments, and there is very 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. 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.[1]

Some schools are combined primary and lower secondary schools, thus there is an obvious correspondence between catchment areas (which relates 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.

In the current analysis we utilize data from the largest local government (by population size) in Norway, Oslo. Oslo has 550,000 inhabitants and is the capital of Norway. The size of Oslo is 453 square kilometers, and can reasonably be thought of as one housing region and one labor market. People living in Oslo can easily commute to every workplace location within Oslo.

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. There are a limited number of private schools, attended by less than 2% of all students, which are not included in our analysis.

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

3. Data

3.1 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. Traditionally, variation in test scores (or other performance measures) across schools has not been publicly available in Norway. In January 2003, grade point averages were published for the first time. Because student performance is determined to a large extent by individual background characteristics, this measure is likely to be an imperfect measure of school quality as such.

On November 18, 2005 a new kind of school quality indicator was published for the first time. 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.[2] This measure of school quality received considerable media attention. Even though the general public was unlikely to fully understand how the school quality indicators were computed, the media presented the new indicators as 'true school quality'.

In the empirical analysis we investigate the housing market response to the 2005 publication of adjusted school quality, which we denote Q_j^A (for school *j*). We also control for the previously available unadjusted school quality indicator (published in 2003), denoted Q_j^U. Both Q_j^U and Q_j^A are standardized by the (national student level) standard deviation of the outcome corresponding to Q_j^U, to simplify interpretation. Thus the scale is comparable in terms of grade points. The scale of Q_j^A will be more compressed, reflecting the fact that variability between schools is greatly reduced when controlling for student composition. More important to note are the considerable differences between schools when focusing on school contribution to student performance. For the schools in our sample, Q_j^U and Q_j^A have means of 0.10 and 0.20, and standard deviations of 0.25 and 0.095, respectively. The national average for both Q_j^U and Q_j^A is zero. Appendix Table A1 provides unadjusted and adjusted school per-

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[2] 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, Kirkebøen, Raaum, and Salvanes (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.

formance at the school level. Appendix Table A2 displays general summary statistics for our sample.

The correlation between adjusted and unadjusted school quality indicators is fairly low. The coefficient of correlation at the school level is only 0.29 between the adjusted indicator published in 2005 and the unadjusted indicator published in 2003. There is a high degree of persistence over time in the unadjusted quality indicator; the correlation coefficient between results published in 2003 and 2005 is 0.86.

3.2 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. All sales records are resale of apartments. Each record contains information on size (square meters), construction year, 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. Out of the total number of transactions we are able to associate 66,063 with a unique lower secondary school where we have school performance indicators.[3] Furthermore, excluding observations with missing data, or observations with dates that seem unreasonable, we are left with 38,562 observations, yielding an average of about 185 observations per week.[4] This average masks an increase from 138 in 2003 to 182 in 2004, 201 in 2005 and 221 in 2006. Details of the attrition are given in Appendix Table A3. The main source of loss of observations is missing variables, especially date of sale and price quote. If this attrition is systematic, it may bias our results. We have no reason to believe this to be the case.

Figures A1 and A2 (Appendix) illustrate the week by week variation in the number of transactions, the date of publication (November 18, 2005) being day one of week zero, indicated with a vertical line. There are large seasonal variations in the number of transactions. In particular, the number of transactions is extremely low around Christmas and New Year and also lower in summer and around Easter. Looking at the 13 weeks immediately preceding and fol-

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[3] 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 pupils between subsequent years.

[4] Specifically, we exclude erroneous data where the date of the price quote is reported to be after the date of sale. We also exclude 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% or above 135% of the actual price. Excluding these observations has a limited impact on the results.

lowing the publication, we see that the number of transactions is fairly stable at just over 200 transactions per week in all weeks before and the week including the publication date. The number then drops off, reaching a minimum around Christmas, and quickly recovers after New Year (the rightmost vertical line in Figure A2).

Table 1 presents descriptive statistics for key variables, each at the school catchment area level. The average apartment in our sample is 66 square meters, built in 1949 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. The average price per square meter range from NOK 16,400 to NOK 35,900.

4. Empirical Strategy

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, one will obtain upward biased estimates of the value of schooling if one does not control for neighborhood characteristics.[5] Several different identification strategies have been applied in the literature, e.g. Black (1999), Kane, Riegg, and Staiger (2006), Bayer, Ferreira, and McMillan (2007), and Fack and Grenet (2007) apply a ‘boundary fixed effects approach’, Gibbons and Machin (2003) rely on instrumental variables based on historically determined school characteristics, Brasington and Haurin (2006) apply spatial econometric techniques and Figlio and Lucas (2004) and Kane, Staiger, and Samms (2003) investigate whether housing markets respond to publication of school performance ratings. Clapp, Nanda, and Ross (2008) study the capitalization mechanism using panel data and also provide an updated review of the extensive literature. Most studies find that school proficiency tests are capitalized into housing prices, but weaker (or no) effects are often reported for value added measures.

