Consumer Benefit of Big-Box Supermarkets:

The Importance of Controlling for Big-Box Presence

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November—2013

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JOB MARKET PAPER

Abstract

Many European countries have instituted regulatory barriers that discourage the entry of out-of-town big-box supermarkets. A key policy question is whether such land use regulations are indeed desirable for society. This paper evaluates the consumer benefit of big-box supermarkets and contributes to inform the policy debate on the efficiency of land use regulations targeted at big-boxes. I propose a novel framework to consistently estimate demand in the context of endogenous store entry (more in general: endogenous product availability). Then, I exploit UK home scanner expenditure data in conjunction with the introduction of Town Centres First (i.e., a tightening in land use regulations) to separately identify preferences for big-boxes from retailers’ entry decisions. In conclusion, I evaluate the consumer welfare consequences of big-box entry. Results reveal three key deviations from conventional estimation approaches. First, the consumer benefit of big-box supermarkets is an order of magnitude smaller due to the implicit sample-selection bias that arises when supermarket entry locations are endogenous to local consumer preferences. Second, the largest part of the increase in consumer welfare of grocery shopping is due to improvements in the quality of small supermarkets (e.g., variety of retailers, total floorspace, and range of product categories sold). Third, disadvantaged households (e.g., those without cars, single parents, and single pensioners) are unable to enjoy most of the benefits generated by big-boxes: they tend to live in deprived areas not profitable for retailers, and have to travel further away for their grocery shopping. The paper’s findings help rationalize Town Centres First, a policy frequently criticized as being anti-consumer.

Keywords: Big-box; demand estimation; endogenous product choice; land use regulation; selection bias; Town Centres First; welfare.

*Special thanks for the support and great discussions to Gregory Crawford, Sascha Becker, Andrés Carvajal, Valentina Corradi, and Fabian Waldinger. Special thanks also to the members of the productivity sector at IFS for their hospitality and for allowing me to use their data, in particular to Rachel Griffith and Martin O’Connell for their constant help and suggestions. I am indebted for the insightful discussions and useful comments to Dan Ackerberg, Dan Bernhardt, Clément de Chaisemartin, Tim Conley, Mirko Draka, Ben Faber, Luigi Pascali, Salvador Navarro, David Rivers, Michelle Sovinski, and the participants of the Pizza Workshop (Warwick), CWIP (Warwick), NIE Colloquium (Nottingham), the 5th ICEEE Conference (Genoa), Applied Seminar (Western Ontario), IO Reading Group (IFS), and 40th EARIE Conference (Évora). I gratefully acknowledge financial support from the European Research Council (ERC) under ERC-2009-AdG grant agreement number 249529. Data supplied by TNS UK Limited. The use of TNS UK Ltd. data in this work does not imply the endorsement of TNS UK Ltd. in relation to the interpretation or analysis of the data. All errors and omissions remained are my responsibility. a.iaaria@warwick.ac.uk.
1 Introduction

The UK grocery market is worth £169.7 billions, accounting for a market share of 54.9% of total retail spending. In the last five years (2008-2012), the UK grocery market has been growing at an average 4.5% faster than the national GDP. Large grocery retailers have been growing larger, while—in spite of the sustained market expansion—high street shops have been growing smaller. Despite the numerous benefits (e.g., wide variety of products, low prices, free parking, etc.), out-of-town big-boxes are perceived to generate negative externalities such as draining shoppers away from town centres, depriving high streets from their vital essence. The number of specialist grocery stores and independents has been shrinking since the 1950s in a downward spiral of decline: as closures reduce footfall, this weakens the high streets, which leads to more vacancies.¹

The escalation of out-of-town big-box supermarkets experienced by the UK in the last decades triggered strong political reactions that resulted in land use regulations design to protect town centres. In 1996 the Town Centres First (TCF) policy was introduced. Town Centre First makes it harder for retailers to develop new out-of-town big-boxes, inducing them to open smaller supermarkets as close as possible to high streets.² Town Centres First has been studied in relation to its detrimental effects on retailers’ loss of productivity and increased unemployment rates.³ However, we still have a limited understanding of the consumer welfare consequences of such land use regulations. To assess the efficiency of Town Centres First, one needs to understand consumer preferences for big-boxes and their services.

