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Adjustments in the housing market after an environmental shock: evidence from a large-scale change in aircraft noise exposure

By Christian Almer[a], Stefan Boes[b], and Stephan Nüesch[c]

[a]Mannheim Institute of Public Health, Social and Preventive Medicine, Medical Faculty Mannheim, Heidelberg University, Ludolf-Krehl-Strasse 7-11, 68167 Mannheim; and Department of Economics, University of Bath, Claverton Down, Bath, BA2 7AY, e-mail: christian.almer@medma.uni-heidelberg.de

[b]Department of Health Sciences and Health Policy, University of Lucerne, Frohburgstrasse 3, PO Box 4466, CH-6002 Lucerne, Switzerland, e-mail: stefan.boes@unilu.ch

[c]Chair of Business Management, University of Münster, Georgskommende 26, D-48143 Münster, Germany, e-mail: stephan.nueesch@wiwi.uni-muenster.de

Abstract

The impact of aircraft noise on housing is a much-debated topic. To meet the increasing demand for air transportation, airports seek to expand their capacities, but studies looking at market responses to a spatial redistribution of noise pollution are scarce. Using online advertisements of rental apartments around a large European airport (ZRH) and an unexpected change in flight regulations, we investigate the post-shock dynamics in apartment rents and tenants' search behaviour. We find that rents take about two years to stabilise to a new equilibrium value. After this period there is a constant markup (discount) for apartments exposed to less (more) aircraft noise. Moreover, the number of advertisement clicks as a proxy for search behaviour and information acquisition is significantly higher during the adjustment period. Our results have implications regarding the calculation of capitalisation effects in quasi-experimental hedonic valuations, which need to take into account off-equilibrium periods.

JEL classifications: C23, D58, Q51, R10.

1. Introduction

Civil aviation has become a central part of human life, with a constant increase in global demand for both cargo and passenger flights (IATA, 2013). At the same time, a steady migration flow to cities can be observed, resulting in a high urban population density and urban sprawl (United Nations, 2012). As a consequence, even though most major airports have originally been located with some distance to residential neighbourhoods, population growth in metropolitan areas leaves an increasing amount of people affected by aircraft noise. A recent example for the clash of interests can be observed at Heathrow Airport in the UK. On the one hand, airport officials announced plans to build an additional runway, which would almost double the airport's capacity. On the other hand, a growing political opposition argues in favour of closing down Heathrow and increasing the capacities of airports located in less populated areas.[1]

Homeowners in areas affected by more aircraft noise typically face a devaluation of their property and possibly an increase in their default risk (e.g., Nelson, 2004). In order to estimate the effect of aircraft noise on housing prices, the literature commonly employs revealed-preference methods because environmental goods, such as quietness, are rarely traded in explicit markets (e.g., Garrod and Willis, 1999; Baranzini *et al.*, 2008). A prominent approach is Rosen's (1974) hedonic pricing model, which is based on the idea that the utility derived from the consumption of a composite product like housing is determined by the utility associated with its constituent parts, i.e., characteristics of the house (e.g., square footage, construction quality), the neighbourhood (e.g., crime rates, population structure, schools), and the environment (e.g., noise pollution, air quality). Empirically, implicit prices can be estimated by a regression of housing values on a comprehensive vector of objectively measured characteristics.[2]

While cross-sectional hedonic regressions provide an important device for the valuation of non-traded goods, they have several drawbacks that impede a meaningful interpretation in terms of the individual's marginal willingness-to-pay, in particular distortions due to omitted variable bias (e.g., Parmeter *et al.*, 2007; Kuminoff *et al.*, 2010; Parmeter and Pope, 2013). More recent hedonic studies have therefore relied on quasi-experiments, e.g., in the valuation of school quality (Black, 1999), clean air (Chay and Greenstone, 2005), hazardous waste (Gayer *et al.*, 2000; Greenstone and Gallagher, 2008), and power plants (Davis, 2011). Exploring non-marginal policy interventions as a source of experimental variation carries at least two problems. First, econometric approaches such as difference-in-differences (DID) methods usually identify capitalisation effects (see Parmeter and Pope [2013] for a general discussion). The link to meaningful welfare measures and the public's willingness-to-pay, however, is not clear in this framework (Kuminoff and Pope, 2014; Banzhaf, 2015). Second, the market of interest may at least temporarily be out of equilibrium. Hence, quasi-experimental results may be upward or downward biased for the true capitalisation effect depending on the post-policy dynamics and timing of data collection.

Our study addresses the latter aspect in the quasi-experimental hedonic literature. The novelty of the paper is to empirically identify the adjustments in the housing market after a large-scale and unexpected policy intervention. In 2003, a change in flight regulations mandated by the neighbouring country Germany significantly altered the exposure to aircraft noise around Zurich airport (ZRH) in Switzerland. In particular, flights were rerouted to

1 See, e.g.: http://www.theguardian.com/environment/2013/jul/17/heathrow-airport-third-runway (accessed 16 November 2016).

2 This hedonic regression is the first stage in the procedure described in Rosen (1974). For a discussion of the second-stage estimation of structural demand and supply functions, and the practical difficulties in the estimation of these functions, see for example Bartik (1987), Epple (1987), Ekeland *et al.* (2004), Bajari and Benkard (2005), and Bishop and Timmins (2015). The focus of our paper will be on the first-stage hedonic price regression.

new paths that were not used prior to the intervention. Using online advertisements of rental apartments over a time frame of eight years, we employ a DID approach with time-varying treatment effects (e.g., Angrist and Pischke, 2009) to investigate the post-policy adjustments in apartment rents and tenants' search behaviour for areas exposed to more and areas exposed to less aircraft noise. Our results suggest that apartment rents in areas exposed to more aircraft noise continuously decreased for about two years after the intervention until they stabilised at a new lower equilibrium value. Consistent with this result we find a constant markup for apartments in regions exposed to less aircraft noise. The number of advertisement clicks is significantly higher during the adjustment period, indicating increased search activity in those areas affected by the intervention compared to the control region. We place our results in the broader literature of market frictions and information acquisition in the housing market (Rosen and Smith, 1983; Wheaton, 1990; Pope, 2008).

