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Estimating Causal Installed-Base Effects: A Bias-Correction Approach

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New empirical models of consumer demand that incorporate social preferences, observational learning, word-of-mouth or network effects have the feature that the adoption of others in the reference group - the "installed-base" - has a causal effect on current adoption behavior. Estimation of such causal installed-base effects is challenging due to the potential for spurious correlation between the adoption of agents, arising from endogenous assortive matching into social groups (or homophily) and from the existence of unobservables across agents that are correlated. In the absence of experimental variation, the preferred solution is to control for these using a rich specification of fixed-effects, which is feasible with panel data. We show that fixed-effects estimators of this sort are inconsistent in the presence of installed-base effects; in our simulations, random-effects specifications perform even worse. Our analysis reveals the tension faced by the applied empiricist in this area: a rich control for unobservables increases the credibility of the reported causal effects, but the incorporation of these controls introduces biases of a new kind in this class of models. We present two solutions: an instrumental variable approach, and a new bias-correction approach, both of which deliver consistent estimates of causal installed-base effects. The bias-correction approach is tractable in this context because we are able to exploit the structure of the problem to solve analytically for the asymptotic bias of the installed-base estimator, and to incorporate it into the estimation routine. Our approach has implications for the measurement of social effects using non-experimental data, and for measuring marketing-mix effects in the presence of state-dependence in demand, more generally. Our empirical application to the adoption of the Toyota Prius Hybrid in California reveals evidence for social influence in diffusion, and demonstrates the importance of incorporating proper controls for the biases we identify.

Keywords: Contagion, Social Interactions, Installed-base Effects, Homophily, Correlated Unobservables, Diffusion, Product Adoption, Toyota Prius.

JEL Codes: C13, C21, C23, C51, L00, M30.

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1 Introduction

We investigate the measurement of causal installed-base effects in marketing models of consumer adoption. We use the term "installed-base" to refer to the set of agents in a user's reference group that have adopted the focal product or service. Agents may care about the adoption behavior of other users because others' actions or welfare directly affects their utility (social preferences); because adoption by others updates the users' beliefs about existence or attributes (observational learning); because feedback from others affects beliefs directly (word-of-mouth); because adoption by others affects the users' value of the product (network effects); or because some combination of these are at play. In Marketing parlance, these structural constructs have been summarily referred to as "contagion" or "social effects". An important goal of empirical models of consumer demand that incorporate contagion is to measure the causal effects of the installed-base on current adoption behavior. In addition, the sign of the installed-base effect may also be of independent interest: for instance, herd behavior (Bikhchandani, Hirshleifer, and Welch, 1992) may result in positive installed-base effects, while exclusivity or snob effects (Leibenstein, 1950) may result in negative installed-base effects. A firm may then be interested in whether the combination of potential herding, snobbery and desire for exclusivity result in net positive or negative social influence. These measures feed into the development of targeted marketing interventions (e.g. seeding key opinion leaders: Yoganarasimhan, 2010) and/or resource allocation decisions that exploit the measured contagion (e.g. penetration pricing in the presence of network effects: Kalish and Lilien, 1983).

In spite of the importance for Marketing, measurement of causal installed-base effects from behavioral data has proven to be very challenging. Installed-base effects have formed the basis for the extensive aggregate diffusion literature in Marketing (Bass, 1969; Mahajan, Muller, and Bass, 1990). This literature treats the entire population of past adopters as the reference group for a representative agent's product adoption decision. With access to more disaggregate data on consumer's social networks, the recent empirical literature has used a more nuanced view of the reference group, leveraging social networks based on self-elicitation (Conley and Udry, 2008; Kratzer and Lettl, 2009; Iyengar, van den Bulte, and Valente, 2010; Nair, Manchanda, and Bhatia, 2010); dorm/work location (Sacerdote, 2001; Dufflo and Saez, 2003; Sorensen, 2006); ethnic/cultural proximity (Bertrand, Mullainathan, and Luttmer, 2000; Munshi and Myaux, 2006); or as in the current application, geographic location (Topa, 2001; Arzaghi and Henderson, 2007; Bell and Song, 2007; Manchanda, Xie, and Youn, 2008; Choi, Hui, and Bell, 2010; McShane, Bradlow, and Berger, 2010; Nam, Manchanda, and Chintagunta, 2010; Bollinger and Gillingham, 2011).

When the reference group is a subset of the population, the immediate concern that arises is one of self-selection: unobserved tastes that cause two individuals to select to be part of the same group (homophily), also may also cause them to behave similarly in product adoption behavior. In the geographic case, the concern is of assortive sorting on unobservables of households into communities. For instance, it is possible that environmentally conscious households prefer to live in "green" communities; at the same time, it may be that *ceteris paribus*, households in green communities tend to adopt environmentally friendly automobiles like the Toyota Prius early. Hence, an observed correlation in the data between the propensity to adopt a Prius, and the installed-base of Prius adopters in a community could simply reflect sorting on unobserved environmental friendliness, and not a causal effect of the past adoption on current behavior. Similar concerns arise due to the presence of spatially and temporally correlated unobservables that make households behave similarly. For instance, Toyota's advertising activity or local promotions targeted at a community could generate correlation in the adoption behavior of community members, and generate clustering in spatial patterns of diffusion. If not controlled for, this could manifest itself as a spurious installed-base effect. Accounting for such correlated, but omitted unobservables has been established to be important to the inference of causal Marketing effects using both spatial (e.g., Bronnenberg and Mahajan, 2001), and temporal (e.g., Rao, 1986) sources of variation in observational data.[1]

It is now taken as *fait accompli* in the empirical literature that the credibility of measures of social influence rests on the extent to which these confounds are appropriately addressed. Recognizing this, researchers now typically include rich specifications of fixed or random effects to control for these unobserved sources of correlation. Random effects that involve distributional assumptions suffer from specification biases if the distributional assumptions are incorrect. The validity of the panel-data random effects estimator also depends on the assumption of independence with included within-group covariates, which is difficult to reconcile with an omitted variables interpretation for unobservables. Fixed-effects address both these concerns; and consequently, one of the most general way to control for confounding is to accommodate a rich specification of fixed-effects. The fixed-effects provide a semi-parametric control for unobservables that assuage the misspecification concerns, and also allow for arbitrary patterns of correlation. In the Prius example, for instance, one may include zip-and-time fixed-effects to flexibly control for unobserved common shocks that may be both spatially and temporally correlated in an unknown way.

Unfortunately, we show that though attractive, fixed-effects estimators of this sort are *inconsistent* in the presence of installed-base effects. As expected, random-effects do not ameliorate

[1] Manski (1993) pointed out a third confound that arises if the adoption decisions of agents in the community are simultaneously determined with that of past adopters. This is likely to be of lesser concern in the case of installed-base effects as consumers are assumed to condition on the adoption of their installed-base, which is taken as predetermined. Fundamentally, this requires that each adopting consumer is myopic, or alternatively, is forward-looking, but forms beliefs under the assumption he is "small" relative to the size of the adopting local community such that he anticipates his individual adoption decision will not significantly influence the adoption behavior of any one consumer in the future.

the issue: in our simulations, random-effects specifications perform even worse. We derive the asymptotic distribution of the fixed-effects estimator and characterize the nature of the bias. Our analysis reveals the tension faced by the applied empiricist wishing to do careful work in this area: a rich control for unobservables increases the credibility of the reported causal effects, but the incorporation of these controls introduces biases of a new kind in this class of models. Our analysis is related to, but conceptually distinct from the "dynamic panel data" literature which discusses the inconsistency generated by the presence of lagged dependent variables in models with fixed-effects or random effects (Nerlove, 1971; Nickell, 1981; Kiviet, 1995; Judson and Owen, 1999; Bun and Carree, 2005).[2] In the spatio-temporal models of diffusion we consider, there are no lagged endogenous variables, but instead, lagged aggregations of past decisions made by other consumers are included as explanatory variables. This is the work-horse empirical model employed by the vast and burgeoning social effects literature.

We then present two solutions to addressing the bias: first, an instrumental variable (IV) approach, and second, a new bias-correction approach, both of which deliver consistent estimates of causal installed-base effects. The IV approach requires access to an exclusion restriction implying a variable that generates exogenous variation in the installed-base. In practice, this variation may be hard to find (we discuss one approach below). On the other hand, the bias-correction approach is tractable in this context because we are able to exploit the structure of the problem to solve analytically for the asymptotic bias of the installed-base estimator, and to incorporate it into the estimation routine. The bias-correction approach utilizes an asymptotic approximation, and requires access to a large dataset. In practice, we expect this requirement to be easily met given the nature of data that Marketers now have access to (for example, our data contain about 11 Million observations on automobile purchases). The approach does not depend on finding valid instruments, and hence is quite attractive in many empirical contexts, where it may be hard to find suitable instruments. We present Monte Carlo simulations that establish the face validity and internal consistency of the approach in our context, and assess its performance relative to a series of alternative estimators.

We then present an empirical application to studying social spillovers in the adoption of the Toyota Prius Hybrid electric car in the state of California. We specify an individual-level model to test for spillovers, and to measure their magnitude. Our individual-level adoption data come from R.L. Polk and Company, and are drawn from local motor vehicle registration records. The

[2]It is easy to see that in the dynamic panel model, $y_{it} = \nu_i + \gamma y_{it-1} + \epsilon_{it}$, with fixed-effect ν_i for unit i, first-differencing is problematic even if the innovations, ϵ_{it} are IID: in the first-differenced model, $y_{it} - y_{it-1} = \gamma\left(y_{it-1} - y_{it-2}\right) + \left(\epsilon_{it} - \epsilon_{it-1}\right)$, the errors, $\left(\epsilon_{it} - \epsilon_{it-1}\right)$ are correlated with the included variable, $\left(y_{it-1} - y_{it-2}\right)$. Clearly, random effects are more problematic because the assumption that ν_i is uncorrelated with the included variable, y_{it-1}, is violated by construction. Similarly, the Least Squares Dummy Variable (LSDV) estimator, $y_{it} - \bar{y}_i = \gamma\left(y_{it-1} - \bar{y}_{i,-1}\right) + \left(\epsilon_{it} - \bar{\epsilon}_i\right)$, is inconsistent for γ because the mean differenced errors, $\left(\epsilon_{it} - \bar{\epsilon}_i\right)$ are correlated with the mean-differenced lags, the included variable, $\left(y_{it-1} - \bar{y}_{i,-1}\right)$. Starting with (Anderson and Hsiao, 1981) and (Arellano and Bond, 1991), it is common to use the history of lagged-levels and lagged-differences as instruments to address the inconsistency.

data comprise the complete census of all Prius purchases in California since its introduction in 2001 till March 2007. We see strong spatial correlation in adoption patterns of the Prius which are suggestive of social spillovers operating over geography. The richness of our individual-level adoption data enable us to accommodate a rich set of controls for unobserved factors: we include a fixed-effect for each zip-quarter combination in the data (over 64,000 fixed-effects). We specify the installed-base at the level of a zip code and month, and explore robustness to different levels of geographical aggregation, including the level of a city and county.

We find the incorporation of fixed-effects is important to obtain a valid measure of the social effects. We find the correlation in the unobservables accommodated by the fixed-effects is not easily approximated by parametric specifications (e.g., multivariate normals), underscoring the importance of the flexible control. In general, we find that controls for homophily and correlated unobservables reduces the magnitude of the installed-base effects. In our most general fixed-effects specification without the corrections we propose, we find the installed-base effect becomes negative and significant, suggesting snob or exclusivity effects. We discuss why this is spurious, and driven by the downward bias we identify. Controlling for this, we find evidence for positive and significant installed-base effects. The flip in the sign has economic consequences for marketers, and illustrate the practical consequences of the biases we identify for empirical work.

For the IV approach, we exploit an institutional feature of the Hybrid market that contagion across spatially co-located households likely occurs via visual observation of consumption. The institutional feature is that competing Hybrid vehicles such as the Honda Civic Hybrid were visually exact versions of their non-Hybrid versions. The identifying assumption is that on account of this aspect, Hybrid adoption of these other brands is not subject to social effects, and may be used as instruments for the Prius installed-base. We also use the installed-bases of flex-fuel vehicles as instruments ("flex-fuel" vehicles can use ethanol-blended gasoline). These identifying assumptions may not be valid, and may be falsified under alternative stories of spillover mechanisms. Hence, we also present estimates using our bias-correction approach. We find the estimates of this approach correspond broadly to those obtained using the IV estimator in our empirical application. In addition, under this approach, the social effects are precisely estimated.

To assess robustness, we expect that social effects that operate via geographic proximity should dissipate when we define the network over larger geographic areas. We find the estimated installed-base effects are indeed weaker when we define the network at the level of the city, and statistically insignificant when we define the network at the level of the county. These results are consistent with social effects that operate over geographic contiguity. Further, our estimator finds no social effects when applied to data on the adoption of the Honda Civic Hybrid, consistent with *a priori* expectations. In our preferred specification for the Prius, we find an average elasticity of about 5.3, i.e., for every 1% increase in the installed-base of the Prius in the zip code of the individual, there is on average a 5.3% increase in the probability of purchase of the Prius (as a percentage of the baseline purchase probability). These numbers suggest the installed-base effects are economically significant for the Prius, and illustrates the empirical feasibility of our approach.

The rest of the paper is organized as follows. In section 2, we investigate the biases that result in models with installed-base effects and fixed-effects. To develop the intuition, we first discuss a model with installed-base as the only covariate, and then generalize the results for a model with other (exogenous) covariates. We discuss IV and bias-correction approaches to addressing the bias. In section 3, we present our empirical application for the Hybrid automobile market. We present IV and bias-corrected approaches to obtain consistent estimates of the installed-base effect and conduct a series of robustness checks. Finally, we conclude in section 4.

2 Consistent Estimation of Installed-Base Effects

2.1 No Exogenous Covariates

We present the discussion in the context of a linear probability model in which the installed-base of past adopters in the local neighborhood of the consumer is the only included covariate (the analysis goes through with little change for other linear models). In the Appendix, we discuss an extension to including additional covariates. Anticipating our empirical application, we assume the analyst has access to individual-level data on the adoption of the focal product, which describes the location (zip code) of the individual, as well as the time of adoption (month). We will work with a *conditional* set-up, in which we model the probability an individual will buy the focal product (e.g, Toyota Prius) in a given month, conditional on the decision to buy a car that month. Specifying a conditional model enables us to abstract from modeling the hazard-rate of adoption, and to avoid specifying structural constructs relating to price and quality expectations for the adopting consumer. A more detailed discussion of the use of a linear probability model, and of the conditional set-up is presented in section (3.3.1), where we discuss our empirical application.

