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Working Paper No. 0906

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Publisher
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Bibliothek (Working Paper)
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CH-8006 Zürich
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New Flight Regimes and Exposure to Aircraft Noise: Identifying Housing Price Effects Using a Ratio-of-Ratios Approach*

Stefan Boes† Stephan Nüesch‡

April 1, 2009

Abstract

In October 2003, a new flight regime was introduced at Zurich airport that significantly changed the levels of noise pollution in surrounding communities. We investigate the impact of the new flight policy on apartment prices using a hedonic price model and a non-linear difference-in-differences identification strategy. Our results suggest that rental prices increased by about 3 to 8 percent less in regions affected by the policy change, controlling for several apartment and location characteristics. The noise discount is still significant, although smaller, even after the inclusion of object-specific fixed effects. However, we do not find evidence of price changes in the sales market.

JEL Classification: C21, Q53, R21

Keywords: Quasi-experimental data, housing market, aircraft noise, hedonic approach, non-linear difference-in-differences, policy evaluation.

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*Acknowledgments: We would like to thank Andrew Oswald, Kevin Staub, Steven Stillman, and Rainer Winkelmann for providing valuable comments. We are grateful to Homegate Corporation and the Swiss Federal Laboratories for Materials Testing and Research for providing the data.

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1 Introduction

The empirical evaluation of policy interventions is a difficult task as the counterfactual world – what would have happened without the policy – can never be observed. Nevertheless, cost-benefit analyses are increasingly necessary to inform authorities about the consequences of already implemented policies, or, an even greater challenge, to forecast the impact of new interventions. The policy intervention we address in this paper is the introduction of a new flight regime at Zurich airport, and the corresponding change in the levels of noise pollution. We focus on one aspect of the cost side, specifically the impact on housing prices, and employ a model-based approach that allows us to evaluate a historical intervention, namely the change in landing regulations in October 2003, as well as possible future interventions.

Since there is no explicit market for quiet, revealed-preference methods have often been used to estimate the economic value of quiet. The hedonic approach (Rosen 1974) uses information from the housing market to infer an implicit price function, based on the idea that utility associated with the consumption of a composite product like housing is determined by the utility associated with its constituent parts, such as structural characteristics (e.g., the number of rooms), neighborhood variables (e.g., crime), and environmental amenities like quiet. Set in a policy evaluation framework, the "classical" hedonic method would require to regress property prices on noise characteristics and other attributes of the property, and then to extrapolate the estimated noise discounts with the changes in noise exposure as implied by the new flight regime.

We follow a different identification strategy here. Instead of using the classical cross-sectional hedonic approach, we combine a theoretically founded hedonic property model with quasi-experimental methods to estimate the policy effect of interest, i.e., the impact of the new flight regime on housing prices. The choice of a quasi-experimental approach is guided by increasing concerns that simple cross-sectional hedonic methods are likely to suffer from omitted variable bias (Parmeter and Pope 2009), and that extrapolation exercises typically hinge on homogeneity assumptions that are hardly justified in practice. Quasi-experimental tests, on the other hand, have become a popular tool in the related literature, and have been successfully employed, for example, to measure the capitalization of urban property crime (Gibbons 2004; Linden and Rockoff 2008), air pollution (Chay and Greenstone 2005), rail access (Gibbons and Machin 2005), or hazardous waste and toxic releases (Gayer *et al.* 2000; Bui and Mayer 2003; Greenstone and Gallagher 2008).

The theoretical model underlying our analysis assumes that buyers and sellers (lessors and lessees) interact in a competitive market under full information. The equilibrium is characterized by tangential bid and offer curves, implying a generally non-linear hedonic price function under heterogenous preferences. For the empirical model we impose a flexible exponential function that accommodates the quasi-experimental setup using a before/after and treatment/control structure. This yields a non-linear difference-in-differences problem. More specifically, the relative time effect in the treatment group is adjusted by the relative time effect in the control group to remove bias associated with a common time trend unrelated to the intervention. Due to the relatively simple interpretation of the estimand, we refer to this as "ratio-of-ratios" approach.

The validity of quasi-experimental results, however, largely depends on whether the selection of treatment is random (see Greenstone and Gayer, 2007, for arguments related to environmental economics). As we do not have a randomized field experiment, it is precarious to assume that apartments afflicted by additional aircraft noise have the same characteristics as apartments in the control group. In order to reduce the potential selection bias, we proceed sequentially. First, we apply a "selection on observables" approach and control for a number of observable and potentially confounding factors, such as the number of rooms, the year of construction, and other apartment and neighborhood attributes. "Selection on observables" assumes exogenous treatment status after conditioning on the vector of covariates.

In a second step, we try to tackle the "selection on unobservables" problem. Formal tests for the presence of omitted variable bias are of course impossible. Altonji *et al.* (2005), however, suggested using the degree of selection on observables as a guide to evaluate the degree of selection on unobservables. Simple summary statistics reveal that in our case, apartments in the treatment group not only tend to be significantly more expensive and larger than apartments in the control group, but also differ in various location characteristics. It is therefore unlikely that treatment assignment is orthogonal to unobserved factors either. In order to solve this problem we make use of panel information for a subsample of the apartments in our dataset and include object-specific fixed effects. In doing so, we are able to eliminate the influence of unobserved but time-invariant apartment heterogeneity.

To the best of our knowledge, we are the first to study noise effects on housing prices using a quasi-experimental identification strategy and object-specific fixed effects. Two papers are somewhat related to ours. Jud and Winkler (2006) employ an event study methodology to analyze the announcement effect of an airport expansion on housing prices. Pope (2008) uses the introduction of a mandatory airport noise disclosure in the residential housing market as a quasi-random experiment. Neither of these two studies controls for unobserved housing attributes. Apart from the two quasi-experimental approaches, there exist numerous cross-sectional studies that analyze the relationship between noise and property prices. An overview is provided by the meta-analyses of Nelson (1980, 2004) and Schipper *et al.* (1998).

This paper proceeds as follows. In the next section, we describe the institutional framework and provide a chronological order of events related to the introduction of the new flight regime at Zurich airport. In Section 3, we identify communities that were and were not affected by the new flight regime. Section 4 gives the technical details. Section 5 describes the data and provides initial results. Section 6 discusses the estimates of the policy effect of interest, and Section 7 concludes.

2 The New Flight Regime at Zurich Airport

Zurich airport is the largest international flight gateway in Switzerland. It operates about 260,000 take-offs and landings per year on three different runways. Figure 1 provides a map overview of the airport. The relative frequencies indicate the distribution of incoming and outgoing aircrafts by flight direction in 2008.

— Insert Figure 1 about here —

Until 2002 over 90 percent of all aircrafts were approaching form the north, more precisely from the northwest on runway 14. Since Zurich airport is located relatively close to the German border, incoming aircrafts fly with an altitude of less than 4000 feet over German communities. In April 2003 the German government released a binding decree that prohibited landings from the north in the early morning and the late evening. The flight ban over German territory covers the times between 6 and 7am and between 9 and 12pm on weekdays, and between 6 and 9am and between 8 and 12pm on weekends. As a result, landings in these time periods had to be redirected to runway 28 (east) as the flight regulations at that time did not allow any other direction.

On May 21, 2003 the Federal Office of Civil Aviation changed the regulations such that after October 30, 2003 landings were also permitted from the south on runway 34. The new landing policy at Zurich airport stated that aircrafts landing between 6 and 7am on weekdays (6 and 9am on weekends) should generally approach from the south, and aircrafts landing between 9 and 12pm on weekdays (8 and 12pm on weekends) should approach from the east. Exceptions to this general rule are only allowed in special weather conditions, namely strong wind or fog and mist, or in the case of emergency flights (Unique 2005, 2007).

The next two figures illustrate the monthly number of landings by flight path and time of the day. Figure 2 shows the monthly averages on the basis of airport operation time, i.e., from 6am to 12pm. We observe a downward trend in the number of landings from the north, with largest drop in 2003, and a significant increase in the number of landings from the east and the south. The monthly landings from the east reached a peak level in summer 2003. The temporary increase in the fall of 2007 was due to the restoration of runway 16/34, and the related redistribution of flights to runway 28 (east). Landings from the south started in October 2003, after the new flight regulation took effect.

— Insert Figures 2 and 3 about here —

Figure 3 shows the monthly landings for the early morning hours (6 to 7am) and for the late evening (9 to 12pm). Before 2003, landings in the early morning were operated from the north, in 2003 mainly from the east, and thereafter from the south. Figure 3 shows a significant decrease in the number of landings from the north in both times, in the early morning and in the late evening. The temporary increase observed in October 2005 is due to the test phase of a new flight path from the northwest over Swiss territory. Because this new landing procedure had to be carried out by a visual approach instead of using the otherwise prevailing instrument landing system, the change in flight regulations was denied for safety reasons by the Federal Office for Civil Aviation (FOCA 2008).

Another observation in Figure 3 relates to seasonal effects and associated weather conditions. According to the new flight regime and the corresponding safety regulations incoming aircrafts are directed to approach from the south when visibility is less than 4300 meters but more than 750 meters. In the case of visibility of less than 750 meters, aircrafts approach from the north. This explains the temporary drop of landings form the east during the wintertime, when the weather in the Zurich region is often very foggy.

Due to the increased number of aircrafts landing from the east and south in the early morning and late evening, the new flight regime also required a moderate redistribution of outgoing flights (Unique 2008). The number of departures from runway 16 (towards the south) dropped, whereas the number of outgoing flights in the northward direction increased. Since aircrafts taking off ascend very steeply, they do not fall under the flight ban over Germany in the early morning and late evening.