Our paper is not the first quasi-experimental study analysing the impact of aircraft noise on housing prices. McMillen (2004) looks at changes in noise contours over time around Chicago O'Hare and finds a 9% reduction in property values close to the airport. Pope (2008) shows that a new airport noise disclosure policy significantly decreased housing prices, even though actual noise levels did not change, supporting the hypothesis of reduced information asymmetry between buyers and sellers. Most closely related to our study is Boes and Nüesch (2011). Using the same intervention, they find an aggregate price discount for apartments exposed to more aircraft noise after the change in flight regulations. Compared to the earlier papers, we specifically address the adjustment processes in the housing market, which we deem relevant to gain a better understanding of the potential consequences of policy interventions that seek to redistribute or enhance flight capacities of airports. We also allow for heterogeneous regional price developments, and we do not restrict the analysis to repeat-rentals. Finally, this paper is the first that analyses individual search behaviour in the housing market after an alteration of aircraft noise exposure, which provides new insights into market responses to environmental shocks.

The remainder of the paper is structured as follows. In Section 2, we briefly review the related literature underlying the hedonic valuation of aircraft noise. In Section 3, we describe our data on aircraft noise exposure and the housing market, also providing details about the flight regulations at Zurich airport and the 2003 intervention. Section 4 outlines our empirical strategy and presents the results. Section 5 discusses the implications of our study.

2. Background and related literature

2.1 Aircraft noise exposure and human well-being

Permanent exposure to aircraft noise is known to have serious negative impacts on physical and mental health (Stansfeld and Matheson, 2003; Black *et al.*, 2007; Jarup *et al.*, 2008; Huss *et al.*, 2010; Boes *et al.*, 2013; Hansell *et al.*, 2013; Correia *et al.*, 2013). Apart from the negative effects on quality of sleep, other health impacts include the increased risks of cardiovascular disease, hypertension, and psychological symptoms like anxiety, depression, and nervousness. Haines *et al.* (2001a,b) and Stansfeld *et al.* (2005) find that steady aircraft noise exposure is negatively associated with children's reading comprehension and long-term memory.

As a consequence of the detrimental health effects, people living close to major airports regularly express their displeasure about the increasing exposure to aircraft noise, for example by means of organised demonstrations or legal disputes. Given the concurrence of a high population density and the increasing demand for air services in metropolitan areas, targets of such protests include leading airports like Atlanta International (Cohen and Coughlin, 2009), Chicago O'Hare (McMillen, 2004), Frankfurt (Geis, 2010), and Heathrow (Griggs and Howarth, 2004).

2.2 Aircraft noise exposure and the housing market

The negative effects of aircraft noise on health and well-being likely reduce the willingness-to-pay for housing in noisy regions. Quietness is considered a valuable good, and individuals either consciously or subconsciously take the exposure to noise into account when looking for a new apartment or house. The meta-analysis of cross-sectional hedonic studies by Nelson (2004) finds aircraft noise discounts to be around 0.6% per decibel. When moving costs and market frictions are non-negligible, then average noise discounts from hedonic studies present a lower bound for the overall costs of aircraft noise (Van Praag and Baarsma, 2005).

Over the past few years, the validity of cross-sectional work has been heavily disputed (e.g., Parmeter *et al.*, 2007; Kuminoff *et al.*, 2010; Parmeter and Pope, 2013). Unobserved housing characteristics like the quality of the neighbourhood are suspected to confound the relationship between noise and housing prices. For example, noisy residential areas are often close to industrial areas or traffic arteries. As a consequence, noise is highly correlated with air pollution, and environmental quality in general. More recently, quasi-experimental approaches have been used to address the problem of confounding (e.g., Chay and Greenstone, 2005; Greenstone and Gayer, 2009; Davis, 2011). Quasi-experimental valuations of aircraft noise can be found in McMillen (2004), Pope (2008), and Boes and Nüesch (2011). These studies identify the impact of aircraft noise on housing prices by using exogenous changes in the exposure to aircraft noise (McMillen, 2004; Boes and Nüesch, 2011) or in the information about noise (Pope, 2008) and by calculating average price changes in affected as opposed to unaffected regions.

3. Data

3.1 Flight regulations around Zurich airport

In this study, we consider the housing market around Zurich airport in Switzerland. Zurich airport is one of the largest airports in Europe, with more than 250,000 take-offs and landings per year. The directions of the three runways are northwest/southeast (runways 14/32), north/south (runways 16/34), and east/west (runways 10/28). In 2002, 89% of the landing aircraft approached from the northwest on runway 14, 5% from the north on runway 16, and 6% from the east on runway 28. And 67% of the aircraft took off in the west direction from runway 28, 10% took off in the north direction (runways 32/34), 21% in the south direction on runway 16, and 2% in the east direction on runway 10.

In 2003, the flight movements around the airport significantly changed. A particular feature of Zurich airport (and the associated exposure to aircraft noise) is the involvement of two countries because of its proximity to the Swiss-German border (dark dash dot line in Fig. 1). As a protective action against noise pollution, the German government issued a binding decree on 17 April 2003 that prohibited landings from the north in the early

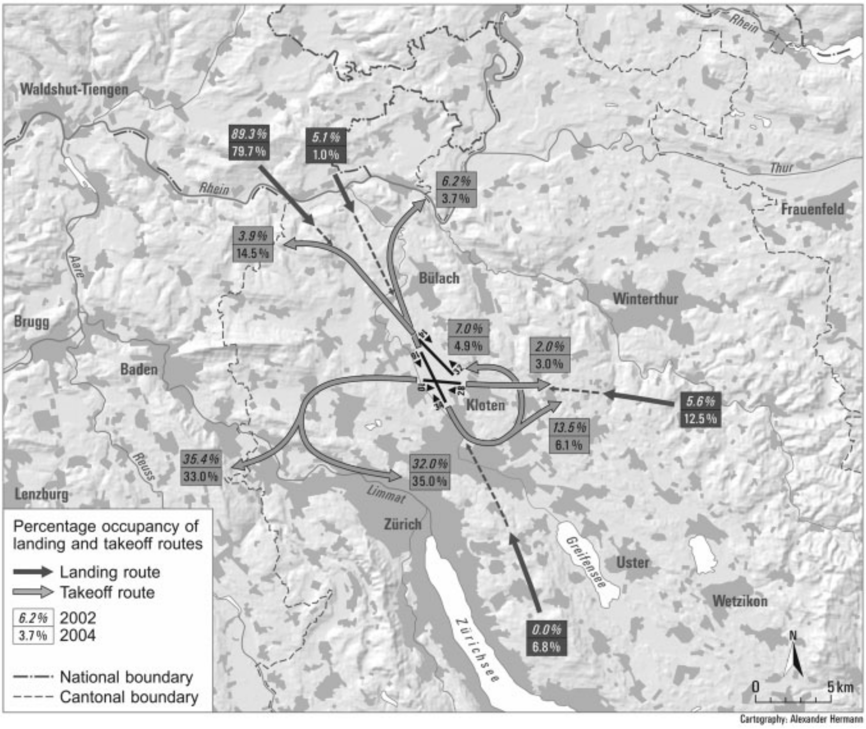

Fig. 1. Zurich airport and relative flight occupancy
Source: Flughafen Zürich AG (2011).
Notes: Percentage occupancy of landing and take-off routes in 2002 and 2004. Grey shaded are settlement areas. Swiss-German border marked with dash dot line. Dashed line marks cantonal border. North/south runway 16/34, northwest/southeast runway 14/32, east/west runway 10/28.

morning and in the late evening. As a result, landing aircraft had to be redirected to approach from the east on runway 28 because at that time the flight regulations did not allow for any other direction.