Consider consumer i, who lives in market m_i and has decided to purchase an automobile in time period (month) t_i. Let y_i denote whether consumer i buys the Toyota Prius in month t_i. Reflecting the conditional model, $y_i = 1$ if the consumer buys the Prius and 0 if he buys another car (i.e., only individuals who buy a car during the observation window are included in the dataset. The choice of whether/when to buy a car is not modeled). Let the installed-base of the Toyota Prius in market m_i and time period t_i be denoted by $X_{m_i t_i}$. Thus,

$$X_{m_it_i} = \sum_{\tau=1}^{t_i-1} \sum_{j=1}^{N_{m_i\tau}} y_j \tag{1}$$

where $N_{m_i\tau}$ is the total number of people who purchase automobiles in market m_i in time period τ. We specify the decision to buy a Prius is related to the installed-base according to a linear probability model,

$$y_i = f_{m_iq_i} + X_{m_it_i}\beta + \varepsilon_i \tag{2}$$

Here, $f_{m_iq_i}$ is a fixed effect specified at the market-quarter level. The market-specific component of the fixed effect controls for unobserved market specific characteristics on which sorting may occur, thereby controlling for across-market selection (or homophily). The time-period specific fixed-effects control flexibly for unobserved time-trends that may generate co-movement in adoption over time thereby controlling for spurious temporal correlation. Here, we allow a very general specification in which the unobservables over time are also allowed to be different for each market in a general way (market specific time-period fixed-effects). This is possible because we observe hundreds of individuals making purchase decisions for each market-month combination. Since the installed-base $X_{m_it_i}$ is the same for all consumers in market m_i and at time t_i, the fixed effect cannot be specified at level of m_i and t_i. Therefore, we specify the fixed effect at the level of market and an aggregation of time. In the empirical application, this aggregation is at the level of the quarter. Thus, we include a fixed-effect for each market-quarter combination. This results in over 64,000 fixed-effects, and is one of the most comprehensive set of controls considered in the literature.

In Equation (2) above, β, which is the parameter of interest, is the installed-base effect, and ε_i is the observation specific unobservable assumed to be independent and identically distributed across consumers, markets and time. The IID assumption also implies the installed-base $X_{m_it_i}$ is independent of ε_i. Least Squares estimation of the above model is equivalent to the *mean differenced* estimator obtained by subtracting the means of both sides of Equation (2) over all observations within each market-quarter (the level of the fixed-effect). The fixed-effects are eliminated in this differenced equation,

$$(y_i - \bar{y}_{m_iq_i}) = \left(X_{m_it_i} - \bar{X}_{m_iq_i}\right)\beta + (\varepsilon_i - \bar{\varepsilon}_{m_iq_i}) \tag{3}$$

where $\bar{y}_{m_iq_i}$, $\bar{X}_{m_iq_i}$ and $\bar{\varepsilon}_{m_iq_i}$ are the means of the respective variables taken over all the $N_{m_iq_i}$ observations within market m_i and quarter q_i. Thus,

$$\bar{y}_{m_iq_i} = \frac{1}{N_{m_iq_i}} \sum_{j=1}^{N_{m_iq_i}} y_j \tag{4}$$

$$\bar{X}_{m_iq_i} = \frac{1}{N_{m_iq_i}} \sum_{j=1}^{N_{m_iq_i}} X_{m_jt_j} = \frac{1}{N_{m_iq_i}} \sum_{j=1}^{N_{m_iq_i}} \left(\sum_{\tau=1}^{t_j-1} \sum_{k=1}^{N_{m_j\tau}} y_k \right) \tag{5}$$

$$\bar{\varepsilon}_{m_i q_i} = \frac{1}{N_{m_i q_i}} \sum_{j=1}^{N_{m_i q_i}} \varepsilon_j \tag{6}$$

where we use the definition of the installed-base in Equation (1) to obtain Equation (5). Assume there are a total of N consumers observed in the data, and T time periods per quarter.

The *within-estimator* of the social effect $\hat{\beta}$, is obtained as,

$$\hat{\beta} = \frac{\sum_i \left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)\left(y_i - \bar{y}_{m_i q_i}\right)}{\sum_i \left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)^2} \tag{7}$$

Proposition 1. The within-estimator $\hat{\beta}$ is inconsistent, and the asymptotic bias is negative.

Proof. We present the proof by computing the probability limit of $\hat{\beta}$ as $N \to \infty$, holding the number of observations within each zip code-quarter (i.e. $N_{M_i q_i}$) fixed. The proof is constructive as this will feed into the development of our bias-corrected estimator. Substituting for $(y_i - \bar{y}_{m_i q_i})$ from (3), we get,

$$\hat{\beta} - \beta = \frac{\sum_i \left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)\left(\varepsilon_i - \bar{\varepsilon}_{m_i q_i}\right)}{\sum_i \left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)^2} \tag{8}$$

It follows from Slutsky's Theorem (ST) and the Mann Wald Continuous Mapping Theorem (MWCMT) that,

$$\underset{N \to \infty}{\text{plim}} \left(\hat{\beta} - \beta\right) = \frac{\underset{\text{N}\to\infty}{\text{plim}} \frac{1}{\text{N}} \sum_i \left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)\left(\varepsilon_i - \bar{\varepsilon}_{m_i q_i}\right)}{\underset{\text{N}\to\infty}{\text{plim}} \frac{1}{\text{N}} \sum_i \left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)^2} \tag{9}$$

and from Khintchine's weak law of large numbers that,

$$\underset{N \to \infty}{\text{plim}} \left(\hat{\beta} - \beta\right) = \frac{\mathbb{E}\left[\left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)\left(\varepsilon_i - \bar{\varepsilon}_{m_i q_i}\right)\right]}{\mathbb{E}\left[\left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)^2\right]} \equiv \frac{\mathcal{A}}{\mathcal{B}} \tag{10}$$

The denominator $\mathcal{B}$ in Equation (10) is non-zero by construction, hence inconsistency of the within-estimator $\hat{\beta}$ is related to the fact that the expectation in the numerator $\mathcal{A}$ is non-zero even with an infinite number of consumers (N). We can write $\mathcal{A}$ as the sum of four terms,

$$\mathcal{A} = \mathbb{E}\left[\left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)\left(\varepsilon_i - \bar{\varepsilon}_{m_i q_i}\right)\right] = \mathbb{E}\left[X_{m_i t_i}\varepsilon_i\right] - \mathbb{E}\left[X_{m_i t_i}\bar{\varepsilon}_{m_i q_i}\right] - \mathbb{E}\left[\bar{X}_{m_i q_i}\varepsilon_i\right] + \mathbb{E}\left[\bar{X}_{m_i q_i}\bar{\varepsilon}_{m_i q_i}\right] \tag{11}$$

Consider the first term. It follows from the IID assumption that, $\mathbb{E}\left[X_{m_i t_i}\varepsilon_i\right] = 0$. For the second term, we use (1) and (6) to get,

$$X_{m_i t_i}\bar{\varepsilon}_{m_i q_i} = \frac{1}{N_{m_i q_i}} \left[\sum_{\tau=1}^{t_i - 1} \sum_{j=1}^{N_{m_i \tau}} y_j\right] \left[\sum_{j=1}^{N_{m_i q_i}} \varepsilon_j\right] \tag{12}$$

To evaluate this, consider observations in the first period of the quarter. First, note the installed-base for the first period includes purchases only in periods before the start of the quarter q_i.

Second, $\bar{\varepsilon}_{m_i q_i}$ contains only ε_j terms for observations in that quarter. Hence, the expectation $E\left[X_{m_i t_i}\bar{\varepsilon}_{m_i q_i}\right] = 0$ for these observations. For observations in the second time period, there will be as many $y_i\varepsilon_i$ terms as there are observations in the *first* period, since the y terms for the first period enter the installed-base for all observations in the second period and will be multiplied by the corresponding ε terms contained in $\bar{\varepsilon}_{m_i q_i}$. For the third period of the quarter, there will be as many $y_i\varepsilon_i$ terms as there are observations in the first and second periods and so on. Assuming that there are no systematic differences in the number of observations across periods - this is not necessary, though it makes the notation simpler - the proportion of observations each period is $1/T$, and $N_{m_i q_{ii}} = nT$ where n is the number of observations within a time period. Therefore, the conditional expectation is,

$$\mathbb{E}\left[X_{m_i t_i}\bar{\varepsilon}_{m_i q_i}|\text{period } t\right] = \frac{[(t-1)n]}{nT}\mathbb{E}\left[y_i\varepsilon_i\right] = \frac{(t-1)\,\sigma_\varepsilon^2}{T} \tag{13}$$

The unconditional expectation is then,

$$\begin{aligned}\mathbb{E}\left[X_{m_i t_i}\bar{\varepsilon}_{m_i q_i}\right] &= \frac{1}{T}\left[\frac{1}{T}(0)\,\sigma_\varepsilon^2 + \frac{1}{T}(1)\,\sigma_\varepsilon^2 + \ldots \frac{1}{T}(T-1)\,\sigma_\varepsilon^2\right] = \frac{1}{T^2}\frac{(T-1)\,T}{2}\sigma_\varepsilon^2 \\ &= \frac{T-1}{2T}\sigma_\varepsilon^2 \end{aligned} \tag{14}$$

With the analysis at a monthly level (i.e. $T = 3$) and assuming no systematic differences in the number of observations in different months of the quarter, we have the second term is,

$$\mathbb{E}\left[X_{m_i t_i}\bar{\varepsilon}_{m_i q_i}\right] = \frac{1}{3}\sigma_\varepsilon^2 \tag{15}$$

Now consider the third term in Equation (11). We can compute,

$$\bar{X}_{m_i q_i}\varepsilon_i = \left[\frac{1}{N_{m_i q_i}}\sum_{j=1}^{N_{m_i q_i}}\left(\sum_{\tau=1}^{t_j-1}\sum_{k=1}^{N_{m_j \tau}} y_k\right)\right]\varepsilon_i \tag{16}$$

To evaluate this, noting that $y_i \perp \varepsilon_j, i \neq j$, we need only focus on those terms in the expansion of this expectation which contain $y_i\varepsilon_i$. Consider an ε_i in the first period. None of the $X_{m_j t_j}$ (installed-base) terms for the first period contain the corresponding y_i term, but each of the installed-base terms for the subsequent periods till T contains a corresponding y_i term. Thus, for every observation in the first period, there are $(T-1)\,n$ terms of the form $y_i\varepsilon_i$. For every ε_i in the second period, there are $(T-2)n$ terms that contain $y_i\varepsilon_i$ and so on. For an ε_i in the final period, there are no corresponding y_i terms. Thus, the expectation conditional on an observation given period t is,

$$\mathbb{E}\left[\bar{X}_{m_i q_i}\varepsilon_i|\text{period } t\right] = \frac{1}{nT}(T-t)\,n\sigma_\varepsilon^2 = \frac{(T-t)\,\sigma_\varepsilon^2}{T} \tag{17}$$

The unconditional expectation is,

$$\mathbb{E}\left[\bar{X}_{m_i q_i}\varepsilon_i\right] == \frac{1}{T}\left[\frac{(T-1)\,\sigma_\varepsilon^2}{T} + \ldots \frac{\sigma_\varepsilon^2}{T}\right] = \frac{1}{T^2}\frac{(T-1)\,T}{2}\sigma_\varepsilon^2 = \frac{T-1}{2T}\sigma_\varepsilon^2$$

With analysis at the monthly level, $T = 3$, and therefore, $\mathbb{E}\left[\bar{X}_{m_i q_i}\varepsilon_i\right] = \frac{\sigma_\varepsilon^2}{3}$.

Finally, the fourth term can be evaluated using a similar logic. The conditional expectation of the fourth term given period t is,

$$\mathbb{E}\left[\bar{X}_{m_i q_i}\bar{\varepsilon}_{m_i q_i}|\text{period } t\right] = \frac{1}{n^2T^2}\left(T-t\right)n^2\sigma_\varepsilon^2 = \frac{T-t}{T^2}\sigma_\varepsilon^2 \tag{18}$$

This gives the unconditional expectation as,

$$\begin{aligned}\mathbb{E}\left[\bar{X}_{m_i q_i}\bar{\varepsilon}_{m_i q_i}\right] &= \frac{1}{T}\left[\frac{(T-1)\,\sigma_\varepsilon^2}{T^2} + \ldots \frac{\sigma_\varepsilon^2}{T^2}\right] = \frac{1}{T^3}\frac{(T-1)\,T}{2}\sigma_\varepsilon^2 \\ &= \frac{T-1}{2T^2}\sigma_\varepsilon^2\end{aligned} \tag{19}$$

With analysis at the month level, this expectation is $\frac{\sigma_\varepsilon^2}{9}$. Putting all four terms together,

$$\mathcal{A} = 0 - \frac{\sigma_\varepsilon^2}{3} - \frac{\sigma_\varepsilon^2}{3} + \frac{\sigma_\varepsilon^2}{9} = -\frac{5\sigma_\varepsilon^2}{9} \tag{20}$$

Finally, defining $\mathcal{B} \equiv \sigma_X^2$, we get,

$$\underset{N\to\infty}{\text{plim}}\left(\hat{\beta} - \beta\right) = -\frac{5}{9}\frac{\sigma_\varepsilon^2}{\sigma_X^2} \tag{21}$$

This is the first result, establishing the inconsistency of the within-estimator for installed-base effects. Further, it establishes the asymptotic bias is always *negative* and its magnitude is proportional to the ratio of the error variance and the within-quarter variance of the installed-base. Hence, including fixed-effects will tend to understate any positive contagion deriving from installed-base effects. Further, depending on the variation in the unobservables relative to the installed-base, this bias can spuriously suggest snob or exclusivity (i.e., negative installed-base effects). We will exploit these results to develop our bias-correction estimator of β. □

To see the next step, note the bias of $\hat{\beta}$ in Equation (21) is a function of the variance of the unobservables, σ_ε^2. Perhaps, we can estimate this bias term by estimating σ_ε^2? Unfortunately, we show below the standard panel-data estimator for σ_ε^2 is also inconsistent.

Proposition 2. The estimator, s^2 for σ_ε^2 is inconsistent, and the asymptotic bias is negative.

Proof. The estimator s^2 given by,

$$s^2 = \frac{\hat{\varepsilon}'\hat{\varepsilon}}{N - N_{MQ} - K} \tag{22}$$

where M is the total number of observations in the data, N_{MQ} is the number of market-quarter combinations in the data (also equal to the number of fixed-effects) and K is the number of regressors, in this case 1.

