Learning About Lemons:
Consumption After Peers' Negative Experiences

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Abstract

We show that individuals reduce consumption of a brand's products if they learn of a peer's negative (i.e., "lemon") experience with the brand. We use warranty claims on consumer durable goods by close neighbors to identify peers' negative experiences. We implement a hyper-local geographic difference-in-differences empirical framework to compare the consumption of very close neighbors to slightly more distant neighbors. Our "lemons" setting differs from the existing consumption peer effects literature, which typically examines how positive (i.e., "peach") experiences of peers increase peer consumption. Because of this setting, we can attribute these estimated peer effects to the social learning channel.

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1 Introduction

Consumers face a lemons problem (Akerlof, 1970) when there is asymmetric information between a seller and a buyer regarding the quality of the product being sold. Various mechanisms have developed by which prospective buyers can learn about unobserved product quality to alleviate this asymmetric information problem (Nelson, 1970). This paper examines one possible mechanism through which consumers can learn about unobserved product quality: their peers' experiences of a good (i.e., "word of mouth"). If a consumer has a negative quality experience of a good and then shares this negative experience with a peer, that negative feedback may influence the peer's future purchase behavior.

A variety of conflicting arguments have been proposed concerning the importance of this "word of mouth" quality disclosure mechanism. On the one hand, Akerlof, in his 1991 Ely Lecture, argued that consumers place a very high weighting on "word-of-mouth" information regarding negative product quality, even though this information from a single peer may be idiosyncratic and inaccurate in the aggregate.[1] On the other hand, Dranove and Jin (2010) in their literature survey on quality disclosure mechanisms argue that more formalized third-party disclosure mechanisms (e.g., disclosure that is government-mandated or industry-sponsored) "provide more precise and comparable information than word-of-mouth" (p. 939). This implies that word-of-mouth information about negative product quality is less useful and therefore less likely to affect consumption behavior.[2] Given these conflicting views on the value of word-of-mouth information regarding product quality, our aim in this paper is to provide causal evidence on the extent to which word-of-mouth information on product quality affects consumers' purchase decisions.

To answer this question, we study the effect of warranty claims on consumer durable goods (e.g., refrigerators, dishwashers, laundry machines, etc.) made by peers. We examine how such warranty claims by peers impact consumers' future purchases of the claimed brand's products. Consumer durables are a classic example of goods whose quality is not ex-ante known to consumers, i.e., goods

[1] Akerlof (1991) (p. 2) describes a thought experiment, where a car consumer determines that Volvo is a high quality brand using aggregate data published in "Consumer Reports." However, an interaction with a single salient peer, who has had a negative experience with the quality of Volvo cars biases the consumer against the Volvo brand.

[2] Dranove and Jin (2010) provide a survey of the various kinds of quality disclosure mechanisms, including those directly provided by the producer (e.g. branding, warranties, licensing), government-mandated quality disclosure (e.g. for airlines, cars, hospitals, restaurants, bank products), industry-sponsored voluntary quality disclosure (e.g. hospitals, movies, universities), and reviews in independent media (e.g. Consumer Reports, The Wirecutter).

where consumers may face a "lemons" problem. A key innovation of our research is that we use warranty claim filings as an indicator of a negative consumer experience with a product. We study whether this negative quality information propagates via word-of-mouth by studying the effect on the purchases of claimed brand products by immediate neighbors of the warranty claimants. Our main finding is that a negative quality experience with a brand (as measured by a warranty claim) significantly lowers the subsequent purchases of that brand by the claimant's peers.

The most important contribution of our study, relative to the existing literature on consumption peer effects, is that we examine how *negative* information sent by the information-sending peer (the warranty claimant) causes information-receiving peers to *reduce* their consumption of the claimed brand's products. The existing consumption peer effects literature, in comparison, typically examines how the choice by one peer to consume a product causes another peer to *increase* consumption of that product. To extend the metaphor of Akerlof (1970), we focus on negative product information transfer between peers (i.e. where the products are "lemons") while most of the existing literature examines information transfer between peers regarding positive product information (i.e. where the products are "peaches").[3]

Providing causal evidence on peer effects regarding "lemons" (rather than "peaches") is important for two reasons. First, our study allows us to shed light on the substantive disagreement between Akerlof (1991) and Dranove and Jin (2010) regarding the magnitudes and importance of consumption peer effects, specifically in cases when the information flows concern the low quality of "lemons".

Second, our "lemons" setting, allows us to specifically study the social learning channel of peer interactions. Broadly speaking, the recent consumption peer effects literature[4] has attempted to differentiate between at least four channels of peer interactions in the standard (i.e., "peaches") setting. These are: (1) the social learning channel, where the receiver learns the information from the sender through the direct interaction between them, (2) the social utility channel, (e.g., "keeping up with the Joneses" type preferences), where a peer receives utility by owning a product owned by

[3]Peer effects for "peaches", where the receiving peer increases consumption, have been extensively documented in economics. While the primary literatures have been focused on consumption (e.g., Agarwal et al., 2020; Bailey et al., 2022; D'Acunto et al., 2024; De Giorgi et al., 2020; Bertrand and Morse, 2016; Kuhn et al., 2011; Moretti, 2011, etc.) and financial products (e.g., Brown et al., 2008; Bursztyn et al., 2014; Duflo and Saez, 2003; Heimer, 2016; Hong et al., 2004; Kaustia and Knüpfer, 2012; Ouimet and Tate, 2020; Pool et al., 2015, etc.), these peer effects have been studied in numerous settings (Hacamo and Kleiner, 2022; Maturana and Nickerson, 2019, e.g.,).

[4]See recent surveys, e.g., Kuchler and Stroebel (2021); Gomes et al. (2021); Mobius and Rosenblat (2014).

another peer, (3) the imitation channel, where the receiver simply imitates the visible consumption of the sender without any direct communication between them, and (4) the network effects channel, where a product (e.g., a software platform) is used because it provides access to an existing network. It has been challenging to empirically disentangle these channels in the standard consumption peer effects ("peaches") setting.

In our "lemons" setting, on the other hand, the only one of these channels that is applicable is (1) the social learning channel. This is becasue, in a "lemons" setting we can *a priori* rule out the social utility channel, the imitation channel, and the network effects channel, because, in all of these channels, an increase in consumption by the sending peer will generate an *increase* in consumption by the receiving peer. In our setting, if the receiving peer learns about the low quality of the "lemon" from the warranty claimant, it should lead to a *reduction* in her consumption. Because the direction of the social learning effect in our setting is in the opposite direction to the other channels, our "lemons" setting enables us to isolate and study the effect of the social learning channel.

We also contribute to the literature on the measurement of product quality. While it is widely agreed that product quality is crucial for consumption decisions, a major challenge in empirical studies of consumption is that product quality is typically unobservable to the econometrician. Our paper introduces a new measure of product quality, warranty claim filings, to the literature.[5] Unlike much of the existing literature, our warranty claim measure of product quality does not require subjective opinions (e.g., from consumers or from "expert" third parties). The one caveat with our new warranty claim measure of product quality is that an individual consumer's perception of quality could be multi-faceted, whereas we only observe one, albeit important, element of quality: the emergence of a post-purchase defect in a product that results in a warranty claim.

As argued by Kuchler and Stroebel (2021), consumption peer effects are not only important in terms of understanding consumer choices, but also in understanding market-level outcomes for brands. An important contribution of this paper is that we directly address the impact of peer effects on market-level outcomes because we measure the impact of the peer effect on the claimed brand relative to all other brands. Bailey et al. (2022) is the only other study which also examines

[5]As product quality is challenging to observe, various measures have been used in the literature, including; (1) self-reported quality by consumers (e.g., Busso and Galiani, 2019), (2) the evaluation of quality by a "skilled quality assessor" (e.g., Atkin et al., 2017), (3) the assumption that brands with a higher market share conditional on price have a higher quality (e.g., Khandelwal, 2010), and (4) experimentally varying quality in an RCT (e.g., Bold et al., 2022).

how consumption peer effects generate changes in market-level outcomes.[6] An important difference between Bailey et al. (2022) and our paper is that they examine a "peaches" setting, while we examine a "lemons" setting.

The data we use to implement our empirical analysis are provided by a major Canadian consumer durable goods retail store. This retailer provided us with detailed information on all purchases of and warranty claims on consumer durable goods filed between 1998 and 2009, which amounted to six million sales transactions across over 200 brands along with 95,000 warranty claim filings. Each observation includes details such as purchase and warranty claim dates and purchaser and claimant home addresses.

To identify the causal effects of social learning from warranty claimants' negative experiences on consumers' purchasing behavior, we leverage the precise geographic location of consumers and precise claim and purchase dates provided by our data to conduct an empirical analysis employing a hyper-local difference-in-differences empirical methodology. This identification strategy has been used extensively in urban economics which also exploits the richness of fine-grained spatial data.[7] In our specification, we examine how information flows from the information-sending peer (the warranty claimant, who provides negative information about product quality to her peers) to information-receiving peers who are close neighbors of the information sender.

The main identification assumption of this hyper-local identification strategy is that very close neighbors of the information sender will receive the information from the sender, i.e. they will be treated, and neighbors who are "slightly further away" from the information sender will not receive the information, i.e., they will be the control group. Following the literature using this hyper-local identification strategy, we assume that whether a household chooses to live in the treated group (inner ring neighbors between 0 m and 100 m of the warranty claimant in our main specification) or in the control group (outer ring neighbors between 100 m and 200 m of the warranty claimant in our main specification) is as good as random. This argument rests on the typically-thin housing markets at this very granular scale (within 200 m) at the time when the household originally arrived in the neighborhood. The advantage of this identification strategy is that, given the very close

[6] Bailey et al. (2022) argue that "within the literature that has studied peer effects in product adoption decisions, we are the first, to our knowledge, to identify important competitive spillovers to other models and brands." (p. 492)

[7] See Anenberg and Kung (2014); Ang (2020); Bayer et al. (2021); Campbell et al. (2011); Currie et al. (2010); Gupta (2019); Kalda (2020); Kuhn et al. (2011); Linden and Rockoff (2008); McCartney and Shah (2022); Baum-Snow et al. (2024), among others.

distances between the treatment and control group households, all of them can be assumed to be similar in terms of neighborhood-level observable and unobservable variables. Thus the hyper-local identification strategy is able to address concerns about differences in unobservable characteristics of treatment and control groups.

As in all peer effects studies, we need to address the well-known “reflection problem” of Manski (1993), which essentially concerns pinning down the exact direction of information flowing between peers. To address the reflection problem, we adopt a strict sample selection strategy, whereby our analysis sample only includes neighborhoods with: (1) exactly one warranty claim event over the entire sample period (i.e. exactly one information-sending neighbor), and (2) no inner ring (i.e. treated) or outer ring (i.e. control) households that overlap into the inner and outer rings of any other warranty claimant. These sample selection procedures effectively require that any information-receiving household (in either the inner or the outer ring) can only receive information about the warranty claim from exactly one information-sending close neighbor (the warranty claimant).

Our main baseline finding is that a warranty claim filing significantly reduces the consumption of the claimed brand’s products relative to other brands’ products by inner ring (treatment group) households relative to outer ring (control group) households. We also find larger peer effects in poorer neighborhoods and low population density neighborhoods. We do not find any significant heterogeneity in the magnitude of the estimated peer effects across two aggregate brand-level measures of quality: (1) brand claims-to-sales ratio and (2) brand market share. Nor do we find evidence that regional business cycles significantly alter our estimated peer effects. These latter findings are consistent with the Akerlof (1991) proposition that the impact of a single salient (but possibly unrepresentative) peer on consumption choices outweighs more aggregate and systematic considerations (e.g., observable brand characteristics and regional economic conditions).

2 Data and Sample

Our data consists of the universe of sales transactions of appliances and other durable goods, as well as the universe of warranty claims made by purchasers for an anonymous Canadian retailer of consumer durable goods, spanning the period from 1998 to 2009.[8] The durable goods retailer has a market share that is among the top four in Canada. It has more than 270 individual stores (which are typically “big box” stores) across all regions in Canada. Prior to data cleaning procedures, our

[8] This data is also used in Bošković et al. (2024).

database contains 6.54 million sales transactions of more than 35,000 different products to over 3 million consumers.

2.1 Sales Data

The sales transactions data provide us detailed information for each product purchased from the retailer. The dataset includes attributes such as product brand, product category (e.g., kitchen, laundry, TV, video, stereo), product code (e.g., refrigerator, dryer), model series number, price, date of purchase, and a unique identifier for each sales transaction.

During the data cleaning process, some observations were dropped due to missing information or data imputation errors. As a result, our cleaned dataset consists of 5.9 million observations on individual sales transactions. The summary statistics of the sales data are presented in Table 1a. Our sales data also includes a unique customer identifier, which allows us to track each consumer's purchases over time. The consumer has an incentive to provide identifying information (including name and address) at the point of sale in order to be able to claim on a warranty for that product.

While all products are typically covered by a manufacturer's warranty (of approximately one year), the consumer also has an option to purchase an extended warranty. As can be seen in Table 1a, an extended warranty is purchased in 36% of all sales transactions. Importantly, regardless of whether a warranty claim is made on a manufacturer's warranty or an extended warranty, the claim is treated identically (i.e., which products will be repaired, who conducts the repairs, and the extent of repairs). As such, in this paper we treat them as interchangeable.[9]

A key element of our sales data is our ability to observe the brand of every product sold. Our sales data includes more than 200 brands, with Table 1b providing a listing of the top 30 brands. Our ability to observe brands for each transaction is central to our methodology because, as described in detail below, our dependent variable consists of the number of sales of the claimed brand minus the number of sales of all other brands in each neighborhood.

2.2 Warranty Claims Data

Our dataset also includes all the warranty claim records during the sample period. A warranty claim record can be linked with the sales transactions data as described above using the unique sales transaction identifier. Within the warranty claims data, we have access to the exact date

[9]Bošković et al. (2024) uses this data to examine the pricing of extended warranties, while in this paper we examine how a warranty claim (on either an extended or manufacturers warranty) impacts neighborhood peer effects.

when each warranty claim occurs, which we define as the claim event date. The summary statistics of all claim events are reported in Table 1c. After cleaning, we have approximately 95,000 claim events in our data.

As explained in detail below, for reasons related to the identification of peer effects and the reflection problem, we only retain a subset of claim events for our analyses.[10] Due to our strict sample selection criteria, we are left with 1,776 warranty claim events for our analyses. The warranty claims data summary statistics reported in Table 1c show that the full data sample of claim events and the analysis sample exhibit similar observable characteristics.