On 21 May 2003 the Federal Office of Civil Aviation changed the regulations such that landings were also allowed from the south on runway 34. The new flight regime took effect in the first week of November 2003 with aircraft landing from the south between 6 am and 7 am on weekdays (6am to 9am on weekends) and aircraft landing from the east between 9 pm and 12 am on weekdays (8 pm to 12am on weekends).[3] These regulations are still in effect today, although there are ongoing negotiations between the Swiss and German governments about future developments of the airport and flight movements in particular. The relative number of flights approaching from the north dropped by 13.7 percentage points from 2002 to 2004 as a consequence of the regime. The incoming flights were redistributed to approach from the east (+6.9 percentage points) and the south (+6.8 percentage points).

The redistribution of incoming flights also affected take-offs. Due to the introduction of landings from the south after October 2003, the fraction of take-offs in the south direction

3 Exceptions to this general flight regulation are only allowed in special weather conditions (strong wind, fog and mist), or in the case of emergency flights (Flughafen Zürich, 2012).

decreased by 9.5 percentage points from 2002 to 2004. Most of these take-offs are now operated from runways 32 and 34 (+8.1 percentage points) in the north direction combined with a left or right turn such that the take-offs do not violate the German flight restrictions.

3.2 Noise exposure

We use high-resolution annual aircraft noise data provided by the Swiss Federal Laboratories for Material Science and Technology (EMPA). The EMPA model calculates aircraft noise exposure around Zurich airport based on effective radar flight track information, statistics of movements per aircraft type and period of day, sound source data of the aircraft type, and environmental characteristics such as terrain with a resolution of 250 m by 250 m, and then interpolates to a 100 m by 100 m grid; see Krebs *et al.* (2010) for details about the EMPA aircraft noise model and Thomann (2007) for model precision. Following the acoustic literature (Tomkins *et al.*, 1998), we use the equivalence metric L_{eq} for our analyses. L_{eq} indicates the steady sound level between 6 am and 10 pm that would produce the same energy as the actual time-varying noise intensity. The units of measurement are A-weighted decibels dB(A). While the available time frame does not allow us to distinguish between the morning and evening redistribution of flights, we capture both aspects together very well with the changes in L_{eq} from 2002 to 2004, and we interpret our estimates as total effects of the flight regime change.

Figure 2 illustrates the local noise exposure in 2002, one year prior to the flight regime change. The dark regions correspond to the highest levels of aircraft noise exposure, which are concentrated around the airport and in the direction of the three runways, as expected.

3.3 Treatment and control structure

We impose the following definitions for treatment and control regions:

- Treatment N^{+}: Increase of L_{eq} from 2002 to 2004 by more than +3 dB(A)
- Treatment N^{-}: Decrease of L_{eq} from 2002 to 2004 by more than -3 dB(A)
- Control: Change of L_{eq} from 2002 to 2004 in the interval $[-2, 2]$

In all cases we constrain the area of interest to have at least 30 dB(A) of aircraft noise exposure in 2002. The latter restriction is imposed to spatially constrain the control group and to conduct the analyses in an area where aircraft noise is deemed a potentially disturbing environmental factor (WHO, 2009). We used ± 3 dB(A) as the threshold values for the treatment regions because only changes above that level can be identified by the human ear (Reindel, 2001). By the same reasoning, we take the interval $[-2, 2]$ as a plausible choice for the control group because it is within the range of no noticeable changes in aircraft noise exposure.[4] Figure 2 marks the positive treatment N^{+} with (+) signs and the negative treatment N^{-} with (−) signs.

While the noise pollution in the north generally decreased—in some areas by more than −6 dB(A)—noise exposure in the south generally increased, with a maximum of +14 dB(A). Only those communities in the south close to the airport are exposed to less

4 Sensitivity checks indicate that our results are robust to modest variations in the definition of treatment and control, in particular using the interval $\pm$ 1.5 for the control and 3.5 dB(A) as a threshold for the treatments (results are available upon request). Using the discrete definition of the treatment does not come with much of a loss of information, as the noise effect is constant across a large range of noise values; see Boes and Nüesch (2011).

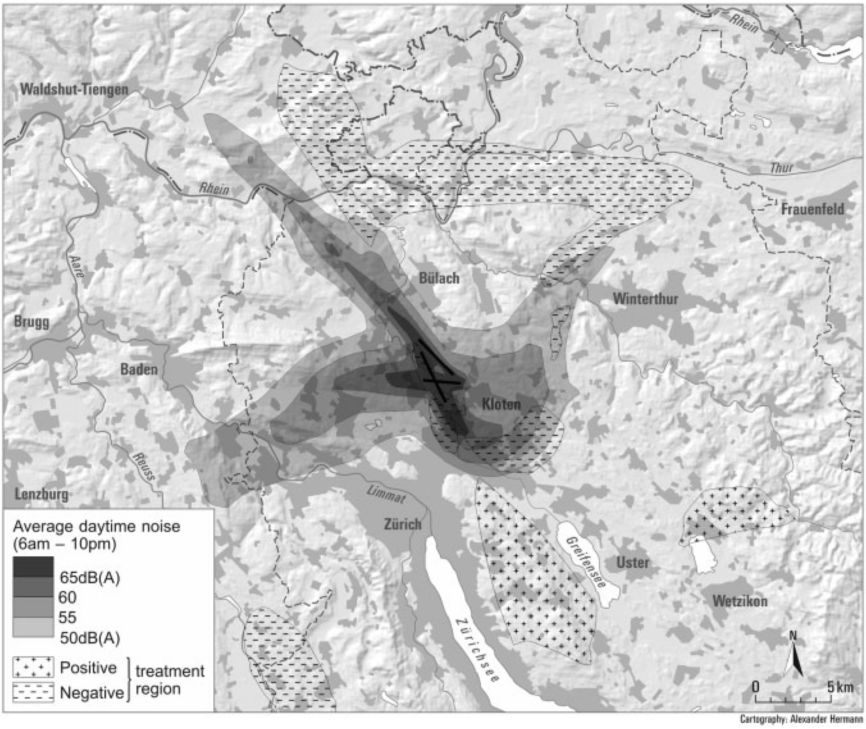

Fig. 2. Daytime noise exposure in 2002
Notes: Contours show average daytime noise L_{eq} from 6 am to 10 pm in 2002. Plus signs mark the positive treatment region N^+ (defined as region affected by change in L_{eq} from 2002 to 2004 by more than 3 dB(A) and average noise exposure in 2002 of more than 30 dB(A)). Minus signs mark the negative treatment region N^- (change in L_{eq} from 2002 to 2004 by less than –3 dB(A) and average noise exposure in 2002 of more than 30 dB(A)).

aircraft noise, due to the substitution of starting and landing aircraft, the latter generating less noise.