This paper exploits home scanner consumer data and the introduction of Town Centres First to further our understanding of the welfare consequences of land use regulations. My paper makes two main contributions. First, it shows that endogenous product availability causes economically significant biases in demand estimation. This leads me to develop an estimation framework that overcomes the empirical challenge that big-boxes are more likely to be observed in places where consumers like them more. Second, it uses this estimation framework to address to the debate on land use regulations using “robust” measures of consumer benefits from big-boxes.⁴, ⁵

Results reveal three key deviations from findings obtained from conventional estimation approaches.⁶

First, consumer benefits from big-box supermarkets are an order of magnitude smaller due to the implicit

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¹The facts reported in this paragraph come from IGD Retail Analysis (2013) and The Portas Review (2011).
²See Office of the Deputy Prime Minister (2005) [ODPM].
³See, for example: Cheshire et al. (2012), Griffith & Harmgart (2008), Haskel & Sadun (2012), and Sadun (2011).
⁵Hausman & Leibtag (2007) and Smith (2006) estimate the consumer benefit of big-box supermarkets; but their estimators do not control for endogenous sample-selection due to big-box presence.
sample-selection bias that arises when supermarket entry locations are endogenous to local consumer preferences. Second, I find that most of the increase in consumer welfare of grocery shopping between 2001 and 2004 is due to improvements in the quality of small supermarkets (e.g., variety of retailers, total floorspace, and range of product categories sold). Third, disadvantaged households (e.g., those without cars, single parents, and single pensioners) are the least able to enjoy the potential benefits generated by big-boxes: they tend to live in deprived areas (unattractive to retailers) and have to travel further away for their grocery shopping.\footnote{Hausman & Leibtag (2007) conclude that, in the US, big-boxes are especially beneficial for poor consumers because of the advantageous prices they offer and the additional competition they engender on surrounding supermarkets. In my sample, in contrast, the extremely high transportation costs faced by disadvantaged households mean that they are “stuck” in deprived areas with no retailers; and this makes it harder for them to enjoy any direct or indirect benefit generated by big-boxes.}

These findings help rationalize Town Centres First, a policy frequently criticized as being anti-consumer.\footnote{Smith (2006), for example, concludes that TCF imposes suboptimal store characteristics to consumers. Differently, my results are closer in spirit to those by Griffith & Harmgart (2008). They show that, in the post-TCF period, the dramatic change in retailers’ entry behaviour towards town centres and smaller formats might be, at least in part, driven by demand considerations rather than by more restrictive regulations (e.g., if Tesco did not open an out-of-town big-box in a certain market, it can be that its expected demand was not “high enough”).}

My paper relates to the recent literature on endogenous product availability.\footnote{The most relevant examples are: Beresteau & Ellickson (2006), Crawford et al. (2012), Draganska et al. (2009), Eizenberg (2012), Ho (2009), Ho et al. (2012), Nosko (2011), and Sweeting (forthcoming). For a survey, see Crawford (2012).} In broad terms, these papers address the consequences of endogenous product availability with structural models of demand and supply.\footnote{Demand is “standard,” while firms not only choose prices but also the range of products they are going to sell (for brevity: product availability).} Despite the great care taken to model firms’ behaviours, none of the papers in the literature controls for endogenous sample-selection due to product availability when estimating demand. Hence, their demand estimates are exposed to selection bias.\footnote{This is not to say that their demand estimates are necessarily wrong: the strength of such form of endogenous sample-selection may vary from application to application.} Some papers do not discuss the issue at all. Others, like Draganska et al. (2009) and Eizenberg (2012), recognize the problem might exist, but they then proceed to assume it away (rather than test it). Sweeting (forthcoming)—in contrast—assumes the problem away, but only after providing evidence that, in his application, endogenous sample-selection due to product availability may not be strong. My paper is the first explicitly to control and to test for endogenous product availability in demand estimation. Moreover, I show—in the UK grocery industry—how policy implications can drastically change if endogenous product availability is mistakenly ignored in the estimation of preferences.

I account for endogenous product availability in demand estimation using classic sample-selection models (i.e., Heckman-type models).\footnote{The availability of products is modeled by “reduced form” selection equations, and then the demand equations are augmented by inverse Mills ratios to account for potential selection on unobservables.} In contrast, existing papers address the consequences of endogenous product availability with structural models of firms’ behaviours.\footnote{See footnote [9].} The estimation of any
structural supply-side model requires knowledge of the demand parameters as an input. Typically, due to computational complexities, demand and product availability are estimated sequentially, starting from demand.\textsuperscript{14} This sequential estimation procedure, with demand first and then product availability given demand, “forces” researchers to estimate demand without controlling for endogenous sample-selection due to product availability. The part of the model that deals with product availability is the supply-side,\textsuperscript{15} but this requires demand parameters to be evaluated. Thus, in a sequential estimation that “starts” from demand, one cannot control for endogenous product availability in such a structural fashion. But, estimating demand without accounting for endogenous sample-selection due to product availability, if indeed there is selection, will generally yield biased estimates of preferences. Consequently, estimated structural models of product choice that rely on such preliminary demand estimates may similarly mislead.