2.3 Geographic Neighborhood Data

For each consumer, we possess information on their home address's six-digit postal code. Canadian six-digit postal codes represent an extremely small geographical area, with the average postal code containing approximately 19 households, and the median postal code containing 13 households. We use the six-digit postal code to identify the geographic location of every individual (both warranty claimants as well as treated and control neighbors) in our study. Because we observe the postal code of every individual in our study, we can calculate the geographic distance between all individuals in our study. As we describe below, we use these geographic distances to define very close (inner ring) neighbors as treated households and slightly further away (outer ring) neighbors as control households. A variety of other papers (e.g., Agarwal et al., 2020; Baum-Snow et al., 2024) also use Canadian six-digit postal code data to measure the locations of individuals as well as the distances between individuals.

In addition to the six-digit postal code, we also make use of slightly larger geographical areas known as a Dissemination Areas (DAs). Each DA has a population of between 400 and 700 individuals. DAs are the smallest geography in Canada for which we observe detailed census data. We are able to match every six-digit postal code to the appropriate DA using the Post Code Conversion File provided by Statistics Canada.

We use the DA-level census data to capture the heterogeneity of demographic characteristics across neighborhoods. The summary statistics of the DA-level census data are shown in Table 1d. In this table, we compare the demographic variables for DAs that are included in our main analysis sample (based on the sample selection criteria described below) with all other DAs in Canada. As

[10] In particular, we only retain claim events where there is a single claim recorded in that neighborhood and no treated or control neighbors appear in neighborhoods of other warranty claims.

can be seen from this Table 1d, the observable demographic criteria for our sample of DAs included in our analysis are very similar to those demographic characteristics of all the other DAs in Canada.

3 Methodology and Identification

In this section, we describe the empirical methodology we employ to identify the consumption peer effects of negative product quality information. First, we explain our use of warranty claims as an exogenous introduction of negative product quality information. Next, we present the regression framework we use to identify the causal impact of warranty claims on the claimants' peers' subsequent purchasing behavior. We also explain how we use claimants' and their neighbors' postal codes to carefully define treatment and control groups. Finally, we lay out how we overcome two potential problems to which our regression framework may expose us.

3.1 Warranty Claims as Information Shocks

One of the main threats to identification of consumption peer effects comes from homophily within peer groups that have been endogenously formed. Because the peers have endogenously selected into their peer group, any observed similarities in consumption between peers (e.g., both peers consume the same product) could be because of peer homophily rather than actual peer effects (i.e., where the sending peer directly transfers information to the receiving peer). One way to overcome this threat to identification is to exploit an information shock which impacts only the sending peer and not the receiving peer. The key identification assumption of this strategy is that the receiving peer can only learn about the information shock through their interactions with the sending peer.

In our paper, we use the warranty claim filed by the sending peer as evidence of a negative information shock experienced by that peer. The negative product quality information generated by this shock, available exclusively to the sending peer, can only be transmitted to the receiving peer by their interactions with the sending peer. While the warranty claim event is obviously endogenously determined for the sending peer, the warranty claim event is also a plausibly exogenous information event for all receiving peers, whose consumption choices are the focus of our study. Our identification strategy thus sets up an environment where the receiving peer can only learn about the low quality of the product through contact with the sending peer.[11]

[11]Our use of the warranty claim event as an information shock is somewhat similar to the consumption peer effects study of Bailey et al. (2022). While we employ a warranty claim as the result of a negative information event that

We implement two key elements in our empirical design to study negative information peer effects as described above. First, we use a hyper-local differencing identification strategy, which uses very close (inner ring) neighbors of the warranty claimant as the treated group, and neighbors slightly further away (outer ring) as the control group. Second, to address the reflection problem (Manski, 1993), we carefully choose our sample to include only those neighborhoods with exactly one warranty claim and no overlapping inner and/or outer rings from other warranty claimants' neighborhoods. We discuss both elements in detail below.

3.2 Hyper-local Differencing

In our setting, we are interested in the flow of information (about warranty claim filings) via word-of-mouth between the warranty claimant and her neighbors. Given the spatial nature of our question, we follow the hyper-local differencing procedure for defining treated and controlled neighbors. A substantial literature in various branches of spatial and urban economics has used this procedure, in contexts such as: (1) real estate [12], (2) health [13], (3) crime [14], and (4) environment [15]. This approach has also been used in previous peer effects studies, where, as in our setting, the peers are neighbors [16]. Our study, like all of the aforementioned literature, exploits fine-grained (hyper-local) geographic location data to assign neighboring households to treatment and control groups. In addition, most of these studies, much like ours, exploit an information shock event that impacts the sending peer but not the receiving peers. We implement this neighbor assignment process using the very fine-grained six-digit postal codes available to us.[17]

The aim of this hyper-local methodology is to compare the impact of a plausibly exogenous information shock (in our case the warranty claim by the information-sending neighbor) on infor-

happens to the sending but not receiving peer, they examine the selection of a new phone (due to an inadvertent destruction of the previous phone) as a positive information event that happens to the sending but not receiving peer.

[12] Anenberg and Kung (2014); Bayer et al. (2021); Gupta (2019); Pool et al. (2015); Campbell et al. (2011); Linden and Rockoff (2008)

[13] Currie et al. (2010, 2011); Currie and Tekin (2015)

[14] Ang (2020)

[15] Currie et al. (2015, 2011)

[16] Agarwal et al. (2020); Bayer et al. (2021); Baum-Snow et al. (2024)

[17] Our Canadian neighborhood setting is similar to papers such as (Agarwal et al., 2020; Baum-Snow et al., 2024) who also use Canadian six-digit postal codes when implementing a hyper-local identification strategy, but the questions in these papers are quite different from ours.

mation recipients who are geographically "very close" (inner ring) to the information event (i.e., the treated group) to the impact of the same information shock on potential information recipients who are geographically "slightly further away" (outer ring) from the information event (i.e., control group).

Our primary specification may be represented as follows:

$$Y_{i,c,t} = \beta_1 Inner_{i,c} + \beta_2 Post_{t,c} + \delta Inner_{i,c} \times Post_{t,c} + \gamma_i + \gamma_t + \gamma_c + \gamma_\tau + \varepsilon_{i,c,t}, \tag{1}$$

where $Y_{i,c,t}$ is a measure of the purchases of the claimed brand relative to all other brands in postal code i in the neighborhood of claimant c in the two years before and after the claim filing ($t \in \{0, 1\}$), $Inner_{i,c}$ indicates whether postal code i is geographically close to warranty claimant c's postal code, $Post_{t,c}$ indicates whether the two-year time period t occurs before or after claimant c files their warranty claim, γ_i, γ_t, γ_c, and γ_τ are postal code, claim-relative time period, warranty claimant, and warranty claim quarter fixed effects, respectively,[18] and δ is the coefficient of interest.[19] If word-of-mouth peer effects affect consumption, we predict that a warranty claim event will reduce the subsequent purchases of the claimed brand relative to all other brands for neighbors living in postal codes that are very close to the claimant's postal code relative to neighbors living in slightly further away postal codes, i.e., we predict that δ is negative.

3.3 Treatment and Control Neighbors

An important feature of all studies using this hyper-local methodology is the use of very fine-grained geographic data to define treatment and control groups. This allows us to address two important econometric issues: (1) endogenous selection into treatment and control groups and (2) differences in neighborhood characteristics between treatment and control groups. We discuss each in turn below.

3.3.1 Endogenous Selection by Neighbors into Treatment and Control Groups

An important challenge in any definition of treatment and control groups is to ensure that there is no systematic selection bias into either group, i.e. that individuals do not (or cannot) choose to

[18]Another way of describing our analysis, due to the warranty claimant fixed effect we employ, is to note that each neighborhood can be considered as a separate sub-experiment with a single event. Our specification essentially stacks all of these different sub-experiments in a way that is similar to Cengiz et al. (2019); Deshpande and Li (2019).

[19]Given that we implement postal code and claim-relative time period fixed effects, our regression analysis does not estimate β_1 or β_2. We include these two terms in our specification for completeness.

be a part of either the treatment or control group. This issue can be potentially very problematic in spatial or geographic settings, where it is possible for an individual to endogenously select into living in a specific neighborhood. For instance, a household may choose to be in a specific area because the observable or unobservable characteristics of the people in that area (e.g., ethnicity, political beliefs) match those of the household.

We argue that our hyper-local differencing strategy is able to address this issue. The argument, frequently made by the literature cited previously, is that, while the individual may endogenously select into a neighborhood because of the characteristics of the neighborhood, whether the individual chooses to live in specific sub-regions (such as the postal codes used to define our treatment and control groups) can be considered "as good as random." Because the treatment and control groups are quite small and defined based on relative distance to the same information event (in our primary specification, the centroids of postal codes assigned to the treatment and control groups have to be in contiguous 100 m concentric rings around the claimant postal code), the distance between treatment and control households is quite small. Often, control group households are on "the other side of the road" from their treatment group counterparts (as postal codes often change from one side of a road to the other). As such, given the geographic tightness of our group assignments, while an individual may choose the wider neighborhood they live in, residential real estate markets are typically sufficiently thin at the scales at which our treatment and control groups are defined that the specific locations of the housing available in the neighborhood at the time that they were searching is nearly impossible to control. Therefore, an individual cannot reasonably select into our treatment or control groups and, as a result, their group assignment can be considered "as good as random."

3.3.2 Neighborhood Unobservables Impacting Treatment and Control Groups

A related problem in any spatially-defined treatment and control groups, is that a neighborhood unobservable (e.g. a mass layoff by a large neighborhood employer, or neighborhood specific preferences for certain products or brands) could impact the treatment and control groups differently and, if correlated with the variables of interest, threaten causal identification. Once again, we follow the literature in arguing that our methodology addresses this issue, specifically because of very fine-grained spatial definitions of treatment and control groups. Because of these very small spatial areas, any unobservable neighborhood feature should impact both the treatment and control groups in very similar ways. For instance, an event such as a mass layoff, a gas leak, or neighborhood-specific preferences, should exert a nearly identical influence on both the treatment and control groups, given their geographic proximity. Thus, we effectively nullify the impact of such unobservables by comparing our treatment group households to their very proximate control group households.

3.4 The Reflection Problem

As we describe above, while the warranty claim is endogenously determined by the claimant, the plausible exogeneity of the information shock (the warranty claim) to receiving peers in both the treated and control groups is important for causal interpretation of results. This is the reflection problem (Manski, 1993), which notes a potential issue for peer effect studies related to the direction of information flow between the peers. In our setting, this could pose a problem if multiple households in the same neighborhood file warranty claims at different times. If that happened, it would be difficult to determine which warranty claim by which peer had a causal impact on the purchase decisions of their peers (i.e., all peers could be both information senders and information receivers).

3.4.1 Sample Selection Criteria to Address the Reflection Problem

To address the reflection problem, (i.e., to pin down that information only flows from a sending peer to the receiving peers) we impose two sample criteria. These are: (1) only include those neighborhoods who have exactly one warranty claim during the whole period, and (2) only include those neighborhoods where the inner ring and outer ring postal codes (and households) do not overlap spatially with the inner or outer ring postal codes around any other warranty claim.

By using these sample criteria, we ensure that both the treatment and control group neighbors of a warranty claim are impacted only by that warranty claim. Because we have no overlapping (inner and/or outer) rings across different warranty claim neighborhoods, we can ensure that no other warranty claim event could be contaminating the flow of information to the information-receiving neighbors in the treatment and control groups.[20]

These sample selection criteria are clearly very restrictive in that, from an initial population of over 95,000 claims, we must exclude nearly 98% of claims. However, without these sample criteria,

[20]There is, of course, a chance that a neighbor receives information about another warranty claim from a (more distant) warranty claimant or via some other social network. But, given the geographic proximity of the treatment and control groups to each other, it is unlikely that the likelihood of receiving such information would be systematically different for these groups. As such, it should not bias the coefficient estimates in our analyses.

the causal interpretation of our analysis (and any others like it) is suspect. Therefore, a valuable advantage of our data is that our original sample covers a sufficiently long time period across all of Canada that, even after we impose these strict criteria, we still have 1,776 warranty claims and their neighborhoods on which to perform our analysis.

3.4.2 Using This Sample Selection Criteria to Address Staggered Events in DiD

The sample selection criteria also allow us to address another econometric issue: estimation bias due to staggered events in difference-in-differences (DiD) empirical models. A large recent literature has raised concerns about DiD models with staggered timing of events when there are heterogeneous effect sizes across treatments.[21] The concern is that using an "already-treated" unit as part of the control group may be problematic because, if effect sizes are heterogeneous, then the effect of the previous treatment on the control will be unclear. Various solutions have been proposed for this problem, including only using control group units that are "later-treated" or "never-treated".

In our setting, if we were to use our full sample (rather than the carefully chosen sample), then we would face these problems related to staggered event timing. This is because the full sample includes many neighborhoods with multiple warranty claim events, made by multiple neighbors, on multiple dates. As a result, many of our control group households would be "already-treated" units.

However, as our analysis sample only includes neighborhoods with exactly one warranty claim event over the entire 10-year period, within each warranty claim's neighborhood, we do not face multiple staggered events over time, but only a single event over the whole time period. In addition, our second sample criterion (no postal codes reside in multiple warranty claims' neighborhoods) ensures that that no other warranty claim events can influence these neighbors. Effectively, for each warranty claim, we run a "sub-experiment" wherein we compare the effect of that unitary warranty claim on treatment group units (inner ring postal codes) against its effect on control group units (outer ring postal codes). There are no other warranty claim events that may affect the treatment status of these units at other dates. In other words, our control group households are effectively "never-treated" units and our treatment group households are only treated once. For these reasons, because of our use of the criteria-created analysis sample, our analyses are not exposed to the problems facing staggered DiD analyses.

[21] See Goodman-Bacon (2021); Baker et al. (2022); Cengiz et al. (2019) and many others.

3.4.3 Using This Sample Selection Criteria to Provide Evidence on Akerlof (1991)

While the sample selection procedure suggested here, of using *exactly one* claimant, is specifically designed to address various econometric issues (i.e. the reflection problem and staggered DiD), we argue that it is also advantageous in our setting for substantive economic reasons. In particular it allows us to provide direct evidence on the proposition of Akerlof (1991), described above. This proposition states that a *single* salient peer will have a large weighting on the consumption choices of the information receiving peer. Thus, our sample selection criteria of restricting our sample to neighborhoods with *exactly one* information sending warranty claimant allows us to provide direct evidence on the Akerlof (1991) proposition, which examines the impact on consumption of a *single* salient peer.