Table 1 summarises the changes in noise exposure for all zip codes in the two treatment regions and in the control region. The reported numbers are based on the changes between the pre-treatment (2002) and post-treatment (2004) average noise levels.

Table 1 indicates that the positive treatment region N^+ consists of 10 zip codes, the negative treatment region N^- of 24. In the positive treatment region, the maximum increase in noise is +14.1 dB(A), with a mean increase of +7.5 dB(A). The negative treatment region experienced a maximum drop in noise exposure of –6.9 dB(A), with a mean decrease of about –3.6 dB(A). The control region consists of 102 zip codes which experienced a mean change of –0.6 dB(A). The final row of Table 1 shows the number of advertisements that we have for each of the regions (7,397 in region N^+, 11,051 in region N^-, and 123,775 in the control region).

3.4 Housing data

We use data for online advertisements from homegate.ch, the major online platform for housing in Switzerland. The website has been online since the end of 2001 and is designed

Table 1. Treatment summary

	Treatment N^+ $\Delta L_{eq} > 3$	Treatment N^- $\Delta L_{eq} < -3$	Control $-2 \leq \Delta L_{eq} \leq 2$
Change in noise exposure ΔL_{eq}			
Mean	7.45	–3.59	–0.57
Minimum	3.10	–6.90	–2.00
Maximum	14.10	–3.10	2.00
Number of zip codes	10	24	102
Number of observations	7,397	11,051	123,775

Source: EMPA noise data, own calculations.
Notes: ΔL_{eq} is the change of daytime noise exposure from 2002 to 2004. L_{eq} is an equivalence metric corresponding to a steady sound level, measured in dB(A), for the 16-hour interval from 6:00 am to 10:00 pm that produces the same energy as the actual time-varying sound level. Units of observation are advertisements.

such that users can enter general parameters for their query, which include at a minimum the location of the apartment (community name, zip code, or region) plus optional information about the number of rooms, square meters, and rental price (in ranges that can be selected from a menu or entered manually). Apartments in the database that match the chosen parameters are shown in a list. More detailed information about each apartment, including the contact information of the owner, is only available after clicking on the link in the search results. Hence, a click on a particular item in the search results indicates at least a basic interest in the advertised apartment, but it is neither necessary nor sufficient for renting it because there might be other channels of renting the apartment and/or there are usually many applicants for the same apartment.

Our time frame starts in January 2002, 15 months prior to the policy intervention, and ends in mid-2010. We only keep listings for residential apartments for rent, and delete those for office space, parking places, and storage. We do not consider the property market because of low turnover rates (Werczberger, 1997), high relocation and transaction costs (Bayer *et al.*, 2009), and the low fraction of homeowners in the canton of Zurich (24.8% in 2000). Advertised rents typically correspond to actual rents in the Swiss housing market because landlords (often represented by professional real estate agencies) offer their apartments without possibilities for negotiation. After carefully checking for data consistency, we obtain a final sample of 142,223 observations (advertisements) in the canton of Zurich, which we use as the basis for our empirical investigation.[5] Data cleaning affected less than 0.3% of the total sample. Average rents by apartment size and the distribution of apartment sizes in the final sample are consistent with the information in the 2000 census. We therefore consider the data from homegate.ch as representative for the housing market in the canton of Zurich.

For every apartment listed, we observe the monthly rent (in CHF, including utilities), the exact date when the advertisement appeared online, the number of clicks per advertisement, the duration of the offer (in days), the size of the apartment (in square meters), the number of rooms, the year built, and the zip code. The exact address information is often missing or misspelled. As a consequence, street information could not be used for

5 We dropped duplicate advertisements and apartments with very low rents (less than CHF 100 per room and month) or very high rents (more than CHF 30,000 per month).

Table 2. Descriptive statistics by treatment region and time

	Treatment N^+		Treatment N^-		Control	
	Mean	SEM	Mean	SEM	Mean	SEM
A. Before flight regime change						
Apartment rent	1887.4	50.7	1532.8	23.2	1528.0	9.9
Clicks per day	470.3	56.6	304.6	36.4	406.4	12.8
Number of observations	356		519		7,270	
Number of zip codes	10		19		86	
B. After flight regime change						
Apartment rent	1818.8	9.3	1643.4	5.6	1720.5	2.9
Clicks per day	543.9	17.5	471.0	12.8	574.3	4.2
Number of observations	7,041		10,532		116,505	
Number of zip codes	10		24		102	

Source: Homegate advertisement data, own calculations. *Notes:* Apartment rents in Swiss Francs (CHF), average number of clicks per day registered on homegate.ch. SEM is the standard error of the mean.

geocoding. The next higher level of spatial resolution, the zip codes, are accurately recorded. We matched the housing data to the aircraft noise data based on the coordinates of the population-weighted centre of gravity for each zip code (provided by MicroGIS). One zip code usually corresponds to one municipality as the smallest political unit in the Swiss legislative system. Larger municipalities are divided into several zip codes; see Statistics Canton of Zurich (2015) for further information.

Descriptive statistics for our main outcomes, apartment rents and the number of clicks per day and advertisement, are displayed in Table 2. Panel A refers to the pre-treatment period, defined as before January 2003, because there is little indication for a policy intervention prior to that date (FOCA, 2003). Panel B refers to the post-treatment period, which for the moment we simply define as the entire period not classified as pre-treatment, i.e., from January 2003 until July 2010. Clearly, this separation is not sharp because the period includes two months of anticipation effects, the first change in flight regulations in April 2003, the second change at the end of October 2003, and finally the adjustment processes of the housing market afterwards. The timing of these events will be explicitly accounted for later in the analysis.

A first observation in Table 2 is that both treatment regions (N^+ and N^-) show higher pre-treatment rents than the control region, and in the case of the positive treatment, significantly so. Using the rough before/after comparisons, we find increases in rents for the negative treatment and for the control region, and a slight decrease for the positive treatment region. The number of clicks per day increased between the two time periods for all groups.