The i^{th} element of the residual vector $\hat{\varepsilon}$ is given by

$$\begin{aligned} \hat{\varepsilon}_i &= y_i - \hat{f}_{m_i q_i} - X_{m_i t_i}\hat{\beta} \\ &= (y_i - \bar{y}_{m_i q_{ii}}) - \left(X_{m_i t_i} - \bar{X}_{m_i q_i}\right)\hat{\beta} \\ &\equiv \tilde{y}_i - \tilde{X}_i\hat{\beta} \end{aligned} \tag{23}$$

This is the residual in the differenced equation. Moving to vector notation, and noting that $\hat{\beta} = \left(\tilde{X}'\tilde{X}\right)\tilde{X}'\tilde{y}$,

$$\begin{aligned} \hat{\varepsilon} &= \tilde{y} - \tilde{X}\hat{\beta} \\ &= \left(\tilde{y} - \tilde{X}\beta\right) + \left(\tilde{X}\beta - \tilde{X}\hat{\beta}\right) \\ &= \tilde{\varepsilon} - \tilde{X}\left(\tilde{X}'\tilde{X}\right)^{-1}\tilde{X}'\tilde{\varepsilon} \end{aligned} \tag{24}$$

Thus, s^2 is,

$$\begin{aligned} s^2 &= \frac{\hat{\varepsilon}'\hat{\varepsilon}}{N-N_{ZQ}-K} = \frac{N}{N-N_{ZQ}-K}\frac{\hat{\varepsilon}'\hat{\varepsilon}}{N} \\ &= \frac{N}{N-N_{ZQ}-K}\left(\frac{\tilde{\varepsilon}'\tilde{\varepsilon}}{N} - \frac{\tilde{\varepsilon}'\tilde{X}}{N}\left(\frac{\tilde{X}'\tilde{X}}{N}\right)\frac{\tilde{X}'\tilde{\varepsilon}}{N}\right) \end{aligned} \tag{25}$$

By Khintchine's weak law of large numbers,

$$\underset{N\to\infty}{\text{plim}}\left(\frac{\tilde{\varepsilon}'\tilde{\varepsilon}}{N}\right) = \mathbb{E}\left[\tilde{\varepsilon_i}'\tilde{\varepsilon_i}\right] = \mathbb{E}\left[\left(\varepsilon_i - \bar{\varepsilon}_i\right)'\left(\varepsilon_i - \bar{\varepsilon}_i\right)\right] = \sigma_\varepsilon^2 \tag{26}$$

We have already seen in computing the bias for $\hat{\beta}$ (numerator $\mathcal{A}$ in Equation 10) that,

$$\underset{N\to\infty}{\text{plim}}\left(\frac{\tilde{\varepsilon}'\tilde{X}}{N}\right) = \mathbb{E}\left[\tilde{\varepsilon}_i'\tilde{X}_i\right] = -\frac{5}{9}\sigma_\varepsilon^2 \tag{27}$$

Also, defining $\mathbb{E}\left[\tilde{X}_i'\tilde{X}_i\right] = \sigma_X^2$ as before (and noting $\underset{N\to\infty}{\text{plim}}\left(\frac{\tilde{X}'\tilde{X}}{N}\right)$ is equal to this expectation), we apply ST/MWCMT to get,

$$\underset{N\to\infty}{\text{plim}} s^2 = \sigma_\varepsilon^2 - \left(-\frac{5}{9}\sigma_\varepsilon^2\right)\left(\frac{1}{\sigma_X^2}\right)\left(-\frac{5}{9}\sigma_\varepsilon^2\right) = \sigma_\varepsilon^2 - \frac{25}{81}\frac{\sigma_\varepsilon^4}{\sigma_X^2} \tag{28}$$

Thus the probability limit with infinite N is,

$$\underset{N\to\infty}{\text{plim}}\left(s^2 - \sigma_\varepsilon^2\right) = -\frac{25}{81}\frac{\sigma_\varepsilon^4}{\sigma_X^2} \tag{29}$$

Hence, the estimator of the error variance is inconsistent as well. Further, the bias is negative. □

2.2 Adding Exogenous Covariates

We now augment the model to include exogenous covariates other than the installed-base. Let the new covariate vector be denoted by Z.

Proposition 3. The within-estimator $\hat{\beta}$ for β, and s^2 for σ_ε^2 are both inconsistent, negatively biased, and the bias terms are:

$$\underset{N\to\infty}{\text{plim}}\left(\hat{\beta} - \beta\right) = -\frac{5}{9}\frac{\sigma_\varepsilon^2}{\sigma_{XM_ZX}^2} \tag{30}$$

$$\underset{N\to\infty}{\text{plim}}\left(s^2 - \sigma_\varepsilon^2\right) = -\frac{25\sigma_\varepsilon^4}{81\sigma_X^2}\left[\frac{\sigma_X^4}{\sigma_{XM_ZX}^4} + \Sigma_{XZ}\Sigma_{ZM_XZ}^{-1}\Sigma_{ZZ}\Sigma_{ZM_XZ}^{-1}\Sigma_{ZX}\right] \tag{31}$$

Proof. See Appendix. □

Discussion We have shown so far the Least Squares estimators for both the installed-base effects and the variance of the disturbances are inconsistent. The asymptotic bias in both cases is negative, and the magnitude of the bias is a function of the error variance and the within market-quarter variance of the installed-base and the included covariates. With positive social influence and assortive sorting, we expect not controlling for unobservables will result in overstating social effects (positive bias). At the same time, the results imply that controlling for unobservables using fixed-effects will understate any positive social effects (negative bias). The combination of these opposing biases can manifest itself in a given dataset in a net positive or net negative way. Credible measurement of social effects requires addressing both issues.

2.3 Two Solutions

2.3.1 Instrumental Variables Approach

The first solution is to find an instrument that is correlated with the installed-base, but uncorrelated with the included errors. While conceptually simple, in practice, this is a difficult endeavor, as it is hard to find exogenous variation that shifts the installed-base over time but holds current adoption fixed. We present two potential instrumental variables strategies in our empirical application, which are based on specific stories about the mechanism of social contagion. Unfortunately however, these stories are not testable with available observational data. In other contexts, the instruments may be weak, and may not have enough power to measure the installed-base effect precisely. Hence, it is preferable to have other practical methods to address the bias in addition to the IV strategy.

2.3.2 Bias-Correction Approach

The basis for the second approach is the set of asymptotic results in section (2.2). The structure of the problem has the feature that the bias results from the correlation between the mean differenced installed-base and the mean differenced error. Exploiting this allows us to characterize the magnitude of the basis as a function of quantities that are either observed or can be estimated. To understand the procedure, recall from Equation (30) that,

$$\underset{N\to\infty}{\text{plim}}\left(\hat{\beta}-\beta\right)=-\frac{5}{9}\frac{\sigma_{\varepsilon}^{2}}{\sigma_{XM_ZX}^{2}}$$

Thus, the asymptotic bias is a function of σ_{ε}^{2} and $\sigma_{XM_ZX}^{2}$. The latter quantity is a function of the data and can be computed directly. Hence, provided we have a consistent estimate for σ_{ε}^{2} (denoted by $\ddot{\sigma}_{\varepsilon}^{2}$), we could find the exact magnitude of the asymptotic bias. We could then find the bias-corrected estimate for β as,

$$\ddot{\beta}=\hat{\beta}+\frac{5}{9}\frac{\sigma_{\varepsilon}^{2}}{\sigma_{XM_ZX}^{2}} \tag{32}$$

The intuition is simple: we adjust the estimate with an estimate of the bias. However, in order to find the adjustment term, we need consistent estimates of the variance of the disturbance, σ_ε^2. For this, we rely on the asymptotic bias in the estimates from the within-regression of this quantity, which we have computed in Equation (31) earlier. We reproduce that result here for convenience:

$$\underset{N \to \infty}{\text{plim}} \left(s^2\right) = \sigma_\varepsilon^2 - \frac{25\sigma_\varepsilon^4}{81\sigma_X^2}\left[\frac{\sigma_X^4}{\sigma_{XM_ZX}^4} + \Sigma_{XZ}\Sigma_{ZM_XZ}^{-1}\Sigma_{ZZ}\Sigma_{ZM_XZ}^{-1}\Sigma_{ZX}\right]$$

The right-hand side is a function of σ_ε^2 and a set of quantities that can all be computed directly from the data. The left hand side is the estimate of the error variance for a large dataset (large N). Then, this implicitly defines a quadratic equation for σ_ε^2. Thus, we can find a consistent estimate for the error variance by solving the above equation for σ_ε^2. This idea is similar to panel-data approaches for estimating the variance components in the FGLS estimation of the random effects model (see for instance, Greene 1997, page 628). Denote,

$$r = \frac{\sigma_X^4}{\sigma_{XM_ZX}^4} + \Sigma_{XZ}\Sigma_{ZM_XZ}^{-1}\Sigma_{ZZ}\Sigma_{ZM_XZ}^{-1}\Sigma_{ZX} \tag{33}$$

We can compute r directly from the data. Thus, the solution of the quadratic equation gives us the following estimator:

$$\ddot{\sigma}_\varepsilon^2 = \frac{81\sigma_X^2}{50r} \pm \frac{9\sigma_X}{50r}\sqrt{81\sigma_X^2 - 100s^2} \tag{34}$$

A solution to the above equation always exists whenever $81\sigma_X^2 > 100s^2$. Lack of existence of a solution is unlikely to be a binding issue in practice as with large samples, we expect the variance in the installed-base σ_X^2 is a large relative to s^2 (especially as the dependent variable of the equation for which s^2 is estimated is either 0 or 1).

A second issue is that there are two roots. Note when $81\sigma_X^2 \gg 100s^2$, the larger of the two roots ($\ddot{\sigma}_\varepsilon^2 = \frac{81\sigma_X^2}{50r} + \frac{9\sigma_X}{50r}\sqrt{81\sigma_X^2 - 100s^2}$) is usually an unrealistically large number (note the dependent variable, y_i is binary). In our Monte Carlo analysis, we found that invariably the viable estimator is,

$$\ddot{\sigma}_\varepsilon^2 = \frac{81\sigma_X^2}{50r} - \frac{9\sigma_X}{50r}\sqrt{81\sigma_X^2 - 100s^2} \tag{35}$$

When it exists, this estimator always gives us a positive estimate (since $\frac{9\sigma_X}{50r}\sqrt{81\sigma_X^2 - 100s^2} < \frac{81\sigma_X^2}{50r}$ as long as the square root exists).

Once we have the estimate $\ddot{\sigma}_\varepsilon^2$, we can plug it into the expression for the bias-corrected estimator $\ddot{\beta}$ in Equation (32) to obtain consistent estimates of the installed-base effect. The standard errors of the bias-corrected estimates can be obtained using a bootstrap procedure, which we employ in our empirical application.

Discussion To summarize, the bias-correction approach is attractive because it exploits the structure of the problem, and does not require IVs. The approach requires an asymptotic approximation, and is therefore appropriate in contexts such as ours where we have a large volume of data (large N). This is not an atypical situation, especially in recent years, wherein increasingly better access is available to large datasets. In the context of measuring installed-base effects, large-scale datasets are also crucially important, since it facilitates the controls for the confounding factors referred to earlier. Further, the large volume of the data is also a factor in identifying instruments. While high-frequency data are increasingly available, often it is hard to find IVs with equivalent variation.

2.4 Monte Carlo Simulations

We now discuss the results of a series of Monte Carlo simulations we conducted to investigate the empirical strategies presented above and to assess the performance of alternative estimators.

Research Design The research design for our simulations is as follows. First, we assume the sample is drawn from N_m zip codes, N_q quarters and the lowest time period in the data is a month. In each month, we observe N purchase decisions. For the simulations reported below, we set $N_m = 100$, $N_q = 3$ and $N = 30$. For each zip code-quarter combination, we generate a fixed effect $f_{m_i q_i}$ from a normal distribution with a variance σ_f^2. Errors ε_i are then drawn from a standard normal distribution (probit specification). A scalar exogenous covariate Z_i is drawn from a normal distribution with mean $m_Z + \rho f_{m_i q_i}$ and standard deviation σ_Z^2. The mean of this distribution allows for correlation between the fixed effect and the covariate Z_i. This is the default presumption for the researcher. When the correlation ρ is non-zero, the fixed-effects have an "omitted variables" interpretation. Finally, the coefficient β for the installed-base X and γ for the exogenous covariate Z are varied across different simulations. The latent propensity of a consumer to purchase is defined as,

$$u_i = f_{m_i q_i} + X_{m_i t_i}\beta + Z_i\gamma + \varepsilon_i \tag{36}$$

The dependent variable, y_i, are simulated by first setting the installed-bases in the first period to 0 in all zip codes. Then, each observation in the first period is simulated as,

$$y_i = 1 \quad \text{if} \quad u_i > 0 \tag{37}$$

$$y_i = 0 \quad \text{if} \quad u_i < 0 \tag{38}$$

The installed-bases for each zip code for the second period are then set to the aggregations of the choices (y_i) of all observations in the first period in that zip code (i.e. using Equation 1). Second period choices are simulated next, and analogously for subsequent periods. Thus, synthetic data are generated using a probit model, with fixed effects drawn from a random distribution, an installed-base variable constructed using observations in the periods up to the focal period, and other covariates drawn from a random distribution. Further, the covariates are correlated with the fixed effect in some simulations.

For ease of reference, we collect the distributions from which various variables are drawn here,

$$\varepsilon_i \sim N(0,1) \tag{39}$$

$$f_{m_i q_i} \sim N\left(0, \sigma_f^2\right) \tag{40}$$

$$Z_i \sim N\left(m_Z + \rho f_{m_i q_i}, \sigma_Z^2\right) \tag{41}$$

Finally, in some simulations, we draw the fixed effects from a gamma distribution instead of a normal distribution.

Results We run a series of alternative estimators on the above simulated dataset. These include linear models estimated using a OLS regression without fixed or random effects, a random effects Generalized Least Squares (GLS) regression, GLS with fixed effects separately specified for zip code and quarters, GLS with fixed effects specified for each zip code-quarter combination, and random effects probit and random effects logit models. Since we are comparing models which the parameters themselves are not directly comparable, we compare marginal effects to assess performance. The standard errors of the marginal effects are computed using the delta method.

Table 1 shows the results of the first set of Monte Carlo simulations. The first row shows the baseline simulation, where parameter values are assumed, fixed effects are drawn from a normal distribution and data generated from a probit model. Further, the installed-base effect is set to *zero*, and the correlation between the fixed effect and the covariate is set to zero. We see that all the models do well in recovering the true effect, with the truth within the 95% confidence intervals of the estimated effects for all the models. We also see the fixed effects linear probability model does as well as the probit and logit models in recovering the effects when the true data generating process is a probit.

In the second row, we keep everything the same as the baseline simulation, but allow the fixed effects to be correlated with the covariate in the data generating process (i.e. $\rho \neq 0$). We see that this correlation biases the results of all models except the most general fixed effects model. A non-zero ρ induces a correlation between the error term (which includes a deviation of the fixed effect from the mean value) and the included covariate, causing an omitted variable bias in the estimates. However, as seen, the fixed effect estimators do not suffer from any bias in the presence of correlation between the fixed effect and covariates. This aspect is very significant when the installed-base variable is included in the model, as then, a correlation is induced between the included installed-base and the individual effect by construction.

The third row of the table shows the effect of misspecification of the distribution of the random effect. Linear models with random effects do not require the specification of this distribution, as the parameters can be consistently estimated using GLS without distributional assumptions. However, models with nonlinear link functions such as the logit or probit require the analyst to specify a distribution for the random effects. The most common assumption in empirical work is of normality. Here, we ask how the assumption of normal random effects biases estimation when the individual effects are in truth drawn from a skewed distribution: specifically, a gamma distribution with shape and scale parameters fixed so the variance and mean of the fixed effect is the same as that in the previous two simulations. Looking at Table 1 we find the linear models do well in recovering the true effect, but the random effects probit and logit models suffer from significant specification bias (for instance, the normality assumption on random effects biases the marginal effect of Z in the probit model upward by about 28%, to 2.9832 from 2.3211, the truth).