To our knowledge this paper is the first to investigate the impact of the quality of compulsory schooling on housing prices in Norway. However, Machin and Salvanes (2007) exploit a

[5] Black (1999) and Bayer, Ferreira, and McMillan (2007) demonstrate this effect. Boundary fixed effects substantially reduces the coefficient on school quality in hedonic price regressions. Bayer, Ferreira, and McMillan (2007) also find that

change of admission policy to identify the relationship between housing prices and school performance on upper secondary education (11th to 13th grade) in Oslo. Their findings indicate that households are willing to pay more to live in catchment areas of better performing schools. Machin and Salvanes use raw grade point averages as indicators of school quality.

The identification strategy proposed here is similar to the fixed effects approach applied by Figlio and Lucas (2004). They evaluate whether the publication of 'school report cards' contributed to changes in housing values in Florida. The 'school report cards' make up the key part of an accountability system rating the school from 'A' through 'F', based on different student test scores. The school report cards are to some extent contingent on student characteristics, because to obtain the best 'grades' one needs to have fairly good results, not only on average, but also across different subgroups of students. Figlio and Lucas 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, Staiger, and Samms (2003) fail to find any effect of publication of test scores on housing prices using data from Mecklenburg County, North Carolina. Pope (2008) applies a similar strategy to investigate how a change in US "seller disclosure" laws impact the implicit price for airport noise in North Carolina and finds a substantial response.

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 semilog model:[6]

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

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. Neighborhood characteristics that do not change over time are soaked up by the school catchment fixed effects, $\boldsymbol{\alpha} = \{\alpha_j\}_{j=1}^J$. $\boldsymbol{\beta} = \{\beta_w\}_{w=1}^W$ is a set of week specific

subsequent inclusion of precise neighborhood control variables reduces the estimate further, by as much as approximately 50%.

6 Cropper, Deck, and McConnell (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.

fixed effects, capturing general variation in apartment prices over time. In addition, we include the vector $\mathbf{x}_{ij}$ which includes apartment size and construction year dummies. $\boldsymbol{\eta}$, $\boldsymbol{\alpha}$, $\boldsymbol{\beta}$, and δ is a set of 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.[7]

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 adjusted and unadjusted school quality indicators. These indicators are in turn functions of some underlying school quality, assumed to be time invariant,[8] but the perceived school quality may depend on the available information. We expect changes in households' information set to lead to a reweighting 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:

$$\text{(2)} \qquad \log P_{ijtw} = \tilde{\alpha}_j + \beta_w + \delta(aD_tQ^A_j + bD_tQ^U_j) + \mathbf{x}_{ij}\boldsymbol{\eta} + \varepsilon_{ijtw} \, .$$

In Eq. (2) the effect on housing prices of the publication of the indicators will be captured by the interaction terms $D_tQ^A_j$ and $D_tQ^A_j$. 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, $\tilde{\alpha}_j$. 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. We cannot identify δ without making assumptions about the 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 unad-

[7] This is done using the cluster option in Stata.

[8] 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.

justed quality, before and after publication of the former. We believe some assumptions with testable implications are justified.

Applying the hedonic price approach we cannot distinguish between general household valuation and parental valuation of school quality. 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 to be strong. Rothstein (2006) analyzes parental valuation of school characteristics in the US and finds that school effectiveness is not a primary determinant of parental choice. He speculates that the explanation for this result is that: (i) schools' contribution to student achievement is modest; (ii) parents prefer other neighborhood or school characteristics to effectiveness, e.g. peer effects or (iii) parents cannot distinguish effective from ineffective schools.

However, no matter what are the parents' preferences, if the indicators do not provide any new information, weights should not change ($a = 0$, $b = 0$). In this case school quality is captured by the model's catchment area fixed effects and we expect to find a coefficient estimate on $D_t Q_j^A$ not significantly different from zero. We also expect this coefficient estimate to be insignificant if parents do not value school quality ($\delta = 0$). 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.

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—either because the information is already partly known (and thus captured by the fixed effects) or because the indicators are not fully accepted as valid measures of school quality—our estimate of δa will be a downward-biased estimate of household valuation of school quality, δ.[9]

The opposite argument can be made for unadjusted school quality: this information was already public prior to the 2005 publication, and was then supplemented, or possibly supplanted, by an additional measure. Thus, its weight should have stayed the same or gone down, implying that $b \leq 0$. If the households do indeed have any preference for school quality, the weights will stay the same only if the indicators provided no new information, otherwise at least some emphasis should 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 3. Because unobservable variables affecting house prices are likely to be systematically correlated with the school performance indicators, we evaluate changes in housing prices tightly bounded around the publication date in order to remove all effects of variables related to each school area that do not vary over time. In the empirical analysis we use the exact date of sale (agreement) and experiment with different time windows around the date of publication, ranging from 2 to 104 weeks (± 1 week to ± 52 weeks).