Given the information in Table 2, we can estimate two average treatment effects. First, the difference in pre- and post-treatment average rental prices between the treatment region N^+ and the control region gives a difference-in-differences (DID) estimate of the average treatment effect on the treated of CHF –261.1 [= (1818.8 – 1887.4) – (1720.5 – 1528.0)]. That is, the average apartment rent under the positive treatment (i.e., the region exposed to more aircraft noise due to the flight regime change) is about 261 CHF lower than that for the average control. Second, the average treatment effect on the negatively treated units (the region exposed to less aircraft noise) is estimated by the same approach as CHF –81.9. This seems rather implausible given that the absence of noise is usually valued positively, *ceteris paribus*, and the willingness-to-pay for an apartment should increase. While simple and straightforward to calculate, the assumptions underlying the basic DID approach, in particular the common trend assumption of treated and control in absence of the treatment, are likely not fulfilled in our context. We therefore refine our estimation approach in the following sections to account for the specific features of our data (imbalanced pre-treatment trends, region and time specific effects, heterogeneity).

3.5 Examining pre-treatment imbalance

We address the common trend assumption by looking at pre-treatment price developments and by selecting only those control units with similar pre-treatment trends as the treated. There are several reasons why the data pre-processing is important. First, our dataset is asymmetric in the sense that we observe advertisements for only 15 months prior to the treatment and for about seven years after. Second, the regions affected by the policy (positively or negatively) and the control regions are very heterogeneous. While the region in the south is characterised by expensive, upper-class neighbourhoods, in particular close to Lake Zurich (see Fig. 2), the residential region directly surrounding the airport and to its east is characterised by a more working-class population and housing in the middle to lower price categories. Third, investments in residential housing varied substantially over municipalities. While some municipalities have received little to no investment in residential housing over the last 10–15 years, others are boom areas where whole new neighbourhoods have been built.

Given that the raw data consist of repeated cross-sections (and not a panel), the unit of observation for balance checking is the average apartment rent and the average number of clicks per day aggregated on the zip code level, adjusted for the size of apartments. The upper part of Table 3 (Panel A) displays the summary statistics of the pre-treatment trends for the main outcomes (apartment rents, clicks per advertisement) aggregated on the zip code level for both treatment groups and the control group. We calculate the pre-treatment trend as the average over the second half-year 2002 minus the average over the first half-year 2002.[6] As a robustness check, we altered the time aggregation and compared the first quarter of 2002 with Q2–Q4 2002, and Q1–Q3 2002 with Q4 2002, which did not affect our results by much. We decided for the half-year separation, as this balances the number of observations in each period.

The average price trends per zip code (row 1) show that the two treatment and the control groups developed rather differently during the pre-treatment period. In particular, prices in zip codes located in region N^+ went down by CHF –14.5 on average, prices in zip codes located in region N^- by even more (CHF –72.7), but prices in the control region, on the contrary, increased by CHF 88.4 on average.

To eliminate the observed pre-treatment differences, we pre-process the data using a matching methodology developed by Iacus *et al.* (2011, 2012). They suggest a coarsened exact matching (CEM) where observations are assigned to strata (or bins). The CEM algorithm ensures that for every pair of treated and non-treated in a given stratum there exists at least one exact match. Unmatched observations are excluded from the analysis. The

6 We were not able to analyse shorter time intervals due to small sample issues (most importantly the sensitivity to outliers, e.g., very expensive apartments in a given month and zip code).

Table 3. Pre-treatment trends and CEM

	Treatment N^+			Treatment N^-			Control		
	Mean	#zips	$L1$	Mean	#zips	$L1$	Mean	#zips	#bins
A. Total sample									
Apartment rent	–14.53	9	0.29	–72.68	14	0.25	88.38	71	
Clicks per day	–327.88	9	0.31	74.55	14	0.26	–66.09	71	
B. CEM sample									
Apartment rent	79.91	4	0.00				80.42	4	500
Clicks per day	–159.30	5	0.00				–164.42	8	150
Apartment rent				8.63	8	0.00	7.58	11	280
Clicks per day				58.78	8	0.00	58.23	14	400

Source: Homegate data, own calculations. *Notes:* Pre-treatment trend is calculated as average per zip code over 2002 H2 minus average over 2002 H1. Coarsened exact matching (CEM) based on separate comparison of positive/negative treatment and control zip codes. $L1$ statistic to measure imbalance between treatment groups and control group as proposed by Iacus *et al.* (2011, 2012). $L1 = 0$ indicates perfect balance (up to the discretisation of the original variable into equal sized bins, number of bins in last column).

procedure allows us to constrain the imbalance in the pre-treatment trends, and the average treatment effect on the treated (ATT) is estimated by a (weighted) DID model using matched observations only.

We adopt the CEM strategy for two main reasons. First, Iacus *et al.* (2011, 2012) demonstrate that CEM outperforms alternatives like propensity score matching in balancing the data. They argue that the key advantage of CEM is to reduce imbalance as a prerequisite and not as a result (as most other matching algorithms do). Second, since our sample consists of repeated cross-sections, disaggregate data, and numerous treated units, the synthetic control approach of Abadie and Gardeazabal (2003) and Abadie *et al.* (2010) is not directly applicable. There are various other (matching) approaches that may be used in this context, but we opted for the CEM approach for its simplicity and straightforward implementation.

Table 3 (Panel B) shows the pre-treatment trends of our main outcomes after applying CEM. For the coarsening, we select the number of bins such that the $L1$ imbalance statistic of Iacus *et al.* (2011, 2012)[7] is zero, indicating perfect balance (up to the coarsening). The number of zip codes in the matched control group decreases sharply, and sample sizes of the three groups are almost balanced. Since we are interested in estimating ATTs, the restriction to fewer control units does not affect external validity. The balance in the pre-treatment trends between the treated and control groups is now extremely high. This is true for both outcomes (rents and clicks) and treatments (positive and negative). For example, the price increase for the positive treatment (row 4) is now CHF 79.9 in the treatment group, compared to CHF 80.4 in the control group.

7 Iacus *et al.* (2011, 2012) suggest checking imbalance using two multidimensional histograms from the cross-tabulation of characteristics. In our case, this is only one variable at a time, i.e., the pre-treatment trend of any of the outcomes in the treatment group and in the control group. The bins for the histogram are chosen in advance, with a larger number of bins yielding more precise matches. Imbalance is defined from the relative frequencies f_k for cells $1, \ldots, K$ in the treatment group and g_k in the control group as $L1 = 0.5 \cdot \sum_k |f_k - g_k|$.

