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Evaluating the effects of supervised consumption sites on housing prices in Montreal, Canada using interrupted time series and hedonic price models

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HIGHLIGHTS

- Few studies evaluate the effects of supervised consumption sites (SCS) on home prices.
- Study used interrupted time series with hedonic price models and spatio-temporal lags.
- Homes sold <200 m of SCS may experience price shocks immediately after implementation.
- Steady price gains following price shocks consistently observed.

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ABSTRACT

Background: In 2017, three brick and mortar supervised consumption sites (SCS) opened in Montreal, Canada. Opponents argued the sites would attract people who use drugs and reduce local real estate prices.
Methods: We used interrupted time series and hedonic price models to evaluate the effects of Montreal's SCS on local real estate prices. We linked the Quebec Professional Association of Real Estate Brokers' housing sales data provided by Centris Inc. with census tract data and gentrification scores. Homes sold within 200 m of the SCS locations between 1 January 2014 and 31 December 2021 were included. We adjusted for internal (e.g., number of bed/bathrooms, unit size) and external attributes (e.g., neighbourhood demographics), and included a spatio-temporal lag to account for correlation between sales. For sensitivity analysis we used site-specific dummy variables to better account for unmeasured neighbourhood differences, and repeated analyses using 500 m and 1000 m radii.
Results: We observed a price shock after the opening of the first two SCS in June 2017 (level effect: −10.5%, 95% CI: −19.1%, −1.1%) but prices rose faster month-to-month (trend effect: 1.1%, 95% CI: 0.7%, 1.6%) after implementation. Following the implementation of the third site in November 2017 there was no immediate impact (level effect: 2.4%, 95% CI: −10.4%, 17.0%) but once more prices roses faster (0.9%, 95% CI: 0.4%, 1.5%) thereafter. When we replaced neighbourhood attributes with a site-specific dummy variable, we observed the same pattern. Sales' prices dropped (level effect: −9.6%, 95% CI: −15.0%, −3.8%) but rose faster month-to-month (trend effect: 0.9%, 95% CI: 0.6%, 1.2%) following June 2017's SCS implementations, with no level effect (4.9%, 95% CI: −7.3%, 18.6%) and a positive trend (0.9%, 95% CI: 0.5%, 1.3%) after November 2017's SCS opening. In most 500 m and 1000 m radii models, there were no immediate shocks following SCS opening, however, positive trend effects persisted in all models.
Conclusion: Our models suggest homes sold near SCS may experience a price shock immediately post-implementation, with evidence of market recovery in the months that follow.

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1. Introduction

Montreal, Canada, introduced three brick and mortar supervised consumption sites (SCS)[1] as part of its harm reduction program in June (n=2) and November (n=1) 2017, with the aim to reduce the negative effects of illicit drug use (Strike & Watson, 2019). Federally approved and staffed by medical professionals trained in addictions medicine, SCS provide critical overdose reversal services, overdose education and naloxone distribution, sterile drug use equipment and disposal of used items, primary care services, safe injection practices and wound care education, and housing and employment support (Government of Canada, 2018).

Despite extensive evidence demonstrating the benefits of SCS on the health and well-being of people who use drugs (PWUD), SCS remain politically controversial (Potier et al., 2014). Local politicians, residents and business owners resist their implementation (Cruz et al., 2007; Small , 2007). Opponents believe SCS attract PWUD to the sites' neighbourhoods (the 'honey-pot effect') and argue this influx of PWUD increases local crime, contributes to physical and aesthetic deterioration, and reduces property value (Kolla et al., 2017; Williams & Ouellet, 2010). Proponents of SCS try to alleviate concerns of the honey-pot effect citing evidence that SCS clients typically reside within 500 m of the site (Marshall BDL et al., 2011; The Evaluation of Overdose Prevention Sites Working Group & Lori Wagar, 2018) and stress sites are situated in high-risk neighbourhoods with a known PWUD population (Supervised Consumption Services Review Committee & Alberta Health, 2020). Supporters also refer to the handful of studies that show no changes in property crime, marginal increases in small-scale drug-dealing, and reductions in public drug use after SCS implementation (Freeman et al., 2005; Kennedy et al., 2017; Kimber et al., 2005; Wood et al., 2006; Wood et al., 2004). This is supported by recent studies that found no visible increase in drug use, a decrease in signs of homelessness (Davidson et al., 2023), and a decrease in neighbourhood crime (Davidson et al., 2021) within 500 m of a newly opened SCS.