To control for endogenous sample-selection due to product availability, one must either estimate demand and product availability simultaneously, or start the estimation sequence with product availability. As in the other papers in the literature, my estimation procedure is sequential. The key difference is that I invert the sequence of estimation: I first estimate product availability, and then demand. Inverting the sequence’s order implies the estimation of product availability without having an estimate of demand, hence the need for a reduced form product availability model. Importantly, once correct demand estimates are obtained, nothing prevents one from doing what the existing papers do: one can use them to estimate a fully structural supply. Thus, the proposed reduced form approach should be seen as a pre-requisite for consistent structural estimation.

The analysis proceeds in several steps. I propose a general model of demand in a setting with both endogenous prices and endogenous product availability. The classic discrete choice model of demand, which allows for endogenous prices with individual-level data (i.e., Berry, Levinsohn, and Pakes (2004) [BLP]), is augmented by a first stage Heckman sample-selection model. The model allows one to test for endogenous product availability in a tractable way. Identification of the model requires, in addition to instruments for price, instruments for product availability that are unrelated to consumer preferences.

Using UK home scanner expenditure data and exploiting the introduction of Town Centres First, I

\textsuperscript{14}None of the aforementioned papers estimates demand and product availability simultaneously.
\textsuperscript{15}Demand-side sources of endogenous product availability are studied in the “consideration set” literature, from marketing. Examples are Draganska & Klapper (2010), Koulayev (2012), Metha et al. (2003), Nierop et al. (2010), and Sovinski (2008). A common feature of these papers relates to the “nature” of heterogeneity in choice sets. Consumers face different choice sets not (necessarily) because some of the products are not available to them, but rather because they are not aware of the full set of available products. In the current paper I take a different stand: I consider consumers as fully rational and the only source of choice set heterogeneity being observable product availability (i.e., a product does not belong to the choice set of a consumer only if it is not physically available to her). A different source of demand-side endogenous product choice is studied in appendix [8.1].
separately identify preferences for big-boxes from retailers’ entry decisions. Estimation results show the importance of accounting for big-box presence in the estimation of supermarket demand: big-boxes are more likely to be located in those markets where consumers like them more. Consequently, if one does not control for selection, estimated preferences for big-boxes are upward biased.

Given the estimated preferences, I evaluate the consumer benefit of big-boxes by decomposing the changes in consumer welfare of grocery shopping realized between 2001 and 2004. Selection bias on the estimation of preferences translates into inflated consumer benefit estimates: not controlling for big-box presence mistakenly induces researchers to believe that big-boxes are highly valued by the population at large, and not just by their most loyal customers.\textsuperscript{16} On the other hand, the largest part of the increase in consumer welfare of grocery shopping between 2001 and 2004 is due to improved unobserved quality of small supermarkets.\textsuperscript{17} I then investigate, with a differences-in-differences strategy, if any of the improvements in the unobserved quality of small supermarkets could be—indirectly—attributed to big-boxes.\textsuperscript{18} This is indeed the case: the benefits consumers obtain from big-boxes extend to the unobserved quality—rather than to lower prices—of nearby small supermarkets. This appears to be especially true for small supermarkets not belonging to the big four retailers (i.e., the independents).\textsuperscript{19} Importantly, disadvantaged households are shown to be the least able to enjoy the potential benefits generated by big-boxes: because they tend to live in areas which are not attractive for retailers, they are forced to travel further away for their shopping, and register the smallest increases in consumer welfare of grocery shopping between 2001 and 2004.

In conclusion, the paper presents the following novel insights. First, it shows that endogenous product availability can give rise to economically significant biases in demand estimation. Second, it proposes and implements a new framework to address this issue. Third, the empirical analysis of the UK grocery industry presents evidence of a rich pattern of consumer welfare implications of land use regulations. In particular, if it is true that big-boxes generate a wealth of benefits for consumers, it is also the case that these are of a smaller order of magnitude than previously thought, and that the most disadvantaged con-

\textsuperscript{16}Without controlling for big-box presence, we would conclude that expected consumer welfare of grocery shopping increased 139% from 2001 to 2004. Of this increase, 53.8 percentage points are directly attributable to big-boxes. However, once one controls for big-box presence, the increase in expected consumer welfare deflates to 47.8%, with only 7 percentage points directly attributable to big-boxes.
\textsuperscript{17}For those households who experience entry of new big-boxes, welfare implications drastically change when controlling for sample-selection due to big-box presence. Without controlling for big-box presence, the increase in consumer welfare due to big-boxes relative to that due to unobserved quality of small supermarkets amounts to 1.08; while controlling for big-box presence, the ratio falls to 0.28. Some of the improvements in unobserved quality of small supermarkets are shown to be in terms of variety of retailers, total floorspace, and range of product categories sold.
\textsuperscript{18}Consumers can obtain direct benefits from big-boxes by just going there for their grocery or, even without going shopping there, they can obtain indirect benefits because of the additional competitive pressure big-boxes generate on other stores (e.g.: lower prices and less frequent stockouts). This sort of indirect benefits have been recently documented by Gould et al. (2005), Hausman & Leibtag (2007), Matza (2011), Sadun (2011), and Schiraldi et al. (2011).
\textsuperscript{19}These results are in line with the findings of Matsa (2011) in relation to Walmart in the US.
consumers—because of their higher transportation costs—seem to be the least able to enjoy such benefits. These findings contribute to a growing academic literature on the welfare effects of retail regulation, and serve to inform the policy debate on the costs and benefits of the expansion of big-box supermarkets.