4 Baseline Specifications

4.1 Dependent Variable

Our main hypothesis predicts that the warranty claim on a specific brand by a neighbor will reduce the sales of the claimed brand relative to the sales of other, unclaimed brands. In our baseline specification in Equation 1 above, we stipulate that our dependent variable should be some measure of the sales of the claimed brand (in the specified time and place) relative to the sales of *all other* unclaimed brands (in that same time and place). Two possible versions of an outcome variable that would allow us to test our hypothesis are: (1) net sales, which is sales of the claimed brand *minus* sales of all unclaimed brands, and (2) market share, which is sales of the claimed brand *divided by* sales of all unclaimed brands.

We choose to use the net sales measure as the market share ratio measure does not work well for us because, in our setting, it is often missing because of a divide-by-zero problem. Our unit of analysis is the postal code-quarter and a large number of postal codes in the neighborhoods in our study (approximately half) have zero transactions of *any* brand in a specific quarter.[22] Because the total sales in these postal code-quarters are zero, these observations have undefined market share values and, as such, have to be dropped from our analysis if we use the market share ratio as the

[22] In our analysis sample, we observe transactions for 6,948 postal code-quarters when we use the net sales (subtraction) measure but, because of the divide-by-zero problem, we only have data for 3,687 postal code-quarters using the market share ratio measure.

dependent variable. For this reason we use the net sales measure (based on subtraction rather than division) as our dependent variable.

4.2 DiD Assumptions: Parallel Pre-Trends

In this section, we provide evidence of parallel pre-trends using a dynamic DiD specification, with the $t-1$ quarter omitted. We examine four quarters before and after the warranty claim event quarter. Our results for these dynamic DiD tests are reported in Table 2. These results report on 12 different specifications with varying ring sizes (the width of the treatment and control rings) and separations (the gap between the treatment and control rings).

Our key finding from Table 2 is that, across *all* of the ring size and separation specifications,we find no significant coefficients in pre-claim quarters, strong evidence consistent with the parallel pre-trends assumption. In addition, Table 2 shows significant negative coefficients, as predicted by our main hypothesis, in many post-claim quarters, especially in quarters 3 and 4 after a claim. We plot the dynamic DiD coefficients for column 1 of Table 2 in Figure 2. The figure also shows that the claim event has a negative effect on peers' purchasing behavior for the claimed brand right away, though it becomes statistically significant only in quarters 3 and 4. Equally importantly, the figure confirms that there is no difference in pre-claim purchasing behavior between inner and outer ring neighbors, confirming the parallel trends assumption.

4.3 Choosing Treatment and Control Ring Width and Separation

When selecting the parameters for our hyper-local DiD methodology-based analysis, we have to make two important decisions regarding our inner and outer rings (i.e., our treatment and control groups): the widths of our rings and the separation between them. The extant literature using this methodology does not provide much guidance on the selection of these parameters and, often, papers in the literature seem to select such parameters somewhat arbitrarily. For this paper, we select these parameters to ensure that our treatment and control groups are as similar as possible and that our treatment group is defined as cleanly as possible, even at the cost of weakening the statistical power of our tests and our ability to identify the peer effect we are studying.

When choosing the width of our rings, our prime consideration is ensuring that the inner ring best reflects the true treatment group. In an ideal experiment, we would know which neighbors talk to the claimant about their warranty claim[23] and place those neighbors in a treatment group. However, we do not know which neighbors talk to the claimant. Rather, we possess spatial data showing the location of claimants and their neighbors. We therefore choose our treatment group on the basis of these location data only. Under the reasonable assumption that closer neighbors are more likely to interact with each other, the smaller the geographic distance is between a claimant and a neighbor, the more likely they are to have discussed the warranty claim. Therefore, as the ring of postal codes closest to the claimant is composed of the neighbors most likely to talk to the claimant, we define our treatment group as the inner ring of households closest to the claimant.

The width of this inner ring determines how cleanly it captures the treatment group. With a narrower inner ring, the likelihood of each member household of the ring having interacted with the claimant is higher than with a wider ring, where the more distant households are increasingly less likely to interact with the claimant. On the other hand, with a narrower inner ring, the size of the treatment group becomes smaller, which weakens the statistical power of our tests. For this research, to be as conservative as possible and at the risk of weakening the statistical power of our tests, we choose, the narrowest possible ring widths for our inner and outer rings: 100 meters.

The prime consideration when choosing the separation between our inner and outer rings is to ensure that the treatment and control groups are as similar to each other as possible, across both observable and unobservable characteristics. In an ideal experiment, where we randomly assign neighbors to treatment and control groups, the randomization ensures that treatment and control group members are similar. In our setting, the relative thinness of residential real estate markets ensures that two areas relatively close to each other geographically contain households that are comparable to each other. As such, if we choose to directly abutt our inner and outer rings, we improve the comparability of our treatment and control groups (across observables and unobservables) by reducing the distance between any two households in the inner and outer rings.

However, having no separation between the rings also increases the likelihood of contaminating our control group by including households who did interact with the claimant, and contaminating our treated group by including households who did not interact with the claimant. Thus this lack of geographic separation between the rings reduces the contrast between the rings and weakens our ability to identify the peer effect in question. In a DiD setting, increased cross-contamination between the treatment and control groups reduces the ability to identify the impact of the treatment on the treatment group. One obvious way to reduce this contamination in our setting is to increase

[23]Of course, in a truly ideal experiment, we would assign who talked to the claimant, as well.

the distance between the rings.

Once again, in this paper, to be as conservative as possible, we choose to have no separation between our inner and outer rings. This choice may bias our estimated peer effect downward, because the lack of separation between the rings increases the possible cross-contamination across the treatment and control groups but, more importantly, it maximizes the comparability of our treatment and control group households.

4.3.1 Empirical Evidence on Ring Size and Separation

In Table 3 and Figure 3, we plot the DiD coefficients of a large number of specifications varying across two dimensions: (1) width of the inner and outer rings (both inner and outer rings can be 100 m, 200 m, 300 m, or 400 m in width) and (2) separation distance between the inner and outer rings (0 m, 100 m, 200 m).[24]

Our main finding from Figure 3 is that the DiD coefficient is largest (in absolute value) for the thinnest-sized inner and outer rings (as indicated in Figure 3 by the red line with inner and outer rings of 100 m thickness), despite our concern about statistical power due to smaller sample sizes. In addition, the DiD coefficient is relatively constant regardless of the separation distance between the two rings, as indicated by all the lines being relatively flat in Figure 3 as the distance between the rings is increased. This consistency of the coefficient implies that reducing the separation distance between rings does not significantly weaken our ability to identify the consumption peer effect. Moreover, by keeping the rings directly abutting each other, we ensure the statistical soundness of our empirical analysis.

To further confirm that separation distance does not alter the DiD coefficient, we fix ring width at 100 m and further test whether increasing separation distances in 100 m increments from 0 m to 800 m changes our estimates. We show the results of this test in Figure 4 and Table 4. We find that the DiD coefficient remains constant even when the separation distance between the inner and outer rings extends to 800 m. Given the aforementioned advantages of having narrow, directly abutting inner and outer rings and the findings reported here, we are confident in our choice of directly abutting 100 m inner and outer rings as our ring parameters for the remainder of this paper.

[24] Table 3 is similar to Table 2 except that it is a classic DiD specification with a single pre- and post-claim event period.

4.4 Alternative Event Windows

A large literature emphasizes that the timing of consumption for consumer durable goods such as the ones in our sample is "lumpy" (e.g., Attanasio, 2000; Eberly, 1994; Caballero, 1990, 1993; Caplin and Leahy, 2006). As described in this literature, consumption of these kinds of products happens only rarely over the lifetime of a consumer, so there may be extended spells during which a consumer (or even a whole neighborhood) does not purchase a consumer durable good.

This argument, that consumption of durables happens only rarely over the life cycle, could imply that word-of-mouth peer effects are less effective.[25] This is because the receiving peer may not remember the product information provided by a sending peer if that information was sent long before the receiving peer chose to make a consumer durable purchase. On the other hand, it is also possible that the word of mouth information may indeed be remembered by the receiving peer, even after a long period, because of the salience of the information as well as the salience of the peer providing the information.

Again, there may be a trade-off between the infrequency of consumer durable purchases and salience of recent word-of-mouth information for our tests when selecting the amount of time before and after the claim events that we study. To choose the most appropriate time window, we run our baseline analysis using eight different time windows around claims. Specifically, we perform our classic DiD regression, where the time periods before and after the claim event range from 1 quarter to 8 quarters. The results of these tests are displayed in Table 5 and Figure 5. Both the table and the figure show that the main DiD coefficient is significant and negative (as predicted) across all of the eight time windows. In addition, the magnitude of the DiD coefficient increases monotonically (in absolute value) as the length of the event window increases from 1 quarter to 8 quarters.

These results imply that the magnitude of the peer effect spillover in consumption gets larger as the time window around the claim event expands. This is consistent with the notion that durable good consumption is "lumpy." The receiving peer, even if they receive the negative information shock when the warranty claimant files, may only use the word-of-mouth information to make a purchase decision when they choose to make a durable good purchase. As we expand our time window around the claim event, the proportion of peers using the information shock to make purchase decisions increases. Though the recency of the claim event does not seem to weaken the word-of-mouth

[25] Dranove and Jin (2010), in its literature survey of various kinds of quality disclosure mechanism (p. 937), states that "word-of-mouth (effects) are ... of limited value when products are infrequently purchased".

peer effect, in order to balance the effects of the infrequency of durable good purchases and of the potential salience of recent information transfers, we choose the four quarter (one year) time window for our baseline analyses.

4.5 Baseline Findings

In the previous sections, we carefully explored the parameters of the hyper-local DiD framework we use for our word-of-mouth peer effects analysis. First, we defined our sample such that each inner and outer ring neighbor only experiences one peer warranty claim event. Next, we chose the width of rings and separation between inner and outer rings such that inner ring neighbors are within 100 m of the claimant and outer ring neighbors are between 100 and 200 m away from the claimant. Finally, we chose to use the four quarters (one year) before and after the warranty claim as the time period during which we conduct our analyses.

Using the above parameters, we find that, in the four quarters after a warranty claim, relative to the four quarters before the claim, the inner ring neighbors of the claimant make 1.6 fewer net purchases of the claimed brand, when compared to outer ring neighbors. This reduction in the net purchases of the claimed brand (in comparison to other brands) is highly statistically significant ($p < 0.01$). Given our thorough exploration of the parameters of the empirical exercise, we know that this reduction is robust to various sorts of perturbations. Overall, our analyses confirm that negative information shocks travel via word-of-mouth networks (i.e., via social learning) and have significant effects on the consumption behavior of peers.

5 Heterogeneity

Having established that there are meaningful peer effects of word-of-mouth negative information shocks on neighbors' consumption choices, in this section, we explore what impacts the strength of these peer effects. First, we explore whether neighborhood characteristics influence the documented peer effects. Then, we examine the impact of brand characteristics. And, finally, we study the impact of regional economic conditions.

5.1 Heterogeneity Across Neighborhood Demographics

5.1.1 Measuring Neighborhood Demographics

As described in our data discussion, our main neighborhood-level data are at the dissemination area (DA) level. The DA level is the smallest level of geography at which census data is made available by Statistics Canada. While not as small as the postal codes that we use as our unit of analysis, DAs are still quite small, with the median DA including 550 people. We use these DA-level census data for neighborhood-level demographic information.

Our methodology in exploring heterogeneities is to run our baseline specification (Equation 1) on quintiles based on the value of the demographic variable being studied for the neighborhood. For each claim, we use the DA-level value for the neighborhood characteristic to determine the in-sample quintile of the postal code. We run our hyper-local DiD analysis on each quintile separately and then compare the magnitudes of the 5 DiD coefficients to assess whether and how the magnitudes of these peer effects vary across each demographic characteristic.

5.1.2 Neighborhood Average Income

Recent work from Chetty et al. (2022), using Facebook data, examines "friending rates" across SES (Social and Economic Status) percentiles. This "friending" study provides evidence that as SES declines, the number of friendships developing from neighborhood interactions increases. In other words, this study shows that poorer individuals are more likely to have friends based on peer interactions from within the neighborhood.[26]

Based on Chetty et al. (2022), we hypothesize that there should be greater neighborhood peer effects in neighborhoods with lower SES status, as word-of-mouth information spreads more effectively within neighborhoods with stronger social networks. In our setting, this hypothesis implies that the the magnitude of the negative consumption peer effects of a warranty claim should increase as the SES of the neighborhood declines.

We proxy for neighborhood SES using Canadian census data on average income in a neighborhood and perform our quintile-based heterogeneity analysis. Our results are reported in Table 6 and Figure 6. As is graphically evident in Figure 6, we find a significantly larger and more negative consumption peer effect for the lowest income quintile (quintile 1) than for all other quintiles. And,

[26] Chetty et al. (2022) use the same data to document that as SES status increases, so a larger fraction of friends are made from interactions at College.

for all but the highest quintile, there is a clear monotonic decrease in the DiD term magnitude as average income increases. These results are consistent with the hypothesis, based on Chetty et al. (2022), that the magnitude of neighborhood peer effects should be the largest for individuals in the poorest neighborhoods.

5.1.3 Neighborhood Population Density

An extensive literature across many disciplines (e.g., urban economics, urban sociology, and geography) has examined how variation in population density (e.g., from densely populated large cities to low density suburbs and rural areas) impacts the strength of the relationship between neighbors. In general, two contradictory hypotheses have been proposed. On the one hand, it is argued that increased population density should generate more social interactions, and thus larger peer effects, because increased density increases the opportunity to interact with more peers. On the other hand, it is argued that increased density causes increased interpersonal frictions, offers other alternatives for establishing social networks, and generates other various negative externalities (e.g., congestion externalities), which, in aggregate, lower the quality of neighborhood-based social interactions, and thus lower the impact of such peer effects.[27]

We examine the heterogeneity across population density of our peer effect using DA-level population density data. We are unable to run our analysis on the lowest density quintile as our sample selection criteria mechanically exclude that quintile.[28] Our results for this specification are reported in Table 7 and Figure 7. As can be seen in Figure 7, the peer effect estimate for the least dense quintile (i.e., quintile 2) is significantly more negative than all other quintiles. Moreover, the peer effect is monotonically decreasing with population density. This finding is consistent with the hypothesis that social interactions are stronger in less dense neighborhoods, because peer interactions in more dense neighborhoods are impeded by issues such as interpersonal frictions, negative congestion externalities, and availability of alternative social networks.