Less easily addressed are opponents' skepticism on the generalizability of results, given most studies have focused on the effects of harm reduction interventions in Vancouver's Downtown Eastside (home to Canada's only SCS prior to 2016), and researchers' disregard for "the interests of larger communit[ies]" (Kolla et al., 2017). Even where community stakeholders acknowledge that these facilities have positive health effects, 'not in my backyard' (NIMBY) resistance persists. Opponents focus on SCS' potential to induce negative effects on their communities' quality of life more than public drug use; and maintain that the downstream consequences are reduced small-business patronage and home values. Given NIMBY sentiments stem from complex social, cultural and political perspectives, it is important to understand the effects of harm reduction interventions on local communities (Bosque-Prous & Brugal, 2016). With minimal exploration of the effects of SCS on neighbourhoods' housing values (Liang & Alexeev, 2023); these perceived threats have repeatedly barred agencies from operating SCS in high-risk communities (Guye , 2021; Supervised Consumption Services Review Committee & Alberta Health, 2020).

Some proponents of SCS believe we should not dedicate critical research efforts to an ostensibly moral question, citing the positive impacts of SCS on PWUD as sufficient justification for their operation. However, we believe it is the scientific community's responsibility to examine the effects of the intervention on the wider population because doing so may reveal key features in the design of SCS and the services they provide (e.g., dedicated social spaces for PWUD post-consumption, higher capacity, extended hours of operation, routine needle/syringe neighbourhood sweeps) that contribute to SCS' successful integration within local communities, and what may cause friction. This research can also alleviate some of the tension expressed in Kolla et al.'s 2017 study by adopting a cross-sector perspective. The alternative, not exploring the effect of SCS on communities, may erode long-term support for SCS - ultimately undermining the intervention's sustainability.

Considering the ongoing tensions, we tested the hypothesis that SCS have an effect on residential real estate prices in Montreal using the city's three recently implemented fixed sites as a natural experiment.

2. Methods

2.1. Study setting

Montreal is an ideal setting to study the effects of SCS on residential real estate. The city has one of the largest and most complex PWUD populations in Canada. Approximately 4000 people inject drugs (Leclerc et al., 2014), the proportion of people who inject daily remains high, and roughly 50% of PWUD consume drugs other than opioids (Bruneau et al., 2012). Further, the PWUD population is geographically scattered (Green et al., 2003), a necessary condition for the purported honey-pot effect. Finally, unlike Vancouver and Toronto's housing prices which grew incredibly quickly in recent years, Montreal's market has enjoyed steady but modest gains, making it more representative of other Canadian cities and less likely to obfuscate the effects of SCS on residential real estate prices.

2.2. Study design

We used a multiple-interventions interrupted times series study design with segmented regression to account for the difference in trends in sales prices pre-SCS implementation. Although the impact of COVID-19 on real-estate markets remains unclear, early evidence from the United States and Norway suggest public health lockdowns and other local forces may have exacerbated price trends (Anundsen et al., 2023; Gamber et al., 2023). Hence, aside from accounting for the effects of the SCS on local real estate prices, we incorporated the potential effect of COVID-19 fiscal measures in our model.

We included residential real estate sales records for homes sold within 200 m of the three SCS locations between 1 January 2014 and 31 December 2021. This radius was selected to study local effects of SCS. In the Canadian urban context, a 100 m radius equates to a land surface area of approximately 31,415 m^2 which is between two and three times the size of traditional city blocks (12,000–15,000 m^2) (Raymond, 2020). By doubling the radius, we captured the effects on more than eight neighbourhood blocks. The observation period was selected to allow sufficient time to observe the effects of SCS on prices post-implementation, while limiting the potential for large changes in neighbourhood demographics. We considered using sales within 200 m of men's homeless shelters as potential controls given similarities in client demographics (e.g., age, employment status, mental health needs), many SCS are based in shelters, and similar 'not in my backyard' resistance dominates discussions of new homeless shelter sites (Oakley, 2017). However, we found sales price trends pre-June 2017 and neighbourhood demographics to be too different to justify their use as controls.