The matching approach, however, also comes at a cost, as we lose some zip codes for both the positive and the negative treatment regions. Table A1 in the online appendix indicates that the matched treatment communities in the N^- region have rather similar characteristics compared to the full sample of treated apartments in that region. In contrast, matched treatment communities in the N^+ region are on average more expensive and receive less clicks than the treated apartments in the full sample. Thus, while the more restrictive matched sample in the N^- region might still allow drawing more general conclusions about the N^- region, drawing such conclusions for the N^+ region is more difficult. We therefore follow the suggestion of Iacus *et al.* (2012) and interpret the estimated treatment effect after applying CEM as local sample ATT (local SATT).

4. Estimation results

4.1 Difference-in-differences model

We specify the following difference-in-differences (DID) model with time-varying effects:

$$Y_{ist} = \alpha_s + \beta_t + \sum_{\tau=0}^{L} \delta_\tau D_{s,t+\tau} + \gamma' X_{ist} + \varepsilon_{ist}. \quad (1)$$

where Y_{ist} denotes the outcome variable (log apartment rents and number of clicks) for advertisement i in zip code s at time t. The model includes fixed effects (FE) for each zip code (α_s) and each half-year (β_t). $D_{s,t+\tau}$ takes value 1 for the treatment region in a specific post-treatment period, and 0 otherwise, and the corresponding parameter δ_τ measures the average treatment effect on the treated (ATT) for that period.[8] The vector X_{ist} summarises apartment-specific covariates and other variables used in all our specifications: dummy variables for the month of the year to control for seasonality patterns, dummy variables for the number of rooms, interactions of district and time FE to allow for differential time trends in the various districts of the canton,[9] and interactions of the number of rooms and time FE.

Despite having information about the exact dates when advertisements were uploaded, we need to aggregate the data in the time dimension for estimation purposes. A reasonable choice is to look at treatment effects over half-years. On the one hand, this ensures that we

8 Interpretation of δ_τ as ATT requires the stable unit treatment value assumption (SUTVA). It asserts that outcomes in one location are not affected by the treatment of another location. SUTVA would be violated, for example if N^- or control regions receive a large inflow of people after the increase in aircraft noise in N^+ regions such that the demand for rental apartments and consequently rents increase compared to the hypothetical situation without a change in flight regulations. Appendix Figs A1 and A2 indicate that the number of movers (incoming and outgoing) on the community level is basically constant in all relevant regions over the considered time frame. Figure A3 indicates that the proportion of vacant apartments slightly increased after 2003. However, this pattern was not specific to treated and matched control but rather part of a general development on the state level. While this is not a proof of the validity of SUTVA, we deem the graphs supportive of SUTVA in our context.

9 Our data consist of 11 districts in the canton of Zurich and 221 zip codes in total, excluding Zurich city. The number of zip codes in one district ranges from 12 to 32, about 20 on average. Because districts consist of treated and control zip codes, we can estimate the ATT even after controlling for interactions of district and time FE.

have sufficient observations per zip code and half-year t to reduce the sensitivity of estimates to outliers, and on the other hand, it still allows us to capture the adjustment dynamics in the housing market after the 2003 intervention in a rather flexible manner.

Due to the timing of the unilateral decree, the treatment period starts with the first half-year 2003, i.e., $D_{s,t+0}$ switches to one for the treated (positively or negatively) in that half-year. On 21 May 2003 the Federal Office of Civil Aviation decided to allow landings from the south on runway 34, and the new flight regulation took effect on 30 October 2003. Hence, the effect δ_0 must be considered as anticipatory, whereas treatment effects in the second half-year 2003 and thereafter ($\tau \geq 1$) are a consequence of the flight regime change. We allow for a relatively long adjustment period by including fifteen half-years post treatment ($L = 15$). We estimate eq (1) both in the CEM weighted sample and in the full sample.

4.2 Adjustment of apartment rents

Figure 3 depicts the time-varying ATTs for the positive treatment region (left panel) and the negative treatment region (right panel) in the full sample. For the *positive* treatment (more aircraft noise), the results suggest that there is a constant decline in the ATT over time, but the estimates are rather imprecise and the 95% confidence interval often does not preclude a zero effect. For the *negative* treatment region (less aircraft noise), there is almost no effect on apartment rents, neither in the short term nor in the long term.

Figure 4 depicts the dynamic local SATTs for the two treatment regions in the CEM weighted sample. As the new flight regime was first announced in March 2003, the first half-year 2003 is likely too early to show any effects on apartment rents, and indeed the results suggest no significant local SATT in 2003. At the beginning of 2004, the apartment rents started to react to the treatment. While rather small and statistically insignificant at first, we find a significantly negative treatment effect in the treatment region N^+ from 2004 on (left panel of Fig. 4). For the average apartment in that region, the decrease in rents amounts to about 13% approximately after two years (after the second half-year 2004).

The dynamic effects in the treatment region N^- (i.e., less aircraft noise) suggest that prices increased with a significant and constant markup of about 6 to 7% from 2005 on (see the right panel of Fig. 4). While the absolute magnitude of the long-term effect seems to be smaller compared to region N^+, the magnitude of the two effects must be related to the change in average noise exposure. The increase by 7.5 dB(A) on average in the positive treatment region compares to –3.6 dB(A) on average in the negative treatment region (see Table 1). Hence, the marginal effects on apartment rents per decibel aircraft noise are about the same size: on average about –1.7% per decibel increase of aircraft noise exposure.

The discrepancy in the results of Figs 3 and 4 may be explained by the likely inclusion of control units in the full sample that do not meet the critical DID assumption of a common time trend in the absence of the treatment. The inclusion of such units makes the estimation more imprecise (standard errors are about twice as large) and masks the dynamics in apartment rents.