2 A Demand Model with Endogenous Sample-Selection

In this section I first outline, in general terms, a discrete choice model along the lines of BLP (2004) [i.e., a demand model for individual-level data that accounts for price endogeneity].\(^{20}\) Second, I describe a (potentially common) source of endogenous sample-selection that might bias classic demand estimators: sample-selection due to product availability. Third, I propose an estimation procedure which controls for selection bias due to product availability.

2.1 Indirect Utility and Choice Probability

As in the standard model of BLP (2004), the indirect utility individual \(i\) obtains from choosing alternative \(j\) in market \(m\) is defined as:

\[
U_{ijm} = \delta_{jm}^* + V_{jm}(p_{jm}, x_{ijm}, \eta_i) + \epsilon_{ijm}; \quad i = 1, \ldots, I; j = 1, \ldots, J; m = 1, \ldots, M. \tag{2.1}
\]

Notice how (2.1) implies that individuals have complete preferences: each individual \(i\) has a well defined \(U_{ijm}\) for any \((j, m)\) combination. As discussed in detail in section [2.2], completeness of preferences turns out to be essential for my sample-selection story. \(\delta_{jm}^*\) represents the market-specific utility shifter for alternative \(j\), \(E_i[U_{ijm}] = \delta_{jm}^*\). This is the portion of indirect utility associated with \((j, m)\) that is common across individuals. The remaining part of (2.1) is idiosyncratic to individual \(i\) and it consists of

\(^{20}\)The equivalent class of models for aggregate data is presented in detail by BLP (1995), Nevo (2001), and Petrin (2002). My point about endogenous sample-selection due to product availability directly applies also to models estimated with aggregate data.
an observable part, $V_{jm}(p_{jm}, x_{jm}, \eta_i)$, and an unobservable part (to the researcher), $\varepsilon_{ijm}$. The observable $V_{jm}(p_{jm}, x_{jm}, \eta_i)$ is a function of the alternative’s price in market $m$, $p_{jm}$, other observable characteristics of individual $i$ (i.e., demographics), alternative $j$, and market $m$, $x_{ijm};$ and individual-specific parameters, $\eta_i$.

I assume that individuals make choices over the set of $(j, m)$ combinations (a set with $J \cdot M$ elements), which I will call products [see chapter 2 of Debreu (1959)]. In other words, individuals consider alternative $j$ in market $m_1$ as a potentially different object from alternative $j$ in market $m_2$. For example, an apple sold next to my house is a different product from an apple sold twenty five-minute drive from my house. Index $j$ refers to the physical characteristics of the alternative (e.g., color, weight, grams of sugar, etc. describing the apple), while index $m$ refers to the attributes of the place where alternative $j$ is sold (e.g., features of the specific shop and surrounding area). An important special case arises whenever each individual is “stuck” in a specific market: then individuals do not choose over dimension $m$, and the choice model reduces to the $j$ dimension [see Golsbee & Petrin (2004)]. In appendix [8.1] I show how, if individuals indeed also choose over dimension $m$, not accounting for it can lead to biased demand estimates.

The unobservable $\varepsilon_{ijm}$ is assumed to be distributed i.i.d. extreme value. The probability of product $(j, m)$ being individual $i$’s first-best is given by a mixed logit model [see McFadden & Train (2000)]. Since this is standard in the literature, the details are in appendix [8.2].

The market-specific utility shifter for alternative $j$ is characterized as:

$$\delta^*_{jm} \equiv \alpha p_{jm} + \beta x_{jm} + \zeta_{jm}; \quad j = 1, \ldots, J; \quad m = 1, \ldots, M. \quad (2.2)$$

The observable part of $\delta^*_{jm}$ depends on the characteristics of alternative $j$ and market $m$, $(p_{jm}, x_{jm})$, and on the average preferences of individuals for such characteristics, $(\alpha, \beta)$. The unobservable (to the researcher) part of $\delta^*_{jm}$, $\zeta_{jm}$, captures the remaining portion of average utility due to further characteristics of product $(j, m)$ not controlled for by $\alpha p_{jm} + \beta x_{jm}$.

At least since Berry (1994), researchers have typically focused on addressing the potential correlation between $p_{jm}$ and $\zeta_{jm}$ in the estimation of (2.2) (e.g., products with more desirable unobservable characteristics, such as design, are sold at higher prices). I propose to control for an additional problem which might result in biased estimates of $(\alpha, \beta)$: endogenous sample-selection due to product availability.