We now report on simulations with the installed-base effect turned on. The results of these simulations are reported in Table 2. As before, we generate data using a probit model, and estimate the effects using various estimators - linear OLS without individual effects, random effects, separate zip and quarter fixed effects, zip-quarter fixed effects, and nonlinear models including random effects probit and random effects logit. We also report on the performance of our proposed bias correction estimator. We compare the marginal effect of the installed-base variable across the models, and report the standard errors of these marginal effects computed using the delta method.

The first row of the simulations show the results for the baseline simulations, where the random effect is drawn from a normal distribution. We find that OLS is biased, due to both misspecification as well as the absence of controls for homophily and correlated unobservables. Allowing for fixed or random effects (control for sorting and correlated unobservables) reduces the magnitude of the installed-base effect from the OLS case as expected. Consistent with the results in section (2.2), the fixed and random effect estimates are biased downward relative to the truth. The nonlinear models do better in recovering the true effect, though they are still significantly different from the true value. The bias-corrected estimator locates the truth within its 95% confidence interval.

In the next row, we switch off the effect of the exogenous covariate in the simulations and find similar results: the bias corrected estimator is again the only one locating the truth within the 95% confidence interval. In rows 3 and 4, we vary the strength and sign of the installed-base effect. In row 4, we allow the true data to reflect a “snob” effect (installed-base effect is negative), and in row 5, we allow for stronger social effects (installed-base effect is fixed at a higher positive level than the base simulation). Finally in the last row, we alter the distribution of the fixed effects to a skewed gamma distribution instead of a normal distribution. In each of these cases, we that all considered estimators except the bias-corrected estimator are unable to recover the truth precisely. Notably, the fixed effect estimate is negatively biased as expected. Overall, we also see the bias-correction estimator does remarkably well.

3 Empirical Application

3.1 Background

As described earlier, our empirical application is to the automobile industry. We study social spillover effects in the adoption of the Toyota Prius Hybrid electric car. The Toyota Prius was introduced in Japan in 1997 and in the United States in 2001, and was the first successful the mass market Hybrid electric car, achieving worldwide cumulative sales of 1.6 million units by early 2010. Hybrid vehicles use regenerative braking to generate electricity. While familiar to consumers now, at the time of introduction, adoption of the Prius was subject to considerable uncertainties about quality. In particular, well-documented concerns existed about acceleration, handling properties, reliability of the regenerative braking system and the performance of the Prius' battery pack; see (Taylor, 2006) for a discussion. These factors imply observational learning may play a role. Observation of the car in use in their local neighborhoods could update consumers' beliefs about quality through explicit interactions with other owners, by mere observation and through inferential learning. These could lead to a positive effect of the number of cars in consumer's neighborhoods on their likelihood of adoption. On the other hand, negative social effects are also plausible. For instance, if observational learning and word of mouth cause consumers to update their prior beliefs about the quality of the car downwards, a larger number of cars observed in the neighborhood could have a negative impact on the likelihood of purchase. Further, it is conceivable there may have been exclusivity or snob effects (Leibenstein, 1950) in the purchase of the Prius, if consumers had a desire to buy cars that were different from those owned by others in their neighborhoods. Hence, the sign and magnitude of the overall social effect are empirical questions to be answered with data.

3.2 Data Description

We have access to an unusually detailed disaggregate dataset on automobile purchases. The dataset was acquired from R. L. Polk and Co., a major provider of data to the US automobile industry. The dataset contains individual-level information on all automobiles registered in the state of California between January 2001 and March 2007, and includes time periods from before the introduction of the Toyota Prius in the US market. We observe the details of the automobile registered, including its make, model, fuel-type, whether it was a Hybrid vehicle etc, the zip code of residence of the owner to whom it was registered, the month and year it was registered, whether it was registered to an individual or a business, whether it was part of a fleet (such as that of a car rental company), and in the case of individuals, some details about the owners' household income (see Shriver, 2010 for an application using similar R. L. Polk data). Overall, there are over 11 million observations in the dataset. Each observation is an individual-level registration event.

First, we discuss some summary statistics from the data. There are 11.1 million observations in the full dataset: to be clear, 11.1 million vehicles were registered in the state of California during the data period. Of these, about 10 million observations involve automobiles registered by individual buyers, and the rest involve institutional buyers, including businesses and corporate entities, Government agencies and fleet purchasers such as car rental firms. A total of 186,276 Hybrid vehicles were registered in California during the period of the dataset, of which 172,094 were registered to individuals. Of these, 102,949 (both individual and institutional) and 95,278 (individual only) constituted Toyota Prius cars. Thus, the Toyota Prius thus an overall adoption rate of 0.95% (i.e. 0.95% of all automobiles registered in California during this period were Toyota Prius). In the last month in the dataset, i.e. March 2007, 3.38% of all automobiles were Priuses.

We first explore spatial patterns in the diffusion of the Prius. Figure 1 shows a map of the state of California and Figures 2 through 8 documents the adoption patterns for the Toyota Prius for 2001-2007 overlaid on the map of the state. The colored dots in these maps represent zip codes. For each zip code, the color of the dot represents the adoption rate of adoption of the Prius, i.e. the percentage of car purchases in that year that are Priuses. The main point to note in these charts is that there is a relatively high degree of spatial clustering across areas. For instance, we see high concentration of Prius adoption in the San Francisco Bay Area in Northern California, more than the other big metropolitan areas in the state's south, i.e. Los Angeles and San Diego. Drilling deeper, Figure 9 shows a map of the the San Francisco Bay Area. Figures 10 through 16 overlay the adoption rate by zip code for 2001-2007 on a map of the region. Once again, we find a high degree of local clustering of adoption of the Toyota Prius within this narrower geographic region. Note these are adoption rates, rather than the number of Toyota Prius cars registered, and hence are not confounded with population concentrations (although maps overlaying number of cars show similar spatial concentrations).

The figures demonstrate there is spatial clustering in adoption rates. This pattern is consistent with social effects but are not conclusive. The first-order alternative explanations include in particular, homophily and correlated unobservables. Hence, controls for these factors are critical for attributing such clustering to social effects. Since there is no experimental variation in the installed-bases in local geographies, we would need to econometrically control for these confounding factors.

3.3 Empirical Strategy

3.3.1 Data Specific Decisions

We first discuss decisions we made in implementing our empirical strategy. These decisions reflect the nature of the data and the specifics of our application.

Level of aggregation The lowest level of geography identified in our data is a zip code, and the lowest time interval is a month, i.e. we observe which zip code and which month a car was registered in. Hence, we specify the installed-base at the level of a zip code and month. Later, in checking for robustness of our specification, we allow for different levels of geographical aggregation, including the level of a city and a county.

We also need to decide the level at which fixed-effects may be specified. The fixed-effects control for homophily and correlated unobservables since individuals in the same geographical neighborhood, purchasing at similar points of time would share fixed-effects. Since the installed-base in our analysis is at the zip code-month level, we cannot include fixed-effects at the level of zip code-month. We could specify separate zip code and month fixed-effects. However, this set of controls would likely be incomplete since it would not be able to control for marketing variables that vary at a local level over time (e.g. the varying availability of Toyota Prius cars as it was rolled out across its dealer network, local-level marketing activities). Also, since the dataset spans several years, there could be varying preferences due to the changing demographics of a neighborhood, or other time-varying factors and these trends themselves might vary across neighborhoods (e.g. different neighborhoods might have different trends in green consciousness). Thus, we should ideally control for preferences and unobservables that vary across both geography *and* time (the importance of this is also suggested by our Monte Carlo simulations reported previously). Thus, we include market and time-specific fixed-effects in our specification. As mentioned before, we aggregate to zip code-quarter fixed-effects due to collinearity with the installed-base. The identifying assumption is that correlated factors such as local-level marketing vary across quarters but not within quarters.

In order to allow for variation amongst months within quarters, we also include fixed-effects for the first or second month of the quarter. This could, for instance, control for variation in availabilities across months within a quarter, quarterly sales quotas for dealers and their sales-persons, etc. We also test for the robustness of this identifying assumption by varying the quarter definition (with the first quarter starting in February or March instead of January, and similarly for other quarters). We found no material changes in our results. Finally, we include a set of dummy variables for the income group that the consumer belongs to as controls. This is the richest set of controls provided in the literature so far, and helps mitigate concerns about unobservables.

Why not Random Effects? As discussed above, random effects are not easy to handle in the model since the consistency of random effects estimators depend on the assumption of the orthogonality of these effects to all covariates in the model. By construction, random effects and the installed-base are not uncorrelated, since the installed-base is the aggregation of purchase decisions of other consumers who share the same random effect. By contrast, panel data fixed-effects approaches treat the fixed-effects as parameters to be estimated, and do not depend on assumption about their orthogonality to observed covariates. Maximum-likelihood estimation of the random-effects model is possible, but is sensitive to misspecification if the true distribution of unobservables differs from the ones assumed. We demonstrated this in our Monte-Carlo simulations. With fixed-effects, the control for unobservables is semi-parametric, addressing this concern.

Linear Specification When there are a large number of markets and/or a large number of time periods, specifying fixed-effects for a nonlinear model using dummy variables quickly becomes infeasible. For instance, in our empirical application of automobile purchases in California, the local market is defined as a zip code. There are over 2000 zip codes in the data. Further, the data is spread over 8 years. Thus, there are thousands of fixed-effects to estimate, and a maximum likelihood based procedure such as one that would be used to estimate a binary logit or a binary probit model would be infeasible due to the large number of parameters to maximize the likelihood over. Further, maximum-likelihood estimates estimates of the fixed-effects in a nonlinear model are inconsistent for fixed T (Hsiao, 2003), and this inconsistency transfers to the estimator of the installed-base effect as well. In a linear model, this inconsistency does not transfer to the estimator of the installed-base effect as one can eliminate the fixed-effects using a differencing strategy. A maximum likelihood estimator for the fixed-effects logit model has been developed using the Neyman and Scott (1948) principle (see Hsiao, 2003 for details on this estimator), but this is estimable only conditional on the total number of purchases observed within units.[3] Hence, we adopt a linear model for our inference. Above, we reported on Monte Carlo simulations which shows the linear probability model with a rich specification of fixed-effects performs well in approximating social effects generated from underlying nonlinear data generating processes.

Conditional Model An observation in our dataset is a car that was registered, and while we know where and when it was registered, we have only limited details on who. In other words, we do not see repeated observations for a particular customer, and we do not know whether the customer owns other cars, or whether the focal car is a replacement for another car. Our view is that the timing of automobile adoption is a dynamic replacement problem, and a timing model

[3]The logit is also a rare exception amongst nonlinear models in enabling this conditionally consistent estimation in the presence of fixed-effects.

is not credible without having access to the key state variable driving the dynamics (i.e. the current car the individual is replacing from). Hence, our data do not allow us to credibly model the decision of the consumer of whether to buy an automobile or not. Hence, we condition on the fact that the consumer buys an automobile in that month, and our dependent variable is whether that automobile is the Toyota Prius or not. While we do not have a panel of individuals for which we see repeated purchases, our data are of a panel nature, in the sense that for a given geography (zip code) and a given time (month), we see many individuals buying automobiles. This allows us to set up a rich specification of fixed-effects as described earlier.

3.4 Empirical Specification

Incorporating these considerations, the empirical specification we use is,

$$y_i = X_{m_i t_i}\beta + f_{m_i q_i} + Z_i\boldsymbol{\gamma} + \varepsilon_i \tag{1}$$

The notation is carried over from section (2), i.e.,

- y_i is consumer i's decision of whether to purchase a Toyota Prius or not, conditional on purchasing an automobile in zip code m_i and month t_i, taking the value 1 if the Prius is purchased and 0 otherwise.
- $X_{m_i t_i}$ is the installed-base of the Toyota Prius in zip code m_i and month t_i and is the total number of Prius cars purchased in the zip code from its introduction up to and including month t_i. It is defined in Equation (1).
- $f_{m_i q_i}$ is the zip code-quarter specific fixed effect.
- Z_i is a set of other controls including income dummies and dummies for the month within the quarter (i.e. month 1, 2 or 3 within a quarter).
- ε_i is an independent and identically distributed unobservable.
- β and $\boldsymbol{\gamma}$ are parameters to be estimated.

3.5 Estimation

IV Strategy We first outline a set of instrumental variables (IVs) that may address the issue of endogeneity. Our IVs are motivated by stories about the institutional features of the Hybrid market. The institutional feature is that other Hybrid vehicles such as the Honda Civic Hybrid were visually almost exact versions of their non-Hybrid counterparts. For instance, the Honda Civic Hybrid was externally distinguished from a Honda Civic non-Hybrid only by a small label on the vehicle's rear. It is plausible then that consumers cannot easily track the installed-base of other Hybrids in their local community. In that case, how many have adopted non Prius-Hybrids is not "visually salient", and may not affect a consumer's beliefs about Hybrid quality generally. This argument suggests the *installed-base* of other Hybrid adopters may not directly influence the decision of the consumer to purchase the Toyota Prius. However, the installed-bases of other Hybrid vehicles is correlated with the Toyota Prius installed-base, as the purchase of both the Toyota Prius and other Hybrids are driven by common factors such as preferences, similarities in commute patterns etc. This suggests using the local *installed-base* of other Hybrid adopters as an IV for the *installed-base* of the Toyota Prius. To add moments, we could also use the levels of the installed-bases of these other Hybrid vehicles as instruments for the differenced installed-base in the estimation equation, thereby adding over-identifying restrictions.

In some specifications, we also use the installed-bases of flex-fuel vehicles (which can use gasoline blended with ethanol as fuel) as instruments. The argument for using these as instruments is similar to that of using installed-bases of other Hybrids - the installed-bases of flex-fuel vehicles and the Toyota Prius Hybrid could be correlated due to common factors such as green consciousness, but since flex fuel vehicles are versions of existing vehicles with no significant outward differences, these installed-bases are difficult for consumers to track and may be excluded from the demand equation for the Toyota Prius.

Clearly, these IVs may not be valid if the underlying stories that motivate them are untrue. Hence, we also compare these results to our bias-corrected estimates.

3.6 Results

In this sub-section, we report the results of our empirical analysis. In order to demonstrate the biases we have outlined, we first estimate a set of naive regressions. The results of these regression are reported in Table 3. The first model we estimate (labeled Model N1 in the table) is one without any geography or time fixed-effects. This model does not control for homophily or correlated unobservables. It is not a surprise this model finds a strongly significant positive effect of the installed-base of the Toyota Prius on the adoption decision of the car. Indeed such a positive coefficient is likely for any product whose adoption rates are increasing over time, which is likely true for most new products. This is not an artifact of the specification, but of the correlation in the data. Moving to a more sophisticated nonlinear specification will not address this. The second model we estimate adds separate market (zip code) and time (quarter) fixed-effects. This implies positive and significant installed-base effects. However, the separate fixed-effects only partially control for homophily and correlated unobservable variables, since it controls for time-invariant effects that vary by geography and geography-invariant effects that vary with time, but not trends that vary with geography or conversely geographical differences that vary with time. Therefore, we estimate a third model, in which we allow for market-time fixed-effects, i.e. one fixed-effect for each zip code-quarter combination. This provides a strong set of controls for homophily and correlated unobservables, but the installed-base effect is *negative and significant* in this case.