Residential real estate sales data were provided by Centris Inc. which captures over 89% of all residential real estate sales for the island of Montreal. Records included each home's listed and purchased price, the duration on the market, number of bedrooms and bathrooms, total living space and property size, year of build, and noteworthy features (e.g., number of parking spots, separate garage, historical building). As this database includes sales that were not at 'arm's length' (e.g., property with a sale price of CAD $1), we removed outliers whose final sale price was less than $20,000. We also excluded units with no listed total living space, fewer than two or more than twenty rooms, where number of bathrooms was missing or zero, number of bedrooms was missing, number of bathrooms or bedrooms exceeded the total number of rooms

[1] List of abbreviations: CAD: Canadian dollar, NIMBY: "not in my backyard", PWUD: people who use drugs, SCS: supervised consumption sites

listed, and sales with missing civic numbers or street names (Fig. 1).

We linked the sales records with Statistics Canada's 2016 census tract data summarizing neighbourhood demographics (i.e., median age of population, average household size, median total income, and proportion of the population that are visible minorities, did not complete secondary school, have a post-secondary education, and are unemployed) (Statistics Canada, 2023). We included the Canadian Urban Environmental Health Research Consortium's gentrification measures which identified areas at risk of gentrification in 2006 and those that underwent gentrification within Canada as of 2016 (Firth et al., 2020). We used the Grube-Cavers indicator which was developed based on census data from 1961, 1971, 1981, 1991, 1996, 2001, and 2006 of Toronto and Montreal, as well as Vancouver's census data from 1986 onwards. A census tract is deemed 'gentrifiable' if "a) the average family income and b) the percent of college degrees are below the metropolitan average" (Firth et al., 2020). A census tract is deemed 'gentrified' if at the subsequent census, the following indicators increased more than the metropolitan area: "average monthly rent, family income, percent of degrees, percent of owner-occupied dwellings, and percent of people in professional occupations" (Firth et al., 2020). This was adapted by the INTErventions, Research, and Action in Cities Team (INTERACT) to also include the proportion of university degrees in the 'gentrified' indicator (Firth et al., 2020).

2.3. Statistical analysis

Because sales prices were not normally distributed, we used the semi-log functional form for our models. This form allows the logarithm to be made for either the dependent or independent variables and enables simple interpretation of outputs.

There is a substantial body of literature that uses hedonic price models to identify both internal and external attributes that affect housing prices. Using these models, studies demonstrate buyers will pay a premium for proximity to desired amenities (e.g., schools, commercial centres, revived city centres) (Ding et al., 2020; Dubé et al., 2017) and sellers will incur a penalty for proximity to dis-amenities (e.g., airports, homeless shelters) (Batóg et al., 2019; Galster et al., 2004). The relationship between amenities and housing prices is so robust that *announcements* of future amenities impact housing prices (Mense & Kholodilin, 2014; Yen et al., 2018).

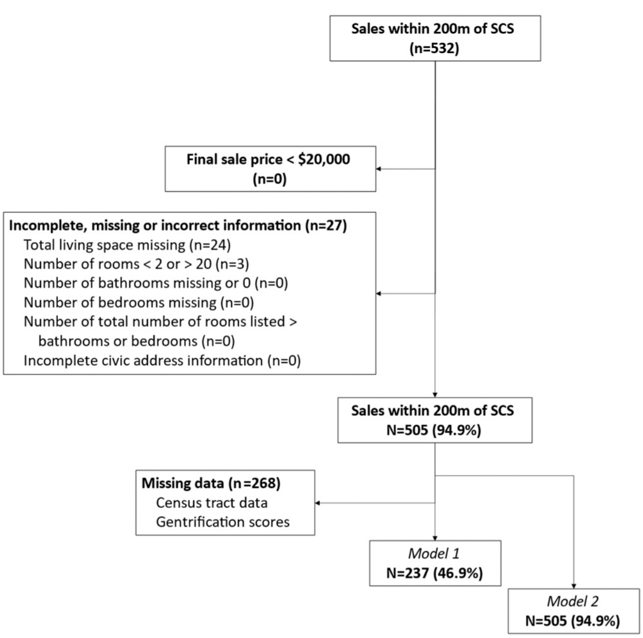

Fig. 1. Flowchart of sales within 200 m of SCS included in subsequent analysis.