Existing studies on publication of school quality indicators rely on monthly data and omit months close to publication. Kane, Staiger, and Samms (2003) match various indicators released in July or August to transactions in the subsequent September through August period. Similarly, Figlio and Lucas (2004) match information released in May or June to transactions in July to April in the subsequent year. This approach may be problematic for two reasons. First, because longer time windows are more subject to unobserved omitted variables or housing market trends that may bias the estimates. Second, because important short-term responses in the housing market may be neglected.

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[9] 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.

5. Results

5.1. Price response

Table 1 displays the results from regressions of log price on different sets of explanatory variables, based on transaction data from 2005. We notice a positive and declining relationship between the size of the item 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 nonmonotonic, 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% 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	0.010	0.010	0.010	0.010	0.010
		[29.61]**	[34.65]**	[29.47]**	[28.47]**	[27.25]**	[26.96]**
Year of construction:							
Before 1900			–0.069	–0.092	–0.107	–0.114	–0.177
			[2.09]*	[3.15]**	[3.27]**	[3.85]**	[8.78]**
Between 1900 and 1919			–0.045	–0.107	–0.080	–0.123	–0.178
			[1.51]	[3.57]**	[2.54]*	[4.08]**	[7.20]**
Between 1920 and 1939			–0.113	–0.155	–0.135	–0.165	–0.209
			[3.54]**	[5.55]**	[4.39]**	[5.90]**	[14.04]**
Between 1940 and 1959			–0.281	–0.297	–0.256	–0.280	–0.226
			[9.20]**	[10.65]**	[9.71]**	[9.29]**	[12.08]**
Between 1960 and 1979			–0.428	–0.423	–0.406	–0.409	–0.243
			[12.02]**	[11.53]**	[10.95]**	[10.28]**	[13.30]**
Between 1980 and 1989			–0.275	–0.276	–0.238	–0.252	–0.164
			[3.53]**	[4.28]**	[3.40]**	[4.09]**	[8.07]**
Between 1990 and 1999			–0.13	–0.129	–0.109	–0.116	–0.093
			[2.79]**	[3.07]**	[2.59]*	[2.94]**	[3.94]**
Q_j^U				0.418		0.378	
				[4.68]**		[3.96]**	
Q_j^A					0.734	0.469	
					[3.05]**	[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%; ** significant at, or below, 1%.

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.[10] 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.

Table 2 presents results based on our empirical strategy laid out in Section 4. We apply eight different time windows, ranging from ± 1 week to ± 52 weeks. Our inference is based on within catchment area changes in housing prices. If publication of school performance indica-

[10] 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 0.25 and 0.095 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.

tors provides new information to households, and households value this information, then housing prices should respond accordingly.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Time window	± 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	0.143	0.179	0.132	0.167	0.139	0.068	0.027
	[1.43]	[1.39]	[2.29]*	[1.73]	[3.05]**	[2.64]*	[1.82]	[0.81]
$D_t Q_j^U$	0.084	0.021	0.002	–0.019	–0.020	–0.024	–0.021	–0.028
	[1.70]	[0.60]	[0.07]	[0.85]	[1.00]	[1.12]	[1.67]	[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 time windows. 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%; ** significant at, or below, 1%.

$D_t Q_j^A$ is found to be statistically significant in regressions based on 6, 16, and 26 weeks windows, consistent with Hypothesis 1. We fail, however, to obtain a statistically significant effect for the narrowest and widest time windows. In every time window, however, we find a positive effect and the estimated magnitude is not very different for the narrow to middle time windows. These 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 5250). This estimate is similar, but somewhat smaller, than earlier papers on the capitalization of school quality into housing prices report (such as Black (1999) and Figlio and Lucas (2004) for the US, Gibbons and Machin (2003) for the UK and Fack and Grenet (2007) for France).[11] Not only does our analysis suggest that households care about school quality, but they seem to respond quickly to this information, which must have been new to them. For the wider time windows, the estimated coefficients are much smaller and not statistically significant at conventional levels, indicating that there may not be a long-term effect.

[11] Bayer, Ferreira, and McMillan (2007) find a smaller response than these studies.