It is tempting to infer these as evidence of snob effects or exclusivity; but as we demonstrated, this estimate now reflects a negative bias in the estimates for the installed-base effect demonstrated in section 2. Thus, it might be that the true causal installed-base effect is positive, but the bias overwhelms this positive effect.

3.6.1 Instrumental variable regression estimates

In Table 4, we report the instrumental variable estimates. Model IV1 includes zip code-quarter fixed-effects and instruments for the installed-base using (the levels of) installed-bases for the Honda Civic Hybrid, other Hybrid cars and flex-fuel vehicles. This model controls for homophily and correlated unobservables, but controls for the bias that we have shown in section 2 using instrumental variables. These are consistent estimates of the true causal effect subject to the identifying assumptions holding true. We find that the effect is positive and significant. We also estimate the model using only the installed-bases of flex fuel vehicles (model IV2). We find it leads to fairly similar estimates as model IV1.

3.6.2 Bias-corrected estimates

The bias-corrected estimator starts by finding the within-estimate for the model with fixed-effects. In other words, we specify the same model as before, but compute the OLS estimates for the mean-differenced model. We then use the estimates to compute the asymptotic bias-corrected estimates as described earlier.

The model we use for the bias-corrected estimator is the fixed-effects model (Model N3) in Table 3. The estimate for the installed-base effect $\hat{\beta}$ is -0.0010268, i.e., negative.[4] On the other hand, our instrumental variables estimates showed a strongly positive installed-base effect. In order to compute the asymptotic bias, we first need to find consistent estimates of the error variance. For this, we solve the quadratic Equation (35), using the smaller of the two roots. The estimate of σ_{ε}^2 so computed is 0.00921226. Plugging this value back in the equation for the bias-corrected installed-base effect (Equation 32) gives us a corrected value of the estimate at 0.0001699. The corrected estimates, along with corrected standard errors are given in Table 7, where we also present the IV estimates discussed earlier, for comparison. The standard errors are constructed using a bootstrap which accounts for the uncertainty associated with computing the error variance for the bias-correction. The bias-corrected estimate of the installed-base effect is close to that obtained using the instrumental variables approach (model IV1).

We next explore the spatio-temporal patterns in the zip code-quarter fixed effects, obtained using the estimates from model N3. We estimate the fixed effect for each zip code-month combination, by first obtaining the zip code-quarter fixed effect (computed as the difference

[4]Note that for ease of reading, the table reports estimates multiplied by 1000.

between the zip code-quarter mean of the dependent variable and its predicted value using the regression estimates), and then adding the estimated month fixed effects. First, we look at the temporal patterns in the data. Figure 17 depicts box plots for the distributions of fixed effects across zip-codes for the various months in the data. We see the fixed effects are generally increasing over months, though there is a significant variance across zip codes in temporal growth. The spread of the distribution is changing over time. It is clear these are hard to capture by a priori specifying a parametric random effects specification. We also document a high degree of temporal dependence as shown in Figure 18, which presents the temporal autocorrelation function for the fixed effects.

We next look at the spatial patterns in the fixed effects. Figure 19 presents a three-dimensional scatter plot of the fixed effects for various zip codes, plotted against the latitudes and longitudes of the zip code centroids. We see the fixed effects are strongly correlated in space, with larger values for zip codes with higher latitudes and longitudes (in absolute value). Further, the correlation is non-systematic. Analogous to the time dimension, it is hard to capture these by specifying a parametric spatial random effects specification. To obtain a sense for where the unobserved covariation is spread geographically, we also depict the spatial patterns in a temperature map of the mean fixed effects for each zip code, overlaid on a map of the state of California (i.e., the map shows fixed effects averages across months for each zip code). In Figure 20, the color for each zip code represents the level of the mean fixed effect for that zip code. The colors range from blue, representing the lowest levels of the fixed effects through red, representing the highest levels. Mirroring the patterns in the raw data (Figures 2 through 8), we find that the fixed effects are highest in the San Francisco Bay Area, with those in the Los Angeles and San Diego metropolitan areas being somewhat lower. The lowest levels are in the smaller cities and rural areas of the state. However, even within these different regions, there is considerable spread in the levels of the fixed effects. Further, we see the spatial correlation is not fully explained by inter-zip code distance. We see the fixed effects are similar within a metropolitan area, where zip codes are very close to each other, but also similar for neighboring zip codes in rural areas, where distances are much greater. This suggests we cannot capture these spatial dependence's as simple, parametric functions of distance. Finally, we compute spatial autocorrelations functions using the *"Moran's I"* measure of similarity, assuming uniformly distributed distance classes. Figures 21 depicts the spatial ACF-s, and reveals a high degree of spatial correlation.

To summarize, the temporal and spatial autocorrelations documented above reflect a pattern of complex correlations over both space and time for the unobservables that is hard to approximate by a parametric specification, underscoring the need for flexible, semiparametric controls of the type afforded by the fixed-effects we employ.

Finally, we report on the economic significance of these estimates. The estimated installed-base coefficient measures the effect of an additional Toyota Prius in the installed-base, on the conditional probability of the purchase of a Toyota Prius by the focal consumer, given he purchases an automobile. The parameter estimates are themselves hard to interpret, and hence we compute the elasticity of the installed-base on the (conditional) purchase probability of the Prius. The elasticity is 5.3, which means that for every 1% increase in the installed-base of the Prius in the zip code of the individual, there is on average a 5.3% increase in the probability of purchase of the Prius (as a percentage of the baseline purchase probability). These numbers suggest that installed-base effects are non-trivial in the diffusion of the Prius.

3.7 Robustness Checks

Finally, we also report on a set of robustness checks on our results. The first, reported in Table 5, replicates the analysis in Model IV2 (Table 4) but with the adoption of the Honda Civic Hybrid as the dependent variable. The rationale for the validity of the installed-base of the Honda Civic as an instrument is that it is not easily observable by consumers. If that is the case, it should also be true that the installed-base of the Honda Civic Hybrid does not have any significant effect on the adoption decision for the Honda Civic Hybrid itself. We therefore conduct a similar analysis for the Honda Civic Hybrid, using the installed-base for flex fuel vehicles as instruments (Model R1). We find that the estimated installed-base effect is insignificant, providing some support to our strategy.

Another set of robustness checks varies the geography for which the installed-base is defined to see if the effects are consistent with social effects. We expect that social effects are stronger when the network is in closer proximity to the focal individual. Thus, a focal consumer is less likely to be affected by changes in installed-base in a geography they are less likely to observe directly. By this logic, if the installed-base is defined at the level of the city, the social effect should be weaker than when it is defined for the zip code, and it should be weaker still if the installed-base is defined at the level of the county. We thus estimate two other models (Models R2 and R3) that respectively define the installed-base at the level of the city and county. The rest of the controls remain the same, including the fixed-effects at the level of a zip code-quarter. These estimates are reported in Table 6. We find that the installed-base effects are indeed weaker (although still significant and positive) at the level of the city compared to the zip code level, and they become insignificant at the county level. These results are consistent with social effects that are moderated by geographic contiguity.

Discussion We close this section with a brief discussion of the importance of accurate measurement of social effects in this category. The increase in recent years in advertising clutter, in the widespread availability and usage of technology to skip television advertisements, in the decline in readership of print media, combined with issues of source credibility in traditional advertising messages imply Marketers have been increasingly adopting non-conventional elements in their marketing communication strategies. Several of these new methods of communicating with consumers rely on the existence of social effects. For instance, Ford conducted a non-conventional marketing campaign during the launch of the Ford Fiesta subcompact car in 2009. This campaign, titled the *Ford Fiesta Movement* involved giving the car away to a hundred people from amongst 4000 applicants for a period of six months. In return, users were asked to sharing their experiences with the car on the Internet (Barry, 2009). Toyota used a similar social media campaign for the Toyota Prius, based on making the car available to celebrities and encouraging them to drive the car to marquee events such as the Academy Awards ceremonies. The firm's actions suggest social contagion in Prius demand. Many industry observers also opine the Toyota Prius was under-priced initially, pointing to the absence of a price skimming strategy for the car despite its early scarcity, and despite the long waiting periods consumers faced between order and delivery of the car. A price penetration strategy is optimal in the context of social spillover effects, if early adopters of the product have a positive effect on the purchase decisions of subsequent adopters. Robust measurement of social effects is therefore an important component of evaluating the return-on-investment of such non-conventional marketing campaigns to firms like Toyota.

4 Conclusion

In this paper, we investigate the identification and estimation of causal installed-base effects. Causal installed-base effects may arise from a variety of social effects including word-of-mouth, network effects, herd behavior, observational learning and exclusivity/snobbery. A valid measure of such causal installed-base effects requires controls for confounding factors such as homophily and correlated unobservables. Controlling adequately for homophily and correlated unobservables is now the *de facto* standard in the literature for empirically establishing the presence of social influence. A robust way to control for these is to specify a rich set of fixed-effects. We address several issues that arise in this specification, most notably, the inconsistency of estimates of installed-base effects in work-horse empirical models of social influence. We characterize the sign and magnitude of the bias.

We present an empirical application to analyzing installed-base effects in the adoption of the Toyota Prius Hybrid car. We use a rich, disaggregate level dataset for the purpose, which allows us to specify a very detailed specification of fixed-effects. We present an instrumental variables method and a new bias-correction method to provide consistent estimates of the installed-base effect. The results of our empirical analysis reveal statistically significant and positive installed-base effects in the adoption of the Toyota Prius. A naive analysis that ignores the bias in the presence of fixed-effects indicates these effects were significantly negative. Thus, the bias changes not just the magnitude of the results, but its sign as well. We show find social effects are effects are economically significant. We conduct a series of robustness checks to establish our estimates are consistent with social effects.

This paper contributes to the literature on identification and estimation of dynamic panel data models, and spatio-temporal models which include lagged aggregations of decisions by other agents as covariates. The bias-correction approach provides a new, practical method to obtain causal installed-base effects when it is hard to find suitable instruments. More generally, this paper cautions the researcher to be careful when estimating dynamic panel data models in the presence of controls for heterogeneity. This is particularly important in the area of Marketing, since both heterogeneity and state dependence are important components of the Marketing scientist's analytical toolkit.