\footnote{It is the deviation of individual $i$’s indirect utility for combination $(j, m)$ from its average across all the individuals, $U_{ijm} - \mathbb{E}_i \left[U_{ijm}\right]$.}

\footnote{For recent surveys regarding the topic, see Nevo (2011) and chapter 13 of Train (2009).}
2.2 Endogenous Sample-Selection due to Product Availability: A Story

Any purchase dataset from which a researcher hopes to infer something about individuals’ preferences can be seen as a set of revealed preferences. Unless every alternative \( j \) is available in every market \( m \), the most complete purchase dataset the researcher can collect has to be a sample of revealed preferences (as opposed to the population). In addition, the way revealed preferences are “sampled” from the population is likely not to be “random,” but rather endogenous to preferences themselves. If this is the case, then any estimator of \((\alpha, \beta)\) that does not control for endogenous sample-selection may be inconsistent.

The researcher observes a sample of market-specific revealed preferences: if an alternative is not sold in a market, the researcher cannot possibly observe any market-specific revealed preferences for it. Thus, the researcher can only observe—in principle—market-specific revealed preferences for those alternatives that are sold in the market.

Furthermore, the sample of market-specific revealed preferences observed by the researcher is endogenous: alternative \( j \) is sold in market \( m \) only if, there, “enough” individuals are expected to like it “sufficiently,” and to buy it in sufficient quantities. This means that the set of alternatives available in each market may be correlated with individuals’ preferences: for example, Italian food is offered only where there are strong enough preferences for it (see Waldfogel (2007) for a book-length treatment of this observation). Hence, there is endogenous sample-selection over the population of market-specific revealed preferences.

More formally, because individuals have complete preferences, \( \{\delta^*_jm\}_{jm=1}^{J\cdot M} \) [the set containing a \( \delta^*_jm \) for every product \((j, m)\)] is the population of market-specific utility shifters. Unless every alternative \( j \) is available in every market \( m \), the researcher will only have access to a sample from \( \{\delta^*_jm\}_{jm=1}^{J\cdot M} \).

Generally, the researcher does not directly observe any of the \( \{\delta^*_jm\}_{jm=1}^{J\cdot M} \). However, the researcher does observe the market shares of the available alternatives in each market, \( \{S_{jm}\}_{jm=1}^{J\cdot M} \). Berry (1994) initiated the investigation of how to “recover” the \( \delta^*_jm \)’s from the market share data (see relationship (8.3) in appendix [8.2]). For a collection of non-degenerate market shares, the market share of alternative \( j \) in market \( m \) can be expressed as:23

\[
S_{jm} \equiv G_{jm} (\delta^*, x, \theta).
\]

\( G \) is defined as (ignoring for simplicity the \((x, \theta)\) arguments) [see Berry et al. (2013)]:

\[
[G_1 (\delta) , \ldots , G_k (\delta) , \ldots , G_K (\delta) ] : \Delta^K \rightarrow (0,1)^K,
\]

where the index \( k \) identifies the products whose observed market shares are non-degenerate and \( \delta^*_k \in \Delta \). Hence, \( G \) is only defined over (and maps to) the group of products whose observed market shares are strictly included in the unit simplex.

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Berry et al. (2013) derive sufficient conditions for the invertibility of $G$:

$$
\delta_{jm} = \begin{cases} 
\delta_{jm}^* \equiv G_{jm}^{-1} (S, x, \theta) & \text{if } 0 < S_{jm} < 1 \\
- & \text{otherwise}
\end{cases}
$$

(2.3)

because $G^{-1}$ is not defined on the boundary of the unit simplex [see footnote 1 of Berry et al. (2013)]. Thus, the researcher can only recover the set $\{ \delta_{jm} \}_{jm=1}^{JM} = \{ \delta_{jm}^* \mid 0 < S_{jm} < 1 \}$, which is smaller than the population $\{ \delta_{jm}^* \}_{jm=1}^{JM}$ whenever some alternative $j$ has zero market share in some $m$. Moreover, if the absence of alternative $j$ from market $m$ (i.e., $S_{jm} = 0$) depends on its unobservable market-specific utility shifter $E_i [U_{ijm}] = \delta_{jm}^*$ (to the researcher, indeed $\delta_{jm} = -$), then the researcher can only recover a non-random sample from $\{ \delta_{jm}^* \}_{jm=1}^{JM}$. In turn, this may induce selection-bias in the estimation of $(\alpha, \beta)$.

Three remarks. First, notice how the threat of selection-bias is independent of that of price endogeneity: even exogenous prices, a researcher could still face endogenous sample-selection due to product availability.