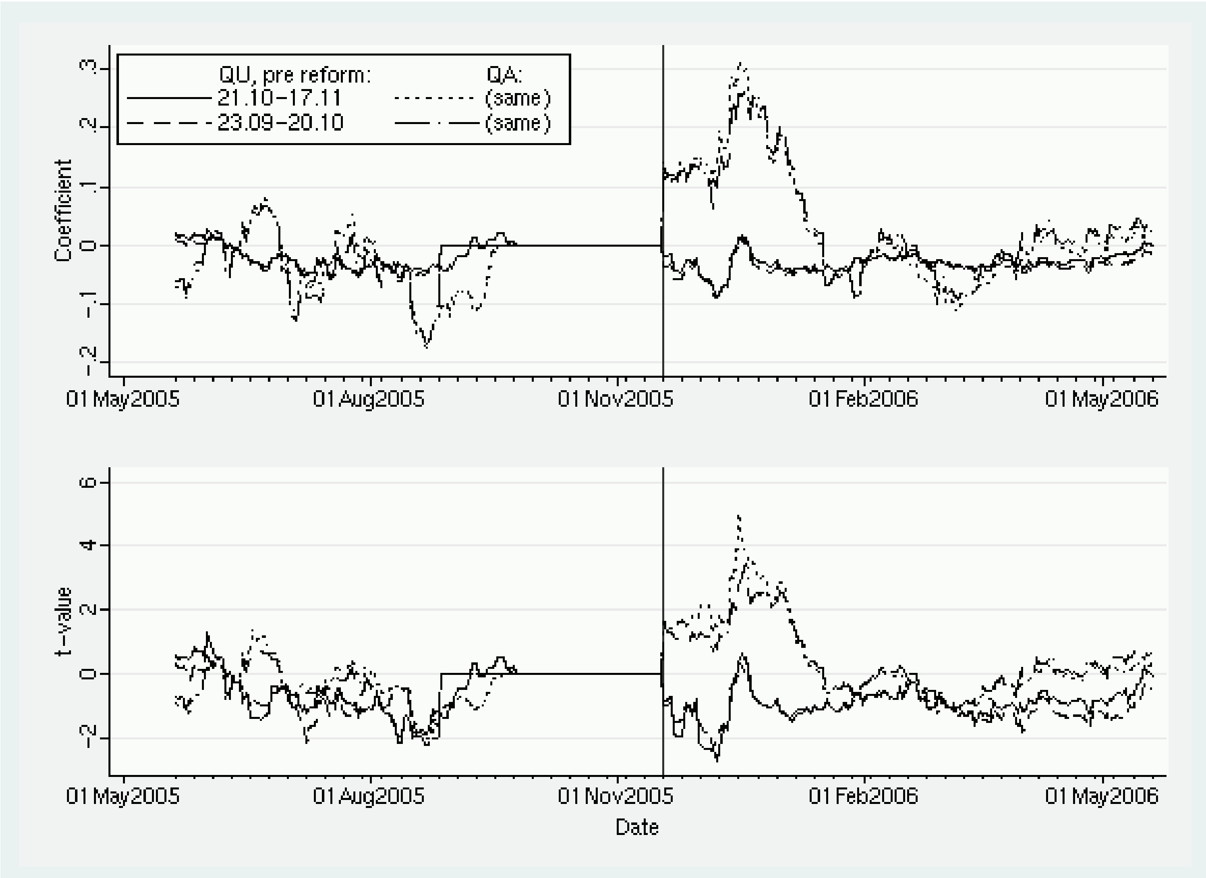
We do not find strong support in favor of Hypothesis 2. We fail to reject the null hypothesis of no impact of $D_t Q_j^U$ for dates closely bracketed around publication. We do, however, find some traces of a statistically significant negative long-term effect, but we are cautious in interpreting this as a causal effect because longer time windows may be more vulnerable to omitted variables and general housing market trends.

5.2 Persistence

In Figure 1, 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 prepublication periods. Looking at the evolution of the postpublication results, there seems to be a short-term effect lasting up to around two months, after which the effect disappears. 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. 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. This illustrates that omitting transactions near the time of publication, as Figlio and Lucas (2004) and Kane Staiger and Samms (2003) do, a substantial short-term response in housing prices may be missed.[12]

Comparing the post publication transactions with alternative transactions preceding publication, e.g. 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 prepublication period used to make the comparisons. Furthermore, looking at the leftmost part of the figures, comparing the prepublication reference periods to other prepublications intervals, we estimate no strong or systematic effects, supporting the above conclusion. Figure 1 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 prepublication period), and the estimated coefficients for almost the entire six months after publication are below zero.

Figure 1: Persistence of 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. The values for each date are based on regressions using postpublication data from the indicated date and four weeks onwards, comparing this with one of two different prepublication periods, as indicated in the legend. Effects are also estimated at times before the actual publication (indicated with a vertical line). For postpublication time windows that overlap with the pre-publication periods, or that do contain dates both before and after publication of the indicators, coefficients and t-values are set equal to zero.