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Figure 1: Map of California

Figure 2: Toyota Prius Adoption Rate - California - 2001

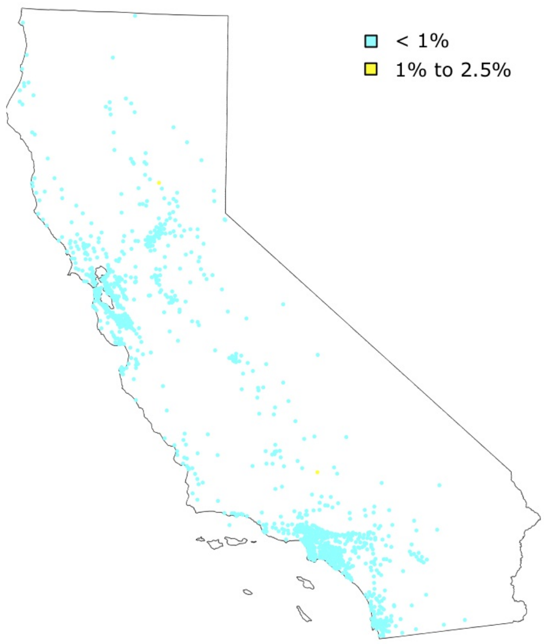

Figure 3: Toyota Prius Adoption Rate - California - 2002

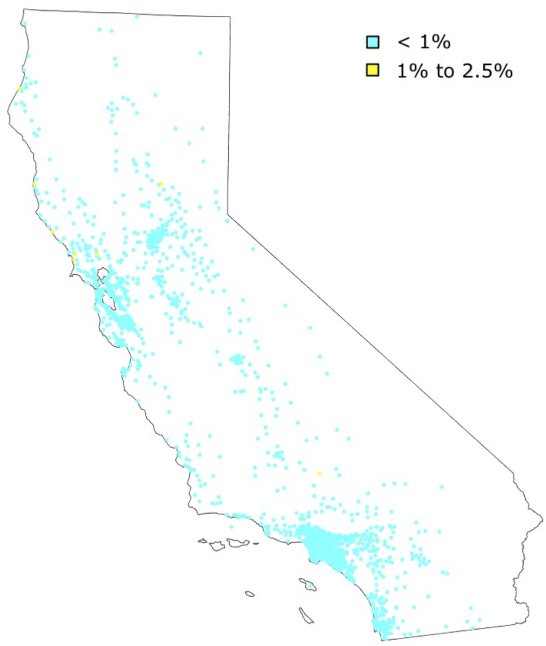

Figure 4: Toyota Prius Adoption Rate - California - 2003

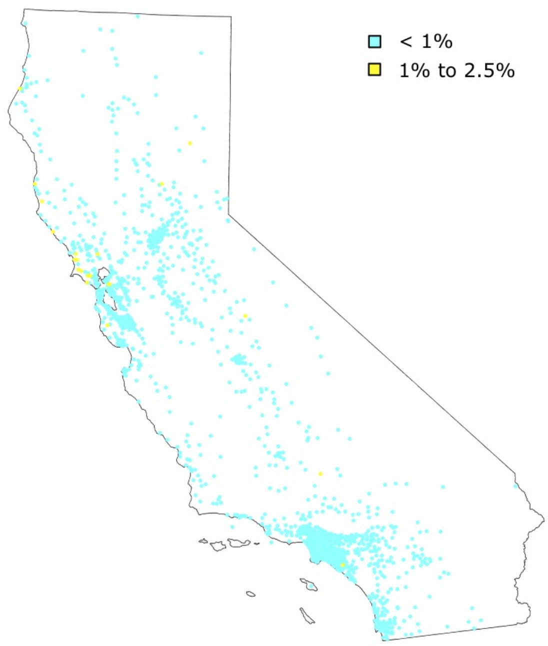

Figure 5: Toyota Prius Adoption Rate - California - 2004

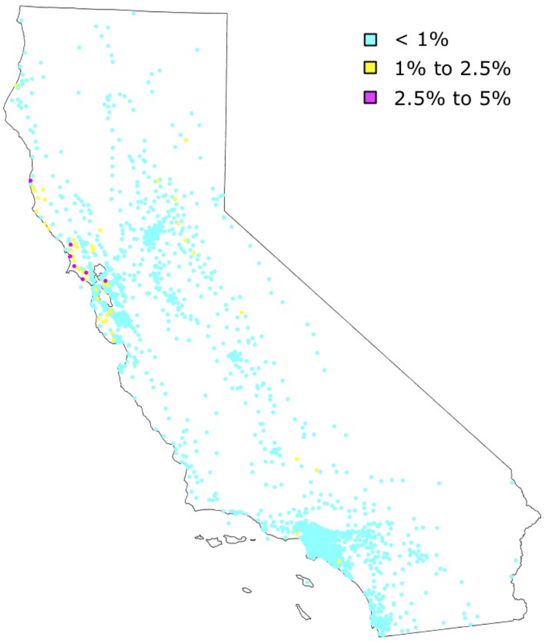

Figure 6: Toyota Prius Adoption Rate - California - 2005

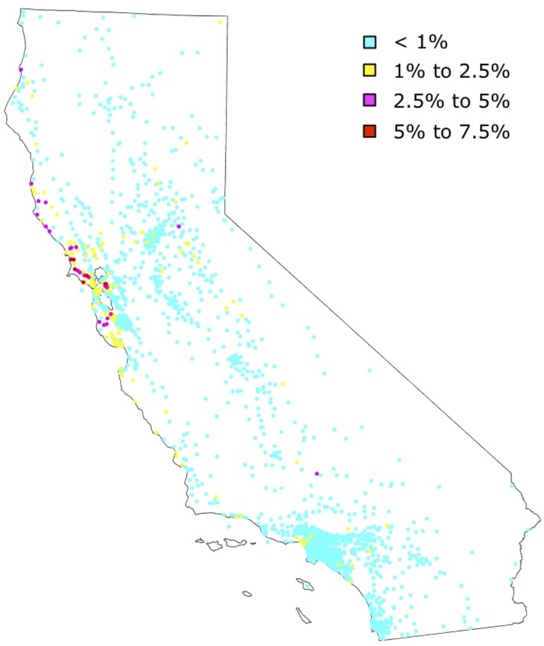

Figure 7: Toyota Prius Adoption Rate - California - 2006

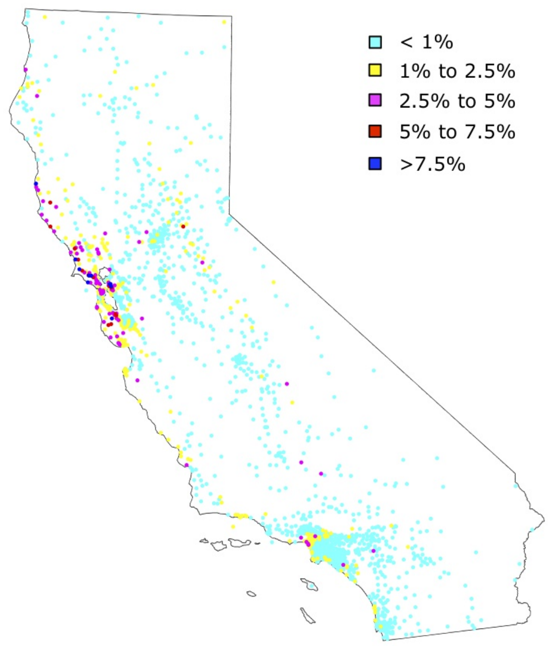

Figure 8: Toyota Prius Adoption Rate - California - 2007

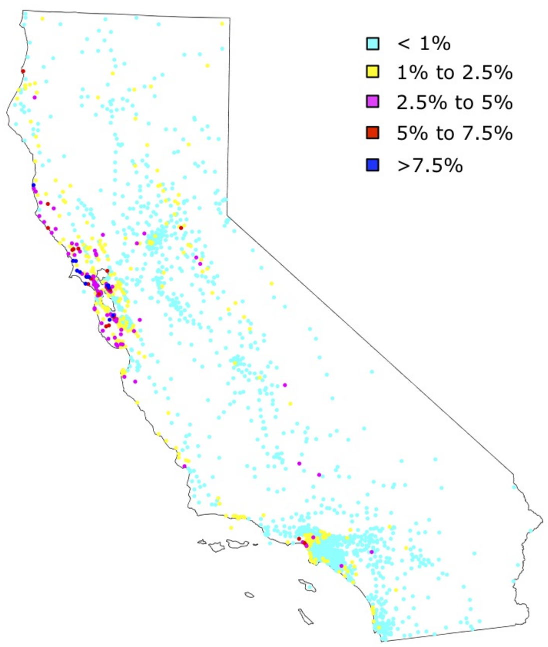

Figure 9: Map of the San Francisco Bay Area

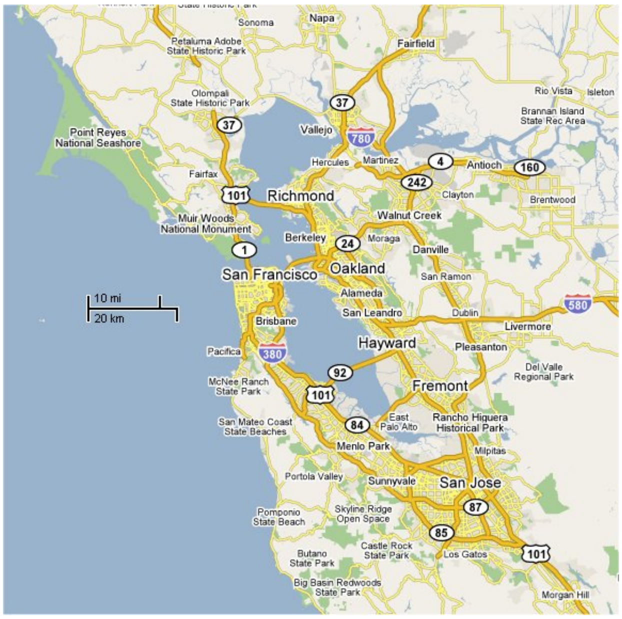

Figure 10: Toyota Prius Adoption Rate - San Francisco Bay Area - 2001

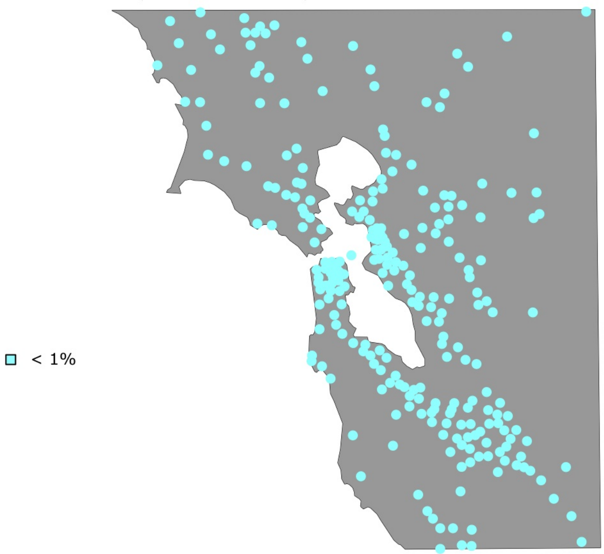

Figure 11: Toyota Prius Adoption Rate - San Francisco Bay Area - 2002

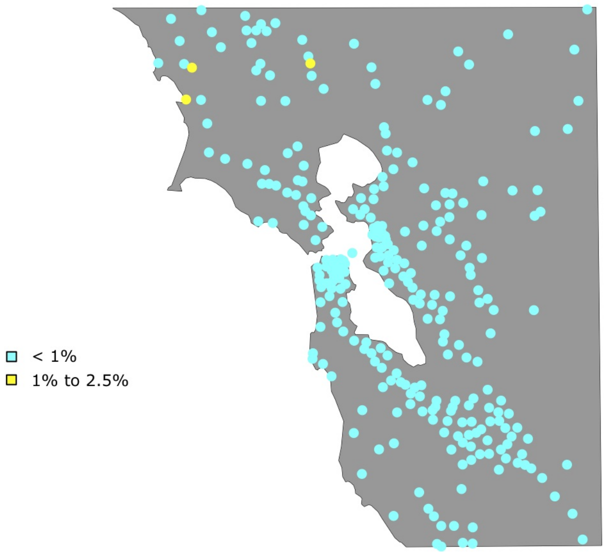

Figure 12: Toyota Prius Adoption Rate - San Francisco Bay Area - 2003

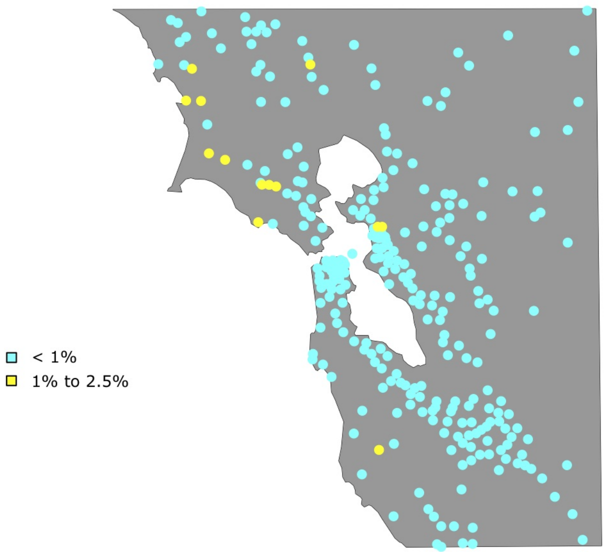

Figure 13: Toyota Prius Adoption Rate - San Francisco Bay Area - 2004

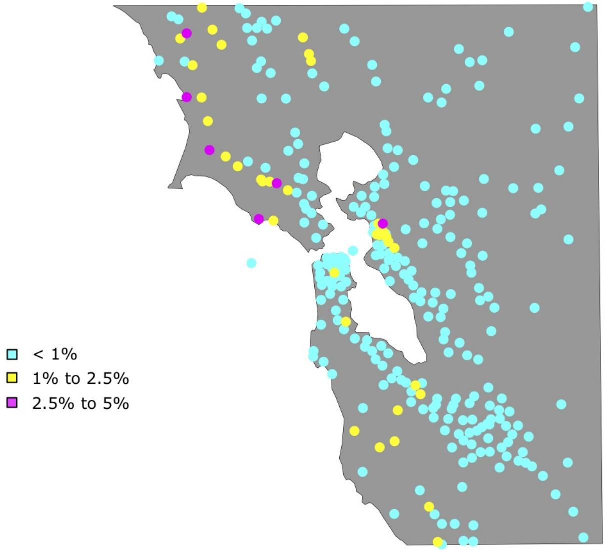

Figure 14: Toyota Prius Adoption Rate - San Francisco Bay Area - 2005

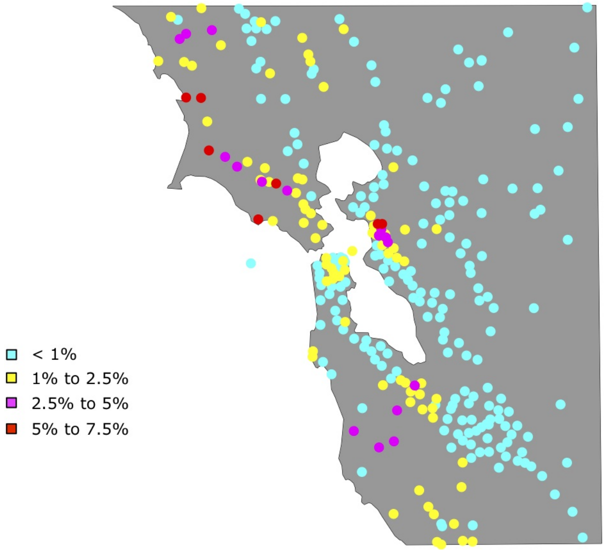

Figure 15: Toyota Prius Adoption Rate - San Francisco Bay Area - 2006

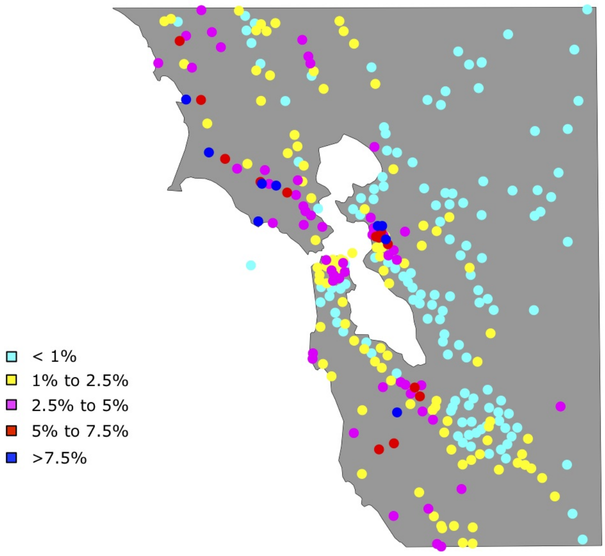

Figure 16: Toyota Prius Adoption Rate - San Francisco Bay Area - 2007

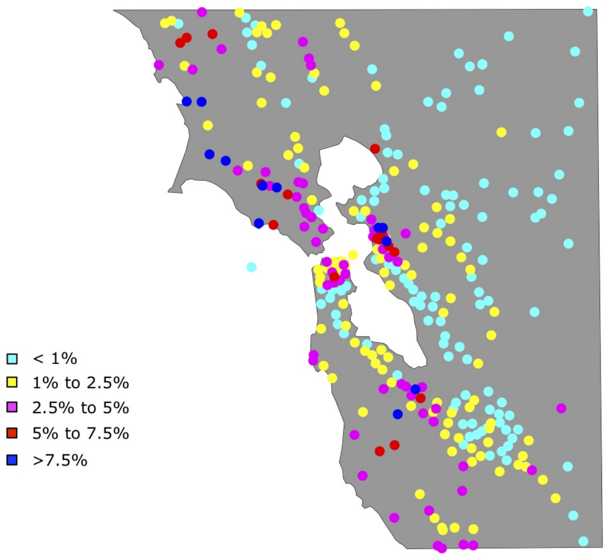

Figure 17: Distributions of Fixed Effects Across Months

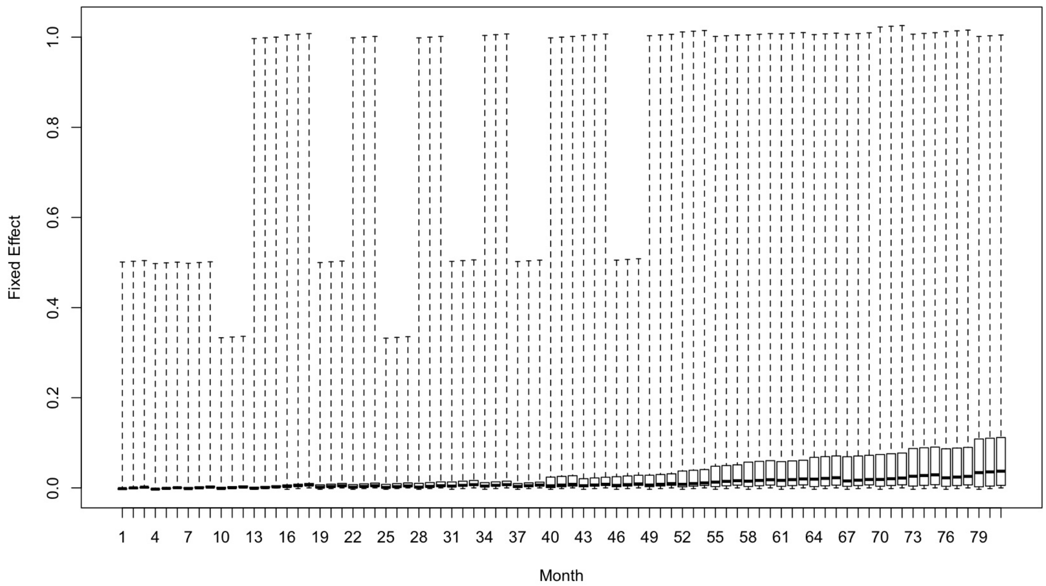

Figure 18: Temporal Autocorrelations of Fixed Effects

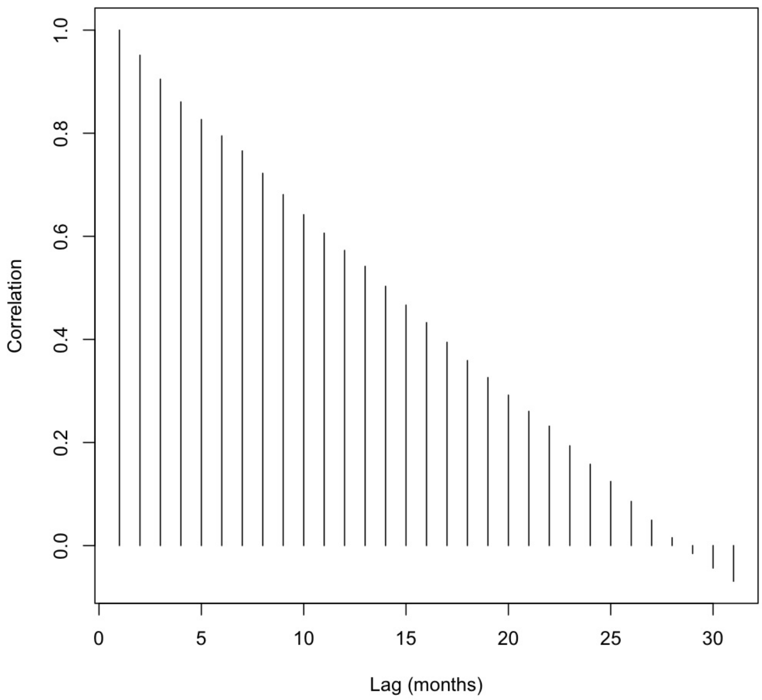

Figure 19: Spatial Patterns in the Estimated Fixed Effects

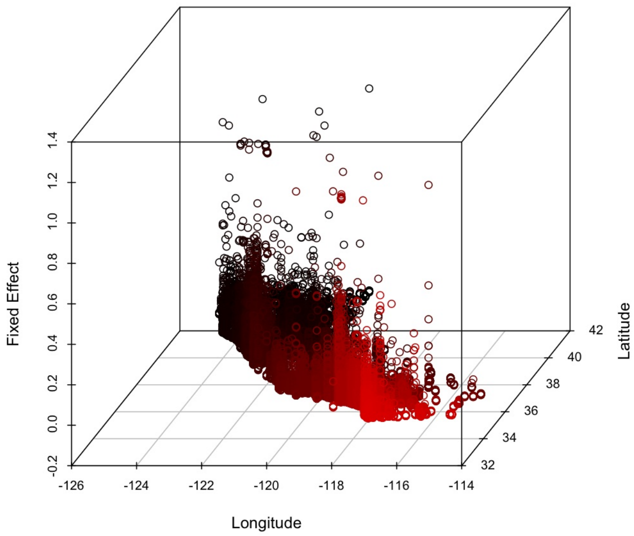

Figure 20: Temperature Map of Mean Zip Code Level Fixed Effects

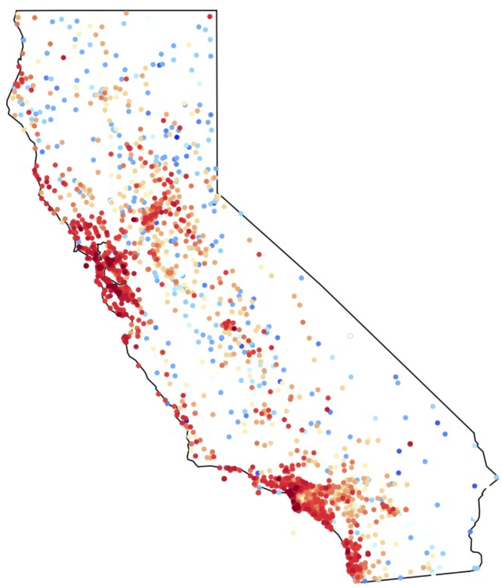

Figure 21: Spatial Autocorrelation of Fixed Effects

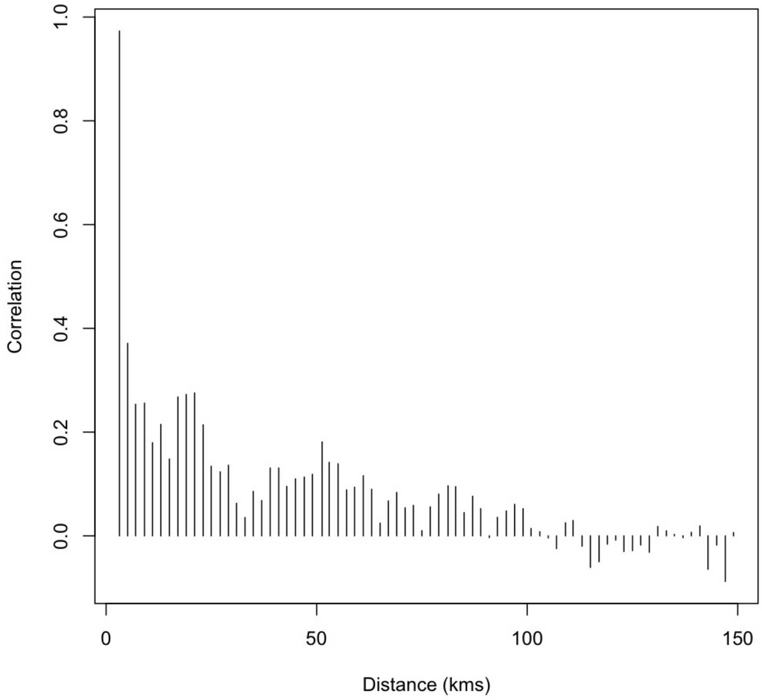

Table 1: Monte Carlo Simulations without Installed-Base Effect

Assumptions of data generating process	Marginal Effect of the Exogenous Covariate x 100 = $100\frac{dy}{dZ}$ (Standard Errors in Parentheses)						
	Truth	Linear Models				Non-linear Models	
		OLS	Random Effects	Fixed-Effects		Random Effects Probit	Random Effects Logit
				Zip & Qtr	Zip-Qtr		
$\beta = 0$, $\gamma = 0.1$, $\rho = 0$, $f_{m_i q_i} \sim N\left(0, \sigma_f^2\right)$, $\sigma_f = 0.4$	3.2959	2.9609 (0.5107)	3.2592 (0.4966)	2.9246 (0.5065)	3.3101 (0.4969)	3.2899 (0.5102)	3.2680 (0.5040)
$\beta = 0$, $\gamma = 0.1$, $\boldsymbol{\rho = 0.5}$**,** $f_{m_i q_i} \sim N\left(0, \sigma_f^2\right)$, $\sigma_f = 0.4$	3.2952	9.8703 (0.4790)	7.7639 (0.4842)	7.9072 (0.4854)	3.3411 (0.4964)	4.5165 (0.5163)	4.4624 (0.5121)
$\beta = 0$, $\gamma = 0.1$, $\rho = 0$, $\boldsymbol{f_{m_i q_i} \sim \Gamma\left(1, 0.4\right)}$	2.3211	2.0823 (0.4366)	2.0761 (0.4355)	2.0752 (0.4363)	2.0694 (0.4331)	2.9832 (0.4358)	2.9624 (0.4331)

Table 2: Monte Carlo Simulations with Installed-Base Effect

Assumptions of data generating process	Marginal Effect of the Installed Base x100 = $100\frac{dy}{dX}$ (Standard Errors in Parentheses)							
	Truth	Linear Models				Non-linear Models		Bias Corr. Esti-mates
		OLS	Random Effects	Fixed-Effects		Random Effects Probit	Random Effects Logit	
				Zip & Qtr	Zip-Qtr			
$\beta = 0.05$, $\gamma = 0.1$, $\rho = 0$, $f_{m_i q_i} \sim N\left(0, \sigma_f^2\right)$, $\sigma_f = 0.4$	0.8011	0.4157 (0.0036)	0.2882 (0.0089)	0.0186 (0.0089)	0.2437 (0.0101)	0.8293 (0.0091)	0.8491 (0.0088)	0.8162 (0.0101)
$\beta = 0.05$, $\boldsymbol{\gamma = 0}$, $\rho = 0$, $f_{m_i q_i} \sim N\left(0, \sigma_f^2\right)$, $\sigma_f = 0.4$	0.8662	0.5196 (0.0041)	0.3644 (0.0101)	0.0546 (0.0094)	0.3087 (0.0115)	0.9313 (0.0096)	0.9397 (0.0101)	0.8859 (0.0115)