[27] Empirical evidence on the relationship between population density and relationships between neighbors is mixed, reflecting the interplay between these contradictory hypotheses. For example, Boessen et al. (2018) finds a positive relationship between density and interactions, while studies including Brueckner and Largey (2008); French et al. (2014); Skjaeveland and Garling (1997); Mouratidis and Poortinga (2020) find a negative relationship.

[28] Recall that we choose neighbors in postal codes within 200 m of the claimant's postal code for our inner and outer rings. Rural postal codes are much larger than that and, as a result, are mechanically excluded from our analyses.

5.1.4 Neighborhood Immigrant Proportion

In this section, we examine whether the fraction of immigrants within a neighborhood affects the impact of word-of-mouth peer effects. A number of factors could drive heterogeneity in these neighborhood peer effects based on the immigrant share of a neighborhood. For instance, Ray and Preston (2009) posits that immigrants may be more socially isolated from their non-immigrant neighbors in the neighborhood. Such socially-driven divisions within a neighborhood would make geography-based peer effects like the one we study here less powerful.

We divide the neighborhoods in our study into five quintiles based on fraction of immigrants in the population, accessed from our DA-level Census data. We run our quintile-based heterogeneity tests for immigrant fraction and report our findings in Table 8 and Figure 8. In the figure, we observe a general trend that the magnitude of the peer effect declines as the fraction of immigrants increases. While the trend is not as dramatic as the average income or population density findings, it seems to be economically relevant, with the neighborhoods with the highest immigrant proportions reporting a 40% weaker peer effect than the lowest immigrant quintile neighborhoods.

5.2 Heterogeneity Across Brand Characteristics: Evidence on Akerlof (1991)

In this section, we examine the effect of variation in brand-level characteristics on our estimated peer effects. Our main motivation for these tests is to provide evidence on Akerlof (1991), where he postulated that the consumption choices of information-receiving peers would depend far more on the (idiosyncratic) information shared by the information-sending peer than on any aggregate brand-specific information, such as brand market share or brand quality ratings. Akerlof (1991) argues that the large weight placed on word-of-mouth information is due to the high salience of a peer relative to any other aggregate brand-level information available to the consumer. The key conclusion of Akerlof (1991) is that even though the information provided by the single salient peer is idiosyncratic and possibly unrepresentative of the product or brand, the salience of the individual peer is so large that the information provided by the peer outweighs the more aggregate and representative brand-level data. In our setting, therefore, the prediction of the argument proposed by Akerlof (1991) is that aggregate brand-level information should not have a significant impact on the strength of the peer effects we document.[29]

[29] In his "thought experiment", Akerlof (1991) (p. 2) describes the salience of the idiosyncratic information from the sending peer as being so high that it outweighs the objective and aggregate brand quality information acquired

In this section, we provide evidence that is consistent with this proposition, by showing that there is no significant variation in the magnitude of the peer effect across two different brand-based characteristics: (1) brand claims-to-sales ratio and (2) brand purchase market share. One important caveat with these brand-level tests is that we cannot determine exactly why the information-receiving peer puts a lower weighting on the aggregate brand-level information. This could either be because the information receiver is not aware of these aggregate brand measures or, alternatively, consistent with Akerlof (1991), the information-receiving peer is aware of these measures, but chooses to ignore them because the idiosyncratic information from the sending peer is more salient. In spite of this caveat, our tests in this section provide support for the main proposition of Akerlof (1991).

5.2.1 Brand Claims-to-Sales Ratio

We first test whether brand quality impacts the documented peer effects using the aggregate warranty claim rate for each brand. The brand-level aggregate warranty claim rate is defined as the total number of claims made against that brand divided by the total number of sales of that brand's products for the claimed product category in the two years before the focal claim event.[30] This ratio is calculated using our complete database of claims and purchases, rather than our restricted analysis sample. We believe that this ratio of claims to sales captures an *objective* aggregate quality measure for each brand and allows us to distinguish between high objective quality brands (with low claims-to-sales ratios) and low objective quality brands (with high claims-to-sales ratios). As in our other heterogeneity tests above, in this section, we divide all our brand-level measures of the claims-to-sales ratio into quintiles and then run our baseline DiD for the five quintiles.

We provide the results for this analysis in Table 9 and Figure 9. Our results indicate that there are no major differences between the estimated DiD coefficients across the five quintiles. This finding is consistent with the Akerlof (1991) conjecture, that variation in objective quality across brands should have a lower weighting on the information-receiving peer's consumption decision, compared to the weighting given to information provided by the salient, but possibly unrepresentative, peer.

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by the receiving peer from Consumer Reports magazine.

[30]We use the product category claims-to-sales ratio so that the measure more accurately reflects product quality. But, we have run these tests using overall brand-level claims-to-sales ratios and the results are qualitatively similar.

5.2.2 Brand Market Share

Our second measure of a brand-level characteristic that may influence consumption decisions is the purchase market share of the brand. The consumer durable goods brands in our data are displayed in Table 1b. A substantial literature in marketing (e.g., Hellofs and Jacobson, 1999) as well as international trade (e.g., Khandelwal, 2010) makes the assumption that, in settings where quality is unobservable by consumers, consumers attribute higher levels of quality to brands with higher market shares (conditional on price). This is based on the argument that the consumer believes that if a large number of other consumers are already purchasing that brand, then the unobserved quality of that brand must also be high.

Given that the retail chain providing us with data is one of the top four retailers of consumer durable goods in Canada, we argue that we can representatively measure the market share for all brands sold in the total Canadian market. By making the assumption that consumers attribute brand market share to reflect unobserved brand quality, we once again test the Akerlof conjecture that idiosyncratic information from the sending peer is more salient than other measures of quality.

To run our tests, we again divide our data into quintiles. In this analysis, we base the quintiles on total purchase market share of each brand over the last two years and then run our baseline DiD for each quintile.[31] We report our results in Table 10 and Figure 10. The main conclusion from these results is that there is no meaningful difference in the estimated DiD coefficients across these five quintiles. In other words, if we assume that consumers use market share as a proxy for unobserved quality, then unobserved brand-level quality does not impact the main peer effect relationship that we document in this paper. This result is thus also consistent with the Akerlof (1991) conjecture that the information-receiving peer will put more weighting on information from a single salient (but possibly unrepresentative) peer and less weighting on more aggregate brand level information.

5.3 Heterogeneity across Local Business Cycles

In this section, we examine whether the strength of the estimated peer effects changes over the course of the business cycle. Our focus on business cycles in this context is motivated by recent research by Bošković et al. (2024), which, using the same data we use, documents that warranty prices are procyclical over regional business cycles. This warranty price procyclicality is driven by

[31] Again, we use the product category purchase market share so that the measure more accurately reflects product quality. But, using overall brand-level purchase market share provides qualitatively similar results.

local stores reducing warranty prices during recessions, as a response to the depressed demand for durable goods, and the higher price sensitivity of consumers. Given their findings, in this section, we examine whether variation across regional business cycles could also impact the magnitude of our estimated peer effects following warranty claim events.

Regional business cycles could impact the peer effects of warranty claims if consumers face larger financial constraints during recessions compared to booms. Individuals with more binding financial constraints (i.e., during recessions) may invest more in costly search for information on product quality (e.g., by interacting with neighbors), compared to individuals in boom periods. As such, consumption peer effects in our study would be larger during recessionary periods.

In order to measure local business cycles, we use the same local house price index data used by Bošković et al. (2024). These regional house price indices are provided by Teranet and the National Bank of Canada. This data is measured at the FSA (Forward Sortation Area) geographic area which is defined as the first three digits of the six digit Canadian post code. There are approximately 1,600 FSA areas across Canada.

As above, we run our baseline peer effects model across five quintiles for the regional house price index (as measured at the FSA-quarter level). We run this test for the regional house price index level in the FSA-quarter, as well as for changes in the index. Our results for the house price index levels are presented in Table 11 and Figure 11. We find no significant trend across the quintiles. We find similar results when examining house price index changes. These results indicate that the consumption peer effects of warranty claims remain equally large and significant across all stages of the business cycle. We interpret these results as furhter confirmation of the Akerlof (1991) proposition concerning the importance of a single salient peer in consumption choices.

6 Conclusion

Akerlof (1991) in his Ely Lecture, describes a vignette where a single salient peer who has had a negative quality experience of a brand (i.e., a "lemon") can deter another peer from purchasing that brand. In this paper, we provide causal evidence testing the validity of this vignette. Our main finding is that a negative experience with a particular product brand significantly reduces the future consumption of that brand's products by that person's geographic peers.

Our paper differs from much of the consumption peer effect literature in that we examine peer effects when the product is a "lemon," while the existing literature examines peer effects when the product is a “peach” (i.e., where positive product information increases peers’ consumption). An important advantage of our “lemons” setting, compared to the typical “peaches” setting, is that it allows us to identify the channel of the peer interactions. A challenge in the standard consumption peer effects literature is that peer effects may be driven by any of at least four channels (e.g., the social learning channel, the social utility channel, the imitation channel and the network effects channel). In our setting, however, only the social learning channel is applicable, making identification of the channel much more straightforward. As such, we can assert that the negative effect on peers’ consumption due to the warranty claims arises because the claimant’s peers learn from her negative experience with the brand, through the social learning channel.

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Figures and Tables

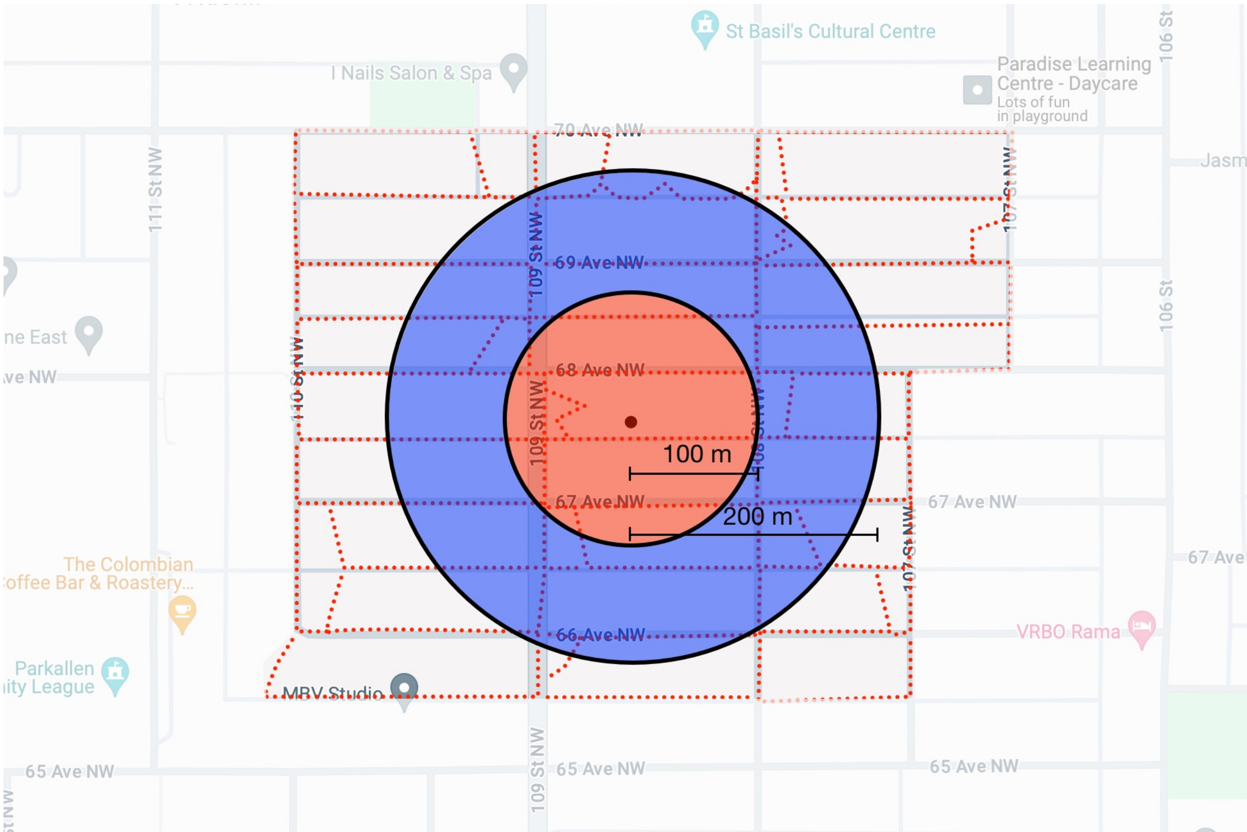

Figure 1: Illustration of Hyper-local Difference-in-Differences Methodology
This figure illustrates the assignment of postal codes to treatment and control groups for the hyper-local difference-in-differences methodology used in this paper. For each warranty claim, an inner circle (red circle with a 100 m radius in this illustrative figure) is drawn around the centroid of the postal code of the claim and all households in postal codes with centroids within the area of the inner (red) circle are in the treatment group (excluding the claimant household). Similarly, an outer ring (blue circle with a 100 m width between 100 m and 200 m from the claimant postcode centroid in this illustrative example) is drawn around the centroid of the postal code of the claim and all households in postal codes with centroids within the area of the outer (blue) ring are in the control group.