Second, I confine the sample-selection problem to the market-specific utility shifters. More generally, one could have selection on the whole indirect utility $U_{ijm}$ rather than only on a component of it. Loosely speaking, I restrict attention to a “market-level selection,” instead of a more general “individual-level selection.” As a consequence, the resulting econometric issues are limited to the estimation of $(\alpha, \beta)$.

Hence, if the question addressed by the researcher does not require correct inference of $(\alpha, \beta)$, recovering $\{ \delta_{jm} \}_{jm=1}^{JM}$ is enough to conduct valid inference on $V_{jm} (p_{jm}, x_{ijm}, \eta_i)$. This second remark also holds for the popular case of price endogeneity [see BLP (2004)].

Third, my setting is a special case of treatment evaluation. In the evaluation of any “treatment” (i.e., welfare effect of some economically relevant event), researchers must choose whether they are interested in the average treatment effect (ATE) (i.e., representative of the population of preferences) or just in the average treatment effect on the treated (ATET) (i.e., representative of the observed sample of preferences). In those situations where preferences are estimated from a random sample of individuals in which “everyone has access to everything” (in the sense detailed above), then ATET and ATE measures of welfare are equivalent, and ATET measures can be used to draw general conclusions.

\footnote{As shown in appendix [8.1], if individuals choose over the set of products $(j, m)$’s, but the researcher incorrectly restricts their choice sets to the $j$ dimension, then the resulting bias could be attributed to individual-level selection. This form of endogenous sample-selection is distinct from that described in the current section; the two can coexist.}

\footnote{For a survey of the treatment evaluation literature, see Heckman & Vytlacil (2005).}

\footnote{In the sense that the observed sample used to estimate preferences is representative of the population.}

\footnote{It is admissible to estimate preferences from the full set $\{ \delta_{jm}^* \}_{jm=1}^{JM}$ and then, in the simulated counterfactual, to remove some product from individuals’ choice sets; for example, see Petrin (2002). The key point is to estimate preferences from the full...}
interesting cases, though, researchers are forced to estimate preferences from a subset of \( \{ \delta^*_jm \}^{J \cdot M}_{jm=1} \). In the latter situation, ATE and ATET measures of welfare differ, and researchers must opt for ATE whenever they wish to draw conclusions that are broadly applicable to any randomly selected individual from the population. My paper talks to those interested in ATE measures of welfare. Also, I refer to “biased estimates” with respect to measures that are representative of the population of preferences.

### 2.3 Endogenous Sample-Selection due to Product Availability: A Fix

As motivated in section [1], the selection story outlined in section [2.2] is modeled in a reduced-form fashion as a type II tobit model.\(^{29}\) The availability of alternative \( j \) in market \( m \) can be expressed as:

\[
y_{jm} = 1 \left( \pi^1_{jm} - \pi^0_{jm} > 0 \right) = 1 \left( \mu_j w_{jm} + u_{jm} > 0 \right), \tag{2.4}
\]

where \( \pi^1_{jm} - \pi^0_{jm} \) is a latent index for the profitability of selling alternative \( j \) in market \( m \); \( 1(\cdot) \) is an indicator function, \( \mu_j \) are alternative \( j \)-specific parameters, \( w_{jm} \) are observable characteristics of product \( (j, m) \), and \( u_{jm} \) is an unobservable error term. Regressors \( w_{jm} \) are always observed, regardless of whether \( y_{jm} \) is 1 or 0.\(^{30}\) Using (2.2) and (2.3) the observed market-specific utility shifter for alternative \( j \) in market \( m \), \( \delta_{jm} \), can be written as:

\[
\delta_{jm} = \begin{cases} 
\alpha p_{jm} + \beta x_{jm} + \zeta_{jm} & \text{if } y_{jm} = 1 \\
& \text{otherwise}
\end{cases}, \tag{2.5}
\]

where I assume that \( (p_{jm}, x_{jm}) \) is not observed if \( y_{jm} = 0 \) (e.g., if an alternative is not sold in a certain market, then, typically, it is not possible to know its counterfactual price without additional assumptions). In the tobit terminology, relationship (2.4) is a selection equation while (2.5) is a truncated outcome equation. In order to make the model operational, I make three standard assumptions [see section 19.6.2 of Wooldridge (2010)]:

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\(^{28}\)For example, see the literature on endogenous product choice discussed in section [1].

\(^{29}\)For a definition of type II tobit model, see chapter 10 of Amemiya (1985).

\(^{30}\)This can limit the use of some alternative \( j \)'s characteristics as regressors in equation (2.4). Typically, this is the case for those characteristics that are thought to be endogenous (i.e., easily modified by firms in the short run). For example, if alternative \( j \) is not sold in market \( m \), then its price \( p_{jm} \) might not be easy to infer. In contrast, markets’ characteristics should not be subject to this problem.
1. \((u_{jm}, \xi_{jm})\) is independent of \(w_{jm}\). \(u_{jm} \sim N(0,1)\). \(\mathbb{E} [\xi_{jm} \mid u_{jm}] = \gamma_j u_{jm}\).