$\boldsymbol{\beta = -0.05}$, $\gamma = 0.1$, $\rho = 0$, $f_{m_i q_i} \sim N\left(0, \sigma_f^2\right)$, $\sigma_f = 0.4$	-0.6467	-0.6034 (0.0202)	-0.8646 (0.0330)	-1.22561 (0.0441)	-1.3760 (0.0494)	-0.6757 (0.0384)	-0.6394 (0.0340)	-0.6542 (0.0494)
$\boldsymbol{\beta = 0.2}$, $\gamma = 0.1$, $\rho = 0$, $f_{m_i q_i} \sim N\left(0, \sigma_f^2\right)$, $\sigma_f = 0.4$	0.8842	0.2385 (0.0026)	0.2253 (0.0065)	0.0999 (0.0077)	0.2198 (0.0076)	1.4623 (0.0194)	1.4494 (0.0193)	0.8857 (0.0076)
$\beta = 0.05$, $\gamma = 0.1$, $\rho = 0$, $\boldsymbol{f_{m_i q_i} \sim \Gamma\left(1, 0.4\right)}$	0.9103	0.5967 (0.0045)	0.4216 (0.0104)	0.1792 (0.0113)	0.3284 (0.0125)	0.9441 (0.0072)	0.9447 (0.0073)	0.9089 (0.0125)

Table 3: Estimates of OLS and fixed-effects Regressions

Variable	Model N1		Model N2		Model N3	
	Estimates (x 1000)	Std. Errors (x 1000)	Estimates (x 1000)	Std. Errors (x 1000)	Estimates (x 1000)	Std. Errors (x 1000)
Prius Installed-Base	0.2076	0.0007	0.1606	0.0012	-1.0268	0.0178
Month 2	-0.1923	0.0752	-0.0520	0.0750	1.6384	0.0792
Month 3	-0.3323	0.0748	-0.2189	0.0748	3.1729	0.0903
Income Level 2	-3.9815	0.1401	-5.7998	0.1566	-5.5847	0.1609
Income Level 3	-3.8062	0.2185	-5.4867	0.2270	-5.3839	0.2296
Income Level 4	-3.7869	0.1494	-5.0281	0.1600	-4.8550	0.1624
Income Level 5	-3.4416	0.1427	-4.4611	0.1522	-4.1586	0.1542
Income Level 6	-3.2661	0.1345	-4.2225	0.1439	-3.8461	0.1455
Income Level 7	-2.8263	0.0897	-3.5888	0.1020	-3.1900	0.1038
Income Level 8	-1.2436	0.0999	-2.1827	0.1094	-2.0859	0.1106
Income Level 9	0.9533	0.1242	-0.3633	0.1307	-0.4585	0.1315
Income Level 10	-4.3000	0.3255	-4.4368	0.3350	-3.9085	0.3481
Intercept	4.6073	0.0681	0.0000	0.0304	4.18375	0.5349
Fixed-effects	None		Zip code & Qtr fixed-effects		Zip code-Qtr fixed-effects	

Table 4: Estimates of Instrumental Variable Regressions

Variable	Model IV1		Model IV2	
	Estimates (x 1000)	Std. Errors (x 1000)	Estimates (x 1000)	Std. Errors (x 1000)
Prius Installed-Base	0.1621	0.0263	0.1932	0.0277
Month 2	-0.0504	0.0837	-0.0945	0.0846
Month 3	-0.2032	0.1055	-0.2914	0.1083
Income Level 2	-5.5577	0.1605	-5.5573	0.1605
Income Level 3	-5.3489	0.2290	-5.3483	0.2290
Income Level 4	-4.8189	0.1620	-4.8184	0.1620
Income Level 5	-4.1241	0.1537	-4.1236	0.1537
Income Level 6	-3.8034	0.1451	-3.8027	0.1451
Income Level 7	-3.1391	0.1034	-3.1382	0.1034
Income Level 8	-2.0237	0.1103	-2.0225	0.1103
Income Level 9	-0.3997	0.1311	-0.3986	0.1311
Income Level 10	-3.3320	0.3213	-3.3316	0.3213
Fixed-effects	Zip code-Qtr fixed-effects		Zip code-Qtr fixed-effects	
Instruments (Installed-Bases of)	Hybrids + Flex Fuel		Flex Fuel	

Table 5: Robustness Check - IV Regression for Honda Civic Hybrid

Variable	Model R1	
	Estimates (x 1000)	Std. Errors (x 1000)
Honda Civic Installed-Base	0.0738	0.0800
Month 2	0.2664	0.0562
Month 3	0.0430	0.0872
Income Level 2	-1.4499	0.0917
Income Level 3	-1.4121	0.1309
Income Level 4	-1.3936	0.0926
Income Level 5	-1.1552	0.0878
Income Level 6	-1.0102	0.0829
Income Level 7	-0.7864	0.0591
Income Level 8	-0.4162	0.0630
Income Level 9	-0.1495	0.0749
Income Level 10	-1.3423	0.1836
Fixed-Effects	Zip code-Qtr Fixed-Effects	
Instruments (Installed-Bases of)	Flex Fuel	

Table 6: Robustness Check: Installed-Base Defined at City and County Levels

Variable	Model R2		Model R3	
	Estimates (x 1000)	Std. Errors (x 1000)	Estimates (x 1000)	Std. Errors (x 1000)
Prius Installed-Base	0.0045	0.0015	0.0003	0.0002
Month 2	0.1325	0.0766	0.1340	0.0799
Month 3	0.1651	0.0810	0.1686	0.0926
Income Level 2	0.1140	0.0594	0.1140	0.0594
Income Level 3	-5.3606	0.1704	-5.3615	0.1704
Income Level 4	-5.1542	0.2361	-5.1545	0.2361
Income Level 5	-4.6185	0.1717	-4.6188	0.1717
Income Level 6	-3.9212	0.1640	-3.9208	0.1640
Income Level 7	-3.6015	0.1559	-3.6015	0.1559
Income Level 8	-2.9377	0.1181	-2.9377	0.1181
Income Level 9	-1.8236	0.1241	-1.8238	0.1241
Income Level 10	-0.2006	0.1430	-0.2009	0.1430
Fixed-Effects	Zip code-Qtr Fixed-Effects		Zip code-Qtr Fixed-Effects	
Instruments (Installed-Bases of)	Hybrids + Flex Fuel		Hybrids + Flex Fuel	