For our primary regression model, the β estimates represent percentage differences in the closing price, not dollar amounts:

$$\ln\left(price_{jkt}\right) = \beta_0 + \beta_1 time_t + \beta_2 level_j + \beta_3 level_j time_t + \beta_4 level_k + \beta_5 level_k time_t + \beta_6 level_m + \beta_7 level_m time_t + \sum \beta_8 X_{jkmt} + \beta_9 lag_{jkmt} + \varepsilon_{jkmt}$$

Where β_0 is the semi-log price intercept; β_1 is the time trend t before any SCS were implemented; β_2 and β_3 describe the level and trend changes post-SCS implementation in June 2017 relative to the pre-intervention period, respectively (first intervention, j); β_4 and β_5 describe the level and trend changes post-SCS implementation in November 2017 relative to the period between June and November 2017, respectively (second intervention, k); β_6 and β_7 describe the level and trend changes post-COVID-19 policies relative to the period after November 2017, respectively (third intervention, m); $\sum \beta_8 X_{jkmt}$ is a vector of internal and external housing attributes (e.g., floor size, number of bed/bathrooms, proximity to SCS, neighbourhood features); β_9 is the spatial-temporal price lag; and ε_{jkmt} is the residual error term.

Distances between sales records and treatment location were confirmed using geocoding and distance matrices in QGIS. Addresses were geocoded to retrieve geographic coordinates using Nominatim/Open Street Maps. Coordinates were then used to create distance matrices between treatment and control locations, and sales records.

We created a spatio-temporal price lag because sales' prices are influenced by the closing prices of neighbouring sales (geographic proximity) and the period of the sale (temporal trends including seasonality). To create the spatio-temporal price lag for each sale, we used Higgins et al.'s proposed methods (Higgins et al., 2019). Briefly, we calculated the spatial proximity between each combination of sales [i,j] using each sale's geo-coordinates and the Euclidean distance method. Based on results from the variogram, we determined the spatial effects of neighbouring transaction j on closing price of sale i became negligible beyond 2400 m and set this as our cut-off. Combinations of sales [i,j] in the same complex were given a correlation value of 1, sales within the cut-off were assigned the inverse of their distance, and sales beyond the 2400 m threshold for correlation were assigned a value of 0 influence. We then pooled the values between each sale combination's spatial weight into matrix S. To account for temporal associations between sales [i,j] we applied Dubé and Legros' 2013 method (Dubé & Legros, 2013). We sorted all the sales in chronological order and allowed for temporal associations between transactions i and j that occurred up to 12 months in the past, and six months in the future. Doing so compensates for plausible 'anchoring' behaviour where property owners set their asking price not just on prices secured for comparable sales in the past, but on future expectations as well (Higgins et al., 2019). We pooled each sale combination's temporal weight into matrix T. Then using the Hadamard product on the matrices ($S \odot T$), we created the spatio-temporal weight matrix W. We normalized W using spectral transformation for row-standardization (Higgins et al., 2019) before applying the weight to a matrix of all closing prices to create each sales' price lag.

Data were organized and models were run in R (version 4.2.2) within R Studio (version 2023.03.0) using tidyverse, readxl, openxlsx, tableone, olsrr, car, ape, spdep, geoR, lubridate, leaps, twang, scales, readr, data.table, and flextable packages.

2.3.1. Sensitivity analyses

As part of our sensitivity analysis, we re-ran our analyses using the same model but adding site-specific dummy variable (β_9, n) to better account for unmeasured neighborhood attribute differences between the sites:

$$\ln\left(price_{jkt}\right) = \beta_0 + \beta_1 time_t + \beta_2 level_j + \beta_3 level_j time_t + \beta_4 level_k + \beta_5 level_k time_t + \beta_6 level_m + \beta_7 level_m time_t + \sum \beta_8 X_{jkmt} + \beta_9 site_n + \beta_{10} lag_{jkmt} + \varepsilon_{jkmt}$$

We also re-ran our primary and dummy variable models expanding the radii of interest to sales within 500 m and 1000 m of each site to discern potential varying effects across larger distances.