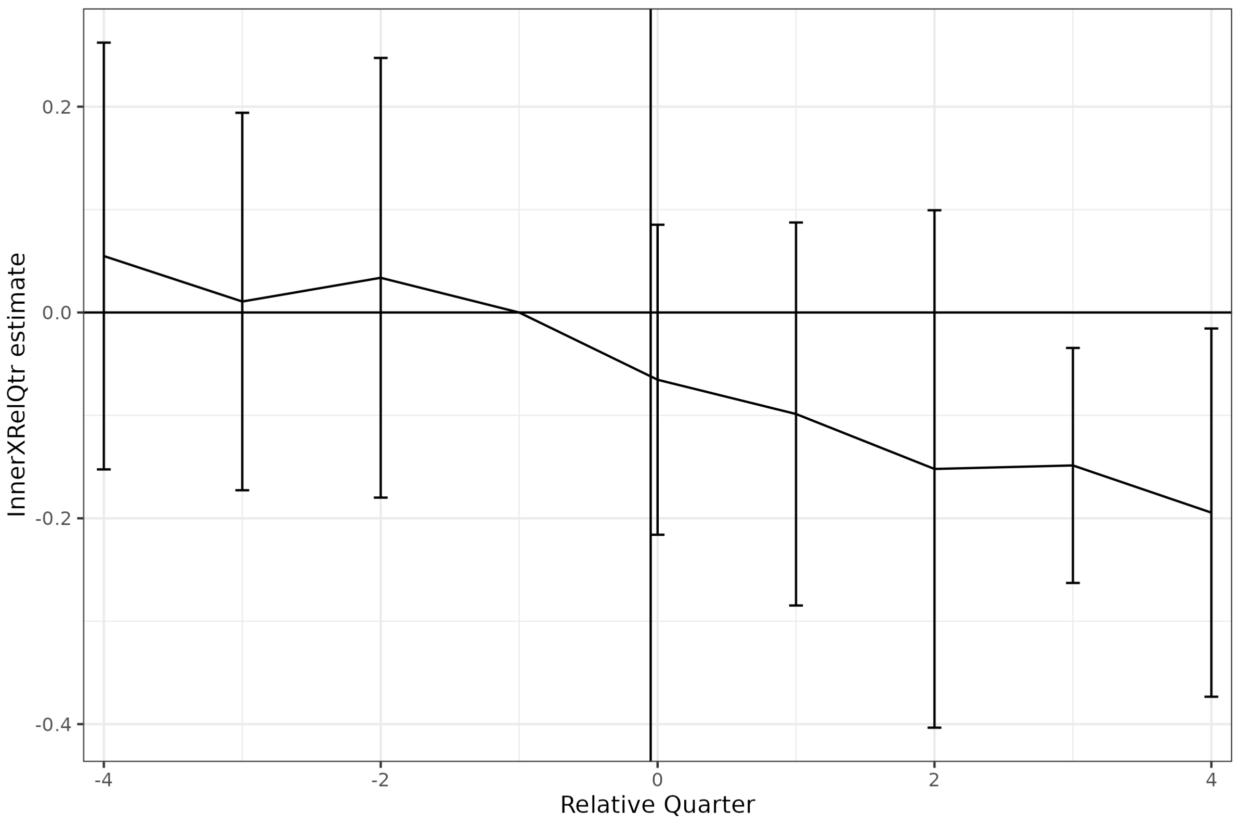

Figure 2: Treatment Effect Dynamics

This figure presents the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households (households in postal codes within 100 m of the warranty claim) as compared to outer ring households (households in postal codes between 100 and 200 m of the warranty claim) for each quarter within one year of the claim, using the quarter just before the warranty claim as a baseline. Inner ring households are defined as households in postal codes within 100 m of the warranty claim and outer ring households are defined as households in postal codes within a ring with a width of 100 m outside of the inner ring, with the gap between the inner and outer rings varying in 100 m increments from 0 m to 800 m. The x-axis presents the quarter of the treatment effect relative to the quarter of the corresponding warranty claim. The solid line is the estimate of the treatment effects across the quarters and the range bars depict the 95% confidence interval for the estimates.

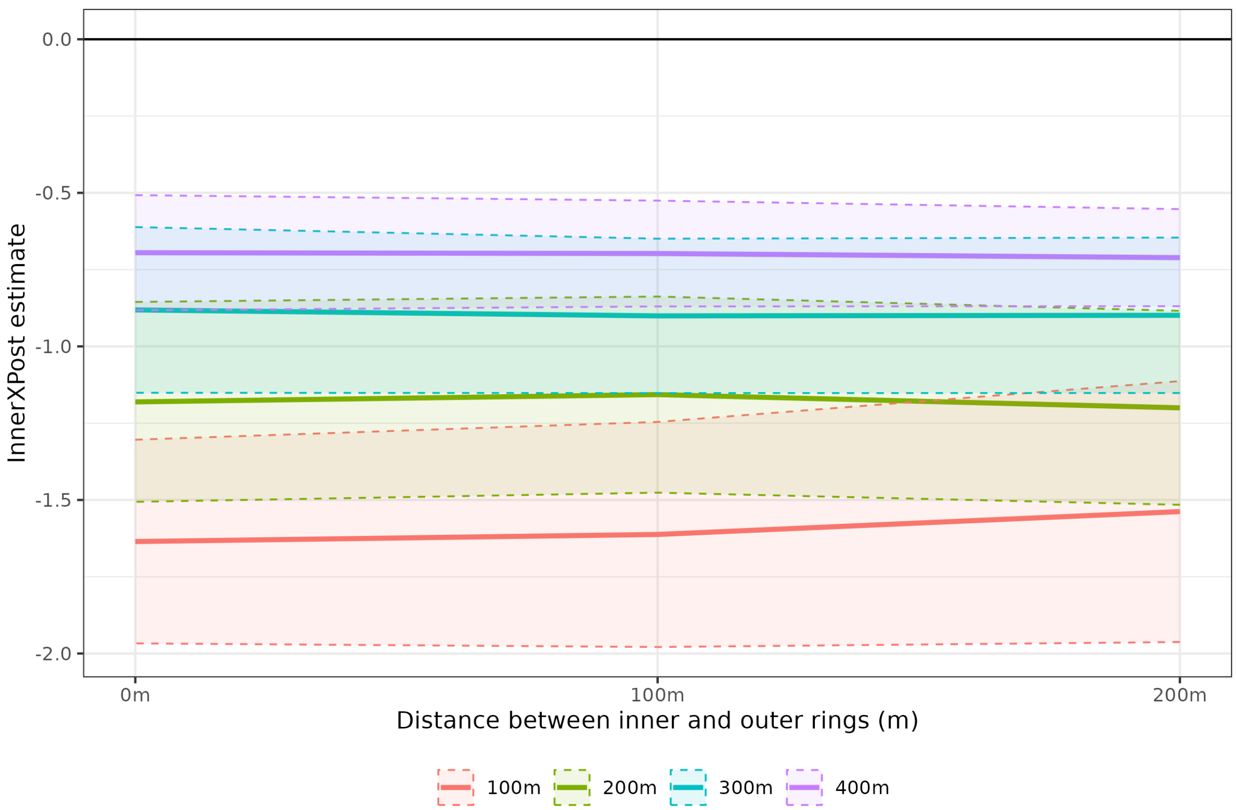

Figure 3: Treatment Effects by Ring Width and Distance between Inner and Outer Rings
This figure presents variation in the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households as compared to outer ring households across two dimensions: ring width and gap between inner and outer rings. The x-axis varies the missing distance (in meters) between the inner and outer rings between 0, 100, and 200 m. The four colored lines represent the treatment effects across four specifications with inner and outer rings of different widths: 100, 200, 300, and 400 m. For each line, the corresponding dashed lines and shaded areas represent the 95% confidence intervals for the estimates.

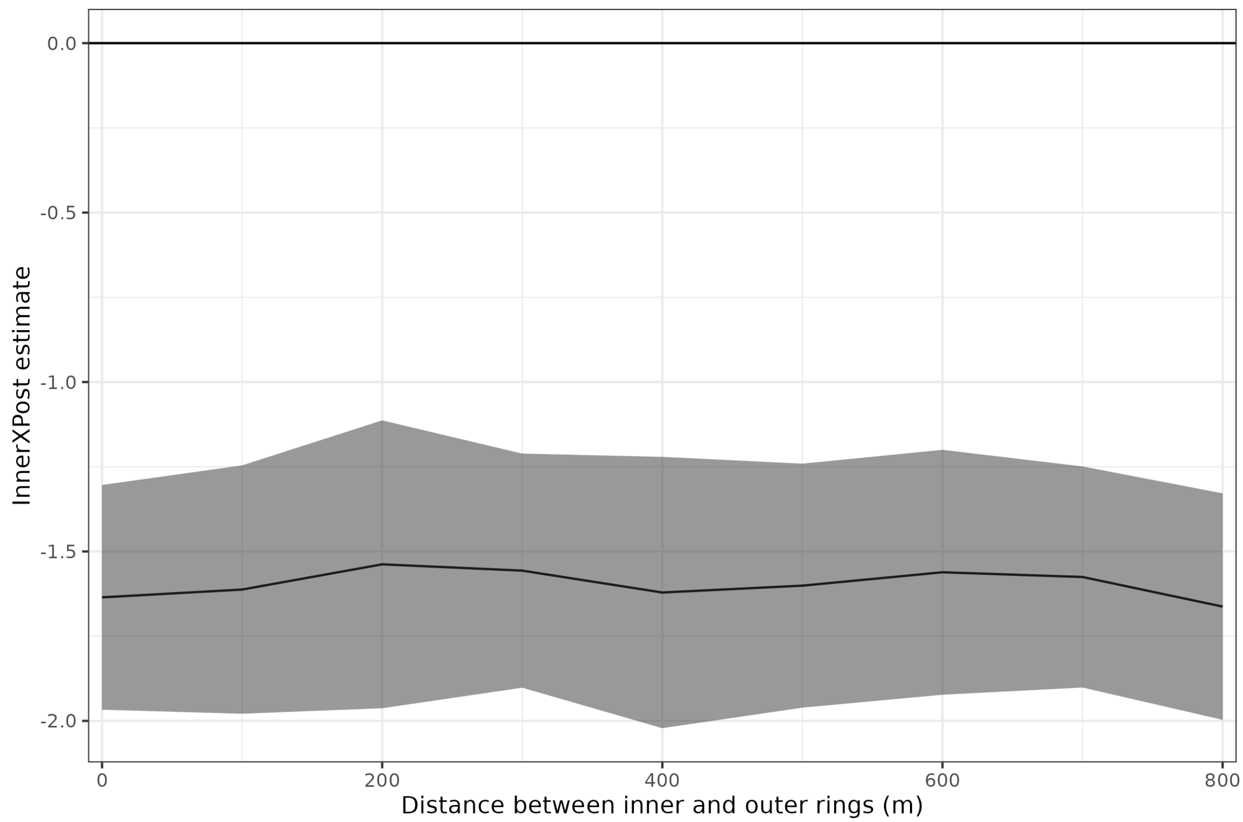

Figure 4: Treatment Effects by "Missing" Distance between Inner and Outer Rings
This figure presents the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households as compared to outer ring households. Inner ring households are defined as households in postal codes within 100 m of the warranty claim and outer ring households are defined as households in postal codes within a ring with a width of 100 m outside of the inner ring, with the gap between the inner and outer rings varying in 100 m increments from 0 m to 800 m. The x-axis presents these gaps (in meters) between the inner and outer rings. The solid line is the estimate of the treatment effects across the gap values and the shaded area depicts the 95% confidence interval for the estimates.

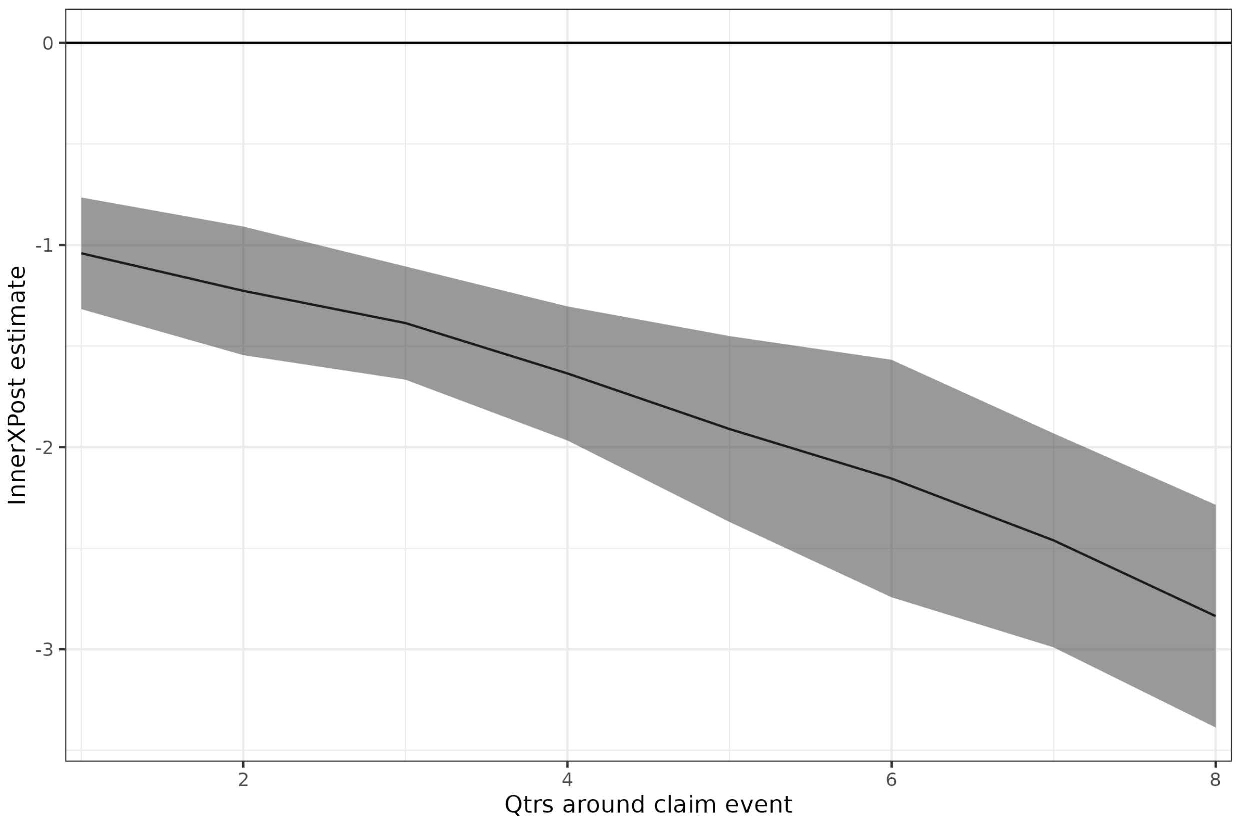

Figure 5: Treatment Effects by Purchase Data Window Length

This figure presents variation in the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households (households in postal codes within 100 m of the warranty claim) as compared to outer ring households (households in postal codes between 100 and 200 m of the warranty claim) using purchase data for differing numbers of quarters around warranty claims, which can vary from one to eight quarters. The x-axis presents the number of quarters of purchases used in the analysis. The solid line is the estimate of the treatment effects across the purchase data window lengths and the shaded area depicts the 95% confidence interval for the estimates.