3 Which Regions Were Affected?

In order to evaluate the location-specific exposure to aircraft noise, we use model-based noise data provided by the Swiss Federal Laboratories for Materials Testing and Research (EMPA). The EMPA model uses effective radar flight track information together with aircraft noise profiles, as well as environmental characteristics such as terrain or prevalent winds, to predict disaggregated noise intensities around Zurich airport. Unlike many other studies that only have access to specific noise contours (e.g., McMillen 2004), EMPA offers longitudinal data on noise exposure on a 100m-by-100m square lattice grid.

Average noise emissions are measured by the L_{eq} metric. L_{eq} is an equivalence metric corresponding to a steady sound level for a given time interval that would produce the same energy as the actual time-varying sound level. Environmental studies commonly proxy noise exposure with the L_{eq} metric, although its use is not uncontroversial (Jones 1997). The available measures differ by the length of the time interval and the time of the day. The first measure, $L_{eq}^{d}(16)$, is the average daytime noise exposure from 6am to 10pm. The second and third, $L_{eq}^{n1}(1)$ and $L_{eq}^{n2}(1)$, measure the average nighttime noise exposure from 10pm to 11pm and from 11pm to 12pm, respectively. Unfortunately, a measure of noise exposure in the early morning hour is not available. The units of measurement in each case are A-weighted decibels, abbreviated by dB(A), and we use the yearly average noise exposure aggregated on the municipality level. For more details about the EMPA model we refer to Pietrzko and Buetikofer (2002).

The change in the landing procedure significantly altered the exposure to aircraft noise around the airport. Regions in the south and the east experienced an increase in aircraft noise, on average, whereas municipalities in the north generally experienced a decrease. Figures 4 to 6 provide a graphical illustration of local noise exposure and show the changes from 2002 to 2004. The graphs differ by the time frame captured, but each shows noise data averaged over the year and aggregated on the municipality level.

— Insert Figures 4, 5, and 6 about here —

Consider Figure 4 first. The upper graph shows the daytime 16-hour equivalent steady noise level from 6am to 10pm, $L_{eq}^{d}(16)$, for the year 2004, i.e., for the year immediately following the introduction of the new flight regime. Zurich airport is indicated by the black dot in the center of the map. The dark regions correspond to the highest levels of exposure to aircraft noise, the white regions to the lowest. As expected, we observe the highest levels of exposure in the municipalities directly surrounding the airport and in the direction of the three runways – consistent with the flight paths shown in Figure 1.

The lower graph in Figure 4 shows the changes in $L_{eq}^{d}(16)$ from 2002 to 2004. The dark shaded regions experienced an increase of more than 2 dB(A) on average, the light grey shaded municipalities experienced changes between -2 and +2 dB(A), and the white shaded municipalities experienced an average decrease of more than 2 dB(A). From this categorization, we define regions with an increase/decrease of more than 2 dB(A) as regions that were affected by the new flight regime. The average increase in noise in municipalities with a change of more than 2 dB(A) is about 5.2 dB(A) (max: 11.4 dB(A)), and the average decrease in municipalities with a change of less than -2 dB(A) is about -2.9 dB(A) (min: -8.4 dB(A)). Regions with changes between -2 and 0 dB(A) are defined as unaffected, since a slight decrease in noise can be explained by the design of quieter jet engines and the replacement of old aircrafts by modern ones. Regions that experienced a slight increase in aircraft noise (changes between 0 and +2 dB(A)) are not considered further, as it is unclear whether such an increase can be attributed to the new flight regime, or to the regular increase in flight loads.

Figures 5 and 6 illustrate the nighttime 1-hour energy equivalent noise level from 10 to 11pm and from 11 to 12pm, respectively. As for the daytime noise metric, the nighttime noise contours show highest levels of exposure in the regions directly surrounding the airport. From 2002 to 2004, a large number of municipalities experienced a change in noise exposure of more than 2 dB(A). The regions affected the most are those in the south and in the northeast. We define these municipalities as being affected by the new flight regime. While the observed changes in the south are directly attributable to incoming aircrafts, the changes in the northeast can be explained partly by incoming aircrafts, and partly by the redistribution of outgoing aircrafts. For nighttime noise from 10 to 11pm, the average increase in municipalities with a change of more than 2 dB(A) is about 5.0 dB(A) (maximum:

12.9 dB(A)), the average decrease in municipalities with a change of less than -2 dB(A) is about -3.0 dB(A) (minimum: -6.1 dB(A)). For the late evening measure, the corresponding values are 5.3 dB(A) (max: 9.8 dB(A)) and -3.9 dB(A) (min: -8.4 dB(A)). As before, we define municipalities that experienced a slight decrease (changes between -2 and 0 dB(A)) as unaffected.

Our definition of affected and unaffected regions crucially depends on the definition of cut-off values that are used to categorize the change-in-noise variable. We assumed the set $\{-2, 0, 2\}$ for all noise measures which immediately raises the question of internal and external validity of our assignment rule. In Section 6.3, we will discuss the sensitivity of our results to alternative cut-off values. It turns out that variation in a reasonable range does not significantly alter the policy effects of interest, so that we deem it reasonable to make this assumption. A second argument in favor of our choice is provided in Figure 7, which shows kernel density estimates of the entire distribution of noise changes for each of the three L_{eq} measures. We observe kinks in the estimated densities near the imposed cut-off values which implies a relatively stable classification of regions if the cut-offs are changed modestly. As a final argument, we use noise data from previous years (not shown) and obtain changes in noise exposure that are consistent with the above definitions of unaffected regions and regions not considered further (in contrast to the pronounced changes observed for regions affected by the new flight regime).

— Insert Figure 7 about here —

It is important to note that the above measures do not separate noise caused by landing aircrafts from noise caused by departing aircrafts. They provide an average of both types of noise. Therefore, the policy effects we identify are restricted to the consequences of general aircraft noise. It is left for future work to refine the analysis to various types of noise as soon as more detailed noise data become available.

4 Identification of Policy Effects

The goal of this study is to evaluate the effect of new flight regime at Zurich airport, and the corresponding changes in exposure to aircraft noise, on housing prices. Having defined affected and non-affected regions, we are now in a position to link the noise data with information about housing prices. In order to formalize the discussion, we let Y_i denote the price of object i. Depending on the market we look at, the price can be either the rental fee excluding utilities, or the apartment's sales price. We let $G_i \in \{0, 1\}$ indicate whether (or not) object i is affected by the new flight regime, and we define two time periods $T_i \in \{0, 1\}$, *pre* and *post* the introduction of the new regulation in October 2003. For a random sample of n observations from all objects in the relevant housing market, the observed data are the triple (Y_i, G_i, T_i), $i = 1, ..., n$.

The intervention of interest is the introduction of the new flight regime for the period after October 2003, formally indicated by $I_i = G_i \cdot T_i$. We let $Y_i(1)$ denote the price of object i if it was exposed to the intervention, and we let $Y_i(0)$ denote the price of the same object if it was not exposed to the intervention. $Y_i(1)$ and $Y_i(0)$ are potential outcomes, i.e., potential prices of an object if it was exposed (or not) to the particular change in aircraft noise caused by the new flight regime. The observed price is given by $Y_i = Y_i(1)I_i + Y_i(0)(1 - I_i)$.

Given this notation, the following policy effects are of interest. The object level effect $Y_i(1) - Y_i(0)$ is the most informative quantity as it gives the change in prices caused by the new flight regime for each object. The object level effect, however, can never be identified from the data triple (Y_i, G_i, T_i), since only one of two potential outcomes can logically be observed. We will focus instead on the average price effect of the new flight regime on those objects that were subject to the intervention, formally defined as $E(Y_i(1) - Y_i(0)|I_i = 1)$. The latter quantity is commonly referred to as the average treatment effect on the treated.

Our analysis will proceed in several steps. In *step one*, we rewrite the average treatment effect on the treated as $E(Y_i|I_i = 1) - E(Y_i(0)|I_i = 1)$ using the observation rule for Y. The first term is the mean price among all treated objects and can be calculated from the observed data. The second term is the mean price among all treated objects if they were not treated, which cannot be identified from the observed data. Nevertheless, we can impose bounds on it in order to identify a range of possible values for the average price effect for all treated objects (Manski 1990). As bounding values we choose either the 5% and 95% percentiles in the distribution of prices in the observed sample, or the upper and lower quartiles. The analysis is refined by conditioning on some object characteristics, such as the number of rooms and the year of construction.

We then exploit various model assumptions, thereby taking into account the special structure of our dataset, to identify the average price in absence of the intervention for all treated objects. We carefully state these assumptions now, and then proceed with the empirical analysis. In light of the previous literature on housing prices (e.g., Diewert 2003), we impose the following model in absence of the intervention:

$$Y_i(0) = \exp(\alpha + \beta T_i + \gamma G_i + \varepsilon_i) \tag{1}$$

where β represents a group-invariant time component, γ represents a group-specific, time-invariant component, and the term ε_i captures unobservable characteristics of the object. In *step two* of our analysis, we assume that $E(\exp(\varepsilon_i)|G_i, T_i)$ is constant and normalized to one. The normalization is inconsequential as long as a constant is included in the model, but the former mean independence assumption is non-trivial and therefore subject to further discussion below. In addition, we assume that the intervention has a constant relative effect on outcomes, formally

$$Y_i(1) = \tau Y_i(0) \tag{2}$$

Assumption (2) together with the model for outcomes in absence of the intervention (1) and the observation rule for Y imply that

$$E(Y_i|G_i, T_i) = \exp(\alpha + \beta T_i + \gamma G_i + \delta G_i \cdot T_i) \tag{3}$$

where $\tau = exp(\delta)$. This is an exponential regression model, which can be estimated by standard methods such as Poisson regression or non-linear least squares. In order to conduct valid inference we should account for clustering at the object level and spatial correlations in the data.