2.4. Ethics review

This study was exempt from ethics review by McGill University's Institutional Review Board.

3. Results

Between January 2014 and December 2021 (inclusive) there were 505 homes sold within 200 m of any SCS. Of these 505 sales, 4 (0.8%) met our definition for house flipping – homes purchased and resold within two years; none of which were flipped more than once (i.e., sold three times with less than two years between sales intervals). The mean closing price pre-intervention (June 2017) was $275,819 (Table 1). Sales pre-intervention were on average 142 m away from future SCS locations, had 1.03 bathrooms, 1.83 bedrooms, and an average floor size of 88.22 m^2. Across neighbourhoods, average household size was 1.70 people; 19.19% of the population were visible minorities, and 11.85% had not completed secondary school education.

Our primary model found the price of homes sold within 200 m of SCS incurred an immediate shock following the opening of the first two sites in June (level effect: −10.5%, 95% CI: −19.1%, −1.1%) but no shock following the third site's opening in November (level effect: 2.4%, 95% CI: −10.4%, 17.0%). Prices rose faster month-to-month after June (trend effect: 1.1%, 95% CI: 0.7%, 1.6%) and November (trend effect: 0.9%, 95% CI: 0.4%, 1.5%) compared with the periods just before. We observed no level effect (-0.3%, 95% CI: −10.5%, 9.9%) but a downturn in sales trends (-0.9%, −1.6%, −0.1%), following COVID-19 policy implementations (Model 1 in Table 2; full model in Supplementary Table 3). When we replaced neighbourhood demographic covariates with site-specific dummy variables, we observed once more a negative price shock for homes sold within 200 m (level effect: −9.6% 95% CI: −15.0%, −3.8%) after June, and no effect following November's SCS implementation (level effect: 4.9%, 95% CI: −7.3%, 18.6%). As before, there was a month-to-month increase (trend effect – June: 0.9%, 95% CI: 0.6%, 1.2%; November: 0.9%, 95% CI: 0.5%, 1.3%) following each SCS implementation; with no level effect (1.3%, 95% CI: −8.2%, 6.2%) and a negative trend effect (-0.7%, 95% CI: −1.2%, −0.1%) following the implementation of COVID-19 policies (Model 2 in Table 2; full model in Supplementary Table 3).

Table 1
Comparison of house and neighbourhood features of sales within **200 m** of treated sites, *prior to implementation of any SCS* (1 January 2014 – 30 June 2017).

	Treated units N=87
Closing price, mean (SD)	$275,818.94 ($90,306.64)
Housing features, mean (SD):	
No. of bathrooms	1.03 (0.18)
No. of bedrooms	1.83 (0.70)
No. of extra rooms	2.53 (1.35)
Floor size (in m^2)	88.22 (28.12)
Distance to closest shelter/SCS in 100 m	1.42 (0.49)
Neighbourhood demographics, mean (SD):	
Age of the population	37.34 (2.02)
Household income in $1000	$28.24 ($3.81)
Household size	1.70 (0.11)
Proportion of population, %:	
Indigenous[b]	1.17
Visible minorities[c]	19.19
Without secondary school completed[d]	11.85
With postsecondary education	73.08
Unemployed (rate)	7.69
Gentrification measure	
Gentrifiable in 2006[e]	0.40 (0.49)
Gentrified in 2016[e]	0.26 (0.44)

[a]Naturalized Canadian citizens, permanent residents, temporary residents
[b] First Nations, Métis, Inuk and/or Registered or Treaty Indians and/or membership in a First Nation or Indian band
[c] Persons, other than Indigenous persons, who are non-Caucasian
[d] No certificate, diploma, or degree
[e] Using the Grube-Cavers indicator

Table 2
Results of variations of interrupted time series for sales within **200 m** of SCS, adjusted for housing attributes and spatio-temporal price lag.