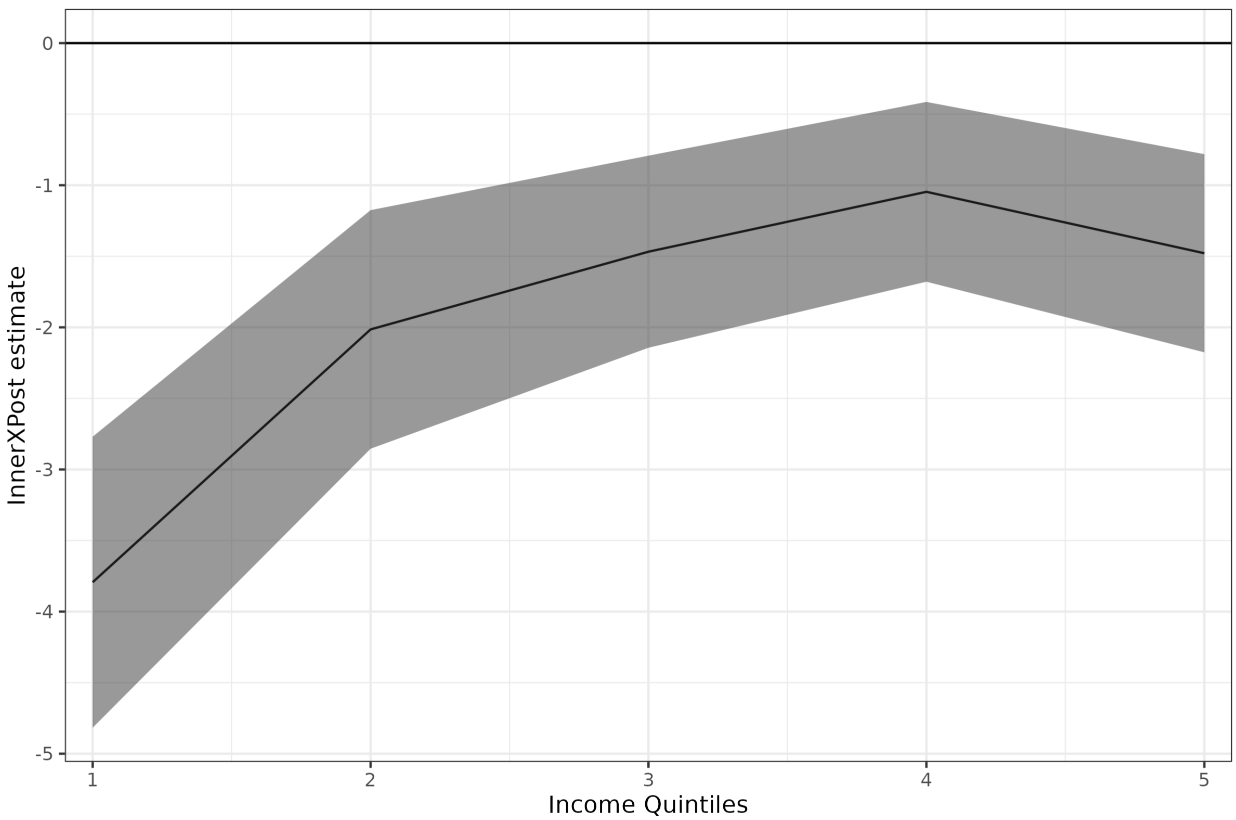

Figure 6: Treatment Effect by Neighborhood Average Income
This figure presents variation in the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households (households in postal codes within 100 m of the warranty claim) as compared to outer ring households (households in postal codes between 100 and 200 m of the warranty claim) across the average income quintiles of the claimant household's dissemination area (DA). The x-axis presents the average income quintiles. Quintile 1 is the poorest quintile. The solid line is the estimate of the treatment effects across the income quintiles and the shaded area depicts the 95% confidence interval for the estimates.

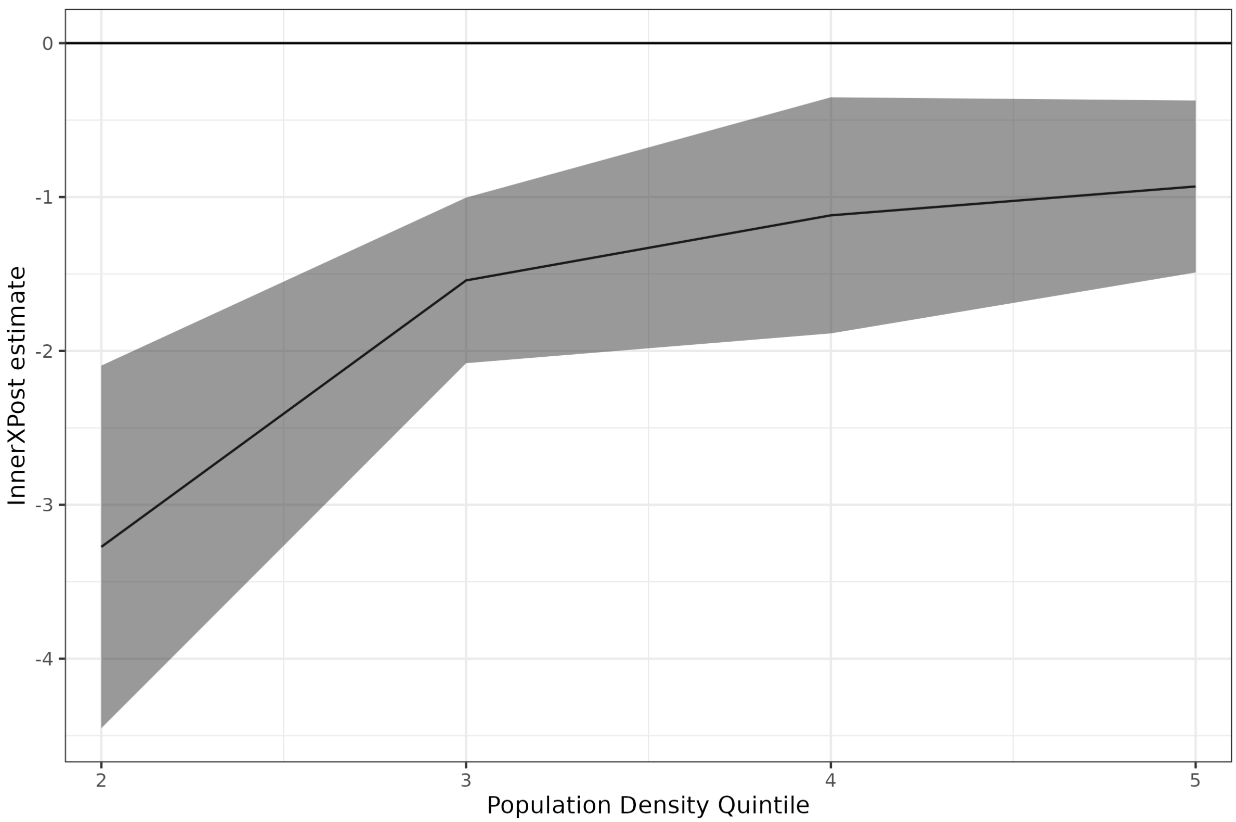

Figure 7: Treatment Effect by Neighborhood Population Density

This figure presents variation in the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households (households in postal codes within 100 m of the warranty claim) as compared to outer ring households (households in postal codes between 100 and 200 m of the warranty claim) across the population density quintiles of the claimant household's dissemination area (DA). The x-axis presents the population density quintile of the DA, with the lowest density quintile, which is composed of rural areas, omitted in the figure as it is in analyses. The solid line is the estimate of the treatment effects across the population density quintiles and the shaded area depicts the 95% confidence interval for the estimates.

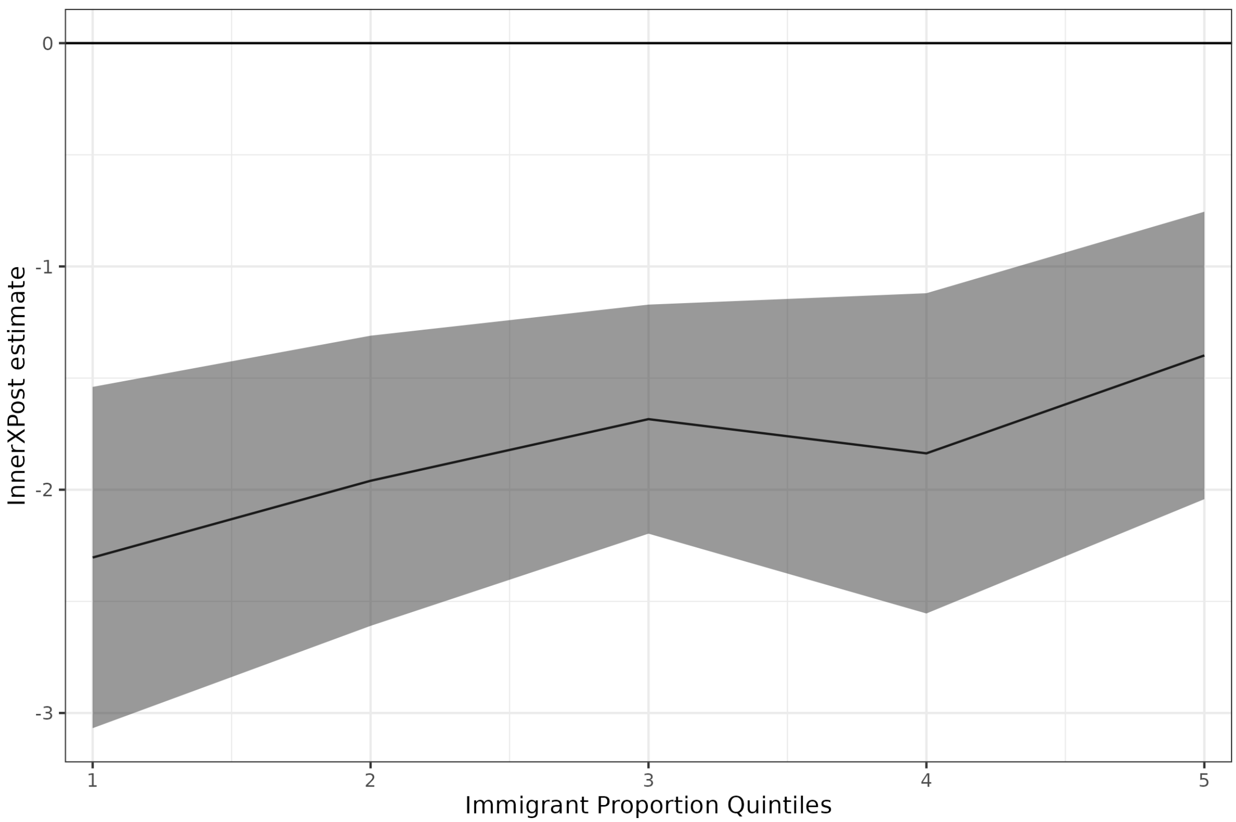

Figure 8: Treatment Effect by Neighborhood Immigrant Proportion
This figure presents variation in the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households (households in postal codes within 100 m of the warranty claim) as compared to outer ring households (households in postal codes between 100 and 200 m of the warranty claim) across quintiles of immigrant proportion of population in the claimant household's dissemination area (DA). The x-axis presents the immigrant proportion quintiles. The solid line is the estimate of the treatment effects across the quintiles and the shaded area depicts the 95% confidence interval for the estimates.

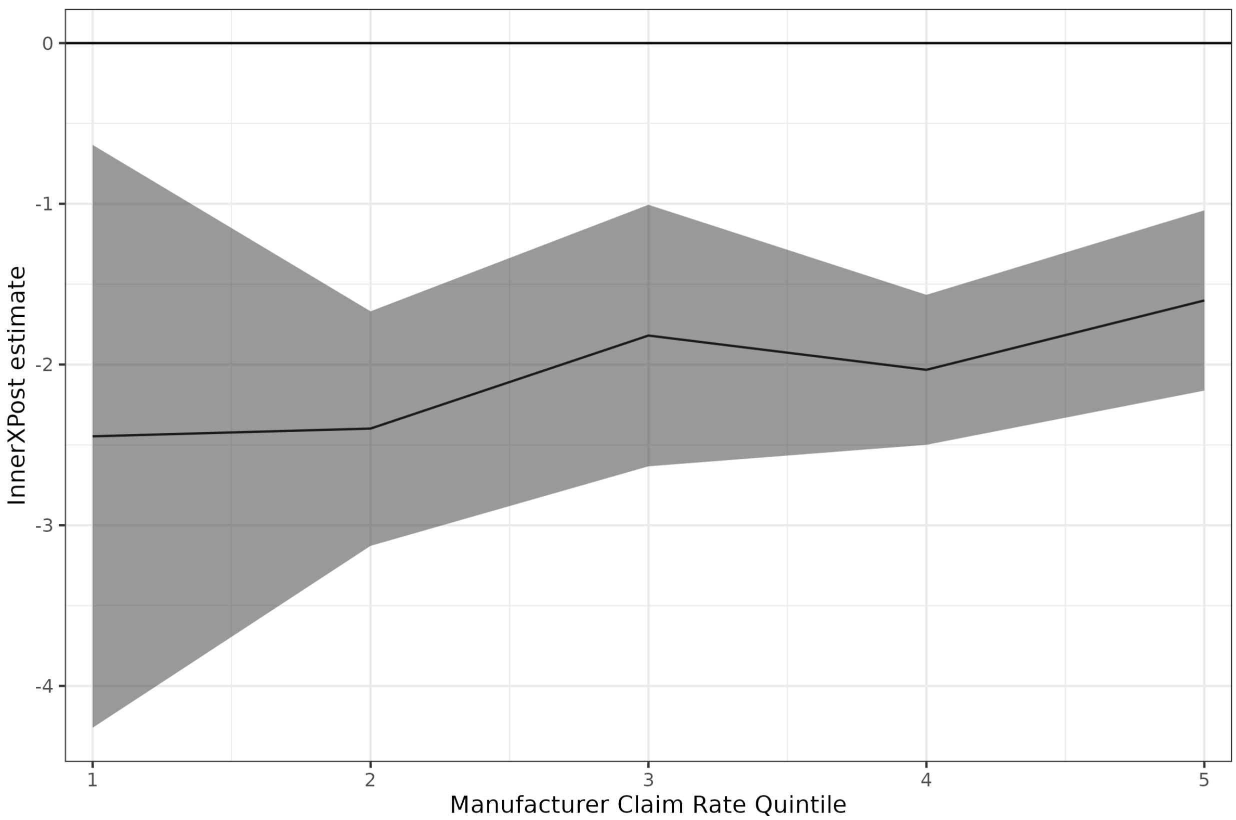

Figure 9: Treatment Effect by Claimed Brand Claims-to-Sales Ratio
This figure presents variation in the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households (households in postal codes within 100 m of the warranty claim) as compared to outer ring households (households in postal codes between 100 and 200 m of the warranty claim) across quintiles of the aggregate claim to sales ratio for the claimed product's manufacturer over the last two years for the claimed product category. The x-axis presents the claimed manufacturer's claim rate quintiles, with the lowest claim rate quintile representing the highest quality quintile. The bar is the estimate of the treatment effects across the product categories and the error bar depicts the 95% confidence interval for the estimates.