One interpretation of the price reversion observed relates to the cost of information acquisition. 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 latter may find it too costly in terms of time and effort to acquire this information through the Oslo municipality web site. However, given our estimates of school quality valuation and reasonable assumptions about time required to find the published school quality indicators, we find this interpretation incomplete. To complement it, we believe insights from the behavioral finance

[12] Applying a time window similar to Figlio and Lucas (2004), 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 3. These indicate no impact of adjusted quality, but some decapitalization of the previously published unadjusted quality.

literature are relevant. This literature has pointed to several human information-processing biases that may explain deviations from full rationality. Because we cannot distinguish between different explanations, we can only speculate about biases that may generate the price reversal. One appealing explanation is the idea of limited attention, where cognitively overloaded agents pay attention to only a subset of publicly available information (Hong and Stein, 2007).[13] If this is a realistic description of how agents make decisions, it is likely to give rise to overweighting of information signals that are more salient at the expense of information that blends in with the background. In our setting, agents may exhibit a tendency to focus on apartment attributes that are more salient. After November 18, 2005, the school quality related to each neighborhood becomes easily accessible. However, as the media turns its attention elsewhere, the housing market agents may respond by putting less weight on school quality, giving rise to the price reversion we observe.

6. Robustness

As noted above, we do find publication effects of reasonably consistent magnitudes for a range of different time windows. Furthermore, as is evident from our analysis of the persistence of the effect, our results do not hinge on the specific pre- or postpublication time window used.

Nonetheless, we perform three more robustness checks: (i) we estimate the model excluding outlying observations, (ii) we estimate similar models for a wide range of (nonpublication) dates, to investigate whether our results reflect secular trends or omitted variables, and (iii) we study price quotes to control for unobserved apartment attributes.

6.1. Outliers removed

We rerun our main regressions, excluding outliers with a very low or very high square meter price. Specifically, we exclude observations below the 1st and above the 99th percentile within each school catchment area for each year. As Table 3 shows, this does not have a large impact on our estimated coefficients, or on the level of statistical significance. Actually, we notice that our main coefficient of interest, the one on adjusted school quality, increases in magnitude and becomes more significant in most of the short- or middle-width time windows. We

[13] Several empirical studies find that salient news tend to carry greater weight in market prices. See Daniel, Hirshleifer, and

have repeated this analysis also for other definitions of outliers, without our results changing much.

Table 3: Housing market reaction to publication of school quality indicators. Regressions where outliers are removed from the data set. The dependent variable is log price

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Time window	± 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
$D_t Q_j^A$	0.194	0.146	0.192	0.144	0.166	0.126	0.067	0.033
	[1.62]	[1.49]	[2.45]*	[1.90]	[3.30]**	[2.66]*	[1.79]	[0.96]
$D_t Q_j^U$	0.098	0.028	–0.001	–0.018	–0.020	–0.014	–0.017	–0.023
	[2.01]	[0.80]	[0.02]	[0.77]	[1.06]	[0.80]	[1.31]	[2.03]*
# obs.	477	897	1260	1605	2755	5168	10763	21387
R^2	0.90	0.88	0.86	0.87	0.87	0.86	0.87	0.87
# obs. pre publ.	223	452	681	903	1813	3024	5570	10239
# obs. post publ.	254	445	579	702	942	2144	5193	11148

Notes: Columns 1 through 8 report coefficients of OLS regressions for different time windows. 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%; ** significant at, or below, 1%.

6.2. Placebo publication effects

For all time periods of specific lengths that contain no dates earlier that January 1, 2003 and no postpublication dates, we estimate artificial publication effects, and compare these with the estimated effect around November 18, 2005. If we found similar types of results in regressions based on artificial publications, it would suggest that omitted (local) trends, other omitted variables or wrong functional form led to the results presented above.

Table 4 reports the t-values from the regression centered on November 18, 2005 and the share of estimated t-values 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 windows we find well below one percent of the placebo t-values to be larger than the actual t-value at November 18, 2005.[14]

Teoh (2002) for a review of this literature.

[14] We have also looked in detail at placebo regressions for November 18 in other years (2003, 2004, and 2006). We never find statistically significant effects for time windows bracketed around November 18 for these years. The exception is the longest time window in 2003 (± 52 weeks). Results are not reported, but available upon request.

Table 4: Estimated "placebo publication effects" relative to the actual publication effect. The dependent variable is log price

Time window (weeks)	Coeff., Nov 18, 2005	t-value, Nov 18, 2005	$\lvert t_{placebo}\rvert \geq t_{18Nov2005}$ (percent)	$t_{placebo} \geq t_{18Nov2005}$ (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.

6.3. Price quote

Despite the apparent robustness demonstrated above, one may still worry about sample selection, i.e. systematic (unobserved) differences among apartments sold before and after November 18, 2005, which may be correlated with the published school quality indicators. To further investigate whether such an argument may bias our results we restrict our analysis to apartments with price quotes set before November 18 and investigate whether those sold at or after November 18 were sold at a higher price relative to the price quote than apartments sold before November 18.