	Model 1[a] (95% CI)	Model 2[b] (95% CI)
Intercept	**$149,180 ($2863, $7,773,446)**	**$153,019 ($140,293, $166,899)**
Time	1.002 (0.999, 1.004)	**1.002 (1.001, 1.004)**
Level – SCS implementation (June 2017)	**0.895 (0.809, 0.989)**	**0.904 (0.850, 0.962)**
Trend – SCS implementation (June 2017)	**1.011 (1.007, 1.016)**	**1.009 (1.006, 1.012)**
Level – SCS implementation (November 2017)	1.024 (0.896, 1.170)	1.049 (0.927, 1.186)
Trend – SCS implementation (November 2017)	**1.009 (1.004, 1.015)**	**1.009 (1.005, 1.013)**
Level – COVID-19	0.997 (0.905, 1.099)	0.987 (0.918, 1.062)
Trend – COVID-19	**0.991 (0.984, 0.999)**	**0.993 (0.988, 0.999)**

bold indicates statistical significance
[a] Controlling for housing and neighbourhood attributes with spatio-temporal price lag
[b] Controlling for housing attributes with spatio-temporal price lag, clustered by SCS

3.1. Sensitivity analysis

When we repeated our analyses using sales within 500 m and 1000 m of SCS we observed similar patterns in price trends but immediate price shocks weakened. When controlling for measured neighborhood demographics, sales within 500 m had no level effects (June: −3.2%, 95% CI: −8.1%, 2.0%; November: 6.2%, 95% CI: −1.1%, 14.0%) but trend effects persisted (June: 0.9%, 95% CI: 0.7%, 1.2%; November 2017: 0.7%, 95% CI: 0.3%, 1.0%). For sales within 1000 m, lack of level effect persisted (June: 1.6%, 95% CI: −0.9%, 4.1%; November: 0.7%, 95% CI: −2.6%, 4.1%) and trend effects (June: 0.3%, 95% CI: 0.2%, 0.4%; November: 0.3%, 95% CI: 0.2%, 0.4%) were attenuated. In our models using site-specific dummies instead, the level effects (June: −7.0%, 95% CI: −11.0%, −2.8%; November: 6.4%, 95% CI: −0.6%, 11.4%) and trend effects (June: 1.0%, 95% CI: 0.8%, 1.2%; November: 0.8%, 95% CI: 0.6%, 1.1%) observed for sales within 200 m of SCS persisted for homes sold within 500 m. Likewise, for homes sold within 1000 m, level (June: −2.3%, 95% CI: −4.7%, 0.1%; November: 3.1%, 95% CI: −0.2%, 6.6%) and trend (June: 0.3%, 95% CI: 0.2%, 0.3%; November: 0.2%, 95% CI: 0.1%, 0.3%) effects were muted.

4. Discussion

Between January 2016 and June 2022, 32,632 Canadians died of opioid toxicity (Special Advisory Committee on the Epidemic of Opioid Overdoses, 2022). Despite the ongoing urgency of the overdose crisis and plethora of studies demonstrating SCS' effectiveness in mitigating drug use related morbidity and mortality, communities continue to resist their implementation. Our study directly examined one ongoing aspect of opponents' arguments against SCS – that their presence impacts residential real estate.

Our outputs suggest homes sold within 200 m of SCS potentially incurred a negative price shock immediately following the implementation of the first two SCS which includes the largest SCS located in the densest neighbourhoods. These results imply neighbourhoods experience a penalty similar to the 5–7% observed by Liang and Alexeev following the implementation of Victoria, Australia's first SCS (Liang & Alexeev, 2023). However, unlike Liang and Alexeev, we also observed positive price trends sufficient to close the price gap within nine months. This positive trend in price observed across all models, including in sensitivity analyses using 500 m and 1000 m radii, may reflect SCS' impact on local crime and drug-use related public nuisance (i.e., fewer reports of public injections and publicly discarded syringes) summarized in a recent systematic review (Levengood et al., 2021).