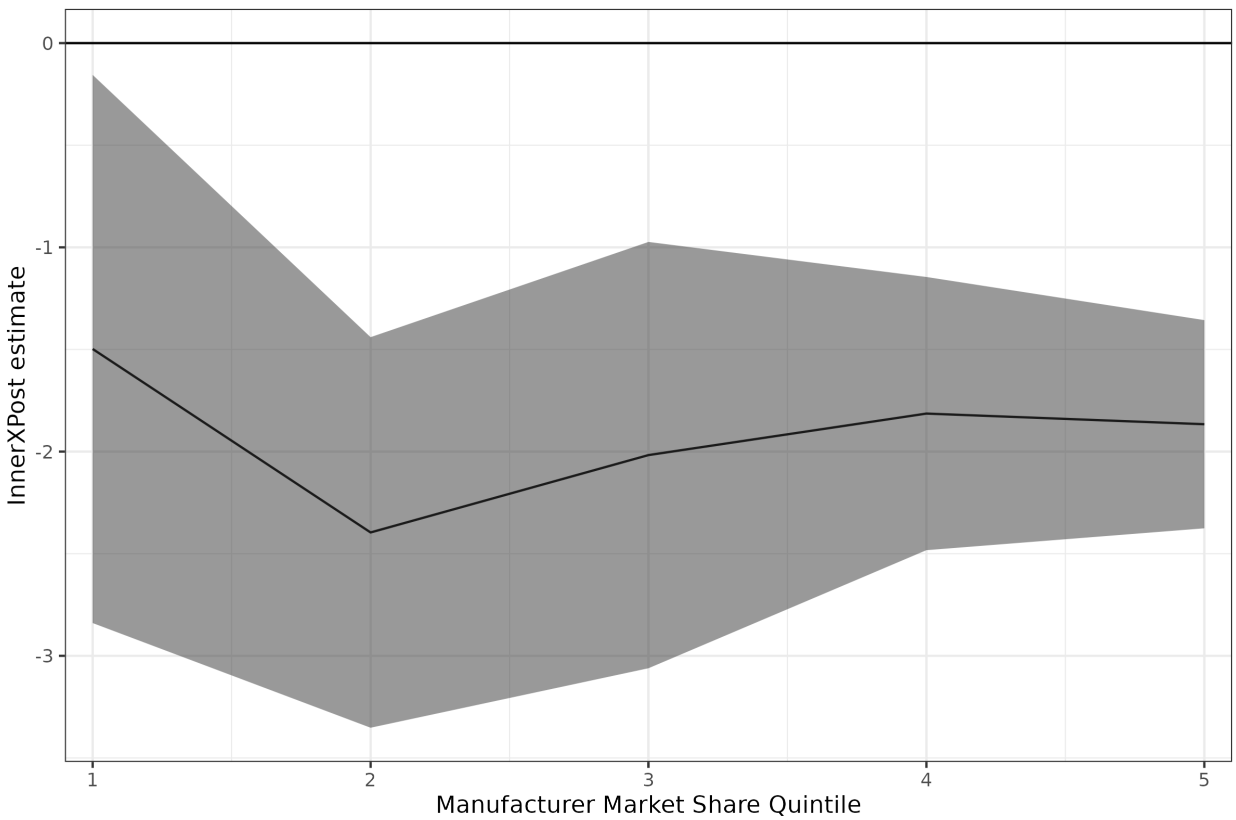

Figure 10: Treatment Effect by Claimed Brand Market Share

This figure presents variation in the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households (households in postal codes within 100 m of the warranty claim) as compared to outer ring households (households in postal codes between 100 and 200 m of the warranty claim) across quintiles of the purchase market share for the claimed product's manufacturer over the last two years for the claimed product category. The x-axis presents the claimed manufacturer's market share quintiles. The bar is the estimate of the treatment effects across the product categories and the error bar depicts the 95% confidence interval for the estimates.

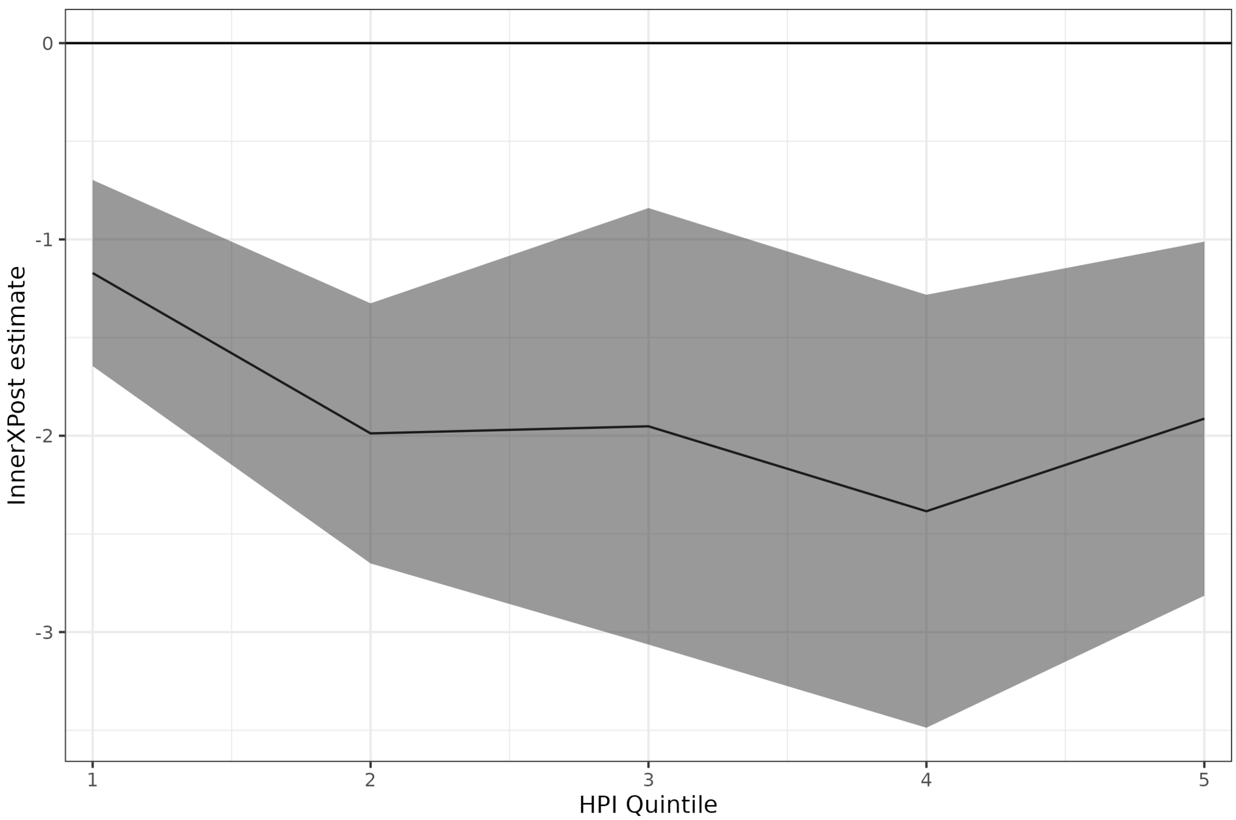

Figure 11: Treatment Effect by Neighborhood House Price Index
This figure presents variation in the treatment effect of a warranty claim on the purchases of claimed brand goods relative to other brands' goods for inner ring households (households in postal codes within 100 m of the warranty claim) as compared to outer ring households (households in postal codes between 100 and 200 m of the warranty claim) across the average House Price Index (HPI) value quintiles of the claimant household's Forward Sortation Area (FSA). The x-axis presents the average HPI quintiles. Quintile 1 is the lowest HPI quintile. The solid line is the estimate of the treatment effects across the HPI quintiles and the shaded area depicts the 95% confidence interval for the estimates.

Table 1: Summary Statistics

These tables present summary statistics for key variables in this paper. Panel A provides statistics on purchases, Panel B provides statistics on all warranty claims, Panel C lists the top 30 brands by purchases along with purchase market share statistics. And Panel D provides statistics on warranty claims used in our analyses, and Panel D provides demographics for dissemination areas within and outside our data.

(a) Purchases

This panel provides summary statistics for purchases of all consumer durable goods purchased from an anonymous Canadian retail store chain between 1998 and 2009. *Purchase Price* is the price of the product purchased (in Canadian Dollars). *Warranty Purchased* is an indicator variable for the purchase of warranty coverage together with the product by a consumer. *Cost of Warranty* is the price of the purchased warranty coverage (in Canadian Dollars).

	25th %ile	Median	75th %ile	Mean	Std Dev	Obs
Purchase Price	199.97	439.96	788.00	609.27	1765.41	5956402
Warranty Purchased	0.00	0.00	1.00	0.36	0.48	5956402
Cost of Warranty	9.99	69.99	124.99	90.56	95.24	2136208

(b) Top 30 Brands by Purchase Market Share

This panel lists the top 30 manufacturer brands by total purchases of consumer durable goods made between 1998 and 2009 across all branches of an anonymous Canadian retail store chain. For each brand, the panel provides the number of purchases recorded as well as the percent of all purchases.

Manufacturer	Count	Percent
Sony	796,122	14.2
Frigidaire	664,920	11.9
Panasonic	360,781	6.4
Whirlpool	309,678	5.5
LG Electronics	307,441	5.5
Samsung	302,170	5.4
Citizen	223,800	4.0
GE Appliances	211,015	3.8
RCA	206,165	3.7
Maytag	199,213	3.6
Danby	136,839	2.4
Electrolux	122,089	2.2
Inglis	114,149	2.0
Performa	91,319	1.6
Hitachi	87,353	1.6
JVC	82,446	1.5
Sharp	78,693	1.4
Toshiba	75,963	1.4
GTech	75,678	1.3
Prima	61,459	1.1
Brada	56,949	1.0
Stitch	48,566	0.9
Kitchenaid	44,682	0.8
Legend	40,109	0.7
Vitel Malta	38,587	0.7
Moffat	35,610	0.6
Hoover	32,610	0.6
Goldstar	29,751	0.5
Philips	28,951	0.5
Retailer	28,668	0.5

(c) Claims

This panel provides summary statistics for warranty claims on consumer durable goods purchased from an anonymous Canadian retail store chain filed between 1998 and 2009. The summary statistics provided in the four leftmost columns are for warranty claims that are used for analysis as per the procedure detailed in Section 3, whereas the summary statistics provided in the four rightmost columns are for all warranty claims. *Purchase Price* is the price of the product purchased (in Canadian Dollars). *Cost of Claim* is the repairing cost of the product under warranty (in Canadian Dollars). *Days to Claim* is the number of days from the purchase date to the warranty claim date. *Days to Repair* is the number of days spent in repairing the product. *In Analysis Sample* is an indicator variable for whether a claim is used for our analyses.

	Analysis Claims				All Claims			
	Median	Mean	Std Dev	Obs	Median	Mean	Std Dev	Obs
Purchase Price	877.94	1140.21	864.99	1776	769.97	1030.17	816.75	95109
Cost of Claim	128.57	210.67	249.82	1776	118.77	175.63	202.78	95109
Days to Claim	668.00	743.73	417.60	1776	664.00	725.19	423.30	95109
Days to Repair	8.00	18.03	33.18	1776	9.00	19.06	36.53	95109
In Analysis Sample	1.00	1.00	0.00	1776	0.00	0.02	0.14	95109

(d) Dissemination Area Demographics

This panel provides demographics for dissemination areas (a geographic area comprising several postal codes) across Canada. The dissemination areas (DAs) are separated into in-sample and out of sample DAs, where in-Sample DAs are DAs containing at least one warranty claim event. *Population* is the number of people living in a DA. *Pct. Male* is the percentage of the DA population that is male. *Pct. Married* is the percentage of the DA population that is married. *Pct. Immigrants* is the percentage of the DA population that has immigrated to Canada. *Avg. Household Size* is the average size of households in the DA. *Avg. Income* is the average income of adults in the DA (in Canadian Dollars).