Although the model is non-linear, the parameter τ (or for that matter δ) has a simple interpretation that is directly related to the standard difference-in-differences (DID) approach in the linear model. Taking double ratios we obtain

$$\frac{E(Y_i|G_i = 1, T_i = 1)}{E(Y_i|G_i = 1, T_i = 0)} \Bigg/ \frac{E(Y_i|G_i = 0, T_i = 1)}{E(Y_i|G_i = 0, T_i = 0)} = \exp(\delta) = \tau \tag{4}$$

Equation (4) states that the relative time effect in the treatment group ($G_i = 1$) is divided by the relative time effect in the control group ($G_i = 0$). As in the standard linear DID world, this removes the bias associated with a common (but now relative) time trend that is unrelated to the intervention. Due to the structure of (4), we may refer to this approach as "ratio-of-ratios". More generally, we could also allow for heterogenous relative effects in (2), but the ratio-of-ratios estimand remains an interesting parameter because it would give an average effect of the new flight policy on all treated objects in this case.

If treatment and control regions differ in their composition of objects, then the mean independence assumption may not necessarily hold. In particular, some observed *pre* treatment characteristics X might be unbalanced between treated and non-treated objects. If these characteristics are also related to the dynamics of housing prices, then the estimation of τ as described above does not yield the desirable result. In a *third step*, we therefore assume that mean independence holds conditional on group membership, time and a vector of object characteristics, formally $E(\exp(\varepsilon_i)|G_i, T_i, X_i) = constant$. There are several ways we can introduce covariates X into the model. One option would be to rewrite the model in absence of the treatment as

$$Y_i(0) = \exp(\alpha + \beta T_i + \gamma G_i + X_i'\theta + \varepsilon_i) \tag{5}$$

and still assume (2). Alternatively, we may introduce X in arbitrary ways and proceed with a matching strategy. In this *fourth step*, we identify for each treated object one object from the pool of non-treated objects that is most similar to it in terms of a Mahalanobis distance measure (see Rubin 1980 for further details on Mahalanobis matching).

A potential problem with the latter two approaches is that we may not be able to control for all relevant characteristics, and hence it may not be reasonable to assume that $E(\exp(\varepsilon_i)|G_i, T_i, X_i)$ is constant. In *step five*, we take advantage of the richness of our data with observations by object before and after the policy change, i.e., we make use of the panel information for some objects in the sample. The mean independence assumption is deemed reasonable in this case conditional on time-varying observable characteristics and object-specific fixed effects. With such a fixed effects strategy we control for *all* time-invariant characteristics of an object, where time-invariant here means that the characteristics do not vary from the *pre* treatment period ($T_i = 0$) to the *post* treatment period ($T_i = 1$), although they may well vary within the two time periods. Given the model assumptions above, we can proceed with Poisson fixed effects regression in order to estimate the parameter τ.

5 Housing Data and Basic Results

The information about housing prices was provided by Homegate Corporation, the largest real estate internet portal in Switzerland. The Homegate website is accessed by all major real estate agencies and by private people to advertise their properties. The Homegate data are representative of the housing market considered here, when compared to the official numbers from the housing census of 2000. Records include housing type, price (with and without utilities), the number of rooms, area in square meters, and age for each advertisement that was published online from October 2001 to December 2006. The database contains additional details about individual room size, kitchen, bathrooms, storage, heating, quality information, and the like, but these are mainly summarized in an open text field from which it is difficult to extract consistent information, so we exclude the latter from our analysis.

We have information about advertisement start and end dates, object specific indicators, and address details. Unfortunately, street information was entered with substantial errors such that the most reliable address detail is given by the postal code. In order to match housing and noise data, we use geographic information provided by the Swiss Federal Statistics Office that allows us to link postal codes, municipalities, and the noise data recorded on a 100m-by-100m square lattice grid. We restrict the sample to housing in the Canton of Zurich as it captures most communities affected by the airport and enables us to use an additional data source, provided by the statistical office of the Canton of Zurich, that contains municipality-level information about population structure and dynamics, unemployment rates, financial situation, tax rates, and environmental characteristics. This gives us a unique dataset, on the object level, including rich information from three different data sources.

We are, to the best of our knowledge, the first to analyze noise effects on housing prices in both the rental and property markets. As we expect these two markets to react differently to noise exposure changes, we split the data in two parts, apartments for rent and apartments for sale. Despite being one of the world's wealthiest nations, Switzerland has the lowest home ownership rate in Western Europe. Only 34.6 percent of Swiss households were homeowners in 2000, while about two thirds of the population rented accommodation built and owned by landlords (FOH 2004). While Swiss properties for sale change ownership only every 20 years on average, rental contracts endure much shorter periods, namely six to seven years (Werczberger 1997). Homeowners are more settled, which considerably increases their (psychological) costs of selling their house. This has at least two consequences: First, the sample size should be much larger for the rental market in comparison to the sales market. Second, and probably more important, the market for rental apartments is likely to react more sensitively to changes in noise exposure as a result of lower relocation and transaction costs for renters than for homeowners.

In order to identify the policy effect of interest, we impose some additional restrictions on the data. First, we define a time frame of adjustment in which prices were reacting to the introduction of the new flight regime in October 2003. The period of adjustment of prices includes the time immediately following the policy change, until a new market equilibrium is reached, but also the time prior to the policy change, if media coverage and professional articles increased public awareness and created expectations of the possible consequences of a potential change in flight regulations. A text analysis of articles published in quality newspapers, weekly magazines as well as reports of press agencies in Switzerland, for example, reveals no reference before March 2003, see Figure 8. Nevertheless, in order to be on the safe side, we exclude all observations between one year before and one year after October 30, 2003. Furthermore, if multiple observations per property are available, we restrict the sample to at maximum one observation immediately preceding and/or following the time of adjustment. Both of these restrictions will be discussed further in Section 6.3.

— Insert Figure 8 about here —

All in all, this leaves us with 31,840 observations (30,915 properties) in the rental market, and 4,020 observations (4,006 properties) in the sales market. We will first discuss the results for the rental market, and then turn our attention to the sales market (Section 6.4). Table 1 shows the number of observations (and relative frequencies) of objects by treatment region and noise exposure. Panel A tabulates the distribution of objects by change in daytime noise from 2002 to 2004. 16 percent of all properties are located in regions that experienced an increase in noise of more than 2 dB(A), 9 percent experienced a change of less than -2 dB(A). These two regions were previously defined as those affected by the new flight regime. About one third of all observations were not affected by the new flight regime ($\Delta L^d_{eq}(16) \in (-2; 0]$). Regarding the nighttime noise measures, panels B and C, we find about one fifth of all observations exposed to additional noise after the intervention. Almost no objects were affected positively, i.e., exposed to less noise, in the time between 10 and 11pm, but about 35 percent of all objects benefited in the late evening hour (between 11 and 12pm).

— Insert Table 1 about here —

The following table reports the mean rental prices excluding utilities by treatment region, noise exposure metric, and time. The standard deviations are given in round brackets, the numbers of observations in square brackets. First, consider the levels of daytime noise exposure (panel A). The mean rental price in the negatively affected region ($\Delta L^d_{eq}(16) \in (2; max]$) in the period after October 2004 is about 1,878 Swiss Francs. In the notation of the previous section, this is an estimate of $E(Y_i|I_i = 1)$, for the negatively affected regions. The second term forming the average treatment effect for this group, $E(Y_i(0)|I_i = 1)$, cannot be identified from the data. We observe, however, the following rental price percentiles: CHF 850 (5%), CHF 1,300 (25%), CHF 2,200 (75%), and CHF 3,590 (95%) in the sample of all treated objects (not reported in the table). Thus, a conservative range of values for the effect of the new flight regime on housing prices in the negatively affected areas, using upper and lower 5% percentiles, is (CHF -1,712; CHF 1,028). A less conservative interval, using upper and lower quartiles, can be defined as (-322; 578), which is plausible if we believe that the distribution of prices is not too heavily skewed.

— Insert Table 2 about here —

A refinement is obtained conditional on the characteristics of the apartments. For example, we may split the sample by size, i.e., apartments with 1 to 1.5 rooms (mean price CHF 879), 2 to 2.5 rooms (CHF 1,286), 3 to 3.5 rooms (CHF 1,676), 4 to 4.5 rooms (CHF 2,102), and 5 and more rooms (CHF 2,876). Imposing the quartiles of the respective distributions as bounds on the counterfactual mean rental price for apartments subject to additional noise yields the following intervals for the average treatment effect on the treated (conditional on the number of rooms): 1 to 1.5 rooms (-71; 213), 2 to 2.5 rooms (-166; 273), 3 to 3.5 rooms (-229; 386), 4 to 4.5 rooms (-241; 502), and 5 rooms and more (-374; 876), all in Swiss Francs. Alternatively, if we distinguish between newly built apartments (aged less than 5 years, mean price CHF 2,484) and "old" ones (aged 5 years or more, mean price CHF 1,727), we obtain the intervals (-200; 684) and (-308; 497), respectively, again using the upper and lower quartiles as bounds.