Table 7: Bias-Corrected Estimates

Variable	Model B1		Model IV1	
	Estimates (x 1000)	Std. Errors (x 1000)	Estimates (x 1000)	Std. Errors (x 1000)
Prius Installed-Base Effect	0.1699	0.0312	0.1621	0.0263

Appendix: Inconsistency of estimator with included covariates

We analyze the asymptotic bias of the least squares estimator in a model that includes exogenous covariates other than the installed-base. The model is given by

$$y_i = f_{m_i q_i} + X_{m_i t_i}\beta + Z_i\gamma + \varepsilon_i \tag{A-1}$$

where Z_i is a vector of exogenous covariates. Let the differenced value of this covariate be denoted by $\tilde{Z}$ (where $\tilde{Z}_i = Z_i - \bar{Z}_{m_i q_i}$). The differenced model can be written in vector notation as,

$$\tilde{y} = \tilde{X}\beta + \tilde{Z}\gamma + \tilde{\varepsilon} \tag{A-2}$$

Define, $M_Z \equiv I - \tilde{Z}\left(\tilde{Z}'\tilde{Z}\right)^{-1}\tilde{Z}$ and $M_X \equiv I - \tilde{X}\left(\tilde{X}'\tilde{X}\right)^{-1}\tilde{X}$. We recognize that,

$$\hat{\beta} = \left(\tilde{X}'M_Z\tilde{X}\right)^{-1}\left(\tilde{X}'M_Z\tilde{y}\right) - \left(\tilde{X}'M_Z\tilde{X}\right)^{-1}\left(\tilde{X}'M_Z\tilde{Z}\gamma\right) \tag{A-3}$$

$$\hat{\gamma} = \left(\tilde{Z}'M_X\tilde{Z}\right)^{-1}\left(\tilde{Z}'M_X\tilde{y}\right) - \left(\tilde{Z}'M_X\tilde{Z}\right)^{-1}\left(\tilde{Z}'M_X\tilde{X}\beta\right) \tag{A-4}$$

To compute the asymptotic bias in the estimate of $\hat{\beta}$ first, we note that from Equations (A-2) and (A-3)

$$\hat{\beta} - \beta = \left(\tilde{X}'M_Z\tilde{X}\right)^{-1}\left(\tilde{X}'M_Z\tilde{\varepsilon}\right) \tag{A-5}$$

Applying ST/MWCMT, we get,

$$\underset{N\to\infty}{\text{plim}}\left(\hat{\beta} - \beta\right) = \underset{N\to\infty}{\text{plim}}\left[\frac{1}{N}\left(\tilde{X}'M_Z\tilde{X}\right)\right]^{-1}\underset{\text{N}\to\infty}{\text{plim}}\left[\frac{1}{N}\left(\tilde{X}'M_Z\tilde{\varepsilon}\right)\right] \tag{A-6}$$

By Khintchine's weak law of large numbers,

$$\underset{N\to\infty}{\text{plim}}\left[\frac{1}{N}\left(\tilde{X}'M_Z\tilde{X}\right)\right]^{-1} = \mathbb{E}\left[\tilde{X}_i'M_Z\tilde{X}_i\right]^{-1} \equiv \left(\sigma^2_{XM_ZX}\right)^{-1} \tag{A-7}$$

Now,

$$\begin{aligned}
\underset{N\to\infty}{\text{plim}}\left[\tfrac{1}{N}\left(\tilde{X}'M_Z\tilde{\varepsilon}\right)\right] &= \underset{N\to\infty}{\text{plim}}\left[\tfrac{1}{N}\left(\tilde{X}'\tilde{\varepsilon}\right) - \tfrac{1}{N}\left[\left(\tilde{X}'\tilde{Z}\right)\left(\tilde{Z}'\tilde{Z}\right)^{-1}\left(\tilde{Z}'\tilde{\varepsilon}\right)\right]\right] \\
&= \underset{N\to\infty}{\text{plim}}\left[\left(\tilde{X}'\tilde{\varepsilon}\right)\right] - \underset{N\to\infty}{\text{plim}}\left[\tfrac{1}{N}\left(\tilde{X}'\tilde{Z}\right)\right]\underset{N\to\infty}{\text{plim}}\left[\tfrac{1}{N}\left(\tilde{Z}'\tilde{Z}\right)\right]^{-1}\underset{N\to\infty}{\text{plim}}\left[\tfrac{1}{N}\left(\tilde{Z}'\tilde{\varepsilon}\right)\right] \\
&= \mathbb{E}\left[\tilde{X}_i{}'\tilde{\varepsilon}_i\right] - \mathbb{E}\left[\tilde{X}_i'\tilde{Z}_i\right]\mathbb{E}\left[\tilde{Z}_i'\tilde{Z}_i\right]^{-1}\mathbb{E}\left[\tilde{Z}_i'\tilde{\varepsilon}_i\right]
\end{aligned} \tag{A-8}$$

For $\mathbb{E}\left[\tilde{Z}_i'\tilde{Z}_i\right]^{-1} \neq 0$ and for exogenous Z (i.e. $\mathbb{E}\left[\tilde{Z}_i'\tilde{\varepsilon}_i\right] = 0$), we have that,

$$\underset{N\to\infty}{\text{plim}} \left(\hat{\beta}-\beta\right) = \left(\sigma_{XZ}^2\right)^{-1} \mathbb{E}\left[\tilde{X}_i'\tilde{\varepsilon}_i\right] \tag{A-9}$$

We have from before that,

$$\mathbb{E}\left[\tilde{X}_i'\tilde{\varepsilon}_i\right] = -\frac{5}{9}\sigma_\varepsilon^2 \tag{A-10}$$

Hence, we have the asymptotic bias is,

$$\underset{N\to\infty}{\text{plim}} \left(\hat{\beta}-\beta\right) = -\frac{5}{9}\frac{\sigma_\varepsilon^2}{\sigma_{XM_ZX}^2} \tag{A-11}$$

Thus, the within-estimator with covariates is biased, and the asymptotic bias in this case is also negative. The magnitude of the asymptotic bias is similar to that in a model with no covariates, except the denominator is modified to include the effect of the covariates.

We now derive the asymptotic properties of s^2:

$$s^2 = \frac{\hat{\varepsilon}'\hat{\varepsilon}}{N - N_{ZQ} - K} \tag{A-12}$$

where,

$$\begin{aligned} \hat{\varepsilon} &= \tilde{y} - \tilde{X}\hat{\beta} - \tilde{Z}\hat{\gamma} \\ &= \left(\tilde{y} - \tilde{X}\beta - \tilde{Z}\gamma\right) + \left(\tilde{X}\beta - \tilde{X}\hat{\beta}\right) + \left(\tilde{Z}\gamma - \tilde{X}\hat{\gamma}\right) \\ &= \tilde{\varepsilon} - \tilde{X}\left(\tilde{X}'M_Z\tilde{X}\right)^{-1}\tilde{X}'M_Z\tilde{\varepsilon} - \tilde{Z}\left(\tilde{Z}'M_X\tilde{Z}\right)^{-1}\tilde{Z}'M_X\tilde{\varepsilon} \end{aligned} \tag{A-13}$$

Thus,

$$\begin{aligned} \hat{\varepsilon}'\hat{\varepsilon} &= \tilde{\varepsilon}'\tilde{\varepsilon} - \left(\tilde{\varepsilon}'M_Z\tilde{X}\right)\left(\tilde{X}'M_Z\tilde{X}\right)^{-1}\left(\tilde{X}'\tilde{X}\right)\left(\tilde{X}'M_Z\tilde{X}\right)^{-1}\left(\tilde{X}'M_Z\tilde{\varepsilon}\right) \\ &\quad - \left(\tilde{\varepsilon}'M_X\tilde{Z}\right)\left(\tilde{Z}'M_X\tilde{Z}\right)^{-1}\left(\tilde{Z}'\tilde{Z}\right)\left(\tilde{Z}'M_X\tilde{Z}\right)^{-1}\left(\tilde{Z}'M_X\tilde{\varepsilon}\right) \end{aligned} \tag{A-14}$$

Consider the second term in the above equation,

$$\tilde{\varepsilon}'M_Z\tilde{X} = \tilde{\varepsilon}'\tilde{X} - \tilde{\varepsilon}'\tilde{Z}\left(\tilde{Z}'\tilde{Z}\right)^{-1}\tilde{Z}'\tilde{X} \tag{A-15}$$

Using a similar approach as before,

$$\begin{aligned} \underset{N\to\infty}{\text{plim}} \frac{1}{N}\left(\tilde{\varepsilon}'M_Z\tilde{X}\right) &= \mathbb{E}\left[\tilde{\varepsilon}_i'\tilde{X}_i\right] - \mathbb{E}\left[\tilde{\varepsilon}_i'\tilde{Z}_i\right]\mathbb{E}\left[\tilde{Z}_i'\tilde{Z}_i\right]^{-1}\mathbb{E}\left[\tilde{Z}_i'\tilde{X}_i\right] \\ &= \mathbb{E}\left[\tilde{\varepsilon}_i'\tilde{X}_i\right] \\ &= -\frac{5}{9}\sigma_\varepsilon^2 \end{aligned} \tag{A-16}$$

which follows from the exogeneity of Z (giving us $\mathbb{E}\left[\tilde{\varepsilon}_i' Z_i\right] = 0$), the fact that $\tilde{Z}'\tilde{Z}$ is positive definite. We have again used the expression for $\mathbb{E}\left[\tilde{\varepsilon}_i'\tilde{X}_i\right]$ evaluated earlier. Similarly,

$$\underset{N \to \infty}{\text{plim}} \frac{1}{N}\left(\tilde{X}' M_Z \tilde{\varepsilon}\right) = -\frac{5}{9}\sigma_\varepsilon^2 \tag{A-17}$$

Also,

$$\underset{N \to \infty}{\text{plim}} \frac{1}{N}\left(\tilde{X}' M_Z \tilde{X}\right)^{-1} = \mathbb{E}\left[\tilde{X}_i' M_Z \tilde{X}_i\right]^{-1} = \left(\sigma^2_{X M_Z X}\right)^{-1} \tag{A-18}$$

$$\underset{N \to \infty}{\text{plim}} \frac{1}{N}\left(\tilde{X}'\tilde{X}\right) = \mathbb{E}\left[\tilde{X}_i'\tilde{X}_i\right] = \sigma_X^2 \tag{A-19}$$

Turning to the last term in Equation (A-14),

$$\tilde{\varepsilon}' M_X \tilde{Z} = \tilde{\varepsilon}'\tilde{Z} - \tilde{\varepsilon}'\tilde{Z}\left(\tilde{Z}'\tilde{Z}\right)^{-1}\tilde{Z}'\tilde{X} \tag{A-20}$$

Thus,

$$\begin{aligned}
\underset{N \to \infty}{\text{plim}} \frac{1}{N}\left(\tilde{\varepsilon}' M_X \tilde{Z}\right) &= \mathbb{E}\left[\tilde{\varepsilon}_i'\tilde{Z}_i\right] - \mathbb{E}\left[\tilde{\varepsilon}_i{}'\tilde{X}_i\right]\mathbb{E}\left[\tilde{X}_i'\tilde{X}_i\right]^{-1}\mathbb{E}\left[\tilde{X}_i'\tilde{Z}_i\right] \\
&= -\mathbb{E}\left[\tilde{\varepsilon}_i'\tilde{X}_i\right]\mathbb{E}\left[\tilde{X}_i{}'\tilde{X}_i\right]^{-1}\mathbb{E}\left[\tilde{X}_i'\tilde{Z}_i\right] \\
&= -\left(-\frac{5}{9}\sigma_\varepsilon^2\right)\left(\sigma_X^2\right)^{-1}\Sigma_{XZ} \\
&= \frac{5}{9}\sigma_\varepsilon^2\left(\sigma_X^2\right)^{-1}\Sigma_{XZ} \\
&= \frac{5\sigma_\varepsilon^2}{9\sigma_X^2}\Sigma_{XZ}
\end{aligned} \tag{A-21}$$

where, $\mathbb{E}\left[\tilde{X}_i'\tilde{Z}_i\right] = \Sigma_{XZ}$. Similarly,

$$\underset{N \to \infty}{\text{plim}} \frac{1}{N}\left(\tilde{Z}' M_X \tilde{\varepsilon}\right) = \frac{5\sigma_\varepsilon^2}{9\sigma_X^2}\Sigma_{ZX} \tag{A-22}$$

where, $\mathbb{E}\left[\tilde{Z}_i'\tilde{X}_i\right] = \Sigma_{ZX}$. Also define $\mathbb{E}\left[\tilde{Z}_i'\tilde{Z}_i\right] = \Sigma_{ZZ}$ and $\mathbb{E}\left[\tilde{Z}_i' M_X \tilde{Z}_i\right] = \Sigma_{Z M_X Z}$. Thus, we obtain,

$$\begin{aligned}
\underset{N \to \infty}{\text{plim}} s^2 &= \underset{N \to \infty}{\text{plim}}\left(\frac{N}{N - N_{ZQ} - K}\right)\underset{N \to \infty}{\text{plim}}\left[\frac{1}{N}\left(\hat{\varepsilon}'\hat{\varepsilon}\right)\right] \\
&= \sigma_\varepsilon^2 - \left(-\frac{5}{9}\sigma_\varepsilon^2\right)\left(\sigma^2_{X M_Z X}\right)^{-1}\sigma_X^2\left(\sigma^2_{X M_Z X}\right)^{-1}\left(-\frac{5}{9}\sigma_\varepsilon^2\right) \\
&\quad - \left(\frac{5\sigma_\varepsilon^2}{9\sigma_X^2}\Sigma_{XZ}\right)\Sigma^{-1}_{Z M_X Z}\Sigma_{ZZ}\Sigma^{-1}_{Z M_X Z}\frac{5\sigma_\varepsilon^2}{9\sigma_X^2}\Sigma_{ZX} \\
&= \sigma_\varepsilon^2 - \frac{25\sigma_\varepsilon^4}{81\sigma_X^2}\left[\frac{\sigma_X^4}{\sigma^4_{X M_Z X}} + \Sigma_{XZ}\Sigma^{-1}_{Z M_X Z}\Sigma_{ZZ}\Sigma^{-1}_{Z M_X Z}\Sigma_{ZX}\right]
\end{aligned} \tag{A-23}$$

Hence, for the model with exogenous covariates,

$$\underset{N \to \infty}{\text{plim}} \left(s^2 - \sigma_\varepsilon^2\right) = -\frac{25\sigma_\varepsilon^4}{81\sigma_X^2} \left[\frac{\sigma_X^4}{\sigma_{XM_ZX}^4} + \Sigma_{XZ}\Sigma_{ZM_XZ}^{-1}\Sigma_{ZZ}\Sigma_{ZM_XZ}^{-1}\Sigma_{ZX}\right] \tag{A-24}$$

The terms inside the parenthesis in this expression are all of quadratic form and hence positive. Thus, we find once again that the asymptotic bias is negative and its magnitude is a modified version of the expression we evaluated for the model without covariates. ♣