	In-sample DAs				Out of sample DAs			
	Median	Mean	Std Dev	Obs	Median	Mean	Std Dev	Obs
Population	557.00	689.88	798.63	1477	516.00	591.39	383.06	51496
Pct. Male	50.61	50.41	2.88	1474	49.26	49.04	3.42	51406
Pct. Married	56.73	53.55	12.14	1474	49.34	47.61	13.52	51406
Pct. Immigrants	7.27	9.50	8.76	1424	11.76	17.85	17.99	50590
Avg. Household Size	2.60	2.61	0.46	1477	2.60	2.58	0.55	51496
Avg. Income	32158.00	34371.32	17959.15	1477	31388.00	33922.92	18089.29	51496

Table 2: Dynamic Treatment Effects on Neighbors' Net Purchases of Claimed Brand Products

This table presents the results of geographic dynamic difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code over the course of one quarter. The reported coefficients represent the change in the difference in the dependent variable between inner ring (treated) and outer ring (control) postal codes in one of the quarters within two years of a warranty claim, relative to the difference in the quarter before the warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. The twelve columns represent four groups of three regressions where the groups have four different widths for the rings (100 m, 200 m, 300 m, and 400 m) and the three regressions within each group have differing distances between the inner and outer rings (0 m, 100 m, and 200 m). For all regressions, we impose postal code, relative quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	T: 0-100m			T: 0-200m			T: 0-300m			T: 0-400m		
	C: 100-200m	C: 200-300m	C: 300-400m	C: 200-400m	C: 300-500m	C: 400-600m	C: 300-600m	C: 400-700m	C: 500-800m	C: 400-800m	C: 500-900m	C: 600-1000m
Inner x Qtr - 4	0.055	0.045	0.045	0.032	0.019	0.015	0.012	0.014	0.037	0.024	0.022	0.023
	[0.519]	[0.449]	[0.434]	[0.415]	[0.246]	[0.214]	[0.219]	[0.250]	[0.726]	[0.602]	[0.564]	[0.556]
Inner x Qtr - 3	0.011	0.003	0.001	-0.001	0.008	0.038	0.026	0.025	0.030	0.027	0.027	0.011
	[0.114]	[0.029]	[0.014]	[-0.010]	[0.115]	[0.557]	[0.536]	[0.547]	[0.624]	[0.688]	[0.726]	[0.268]
Inner x Qtr - 2	0.034	0.061	0.049	0.046	0.049	0.065	0.045	0.041	0.032	0.027	0.026	0.014
	[0.309]	[0.675]	[0.513]	[0.666]	[0.687]	[0.978]	[0.870]	[0.786]	[0.628]	[0.697]	[0.651]	[0.348]
Inner x Qtr 0	-0.065	-0.089	-0.077	-0.067	-0.039	-0.028	-0.020	-0.025	-0.030	-0.014	-0.019	-0.019
	[-0.851]	[-1.176]	[-0.897]	[-1.049]	[-0.603]	[-0.449]	[-0.381]	[-0.518]	[-0.624]	[-0.380]	[-0.530]	[-0.549]
Inner x Qtr + 1	-0.099	-0.093	-0.095	-0.070	-0.072	-0.076	-0.057	-0.062	-0.060	-0.047	-0.053	-0.058
	[-1.039]	[-0.952]	[-0.914]	[-0.912]	[-0.926]	[-1.031]	[-0.978]	[-1.171]	[-1.156]	[-1.195]	[-1.339]	[-1.439]
Inner x Qtr + 2	-0.152	-0.170	-0.156	-0.126	-0.134	-0.139	-0.100	-0.106	-0.089	-0.074	-0.077	-0.082
	[-1.185]	[-1.300]	[-1.100]	[-1.176]	[-1.223]	[-1.323]	[-1.209]	[-1.355]	[-1.239]	[-1.360]	[-1.461]	[-1.687]
Inner x Qtr + 3	-0.149**	-0.120**	-0.087	-0.066	-0.051	-0.036	-0.019	-0.025	-0.031	-0.024	-0.024	-0.045*
	[-2.551]	[-2.100]	[-1.618]	[-1.619]	[-1.205]	[-0.877]	[-0.625]	[-0.901]	[-1.038]	[-0.986]	[-0.999]	[-1.768]
Inner x Qtr + 4	-0.194**	-0.159*	-0.145	-0.105	-0.106	-0.109*	-0.078	-0.081	-0.067	-0.055	-0.051	-0.068*
	[-2.131]	[-1.941]	[-1.624]	[-1.640]	[-1.617]	[-1.700]	[-1.466]	[-1.664]	[-1.321]	[-1.397]	[-1.327]	[-1.838]
N	31266	34110	36378	54162	57789	60939	83835	87939	92925	119448	126765	131985
R-sq	0.718	0.718	0.718	0.711	0.707	0.707	0.696	0.692	0.691	0.677	0.676	0.673
Postal code FE	X	X	X	X	X	X	X	X	X	X	X	X
Rel qtr FE	X	X	X	X	X	X	X	X	X	X	X	X
Claim ring FE	X	X	X	X	X	X	X	X	X	X	X	X
Claim quarter FE	X	X	X	X	X	X	X	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 3: Treatment Effects by Ring Width and Distance between Inner and Outer Rings

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between households in inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. The columns are in four groups of three regressions. Within each group, the three regressions vary the distance between inner and outer rings across three values, 0 m, 100 m, and 200 m. The four groups differ in the width of the inner and outer rings. The first group sets ring widths at 100 m, the second group at 200 m, the third group at 300 m, and the fourth group at 400 m. For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	T: 0-100m			T: 0-200m			T: 0-300m			T: 0-400m		
	C: 100-200m	C: 200-300m	C: 300-400m	C: 200-400m	C: 300-500m	C: 400-600m	C: 300-600m	C: 400-700m	C: 500-800m	C: 400-800m	C: 500-900m	C: 600-1000m
Inner x After	-1.635***	-1.612***	-1.538***	-1.181***	-1.157***	-1.200***	-0.881***	-0.901***	-0.899***	-0.695***	-0.698***	-0.711***
	[-9.667]	[-8.624]	[-7.097]	[-7.111]	[-7.102]	[-7.449]	[-6.404]	[-7.023]	[-6.962]	[-7.267]	[-7.937]	[-8.817]
N	6948	7580	8084	12036	12842	13542	18630	19542	20650	26544	28170	29330
R-sq	0.911	0.911	0.911	0.909	0.908	0.909	0.906	0.904	0.905	0.901	0.899	0.899
Postal code FE	X	X	X	X	X	X	X	X	X	X	X	X
Post-claim FE	X	X	X	X	X	X	X	X	X	X	X	X
Claim ring FE	X	X	X	X	X	X	X	X	X	X	X	X
Claim quarter FE	X	X	X	X	X	X	X	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 4: Treatment Effects by Distance between Inner and Outer Rings

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between households in inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. Inner ring postal codes are postal codes within 100 m (0.1 km) of the claimant postal code. Outer ring postal codes are postal codes in a 100 m ring around the claimant postal code with differing distances from the inner ring for each column. The nine columns vary the distance between inner and outer ring postal codes by 100 m, with the first column placing them next to each other and the ninth column placing them 800 m apart. For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	0-100 v 100-200m	0-100 v 200-300m	0-100 v 300-400m	0-100 v 400-500m	0-100 v 500-600m	0-100 v 600-700m	0-100 v 700-800m	0-100 v 800-900m	0-100 v 900-1000m
Inner x After	-1.635***	-1.612***	-1.538***	-1.557***	-1.621***	-1.601***	-1.561***	-1.575***	-1.663***
	[-9.667]	[-8.624]	[-7.097]	[-8.833]	[-7.937]	[-8.716]	[-8.469]	[-9.466]	[-9.751]
N	6948	7580	8084	8386	8784	8996	9494	10012	9944
R-sq	0.911	0.911	0.911	0.910	0.912	0.909	0.912	0.909	0.911
Postal code FE	X	X	X	X	X	X	X	X	X
Post-claim FE	X	X	X	X	X	X	X	X	X
Claim ring FE	X	X	X	X	X	X	X	X	X
Claim quarter FE	X	X	X	X	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 5: Treatment Effects by Purchase Data Window Length

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between households in inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. Inner ring postal codes are postal codes within 100 m (0.1 km) of the claimant postal code and outer ring postal codes are postal codes between 100 and 200 m (0.2 km) of the claimant postal code. The eight columns vary the number of quarters included in the pre- and post-claim windows, with the first column including one quarter before and after the claim and the eighth column including eight quarters before and after the claim. For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	1 qtr	2 qtrs	3 qtrs	4 qtrs	5 qtrs	6 qtrs	7 qtrs	8 qtrs
Inner x After	-1.041***	-1.227***	-1.386***	-1.635***	-1.910***	-2.155***	-2.461***	-2.836***
	[-7.387]	[-7.560]	[-9.701]	[-9.667]	[-8.144]	[-7.191]	[-9.114]	[-10.089]
N	6948	6948	6948	6948	6948	6948	6948	6948
R-sq	0.796	0.871	0.903	0.911	0.906	0.901	0.898	0.895
Postal code FE	X	X	X	X	X	X	X	X
Post-claim FE	X	X	X	X	X	X	X	X
Claim ring FE	X	X	X	X	X	X	X	X
Claim quarter FE	X	X	X	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 6: Treatment Effects by Neighborhood Average Income

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. Inner ring postal codes are postal codes within 100 m (0.1 km) of the claimant postal code and outer ring postal codes are postal codes between 100 and 200 m (0.2 km) of the claimant postal code. The five columns separate warranty claims into quintiles based on the average income level of the dissemination area (DA) of the warranty claim, with the first column having the lowest average income and the last column having the highest. For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	Median Income Quintiles				
	Bottom quintile	Quintile 2	Quintile 3	Quintile 4	Top quintile
Inner x After	-3.793***	-2.015***	-1.468***	-1.046***	-1.479***
	[-7.262]	[-4.704]	[-4.257]	[-3.243]	[-4.157]
N	925	1494	1350	1582	1494
R-sq	0.948	0.938	0.872	0.952	0.898
Postal code FE	X	X	X	X	X
Post-claim FE	X	X	X	X	X
Claim ring FE	X	X	X	X	X
Claim quarter FE	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 7: Treatment Effects by Neighborhood Population Density

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. Inner ring postal codes are postal codes within 100 m (0.1 km) of the claimant postal code and outer ring postal codes are postal codes between 100 and 200 m (0.2 km) of the claimant postal code. The four columns separate warranty claims into quintiles based on the population density of the dissemination area (DA) of the warranty claim, with the first column showing results for the second lowest population density quintile and the last column for the highest. The lowest density quintile, which is composed of rural areas, is omitted in the table as it is in analyses. For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	Population Density Quintiles			
	Quintile 2	Quintile 3	Quintile 4	Top quintile
Inner x After	-3.273***	-1.542***	-1.119***	-0.931***
	[-5.446]	[-5.617]	[-2.859]	[-3.268]
N	608	784	1058	1962
R-sq	0.917	0.975	0.915	0.802
Postal code FE	X	X	X	X
Post-claim FE	X	X	X	X
Claim ring FE	X	X	X	X
Claim quarter FE	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 8: Treatment Effects by Neighborhood Immigrant Proportion

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. Inner ring postal codes are postal codes within 100 m (0.1 km) of the claimant postal code and outer ring postal codes are postal codes between 100 and 200 m (0.2 km) of the claimant postal code. The five columns separate warranty claims into quintiles based on the immigrant proportion of the population of the dissemination area (DA) of the warranty claim, with the first column having the lowest immigrant proportion and the last column having the highest proportion. For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	Immigrant Proportion Quintiles				
	Bottom quintile	Quintile 2	Quintile 3	Quintile 4	Top quintile
Inner x After	-2.304***	-1.960***	-1.684***	-1.837***	-1.399***
	[-5.909]	[-5.912]	[-6.436]	[-5.021]	[-4.259]
N	1266	1438	1236	1442	1548
R-sq	0.956	0.951	0.931	0.822	0.831
Postal code FE	X	X	X	X	X
Post-claim FE	X	X	X	X	X
Claim ring FE	X	X	X	X	X
Claim quarter FE	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 9: Treatment Effects by Claimed Brand Claims-to-Sales Ratio

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. The five columns examine warranty claims across quintiles of the aggregate claim rate for the claimed product's manufacturer over the last two years for the claimed product category, with the first column containing the lowest claim rate (i.e., highest quality) brands and the fifth column containing the highest claim rate brands (i.e., lowest quality). For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	Manufacturer Claim Rate Quintiles				
	Lowest quintile	Quintile 2	Quintile 3	Quintile 4	Highest quintile
Inner x After	-2.447**	-2.399***	-1.820***	-2.033***	-1.601***
	[-2.645]	[-6.443]	[-4.383]	[-8.541]	[-5.596]
N	990	928	1240	1274	1856
R-sq	0.734	0.940	0.891	0.939	0.958
Postal code FE	X	X	X	X	X
Post-claim FE	X	X	X	X	X
Claim ring FE	X	X	X	X	X
Claim quarter FE	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 10: Treatment Effects by Claimed Brand Market Share

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. The five columns examine warranty claims across quintiles of purchase market share for the claimed product's manufacturer over the last two years for the claimed product category, with the first column containing the lowest market share brands and the fifth column containing the highest market share brands. For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	Manufacturer Market Share Quintiles				
	Bottom quintile	Quintile 2	Quintile 3	Quintile 4	Top quintile
Inner x After	-1.498**	-2.396***	-2.017***	-1.814***	-1.866***
	[-2.187]	[-4.912]	[-3.788]	[-5.316]	[-7.176]
N	1086	1054	1506	1422	1776
R-sq	0.906	0.958	0.820	0.879	0.950
Postal code FE	X	X	X	X	X
Post-claim FE	X	X	X	X	X
Claim ring FE	X	X	X	X	X
Claim quarter FE	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr

Table 11: Treatment Effects by Neighborhood House Price Index Value

This table presents the results of geographic classical difference-in-differences (DID) regressions on the change in neighbors' purchasing behavior following a warranty claim. The dependent variable in all specifications is the difference between the number of purchases of the claimed brand and purchases of all other consumer durable good brands in a given postal code in the two years before or after a warranty claim. The reported coefficient represents the change in the difference in the dependent variable between inner ring (treated) and outer ring (control) postal codes from before to after a warranty claim, with the corresponding t-statistic reported in brackets below each coefficient. Inner ring postal codes are postal codes within 100 m (0.1 km) of the claimant postal code and outer ring postal codes are postal codes between 100 and 200 m (0.2 km) of the claimant postal code. The five columns separate warranty claims into quintiles based on the average House Price Index (HPI) value of the Forward Sortation Area (FSA) of the warranty claim, with the first column having the lowest average HPI values and the last column having the highest. For all regressions, we impose postal code, post-claim quarter, warranty claim event, and claim quarter fixed effects. Standard errors are clustered at warranty claim and claim quarter levels. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

	HPI Quintiles				
	Bottom quintile	Quintile 2	Quintile 3	Quintile 4	Top quintile
Inner x After	-1.171***	-1.988***	-1.952***	-2.385***	-1.913***
	[-4.843]	[-5.882]	[-3.441]	[-4.239]	[-4.158]
N	1170	1400	1574	1690	1410
R-sq	0.860	0.911	0.810	0.926	0.962
Postal code FE	X	X	X	X	X
Post-claim FE	X	X	X	X	X
Claim ring FE	X	X	X	X	X
Claim quarter FE	X	X	X	X	X
SE clustering	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr	Ring, qtr