Intervals for the positively affected regions and the regions affected according to the nighttime noise measures can be constructed similarly. Unfortunately, all these intervals include zero as a possible value, and are generally relatively wide, such that it is not possible to draw informative conclusions with respect to the efficacy of the new flight regime, i.e., the sign of the policy effect. Nevertheless, such intervals are interesting because they define a range of values consistent with the empirical evidence alone, without imposing any model assumptions. They can be used as a benchmark against which to compare the implications of more restrictive assumptions. The purpose of the remainder of this paper is to explore and discuss several such assumptions. In particular, we will follow a difference-in-differences-type strategy to identify the term $E(Y_i(0)|I_i = 1)$, the mean price of objects in the treated group if they were not subject to the intervention. As outlined in Section 4, the primary focus will be on the average treatment effect on the treated.

6 Estimation Results

In order to provide an initial intuition about our results, consider Table 2 and the daytime noise again. The mean rental price in the negatively affected region before October 2002 was about 1,721 Swiss Francs; after October 2004 it was about 1,878 Swiss Francs. Thus, over time this corresponds to an increase in rental prices by about 9.1 percent for the treatment group. The mean rental price in the control region ($\Delta L^d_{eq}(16) \in (-2; 0]$) before October 2002 is about 1,341 Swiss Francs, after October 2004 it increased to about 1,593 Swiss Francs. Thus, in the control group the time effect is about 18.8 percent. If we correct the relative time effect in the treatment group by the relative time effect in the control region, then we obtain a relative decrease in rental prices by about 8.1 percent in the treatment group (ratio-of-ratios calculation: $(1,878/1,721)/(1,593/1,341) - 1 = 0.081$). This is the average treatment effect on the treated, without controlling for any covariates, and therefore implicitly assuming that the properties in the treatment and control region are directly comparable. This effect will be discussed in detail in the following four sections, for the rental market, Sections 6.1 to 6.3, and for the sales market, Section 6.4.

6.1 Apartments for Rent - Increased Daytime Noise

Table 3 reports the estimation results for the daytime noise metric with treatment group defined as the negatively affected regions. The effect of interest is shown in the third row, which is the coefficient of the interaction of the time indicator *after policy* (T) and the treatment indicator *noise region* (G). The reported number is the estimated coefficient δ in model (3), which, according to (4), can be interpreted as relative price effect of the new flight regime for the treated group. Column (1) redisplays the effect obtained without controlling for covariates (see above); more precisely, the average treatment effect on the treated is estimated as $[\exp(-0.084) - 1] \cdot 100 = -8.1$ percent. The other two coefficients reported in column (1) show the common time trend (rental prices for apartments increased by about 18.8 percent from the *pre* to the *post* treatment period), and the *pre* treatment period difference between treated and control, which is about 28.3 percent (i.e., apartments in regions that experienced additional noise after introduction of the new flight regime are on average much more expensive than apartments in the control region).

— Insert Table 3 about here —

As outlined above, a major concern about the estimates in column (1) is that apartments in the treatment and control regions cannot be directly compared because they might differ in terms of observable and/or unobservable characteristics. For example, the mean number of rooms in the treated region is 3.72 (standard error 0.016) as compared to the non-treated region with mean 3.53 (standard error 0.011). A simple mean comparison test rejects the null hypothesis of equal mean numbers of rooms at the 0.1% significance level. The same holds true for various location characteristics such as the population structure, financial situation, tax rate, and the unemployment rate, but not for the average age of the apartments. If these characteristics are also related to the dynamics in rental prices, then the estimated policy effect in column (1) is likely to be biased.

For this reason, we extend the hedonic model stepwise by including various object and location characteristics. The set of object-specific control variables comprises the number of rooms and a second order polynomial in age. The location variables consist of mean population shares (4 variables), mean proportions of women and foreigners, the mean number of inhabitants, a mean index of financial power, the mean tax rate, the mean investment in housing, and the mean building volume of housing, each averaged over 5 years (1998-2002) prior to the change in landing regulations. We capture *pre* treatment dynamics in location characteristics, such as dynamics in the unemployment rate and the number of single family houses, each in levels for the years 1998, 2000, and 2002. In order to control for spatial correlations in the data we account for the distance to Zurich City.

These characteristics are introduced in the model via linear index functions, columns (2) and (3), and nonparametrically using a matching strategy, column (4). The estimated average treatment effect on the treated in the parametric models increases to -5.1 percent (object variables only) and -3.0 percent (object and location variables). The estimates indicate a downward bias in the raw policy effect due to omitted variables. Objects in the treated group tend to be "better" in terms of valuable characteristics, as suggested by the simple mean comparisons of observable characteristics between the treatment and control groups, and these characteristics also tend to converge over time. This explains the estimated decrease in the *pre* treatment difference between both groups and the estimated decrease in the time trend. Matching is carried out using a Mahalanobis distance measure, choosing for each treated object the closest match from the pool of non-treated objects. The average effect of the new flight regime on objects in the affected regions is now estimated as -8.2 percent. The matching estimator is relatively imprecise in comparison to the parametric estimators, and we cannot reject the hypothesis that there is no difference between the estimated effects.

As a final step of our analysis we consider the selection on unobservables problem. As indicated above, the treatment and control regions significantly differ in their observable *pre* treatment characteristics. It is therefore very unlikely that these objects do not differ in terms of unobservable factors as well. For a subsample of 454 objects in our dataset, we have panel information available, i.e., we have observations both before and after the introduction of the new flight regime. Hence, we can implement a non-linear fixed effects methodology and thereby eliminate all time-invariant unobserved factors, potentially related to the treatment status and to the dynamics in rental prices.

The estimated coefficients of the fixed effects model are reported in column (5). The results indicate that apartments in the treated region are predicted to have an average decrease in rental prices by about 1 percent caused by the new flight regime. This estimate is larger (smaller in absolute magnitude) than all the previous estimates, thus indicating the presence of a selection on unobservables problem. The estimated effect, however, might be obtained in a sample that is no longer randomly drawn from the population of interest, as those objects with panel information are characterized by comparably high turnover rates. Such objects are typically negatively selected. We expect a positive bias in the estimated policy effect in column (5) for two reasons. First, negatively selected objects on average have less valuable characteristics, and for the Zurich housing market it is reasonable to assume that the disparities diminish over time due to a high level of activity in restoration and modernization. Second, less expensive apartments, controlling for all time-invariant characteristics and in particular location, might be subject to smaller noise discounts in absolute magnitude than more expensive apartments. We therefore interpret the fixed effects estimate as an upper bound on the noise discount factor for all apartments subject to additional daytime noise.

6.2 Apartments for Rent - Alternative Noise Regions

The results of the ratio-of-ratios model for alternative definitions of the treatment region are shown in Table 4. The reported numbers are the estimated coefficients δ of the interaction term *after policy* times *noise region*. The standard errors are shown in round brackets, and the number of observations in square brackets. The columns correspond to the different model specifications and estimation methods as in Table 3. In panel A, the treated objects are defined as apartments that experienced an increase in nighttime noise exposure from 10 to 11pm by more than 2 dB(A), and the control group is chosen as before, as all apartments that experienced changes in noise exposure between -2 and 0 dB(A). The sample size is 13,444, of which 7,442 objects are in the treatment region and 6,002 objects are in the control region.

The raw effect, i.e., the average treatment effect on the treated without controlling for other factors, is estimated as $[\exp(-0.048) - 1] \cdot 100 = -4.7$ percent, and is significantly different from zero at the 5% level. If we include additional object characteristics (the number of rooms and a second order polynomial in age), then the estimate remains about the same. Controlling for further location and neighborhood variables reduces the effect of interest to about -1 percent in the linear specification, but yields about the same as the raw effect using the more flexible matching method. If we account for a potential selection on unobservables problem and control for time-invariant unobserved heterogeneity, column (5), then the effect of the new flight regime on rental prices disappears (the estimated coefficient is not statistically or practically different from zero). As argued above, we may interpret the latter estimate as an upper bound due to negative selection issues, and interpret the former estimates obtained under the selection on observables identification strategy as lower bounds (if unobservables are valuable regarding rental prices and the differences diminish over time).

— Insert Table 4 about here —

Panel B displays the estimated effects of the new flight regime for the treatment and control regions defined in terms of late evening noise exposure (11 to 12pm). Objects are defined as treated if the change in noise is above 2 dB(A), and we compare these objects to objects with a change in noise between -2 and 0 dB(A). Consistent throughout the different specifications and methods, the average treatment effect on the treated is estimated as about

-3 percent.

Panel C considers daytime noise again, but now the treatment group is defined as all the apartments subject to a decrease in noise from 2002 to 2004 (less than -2 dB(A)). Thus, the treated group is better off in terms of noise exposure after the introduction of the new flight regime. The control group, as before, is defined as all apartments with a change in noise exposure between -2 and 0 dB(A). *A priori* we would expect the average treatment effect on the treated to be positive if noise is considered a disturbing factor, and hence less exposure to aircraft noise should increase the value of an object. Our results, however, do not provide a clear picture of the sign of the policy effect in this case. The raw effect is estimated to be negative, at -4.1 percent. Controlling for potential selection on observables this effect is confirmed. If we tackle the problem of selection on unobservables, however, then the estimated average treatment effect on the treated turns positive and is about 2.0 percent, as expected. The negative raw effect might be explained by the type of communities included in the treatment group. This group primarily consists of apartments in the structurally weak communities in the very north of the Canton (see Figure 4), with the slowest population and economic growth of all communities.