2. There are valid instruments \(z_{jm}\) for \((p_{jm}, x_{jm})\) such that \(\mathbb{E} [z_{jm} \xi_{jm}] = 0\); i.e., some instrument for price.

3. At least one element of \(w_{jm}\) is not included in \(z_{jm}\); i.e., some, different, instrument for selection.

Assumption (1) imposes three restrictions. It requires the marginal distribution of the error term in (2.4) to be standard normal. This allows one to estimate \(\mu_j\) and the derived inverse Mills ratio, parametrically via a probit model.\(^{31}\) The restriction on \(\mathbb{E} [\xi_{jm} \mid u_{jm}]\) imposes structure on the relationship between the unobserved portion of utility, \(\xi_{jm}\), and the unobserved portion of the latent profitability index, \(u_{jm}\). In line with the suggestion of BLP (2004), I do not impose any specific distribution for \(\xi_{jm} \mid (p_{jm}, x_{jm})\). Moreover, it is necessary not to have any endogeneity issue in the selection equation.

Assumption (2) is standard in the BLP framework. With valid instruments, one can account for the potential correlation between \(p_{jm}\) and \(\xi_{jm}\) using a 2SLS estimator. Notice that, jointly, assumptions (1) and (2) do not particularly restrict the identification requirements on \(\xi_{jm}\) with respect to a standard BLP framework.

Assumption (3) ensures that selection and preferences are separately identified. Notice that, jointly with assumption (1), it is quite stringent: the exogenous regressor cannot be either an element of \(z_{jm}\) or related to \(\xi_{jm}\). In other words, it is required some exogenous (with respect to preferences) “shock” to the availability of alternative \(j\) across markets.

A natural question at this point is: when can sample-selection be ignored and \((\alpha, \beta)\) be consistently estimated via a standard BLP procedure (that is, given assumption (2), when can one estimate by 2SLS equation (2.5) on the selected sample of \(\delta_{jm}\)’s)? The crucial condition for consistent estimation of \((\alpha, \beta)\) by 2SLS of (2.5) is [see section 19.4 of Wooldridge (2010)]:

\[
\mathbb{E} [y_{jm} z_{jm} \xi_{jm}] = 0. \tag{2.6}
\]

It follows, then, how assumption (2) alone may fail to deliver consistency of the 2SLS estimator: \(\mathbb{E} [z_{jm} \xi_{jm}] = 0\) does not necessarily imply (2.6). There are two special cases in which assumption (2) alone implies condition (2.6). First, if selection is at “random:” \(y_{jm}\) is independent of \((z_{jm}, \xi_{jm})\). Second, if selection is on

\(^{31}\)This strong distributional assumption greatly simplifies the empirical implementation of the product choice model. There are margins to relax normality [see Das et al. (2003)], even though the practical payoff from doing so (balancing for simplicity) is not clear. Hence, before investing resources in relaxing distributional assumptions, my plan is to perform the analysis in the simplest possible way, and then to see if the results are suggestive enough to motivate such an investment. In any case, from a methodological perspective, the message of the paper is not affected by distributional assumptions.
“observables:” \[ E \left[ \xi_{jm} | z_{jm}, y_{jm} \right] = 0 \]; i.e., \( y_{jm} \) can be correlated with \( z_{jm} \) but not with \( \xi_{jm} \).

If selection is on “unobservables” (i.e., \( u_{jm} \) is correlated with \( \xi_{jm} \)), then to satisfy condition (2.6) the researcher needs to make further assumptions along the lines of assumptions (1) and (3). In addition, she needs to augment estimating equation (2.5) with the inverse Mills ratio:

\[
\delta_{jm} = \alpha p_{jm} + \beta x_{jm} + \gamma \lambda \left( \mu_j w_{jm} \right) + \text{error}_{jm}. \quad (2.7)
\]

The researcher does not directly observe the inverse Mills ratio, \( \lambda \left( \mu_j w_{jm} \right) \), so she needs to estimate it. Given assumption (1), estimating in a first-step the probit model (2.4) allows one to compute \( \hat{\lambda}_{jm} = \lambda \left( \hat{\mu}_j w_{jm} \right) \) and to plug it into (2.7).\(^{32}\) Then, in a second-step, equation (2.7) can be estimated by 2SLS on the selected-sample of \( \delta_{jm} \)’s.

Importantly for practical purposes, one can test the hypothesis of “no selection problem,” \( H_0 : \gamma = 0 \), using the usual 2SLS t-statistic for \( \hat{\gamma} \). This is a crucial difference with respect to the existing literature. Even those authors who are extremely clear about the possibility of endogenous sample-selection due to product availability, usually assume the problem away without gathering much supporting evidence.\(^{33}\) My proposed method, in contrast, allows one to test for selection on unobservables in a tractable way.