Most apartments are sold shortly after the price quote is set. In the first week after November 18, 2005, 97% of the apartments had a price quote set before November 18. However this number drops to 40% in week 2, and 20% in week 3 and week 4. Consequently, the narrow time windows are of most interest in Table 5. As can be seen from the number of observations, extending the time window 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 time windows we do find a 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 the price quote, than apartments in the catchment areas of lower-scoring schools, reinforcing our main results.[15]

[15] 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.

There may 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 postpublication. 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 5, 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.[16]

Table 5: 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)
Time window	± 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	0.123	0.099	0.114	0.113	0.088	0.097	0.090
	[3.40]**	[2.05]*	[2.05]*	[2.74]**	[3.16]**	[2.88]**	[3.23]**	[2.85]**
$D_t Q_j^U$	–0.012	–0.006	–0.014	–0.014	–0.02	–0.027	–0.031	–0.029
	[0.55]	[0.25]	[0.67]	[0.77]	[1.28]	[1.79]	[2.34]*	[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 time windows. 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%; ** significant at, or below, 1%.

As a final robustness check, we also run placebo regressions for this specification, reported in Table 6. Once again, the publication effect stands out compared with placebo publication effects. For eight-week time windows and longer we never find as high t-values in placebo re-

[16] These results are not reported, but are available upon request.

gressions as we do at the actual publication date. Furthermore for the most narrow time window, 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.

Table 6: Estimated "placebo publication effects" relative to the actual publication effect. The dependent variable is log of sale price relative to price quote

Time window (weeks)	Coeff., Nov 18, 2005	t-value, Nov 18, 2005	$\lvert t_{placebo}\rvert \geq t_{18Nov2005}$ (percent)	$t_{placebo} \geq t_{18Nov2005}$ (percent)
± 1	0.203	3.40	3.27	0.38
± 2	0.123	2.05	15.61	1.56
± 3	0.099	2.05	13.35	0.89
± 4	0.114	2.74	5.72	0.00
± 6	0.104	2.75	4.44	0.00
± 8	0.113	3.16	1.81	0.00
± 10	0.092	2.92	3.83	0.00
± 13	0.088	2.88	4.71	0.00

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. Regressions are based only on observations with price quotes set before placebo publication date. All estimations control for size, year of construction (dummies), week fixed effects, and catchment area fixed effects.

From the above analysis, it is not obvious whether home buyers, home sellers, or both react to new information on school quality. By studying price quotes set shortly before and after publication, we may get further information on which mechanisms are driving the price response. In Table 7 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. Given the limited samples in the narrowest time intervals, we do not get precise results, making strong conclusions difficult. However, the fact that we did find a significant first week effect comparing prices with price quotes of sold apartments and no effect for price quotes indicate that home buyers are primarily driving the housing market response.

Table 7: 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)
Time window	± 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
$D_t Q_j^A$	–0.004	0.080	0.124	0.074	0.099	0.116	0.068	0.040
	[0.03]	[0.70]	[1.59]	[1.03]	[1.72]	[2.50]*	[2.26]*	[1.47]
$D_t Q_j^U$	0.105	0.038	0.019	–0.003	–0.007	–0.01	0.004	–0.007
	[1.98]	[1.03]	[0.60]	[0.16]	[0.38]	[0.58]	[0.30]	[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 time windows. 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%; ** significant at, or below, 1%.

7. Concluding Remarks

This paper applies a clean identification strategy to investigate household valuation of school quality. We find a strong short-term response to first time publication of school quality indicators, suggesting that households value better-scoring schools. As only parents (or potential parents) are expected to have a fundamental preference for school quality, we find it reasonable to assume that parental valuation of school quality is the main driving force behind the housing market reaction. However, other households expecting parental valuation to change may also adjust their willingness to pay. In future research we aim to further investigate different households' valuation of school quality by analyzing changes in migration flows around the time of publication.

The impact of the change in the information environment is robust to a number of sensitivity checks, but the publication effect does not seem to be permanent. We point to heuristic biases known from behavioral finance as one plausible explanation. The housing market seems to respond quickly to salient information, and then reverts to prepublication valuations dominated by other characteristics as the school quality becomes less salient.

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Table A1: Descriptive statistics, by school