We conclude that the new flight regime had a negative effect on rental prices for those apartments that were negatively affected in terms of higher noise exposure. Depending on whether the change is mainly observed during the day or in the evening hours, we find average noise discounts of between 1 and 6-8 percent. Our results do not provide clear evidence of whether additional noise exposure is more relevant in the evening or during the day. For apartments that experienced less daytime noise after the introduction of the new flight policy, we do not find clear evidence of a negative or positive effect on rental prices.

6.3 Sensitivity Analysis

The analysis presented above is based on certain (disputable) assumptions and constraints on the objects selected into the sample (treated and control). However, we find a relatively stable pattern of estimated policy effects if some of these constraints are relaxed. First, we allowed for multiple observations per object before and after the introduction of the new flight policy. This has at least two consequences. On the one hand, more frequently advertised apartments are typically characterized by lower quality which in turn has a negative effect on prices. On the other hand, measurement error is averaged out by including additional observations per object before and after the policy change. Second, we chose a tighter time frame of adjustment, reducing the number of months from 24 (±12 months) to 12 (±6 months). This reduces the probability of capturing price effects not attributable to the policy of interest, but a new market equilibrium might not yet have been achieved, so the effect of the new flight regime may be underestimated (in absolute terms). An immediate implication of loosening these restrictions is a larger sample size. Table 5 summarizes the results.

— Insert Table 5 about here —

The four columns in Table 5 correspond to the ratio-of-ratios models without covariates, object specific covariates only, object and location characteristics, and fixed effects, respectively (we dropped the matching results due to space limitations). For each noise metric, we report the coefficients of the interaction term between the group indicator and the time indicator. Consider the increased daytime and nighttime noise measures in panels A, B, and C first. In each case, the noise discount factors are in the range of the estimates obtained before, under the more restrictive sample schemes. Again, we can observe a difference between the estimates based on the selection on observables identification strategy and the fixed effects approach. In general, the estimates are slightly more precise, but this is mainly due to the larger sample sizes. It should be noted that allowing for more than one observation per object increases the panel dimension substantially, indicating that some apartments tend to have a high turnover rate.

The results for the model in which the treatment region is characterized by decreased daytime noise are in line with the results obtained before. Again, we find negative effects of the new flight regime on rental prices if we look at the raw effect and the selection on observables argument, but the policy effect turns positive (and highly significant) if we consider the fixed effects estimates. Overall we conclude that our results are not sensitive to the restrictions imposed in terms of the adjustment time and the number of observations before and after the introduction of the new flight regulation.

Another concern about the validity of our results is related to the definition of the treated and control regions. In particular, we imposed three somewhat arbitrary cut-off values to divide changes in noise exposure into 4 categories. As noted in Section 3, we compared our noise maps with years in which no changes in flight regulations were undertaken. These comparisons yield maps that are consistent with our definition of non-affected regions and regions not considered further. Furthermore, we altered the cut-off values from $\{-2, 0, 2\}$ to $\{-1, 0, 1\}$, $\{-1.5, 0, 1.5\}$ and $\{-3, 0, 3\}$. The results (not shown in the tables) indicate that the policy effects with the alternative cut-off values $\{-1, 0, 1\}$ are still negative, but smaller in magnitude and insignificant. For the second alternative, we obtain estimates of the average treatment effect on the treated that are slightly smaller in magnitude, but still significant and similar in pattern. Finally, the estimates obtained with the set of larger cut-off values are about the same as the results reported in the tables.

6.4 Apartments for Sale

The previous discussion was entirely focused on apartments offered in the rental market. We now turn our attention to the sales market, and the results we obtain for the average effect on sales price of the new flight regime at Zurich airport. The sales market is characterized by substantially smaller turnover rates, so our sample size is much smaller than in the rental market (about 1/8). Again we distinguish between different treatment regions, depending on the noise metric and the changes in aircraft noise from 2002 to 2004. Table 6 reports the logarithmic ratio-of-ratios estimates of the policy effect of interest.

— Insert Table 6 about here —

First consider panel A and the daytime noise measure. The treated objects are defined as all apartments that experienced an increase in aircraft noise of more than 2 dB(A), and the control group comprises all apartments that experienced a change between -2 and 0 dB(A). The first column shows a raw policy effect of about 4.8 percent, which implies that the relative time effect in the treatment region, after correcting for the relative time effect in the control region, is positive. The estimate is insignificant, however, so we cannot reject the hypothesis that the new flight regime did not affect sales prices. The point estimate remains positive, with one exception for the short-term specification, but is insignificant throughout.

If we consider the nighttime noise measures, from 10 to 11pm and from 11 to 12pm, we obtain basically the same pattern of policy effects, positive but insignificant. The estimated average treatment effect on those apartments that experienced a decrease in daytime noise exposure is negative, but insignificant as well. Thus, our results for the sales market do not provide evidence whether the introduction of the new flight regime had a positive effect, a negative effect, or no effect at all on sales prices. This result can be explained by several factors. First, the sales market is fundamentally different from the rental market. Transaction costs in the rental market are much smaller than in the sales market so that tenants are able to move to new apartments without great financial burden, typically the only binding constraint being a three-month period of notice. In contrast, homeowners usually have mortgages on their properties, thus hindering short-term transactions.

Second, homeowners have at least to some degree different interests when selling their properties than tenants have when looking for a new apartment. While tenants' major objective in moving is an improvement in living standards (which are generally reduced under increased aircraft noise pollution), homeowners have additional interests in financial gains. This implies that homeowners may not be willing to sell their properties if they can expect a substantial loss due to additional aircraft noise. Supporting this argument, the Swiss legislative system allows the public to reverse historical policy interventions or initiate new interventions by means of referenda. Since the new flight regime is not entirely settled yet, a homeowner might prefer to postpone the sale of his/her property to avoid a financial loss.

7 Conclusion

This study has used the housing market to estimate one aspect of the cost side of a new flight regime at Zurich airport, due to a flight ban on the part of the neighboring country Germany. The results show that the rental prices of apartments in the regions exposed to additional aircraft noise increased between 1 to 8 percent less than the rental prices in the control group. The negative effect of aircraft noise on rental prices is robust to the inclusion of the number of rooms, construction year and various community attributes as additional explanatory variables, and remains significant even in the fixed-effects regression of a subsample of repeat-sales apartments. Regarding future research on the evaluation of environmental and other nonmarket goods, two main conclusions can be drawn from our analysis.

First, quasi-experiments and non-linear ratio-of-ratios estimates offer a powerful tool to evaluate the value of environmental and other nonmarket goods. The most traditional method of reducing the problem of omitted-variable bias is to use multiple regression techniques to control for as many observable house-price predictors as possible and to hope that the remaining price variation is due to random noise. However, even "kitchen-sink regressions" – describing the hedonic studies that include a great deal of housing attributes – are not able to capture all relevant housing and neighborhood characteristics (Gibbons and Machin 2008). This paper uses a policy change as a quasi-random experiment, and suggests drawing inferences from unequal price trends between the treatment and control group. The potential non-randomness of the treatment assignment is reduced by including several housing and community attributes and/or object-specific fixed-effects.

Secondly, relocation costs have to be negligible in order for hedonic frameworks using housing data to provide plausible estimates of the environmental good. The hedonic price approach implicitly assumes that households continuously re-evaluate their consumption bundles in light of changing circumstances and perfectly adjust their consumption patterns. This requires that individuals can move without transactions costs from one location to another. In reality, however, people experience monetary and psychic relocation costs, which are much higher for homeowners than for renters. This explains why we find a significantly negative noise discount for rental apartments but not for apartments for sale.

As the implicit hedonic prices might not fully reflect the household's marginal willingness to pay for environmental attributes such as quiet, the noise discounts estimated in this paper have to be considered as lower bounds (in absolute magnitude) for the overall negative welfare effect of noise pollution. Recently, life satisfaction approaches to valuing environmental goods evolved to measure the shadow costs of noise using general happiness surveys (Van Praag and Baarsma 2005; Rehdanz and Maddison 2008). Thus, it remains the task of future research to evaluate the overall welfare effects of the new flight regime.