Prior research demonstrates the effects of dis-amenities are distance dependent. For example, housing prices within the first 400 m of a commuter train station decline, while prices immediately outside this radius (e.g., 400 – 800 m) but still within 'walking distance' of the station, increase (Dubé et al., 2017). Marshall et al.'s seminal study observed that over 70% of Vancouver's Insite clients lived within four blocks of the supervised consumption site (Marshall BDL et al., 2011), and more recent evaluations of SCS have observed impacts on health service use within 500 m of sites but not beyond (The Evaluation of Overdose Prevention Sites Working Group & Lori Wagar, 2018). For this reason, we focused on the 200 m radius as our main analyses and explored the effects of SCS on sales within 500 m and 1000 m, respectively, as part of our sensitivity analyses (Supplementary Tables). Our trend findings were robust across all models. Level effects after initial SCS implementation were robust within 500 m. This suggests we need to identify and test design features of successfully integrated SCS that may reduce the risk of price shocks, as well as consider policies to compensate local homeowners for the public good gained from SCS.

Our study had several strengths. We used advances in econometrics to account for traditionally neglected spatio-temporal correlation for a more nuanced examination of consumers' revealed preferences. By accounting for the two-dimensional correlation structure of sales data we reduced bias in our outputs which can otherwise lead to overestimation of the intervention effect. We also examined our records to understand the magnitude and potential effect of house flipping – and observed limited impact of this phenomenon in our sales records. By focusing on a large city with a dispersed population of PWUD and the effects of SCS across multiple neighbourhoods, we were able to account for the potential honey-pot effect and reproducibility of our results, respectively. Further, by selecting a city that was not experiencing a frenzied housing market during much of the observation period, we reduced the potential for housing market trends to obscure the effects of the intervention. Using over three years of pre- and post-implementation data sufficiently powered our study to observe very small effects of SCS on real estate price trends. The multiple-interventions interrupted time series approach allowed us to estimate the effects of the SCS and COVID-19 measures on housing prices.

The study also had multiple limitations whose effects we aimed to mitigate. First, of the 505 sales that were within 200 m of a SCS and met our data quality screening criteria, only 237 (46.9%) were successfully linked with census tract and gentrification data. This limited our ability to adjust for neighbourhood attributes to a small subset of sales. As a work around, we also conducted analysis using site-specific dummy variables (Model 2) which allowed us to control for both observed and unobserved neighbourhood attributes. We were unable to identify control neighbourhoods whose attributes sufficiently matched treated neighbourhoods'. Thus, we relied on pre-intervention trends of the same communities as sufficient controls and limited our primary analysis to sales very close to the intervention sites to reduce the risk of other co-occurring interventions affecting our models. Given the sites were implemented in highly urban communities, we lacked sufficient sales to restrict our analysis to within 100 m of the SCS. This would have been the preferred scale of analysis to identify truly localized 'backyard' effects. However, we did identify statistically significant impacts on local prices within 200 m. This radius also aligns closely to Marshall et al.'s study which noted the majority of clients come from within four blocks of SCS (Marshall BDL et al., 2011). Despite SCS operating in almost every province in Canada, we were unable to secure sales records from other cities. This may affect the generalizability of our findings. Finally, although we note a positive trend in monthly housing prices post-implementation, we can only postulate on why. Elsewhere, SCS have been shown to improve local communities' physical environments via reductions in public drug use and drug-related litter (Freeman et al., 2005; Wood et al., 2004).

Our study provides evidence that the implementation of SCS can have a negative immediate effect on local residential real estate prices, but that this effect may correct over time. However, more research is needed to understand the mechanisms behind this effect, whether it is unique to the Montreal context, and the long-term sustainability of price trends observed post-implementation. Nevertheless, our results provide a valuable contribution to the current debate surrounding SCS and their impact on local communities.

Author disclosures

Role of funding source

This study was supported by an Insights Development Grant from the Social Sciences and Humanities Research Council of Canada (Grant # 430-2022-00061) to DP. DP is also supported by Tier 2 Canada Research Chair in Economics of Harm Reduction. The funders of the study had no role in the study design, data collection, data analysis, data interpretation, or writing of the report.

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CRediT authorship contribution statement

Dimitra Panagiotoglou: Conceptualization, Funding acquisition, Methodology, Writing – original draft, Writing – review & editing. **Maximilian Schaefer:** Formal analysis, Methodology, Software, Visualization, Writing – review & editing.

Declaration of Competing Interest

We declare no competing interests.

Data Availability

Due to commercial restrictions, supporting data is not available.

Appendix A. Supporting information

Supplementary data associated with this article can be found in the online version at doi:10.1016/j.dadr.2024.100242.

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