School	# of transactions	Q_j^U	Q_j^A	Log(Price)	Price per sq. meter (1000 NOK)	Size	Year of construction
ALL	38562	0.10	0.20	14.33	28.0	66	1949
School 1	627	–0.21	0.05	13.98	16.4	76	1973
School 2	460	–0.05	0.10	14.03	17.2	77	1983
School 3	412	–0.02	0.09	14.07	17.6	78	1976
School 4	450	–0.16	0.10	14.03	17.7	75	1975
School 5	244	–0.07	0.26	14.05	17.9	74	1993
School 6	323	–0.04	0.00	14.15	18.5	77	1990
School 7	469	–0.06	0.11	14.15	18.5	80	1972
School 8	189	0.11	0.15	13.97	18.7	67	1977
School 9	266	0.10	0.31	14.21	19.0	86	1974
School 10	497	–0.34	0.12	14.09	19.5	71	1976
School 11	322	–0.22	0.07	14.15	19.7	75	1956
School 12	516	–0.29	0.11	14.00	19.9	67	1973
School 13	349	–0.07	0.13	14.10	20.3	73	1977
School 14	63	0.08	–0.01	14.19	21.1	73	1979
School 15	1361	–0.05	0.29	14.08	21.5	65	1970
School 16	802	0.16	0.15	13.94	21.6	58	1965
School 17	853	–0.17	–0.01	14.04	22.0	60	1958
School 18	845	–0.01	0.19	14.11	22.1	63	1968
School 19	360	–0.16	0.17	14.14	22.6	64	1957
School 20	1147	0.04	0.11	14.13	23.2	61	1958
School 21	522	0.14	0.22	14.21	23.6	65	1971
School 22	990	0.23	0.24	14.20	23.6	65	1963
School 23	109	0.28	0.21	14.20	24.0	63	1961
School 24	118	0.52	0.26	14.18	24.0	63	1968
School 25	737	0.30	0.22	14.24	25.9	62	1959
School 26	1562	0.08	0.17	14.24	26.2	62	1954
School 27	388	0.59	0.21	14.48	27.3	78	1967
School 28	710	0.38	0.04	14.45	27.9	72	1968
School 29	402	0.23	0.09	14.46	28.2	74	1960
School 30	453	0.42	0.03	14.76	29.0	95	1981
School 31	2672	–0.33	0.14	14.30	29.2	60	1944
School 32	1397	0.19	0.20	14.19	29.4	52	1944
School 33	486	0.48	0.09	14.41	29.5	65	1964
School 34	4242	–0.04	0.28	14.31	30.7	57	1929
School 35	3227	0.07	0.24	14.30	30.8	56	1944
School 36	596	0.56	0.25	14.36	31.8	60	1960
School 37	320	0.46	0.06	14.74	32.8	86	1974
School 38	2844	0.37	0.24	14.53	34.1	67	1923
School 39	1904	0.34	0.24	14.66	35.0	75	1928
School 40	1370	0.38	0.19	14.73	35.3	82	1931
School 41	1629	0.18	0.29	14.66	35.8	74	1941
School 42	1329	0.40	0.39	14.78	35.9	87	1920

Table A2: Summary statistics

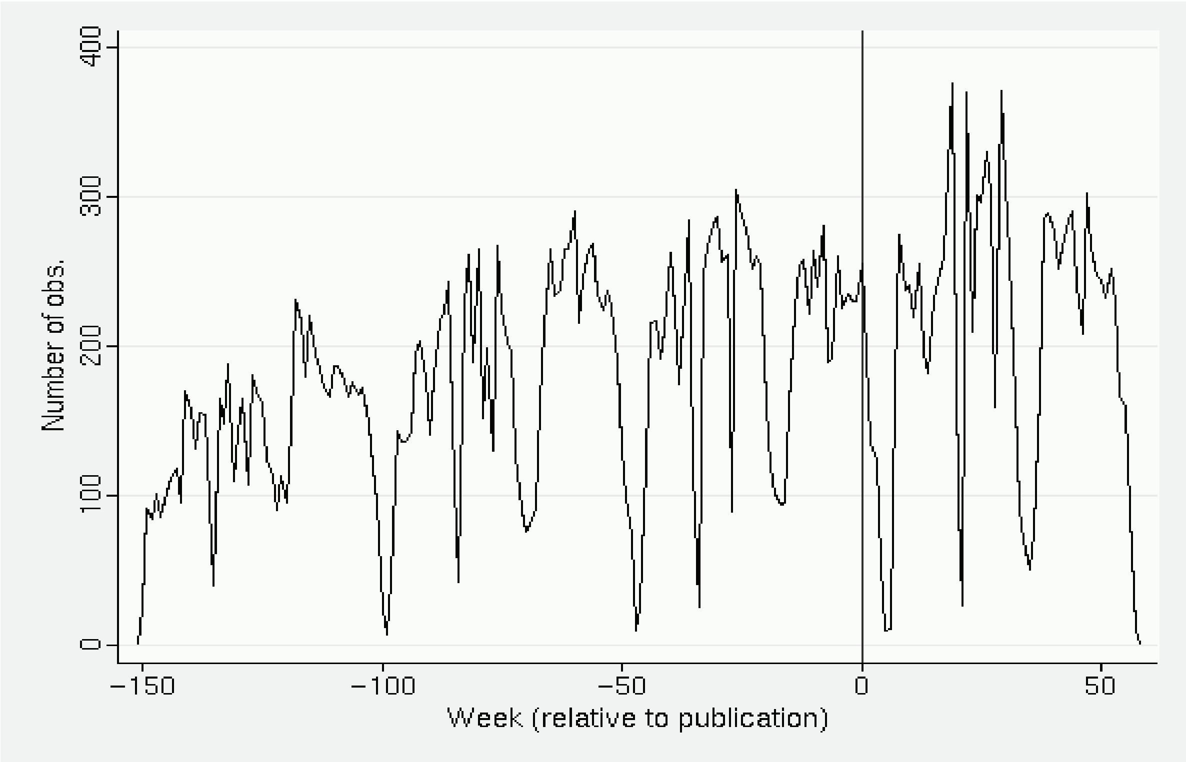
	Mean	Std. dev.
Price (1000 NOK, including financial liability)	1810	837
Price per sq. meter (1000 NOK, including financial liability)	28.04	8.64
Log(Price) (including financial liability)	14.33	0.38
Log(Price quote) (including financial liability)	14.27	0.39
Log(Price/price quote)	0.05	0.07
Q_j^U	0.10	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	