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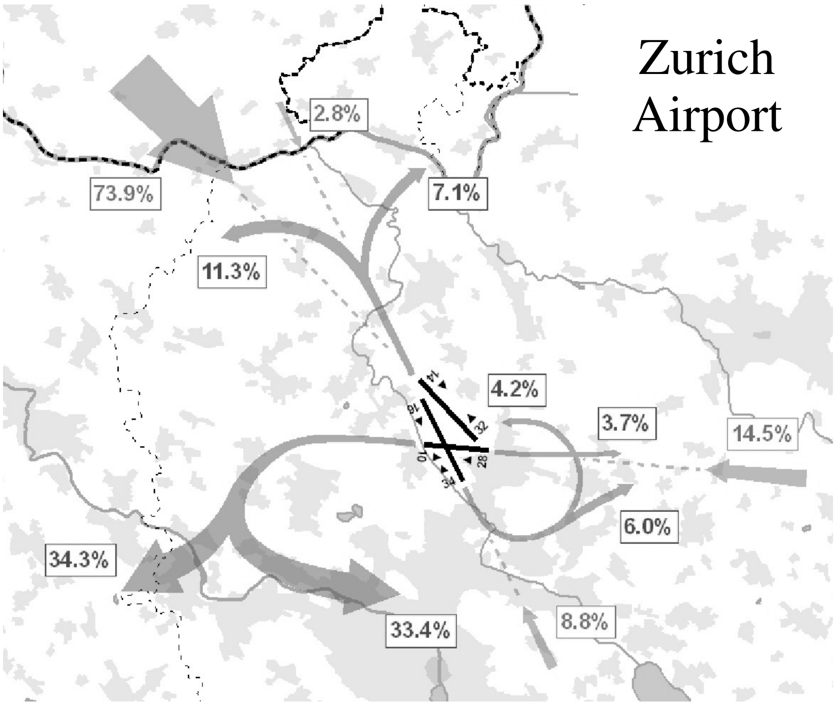
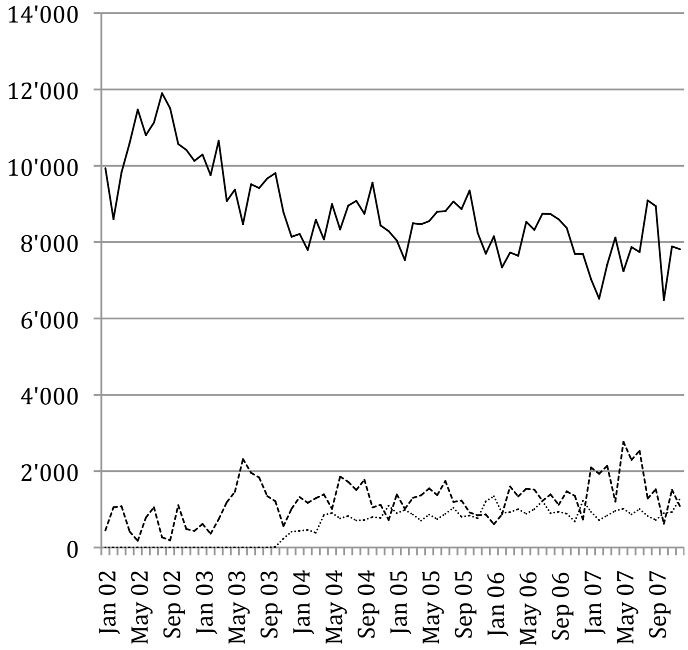
Figures

Figure 1: Orientation Zurich Airport: Runways, Departure/Landing Schemes



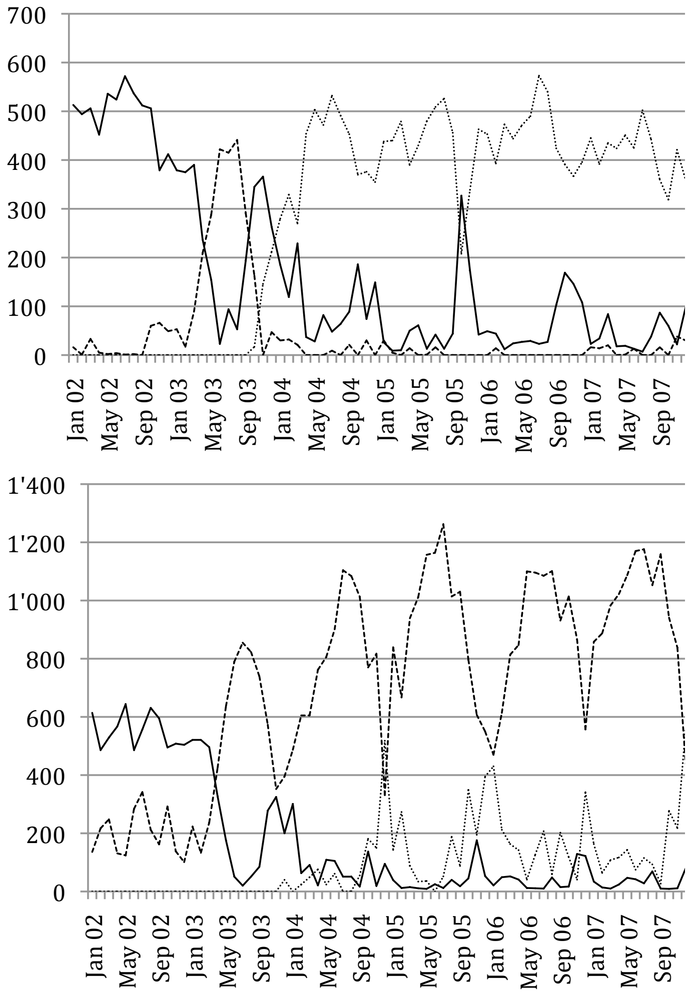
Source: Unique, vector@swisstopo.ch, gg25@swisstopo.ch. Percentages of all flights in 2008.

Figure 2: Monthly Landings – Whole Day



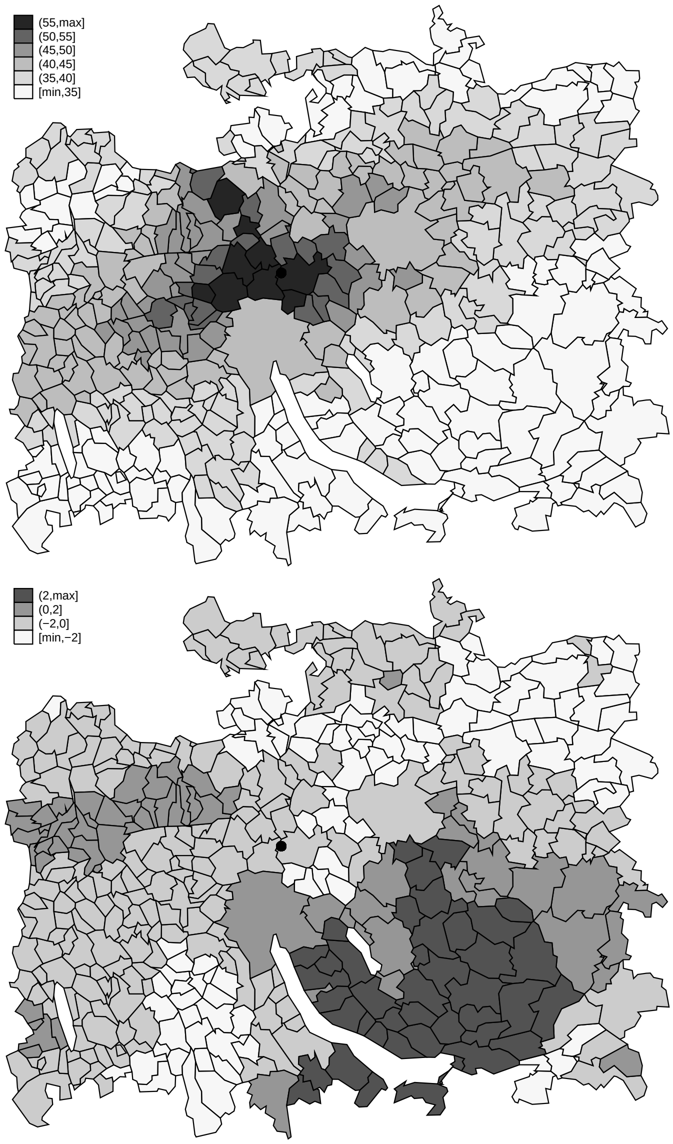
Source: Unique, own calculations. Landings from the north (runways 14 and 16; solid), from the east (runway 28; dashed), and from the south (runway 34; dotted).

Figure 3: Monthly Landings – Morning and Evening



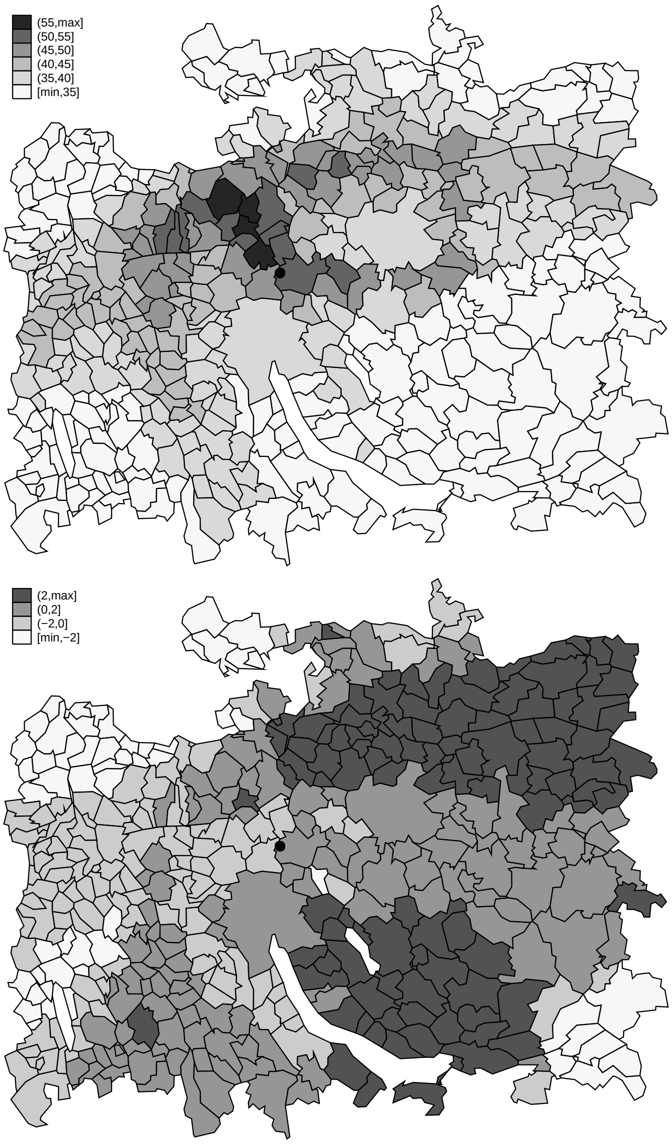
Source: Unique, own calculations. Landings from the north (runways 14 and 16; solid), from the east (runway 28; dashed), and from the south (runway 34; dotted). Upper graph: from 6am to 7am. Lower graph: from 9pm to 12pm.