Overall, we find compelling evidence that apartment rents converge to a new equilibrium, with significantly different prices. In the positive treatment region N^+ (more aircraft noise), the new equilibrium is reached after the second half-year 2004, about two years after the policy intervention. For the negative treatment region N^- (less aircraft noise), we also find an adjustment period of about two years. However, whereas the noise *increase*

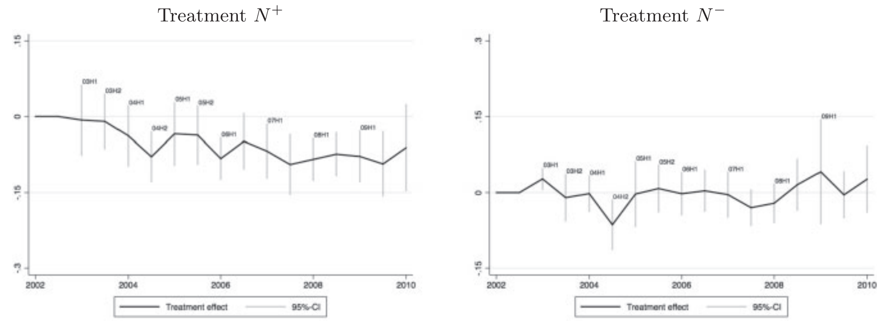

Fig. 3. Adjustment of log apartment rents—full sample
Notes: Vertical axis shows DID estimates in model for the log of apartment rents with the treatment (N^+ or N^- interacted with dummies for each half-year since January 2003). Model controls for zip code FE, time FE (half-years), month of the year (seasonality), number of rooms, interactions of district FE and time FE, and interactions of the number of rooms (5 categories) and time FE. 95% CI based on zip code cluster-adjusted standard errors.

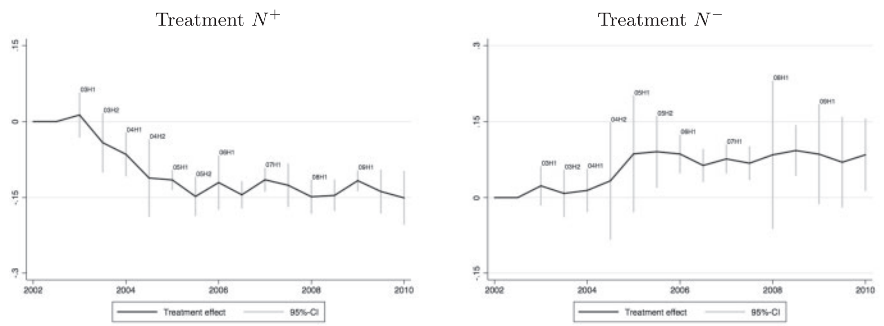

Fig. 4. Adjustment of log apartment rents—CEM sample
Notes: See Fig. 3. Estimation based on CEM weighted sample, treatment effects are local SATTs.

had an almost immediate effect on apartment rents (although small and insignificant at the beginning), the effect of the noise *decrease* showed a lag of about one year. In the following section, we confirm the two-year adjustment period by looking at a proxy for search behaviour, the number of advertisement clicks. We also investigated the impact of the intervention on several supply side measures, including the number of housing units (Greenstone and Gallagher, 2008), but did not find any significant effects. Results are available from the authors upon request.

4.3 Adjustment of advertisement clicks

In a next step, we analyse how people searching for apartments adjust their behaviour in response to the flight regime change. As an indicator for search behaviour, we use the number of clicks that the advertisement received per day online. Clicks have been used as a demand side indicator in several markets, including movies, video games, and music (Goel *et al.*, 2010), but also online retail (Baye *et al.*, 2009) and search advertising (Jeziorski and Segal, 2015).

The two treatment regions do not differ only in their exposure to aircraft noise (more versus less) but also in their attractiveness as a residential neighbourhood. The treatment region N^{+} in the south between the two lakes (Lake Zurich and Lake Greifen) is considered one of the most desirable regions to live in the whole canton of Zurich. This is reflected by the high number of clicks per advertisement and day registered on the homegate.ch website. After the introduction of the new flight regime, the region lost some of its attractiveness due to the additional aircraft noise. On the one hand, we would expect that the demand for housing decreases in response to this negative shock. On the other hand, there could be a stimulating market effect because (i) the possibility of price discounts in a desirable region attracts people previously unable to afford to live there, and (ii) media coverage increases public attention.

Figure 5 provides evidence for the stimulating effect. We estimate an increase in the number of clicks per advertisement and day immediately following the policy intervention. The increase is substantial, with about 300 to 350 additional clicks in the positive treatment region compared to the average control. Relative to the baseline of about 470 clicks (Table 2), this corresponds to an increase of more than 60 percent. The effect vanishes at about the same time as prices converge to the constant discount, i.e., the number of clicks are comparable to those in the control region about two years after the policy intervention and remain at that level thereafter.

In the negative treatment region N^{-}, we find a similar pattern in the number of clicks as for the positive treatment region. The increase in clicks is about 150 per advertisement and day (statistically significant at the 5% level), which corresponds to an increase of about 50% over the baseline of approximately 300 clicks per day.

4.4 Long-term pre-treatment trends and placebo effects

Two potential concerns in the above DID specifications are the relatively short pre-treatment period in the sample at hand and the heterogeneity in the developments (trends) of the outcomes of interest (rents and clicks). More specifically, for the CEM samples we have to assume that balancing the pre-treatment trends in 2002 (see Table 3) sufficiently removes all medium- to long-term differences in the trends of apartment rents and clicks. This might not necessarily be the case. Unfortunately, no data are available from homegate.ch prior to 2002, so we cannot test for the long-term pre-treatment trends of rents and clicks in our sample. Using data from the decennial housing census 1970 to 2000, we compiled average (log) rental prices for all communities included in our study to assess the long-term pre-treatment trends.

Figure 6 shows the pre-treatment developments for both positive (left) and negative (right) treatment regions and two subgroups of the control region, the matched CEM zip-codes and the unmatched controls. The graphs indicate that the treated groups and matched control groups share a common trend for both treatments over the entire time period. In contrast, when looking at the unmatched controls, we find a development in prices that is different to the treated group, in particular for the positive treatment. Figure 6 hence supports our DID approach because i) in general rental prices developed differently in the treated and control areas, but ii) the application of CEM helps to overcome these differences, even in the long term.

Our second concern is that there might be differences in the development of rental prices and clicks between the treated and control regions after the policy change that are not

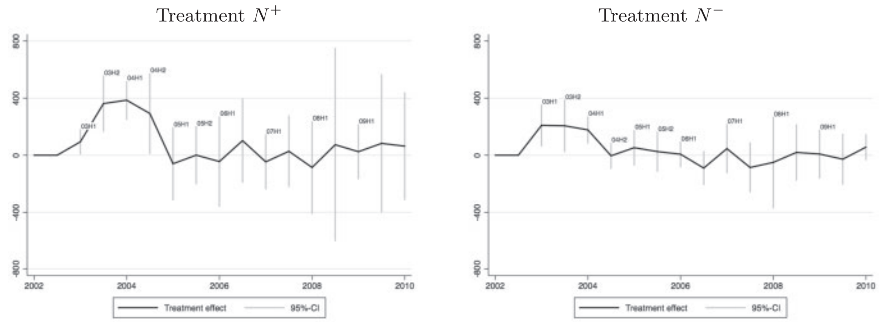

Fig. 5. Adjustment in number of clicks per day—CEM sample
Notes: See Fig. 3. Vertical axis shows DID estimates in model for the number of clicks per day. Estimation based on CEM weighted sample, treatment effects are local SATTs.