Table A3: Attrition

	Change	Observations
Original data set		79322
No valid residential address	–3190	76748
No unique secondary school	–6547	69585
Both indicators not published for school	–4750	64835
Missing one or more variables:	–25175	39660
Date of sale	*–18716*	
Price	*–4381*	
Price quote	*–14745*	
Area	*–8464*	
Year of construction	*–8252*	
High or low price, relative to price quote	–312	39348
Negative or very high time to sell	786	38562

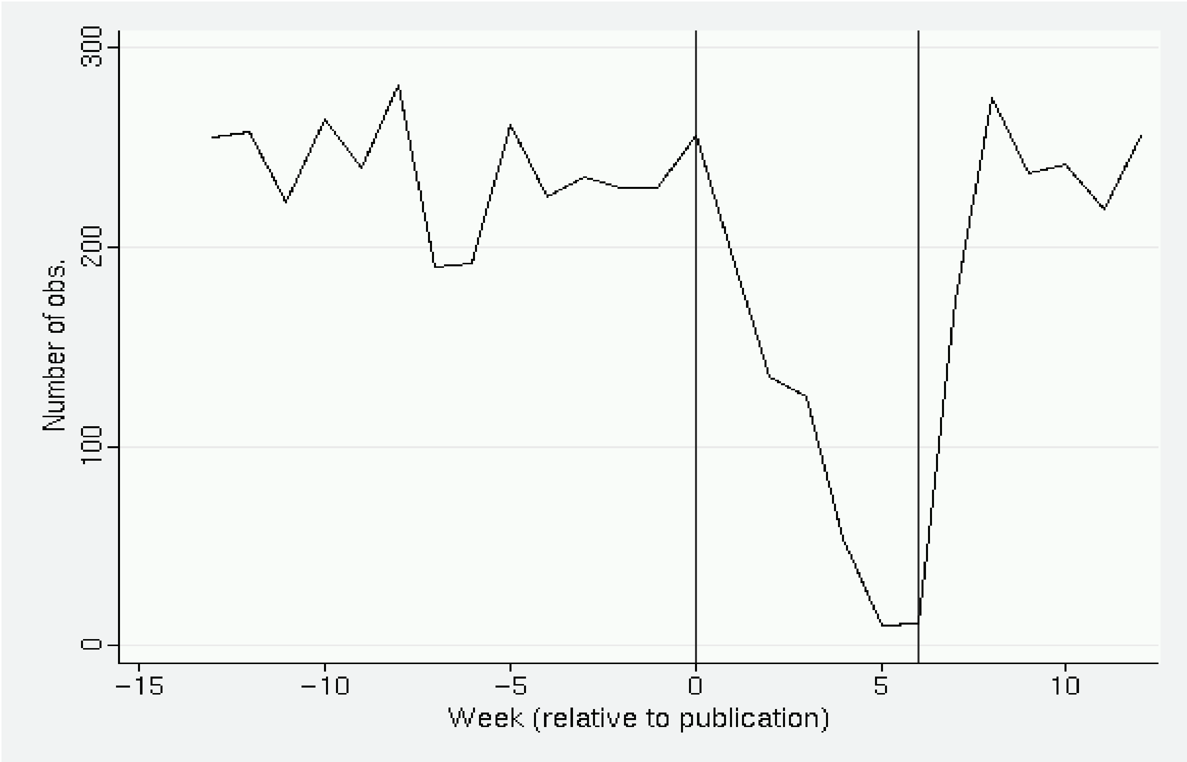
Notes: Some observations are missing several variables, thus the listing in italics is not mutually exclusive, and the number of unique transactions excluded sum to the number indicated above.

Figure A1: Number of transactions



Notes: Transactions per week. Date of publication (November 18, 2005) being day one of week zero, indicated with vertical line.

Figure A2: Number of transactions



Notes: Transactions per week. Date of publication (November 18, 2005) being day one of week zero. Leftmost vertical line denotes date of publication, rightmost denotes New Year.