Figure 4: Daytime Average Noise Exposure from 6am to 10pm



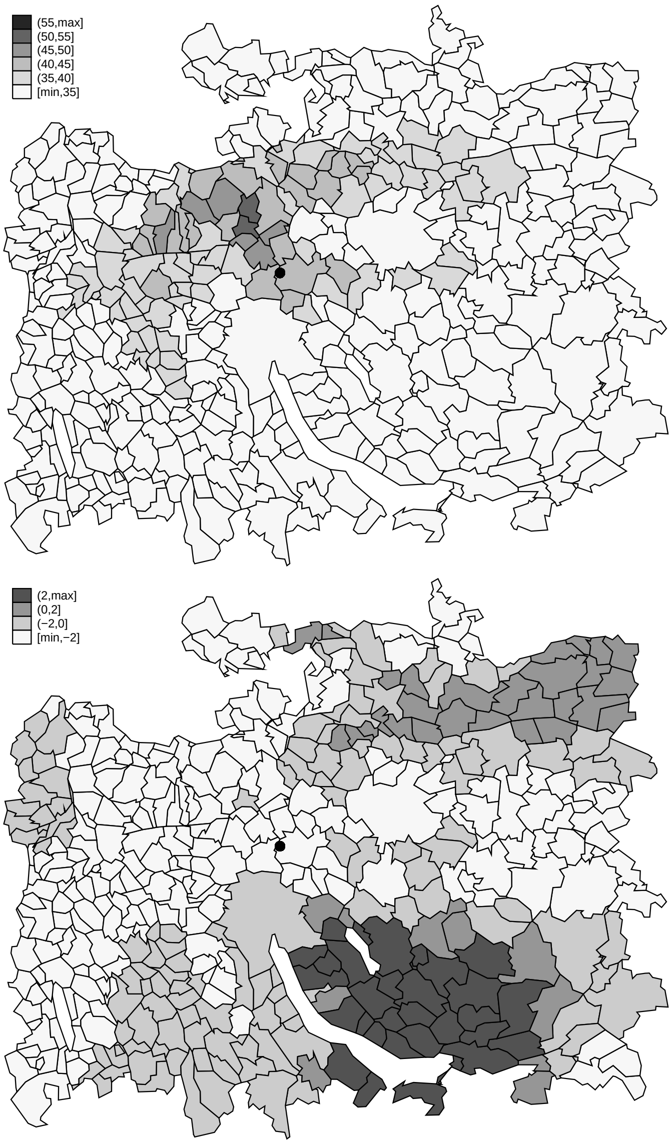
Source: EMPA, own calculations. Upper map: average noise exposure in dB(A) from 6am to 10pm ($L^d_{eq}(16)$) in 2004. Lower map: changes from 2002 to 2004. Zurich airport indicated by the black dot.

Figure 5: Nighttime Average Noise Exposure from 10pm to 11pm



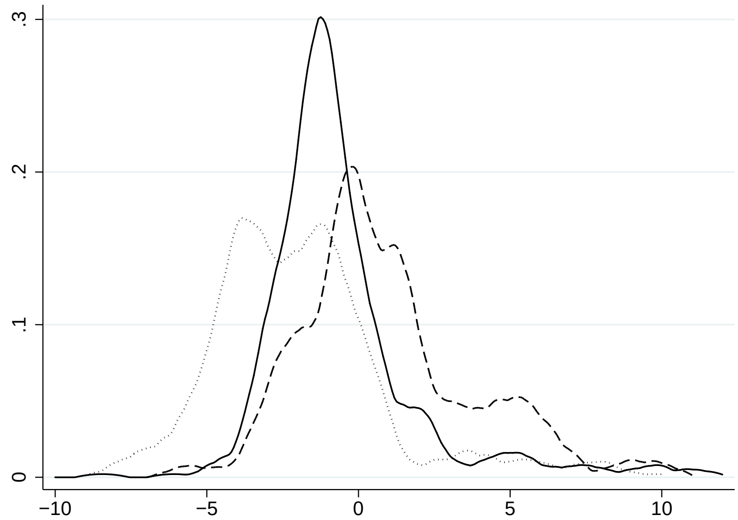
Source: EMPA, own calculations. Upper map: average noise exposure in dB(A) from 10pm to 11pm ($L_{eq}^{n1}(16)$) in 2004. Lower map: changes from 2002 to 2004. Zurich airport indicated by the black dot.

Figure 6: Nighttime Average Noise Exposure from 11pm to 12pm



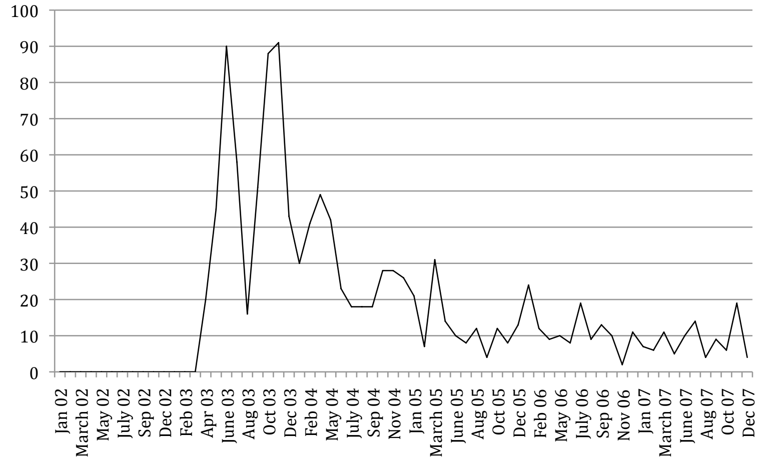
Source: EMPA, own calculations. Upper map: average noise exposure in dB(A) from 11pm to 12pm ($L_{eq}^{n2}(16)$) in 2004. Lower map: changes from 2002 to 2004. Zurich airport indicated by the black dot.

Figure 7: Kernel Density Estimation of Changes in Noise Levels 2002-2004



Source: EMPA, own calculations. Changes are from 2002 to 2004. Upper graph: Changes in $L_{eq}^{d}(16)$ (solid), $L_{eq}^{n1}(1)$ (dashed) and $L_{eq}^{n2}(1)$ (dotted) from 2002 to 2004.

Figure 8: Articles Mentioning the New Flight Regime



Source: LexisNexis, own calculations.

Tables

Table 1: Number of Observations by Treatment Region

	Changes in noise exposure from 2002 to 2004				
Intervals	[min; -2]	(-2; 0]	(0; 2]	(2; max]	Total
A. Daytime noise (6am-10pm): $\Delta L_{eq}^{d}(16)$					
	2,870	10,325	13,513	5,132	31,840
	(9.01%)	(32.43%)	(42.44%)	(16.12%)	
B. Nighttime noise (10-11pm): $\Delta L_{eq}^{n1}(1)$					
	143	6,002	18,253	7,442	31,840
	(0.45%)	(18.85%)	(57.33%)	(23.37%)	
C. Nighttime noise (11-12pm): $\Delta L_{eq}^{n2}(1)$					
	11,274	13,586	1,369	5,611	31,840
	(35.41%)	(42.67%)	(4.30%)	(17.62%)	

Notes: Matched housing-noise data, apartments for rent only.

Table 2: Mean Rental Prices by Treatment Region and Time

	Changes in noise exposure from 2002 to 2004				
Intervals	[min; -2]	(-2; 0]	(0; 2]	(2; max]	Total
A. Daytime noise (6am-10pm): $\Delta L_{eq}^{d}(16)$					
Before Oct '02	1417.8	1341.4	1492.8	1721.3	1461.4
	(470.9)	(566.1)	(850.1)	(941.9)	(751.8)
	[524]	[1,840]	[2,141]	[680]	[5,185]
After Oct '04	1614.0	1592.5	1683.2	1878.2	1680.8
	(509.7)	(749.5)	(851.6)	(941.0)	(817.5)
	[2,346]	[8,485]	[11,372]	[4,452]	[26,655]
B. Nighttime noise (10-11pm): $\Delta L_{eq}^{n1}(1)$					
Before Oct '02	1585.3	1458.2	1411.1	1612.9	1461.4
	(442.3)	(596.2)	(761.9)	(833.3)	(751.8)
	[10]	[1,005]	[3,120]	[1,050]	[5,185]
After Oct '04	1608.2	1695.5	1631.6	1787.2	1680.8
	(526.1)	(844.9)	(788.0)	(857.9)	(817.5)
	[133]	[4,997]	[15,133]	[6,392]	[26,655]
C. Nighttime noise (11-12pm): $\Delta L_{eq}^{n2}(1)$					
Before Oct '02	1338.4	1512.2	1502.2	1660.2	1461.5
	(487.8)	(888.0)	(702.7)	(902.4)	(751.8)
	[2,157]	[2,066]	[194]	[768]	[5,185]
After Oct '04	1512.8	1743.4	1831.3	1811.7	1680.8
	(525.2)	(929.2)	(983.7)	(886.4)	(817.5)
	[9,117]	[11,520]	[1,175]	[4,843]	[26,655]

Notes: Matched housing-noise data, apartments for rent only. Rental prices in Swiss Francs not including utilities. Standard deviation in round brackets, number of observations in square brackets.