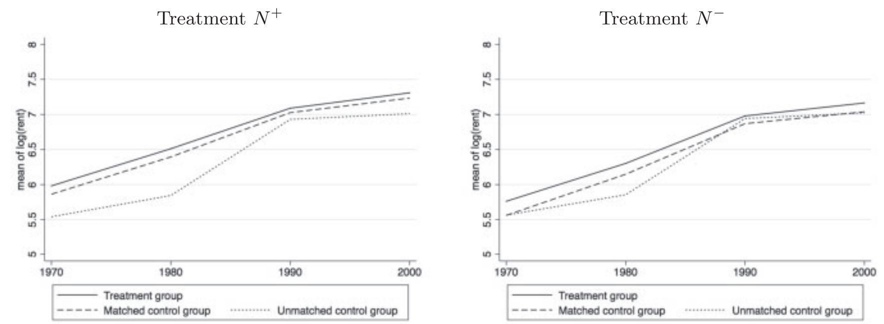

Fig. 6. Long-term pre-treatment trends for apartment rents
Source: Swiss Federal Statistical Office (BFS), own calculations. *Notes:* The sample includes all communities in the Canton of Zurich, positive and negative treatment and control assigned as in the main analysis.

caused by the change in flight regimes itself. More specifically, potential changes in price patterns in parts of the control group would invalidate our findings. In order to test for this, we conducted placebo studies where we randomly selected 25% of the matched control groups (for the positive and for the negative treatment) as the hypothetically treated group (placebo) and ran regressions identical to our core specification while restricting the sample to the matched controls, excluding the actually treated regions. The results are displayed in Table 4.

There is no evidence of placebo effects, except for a negative effect (significant at the 5% level) for the number of clicks and the positive placebo treatment. Given that the average effect in our core specification (+331.2; Fig. 5) is more than eight times larger in absolute terms, the estimated placebo treatment effect (–39.79) seems negligible compared to the mean in the control group.

5. Discussion and concluding remarks

Despite the knowledge that sources of frictions and imperfections cause the housing market to adjust slowly (Smith, 1974; Rosen and Smith, 1983; Wheaton, 1990), quasi-

Table 4. Placebo effects

	Apartment rent		Clicks per day	
	Total sample	CEM sample	Total sample	CEM sample
Treatment N^+				
ATT	0.00159	0.0235	36.73	−39.79
	(0.0158)	(0.0384)	(51.52)	(15.46)
Number of observations	124131	7350	28042	3358
Treatment N^-				
ATT	0.0218	0.0229	49.37	−5.430
	(0.0136)	(0.0173)	(42.09)	(21.66)
Number of observations	124294	20708	28205	5844

Notes: Zip code cluster-adjusted standard errors in parentheses. Placebo treatment for a random selection of 25% of the matched control groups. Sample for the number of clicks per day 2002–2004.

experimental papers on the hedonic valuation of non-market goods often ignore adjustment processes and implicitly assume immediate and constant effects; see Parmeter and Pope (2013) for an overview. Notable exceptions include Ahlfeldt and Kavetsos (2014) on the impact of sports facilities and McMillen and McDonald (2004) on the effect of a new transit line on housing prices. They find capitalisation effects that occur quickly after the effective implementation, or even in anticipation.

In this paper, we analyse the adjustments in the housing market after a large policy-invoked change in flight regulations at Zurich airport in Switzerland. The policy change altered the exposure to aircraft noise around the airport, leaving some communities with significantly more noise and some communities with less noise. Our results indicate that the market for rental apartments adjusted for about two years until a new equilibrium was reached, with relatively stable price differences between the treated and control regions afterwards. Specifications that ignore the adjustment period would tend to understate the targeted capitalisation effect. Our findings also suggest that online advertisements of apartments in both treatment regions (N^+ and N^-) attracted significantly more clicks during the adjustment period, indicating a higher search effort and increased market activity in the affected regions.

There are at least four possible explanations for the observed adjustment. First, there may be noise-based residential sorting. From our results we may conclude that after about two years noise-sensitive people have found a new apartment in a more quiet region. While it is plausible that the process of finding a new apartment takes time, in particular in the Greater Zurich area where demand is high and the rate of vacant apartments low, a non-negligible adjustment period due to preference-based sorting has implications for the translation of capitalisation effects into welfare measures. The reason is that the hedonic gradient may change over time and adjustment is not immediate after the policy intervention. Recent methods to link capitalisation effects with the public's marginal willingness-to-pay in a longitudinal setting (e.g., Kuminoff and Pope, 2014; Banzhaf, 2015) should take into account the timing of post-policy calculations. Second, there may exist uncertainty about how pronounced and lasting the change in aircraft noise will be at the time of the policy change. In line with Pope (2008), this would imply a delay until (i) the information becomes common knowledge, and (ii) the market reacts to the change. Third, there are legal restrictions that do not allow both landlords to change prices and tenants to move out of the apartment from one day to the other. Rental contracts are typically binding, with a period of notice of at least three months, sometimes even longer. Fourth, the behaviour of tenants may be characterised by adaptations, in particular in those regions affected by more noise (e.g., sleeping with closed windows, substitution of outside activities). Such adaptations would imply a change in the hedonic function after the change in flight regulations; see McMillen (2008) and Kuminoff and Pope (2014) for a related discussion. While our data do not allow us to further disentangle the likely related reasons, this would be interesting to address in future research.

Finally, our analyses indicate that a flexible DID model with time-varying treatment effects estimated in a sample of treated and carefully selected controls (using a matching approach that balances the pre-treatment trends) can provide a powerful econometric tool to identify adjustment processes. In our case, for example, the effects of aircraft noise on apartment rents and advertisement clicks would look quite different without using the data pre-processing in a first stage. This difference is highly important, as the policy implications are diametric. Coarsened exact matching (CEM, Iacus *et al*., 2011, 2012) can transparently eliminate observed differences in the pre-treatment time trends as required in the DID model, which is very much in the spirit of the recent methodological efforts to make the critical assumption of a common trend for all groups in the absence of the treatment more credible (e.g., Abadie, 2005).

Supplementary material

Supplementary material in the Appendix is available online at the OUP website. The core data used in this paper is confidential data obtained from two sources: homegate.ch (housing) and Zurich Airport (aircraft noise). However, any academic can contact both data providers to apply for the data. We have made available the STATA syntax files that reproduce the results we provide in the paper.

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