Table 3: Estimation Results for RoR Model – Increased Daytime Noise

Dependent variable: *rental price*					
	(1)	(2)	(3)	(4)	(5)
After policy T (0/1)	0.172	0.095	0.064		-0.015
	(0.011)	(0.007)	(0.006)		(0.003)
Noise region G (0/1)	0.249	0.172	0.050		
	(0.023)	(0.017)	(0.017)		
After policy · *Noise region* (0/1)	-0.084	-0.052	-0.031	-0.083	-0.010
	(0.024)	(0.018)	(0.015)	(0.041)	(0.004)
Number of rooms		0.248	0.238		
		(0.003)	(0.003)		
Age $\cdot 10^{-2}$		-0.390	-0.462		-0.858
		(0.026)	(0.026)		(0.091)
Age squared $\cdot 10^{-4}$		0.059	0.069		1.380
		(0.007)	(0.007)		(0.145)
Further controls	No	No	Yes	Yes	
Fixed effects					Yes
Mahalanobis Matching				Yes	
Number of observations	15,457	15,457	15,457	15,457	908

Notes: Matched housing-noise data, apartments for rent only. Rental prices in Swiss Francs not including utilities. *Noise region* defined via daytime noise exposure as $\Delta L_{eq}^{d}(16) \in (2; \max]$ versus $\Delta L_{eq}^{d}(16) \in (-2; 0]$. *After policy* indicates the period after October 2004, as opposed to before October 2002. The set of control variables comprises mean population shares (4 variables), mean proportions of women and foreigners, the mean number of inhabitants, a mean index of financial power, the mean tax rate, the mean investment in housing, and the mean building volume of housing, each averaged over 5 years (1998-2002) prior to the policy change, the dynamics in the unemployment rate and the number of single family houses, in levels for 1998, 2000, and 2002. The reported numbers are logarithmic ratio-of-ratios estimates. Robust standard errors are shown in parentheses.

Table 4: Estimation Results for RoR Model – Alternative Noise Regions

Dependent variable: *rental price*					
	(1)	(2)	(3)	(4)	(5)
A. Increased nighttime noise (10-11pm): $\Delta L_{eq}^{n1}(1) \in (2; \max]$ versus $(-2; 0]$					
	-0.048	-0.058	-0.008	-0.054	0.00001
	(0.022)	(0.016)	(0.013)	(0.021)	(0.004)
	[13,444]	[13,444]	[13,444]	[13,444]	[756]
B. Increased nighttime noise (11-12pm): $\Delta L_{eq}^{n2}(1) \in (2; \max]$ versus $(-2; 0]$					
	-0.055	-0.028	-0.029	-0.019	-0.029
	(0.025)	(0.019)	(0.015)	(0.025)	(0.004)
	[19,197]	[19,197]	[19,197]	[19,197]	[1,036]
C. Decreased daytime noise (6am-10pm): $\Delta L_{eq}^{d}(16) \in [\min; -2] versus (-2; 0]$					
	-0.042	-0.071	-0.034	-0.084	0.020
	(0.019)	(0.012)	(0.011)	(0.025)	(0.005)
	[13,195]	[13,195]	[13,195]	[13,195]	[804]
Controls 1	No	Yes	Yes	Yes	
Controls 2	No	No	Yes	Yes	
Fixed effects					Yes
Mahalanobis Matching				Yes	

Notes: Matched housing-noise data, apartments for rent only. Rental prices in Swiss Francs not including utilities. *Noise regions* are defined as indicated in the table, *after policy* indicates the period after October 2004, as opposed to before October 2002. All models include the treatment and time dummies. Control set 1 includes the number of rooms, and a second order polynomial in age. Set 2 of control variables comprises the variables listed in the notes to Table 3. The reported numbers are logarithmic ratio-of-ratios estimates of the policy effect, i.e., the interaction term *after policy* · *noise region*. Robust standard errors are shown in parentheses, and numbers of observations in square brackets.

Table 5: Sensitivity Analysis of RoR Estimates by Noise Region

Dependent variable: *rental price*	(1)	(2)	(3)	(4)	Number of Observations
A. Increased daytime noise (6am-10pm): $\Delta L^{d}_{eq}(16) \in (2; \max]$ versus $(-2; 0]$					
± 12 months / all observations b/a	-0.091	-0.058	-0.043	-0.022	23,669
	(0.026)	(0.019)	(0.015)	(0.003)	/ 13,152
± 6 months / one observation b/a	-0.090	-0.049	-0.020	0.006	18,362
	(0.020)	(0.015)	(0.012)	(0.003)	/ 1,656
± 6 months / all observations b/a	-0.090	-0.056	-0.030	-0.006	28,703
	(0.022)	(0.016)	(0.012)	(0.002)	/ 16,572
B. Increased nighttime noise (10-11pm): $\Delta L^{n1}_{eq}(1) \in (2; \max]$ versus $(-2; 0]$					
± 12 months / all observations b/a	-0.068	-0.080	-0.034	-0.006	20,643
	(0.026)	(0.019)	(0.017)	(0.003)	/ 11,448
± 6 months / one observation b/a	-0.055	-0.053	-0.002	0.009	15,952
	(0.018)	(0.014)	(0.012)	(0.003)	/ 1,344
± 6 months / all observations b/a	-0.067	-0.073	-0.025	0.001	24,936
	(0.021)	(0.016)	(0.013)	(0.002)	/ 14,293
C. Increased nighttime noise (11-12pm): $\Delta L^{n2}_{eq}(1) \in (2; \max]$ versus $(-2; 0]$					
± 12 months / all observations b/a	-0.056	-0.039	-0.041	-0.043	28,784
	(0.027)	(0.020)	(0.017)	(0.003)	/ 15,422
± 6 months / one observation b/a	-0.053	-0.030	-0.023	-0.011	23,106
	(0.020)	(0.016)	(0.013)	(0.003)	/ 1,964
± 6 months / all observations b/a	-0.053	-0.042	-0.034	-0.028	35,537
	(0.022)	(0.017)	(0.014)	(0.002)	/ 20,045
D. Decreased daytime noise (6am-10pm): $\Delta L^{d}_{eq}(16) \in (\min; -2]$ versus $(-2; 0]$					
± 12 months / all observations b/a	-0.029	-0.077	-0.046	0.019	20,305
	(0.023)	(0.013)	(0.013)	(0.004)	/ 11,377
± 6 months / one observation b/a	-0.034	-0.057	-0.025	0.012	15,723
	(0.016)	(0.010)	(0.010)	(0.004)	/ 1,448
± 6 months / all observations b/a	-0.026	-0.065	-0.038	0.016	24,667
	(0.020)	(0.012)	(0.011)	(0.003)	/ 14,339
Controls 1	No	Yes	Yes		
Controls 2	No	No	Yes		
Fixed Effects				Yes	

Notes: Matched housing-noise data, apartments for rent only. Rental prices in Swiss Francs not including utilities. Definitions of *after policy* and *noise region* as indicated in the table. For the set of control variables, see the notes to Tables 3 and 4. b/a is short for before/after. The first number in the last column refers to the number of observations in models (1)-(3), the second to the number of observations with panel information, model (4). The reported numbers are logarithmic ratio-of-ratios estimates of the policy effect, i.e., the interaction term *after policy* · *noise region*. Robust standard errors are shown in parentheses.

Table 6: Estimation Results for RoR Model – Apartments for Sale

Dependent variable: *sales price*	(1)	(2)	(3)	(4)	(5)	(6)
A. Increased daytime noise (6am-10pm): $\Delta L^{d}_{eq}(16) \in (2; \max]$ versus $(-2; 0]$						
	0.048	0.054	0.042	0.069	-0.051	0.012
	(0.060)	(0.048)	(0.034)	(0.048)	(0.042)	(0.042)
	[2,576]	[2,576]	[2,576]	[4,176]	[3,189]	[5,126]
B. Increased nighttime noise (10-11pm): $\Delta L^{n1}_{eq}(1) \in (2; \max]$ versus $(-2; 0]$						
	0.037	0.006	0.022	0.061	-0.051	0.014
	(0.064)	(0.049)	(0.037)	(0.059)	(0.040)	(0.048)
	[2,391]	[2,391]	[2,391]	[3,848]	[2,988]	[4,771]
C. Increased nighttime noise (11-12pm): $\Delta L^{n2}_{eq}(1) \in (2; \max]$ versus $(-2; 0]$						
	0.039	0.082	0.039	0.022	-0.060	-0.019
	(0.062)	(0.051)	(0.038)	(0.048)	(0.046)	(0.043)
	[2,504]	[2,504]	[2,504]	[3,959]	[3,123]	[4,906]
D. Decreased daytime noise (6am-10pm): $\Delta L^{d}_{eq}(16) \in (\min; -2]$ versus $(-2; 0]$						
	-0.007	-0.043	-0.063	-0.041	-0.052	-0.028
	(0.051)	(0.041)	(0.037)	(0.052)	(0.030)	(0.041)
	[1,671]	[1,671]	[1,671]	[2,750]	[2,076]	[3,407]
Controls 1	No	Yes	Yes	Yes	Yes	Yes
Controls 2	No	No	Yes	Yes	Yes	Yes
Months b/a policy	12	12	12	12	6	6
Observations b/a	1	1	1	All	1	All

Notes: Matched housing-noise data, apartments for sale only. Sales prices in Swiss Francs. *Noise regions* and *after policy* are defined as indicated in the table. For the set of control variables, see the notes to Tables 3 and 4. The reported numbers are logarithmic ratio-of-ratios estimates of the interaction term *after policy* · *noise region*. Robust standard errors are shown in parentheses, and numbers of observations in square brackets.

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