Equation (2.7) aids in building up intuition with respect to the selection bias. If sample-selection is mistakenly ignored, then the inverse Mills ratio will be an omitted variable. Hence, loosely speaking, the selection bias on \( \left( \hat{\alpha}, \hat{\beta} \right) \) can be expected to be worse, when the correlation between \( \left( p_{jm}, x_{jm} \right) \) and \( \lambda \left( \mu_j w_{jm} \right) \) is higher. If instead, the correlation between the included regressors and the inverse Mills ratio is low, then only the estimated intercept will be affected.

3 Data

I estimate demand model (2.1), (2.4), and (2.7) using three main sources of data.\(^{34}\) Broadly, each source of data contains information about one dimension of the demand model: 2001 English Census and ODPM (2002) (markets), Institute of Grocery Distribution (supermarkets), and Kantar (former TNS) World Panel (individuals). In this section I describe each source of data and how I combine them.

\(^{32}\)This first-step greatly depends on the application and the assumptions the researcher is ready to make about the joint distribution of the unobservables \( u_{jm} \) and \( \xi_{jm} \) across \( j \)’s [i.e., assumption (1)]. If there are many selection equations, it might be sensible to consider them jointly as a system, and then to have a multivariate probit, for example. In this case, also the relationship between the \( u_{jm} \)’s and the \( \xi_{jm} \)’s might be more complex. Because in my application I only have one selection equation, I limit the discussion to the simplest case of independence of the unobservables across \( j \)’s. In any case, these sorts of extensions would not be conceptually too demanding.

\(^{33}\)See, for example, Draganska et al. (2009) and Eizenberg (2012).

\(^{34}\)I kindly thank the Institute for Fiscal Studies for allowing me to use the data described in this paper. Above all, I am particularly grateful to Rachel Griffith.
3.1 Market-Definition and Market-Level Data.

I define a market $m$ as an English town centre (or high street). This decision is motivated by existing research on the topic [Griffith & Harmgart (2008), ODPM (2004), and Schiraldi et al. (2011)] and by the following observations.

Market-definition is particularly important for the selection model (2.4). In my demand model, individuals have preferences for supermarkets’ locations. Hence, retailers may choose where to locate their supermarkets taking into consideration such preferences. In order to control for endogenous sample-selection due to big-box presence, market-definition has to be as close as possible to what retailers actually consider as a “location” in which to open supermarkets. As it will be better explained in section [3.2], I estimate (2.4) with data on entry of supermarkets in the period 1997-2004 (i.e., post-Town Centres First). TCF requires retailers to open their new supermarkets as close as possible to town centres and, since then—willingly or not—the major retailers have indeed been opening their new supermarkets mainly nearby town centres.\(^{35}\)

Data about English town centres and their demographics come from the Office of National Statistics (ONS) and the ODPM and refer to the period 2001-2002. These data also include planning applications related to Town Centre First. Retailers interested in opening a new big-box supermarket have to send an application to the competent Local Planning Authority (LPA). Each LPA, then, decides if to grant or to reject the various applications received. The ODPM data include the number of applications both received and granted by each LPA in the period 1995-2003. Table 1 reports summary statistics of such data.

\begin{table}[h]
\centering
\begin{tabular}{llll}
\hline
 & & & \\
\hline
I have complete data for 753 out of 973 town centres in England and Wales [see Griffith & Harmgart (2008) for more details on these data]. “Density” refers to the number of people per hectare. “Area” is the town centre’s area measured in hectares. “Closest” is the distance, measured in tens of Km, of the town centre to its closest neighbouring town centre. Each town centre belongs to one of 320 Local Planning Authorities; on average a LPA has 2.7 town centres. The LPA which encompasses the highest number of town centres, 16, is Leeds.\(^{36}\) An in-depth description of the “grants” variable can be found in appendix [8.3.1].

\(^{35}\)Since 1996, the entry behaviour of major retailers has dramatically changed: in 1994 only around 25% of Tesco’s new supermarkets were in-town, by 2000 virtually all of them. Similarly, Sainsbury’s passed from 12% to 85% of in-town new openings from 1995 to 1999 [see Cheshire et al. (2012)].

\(^{36}\)London is divided into many different LPAs.
3.2 Alternative-Definition and Supermarket Data

Supermarket data come from the Institute of Grocery Distribution (IGD). The data lists supermarket characteristics of all chain stores and all other large stores, as well as around 80% of independent smaller stores. I use IGD data up to 2004 (the last year these data were collected by IGD). I match the supermarket data with the town centre data via the location information.\footnote{Each supermarket is matched to the closest, in Euclidean terms, town centre.}

Table 2 shows that retailer presence across LPAs is very heterogeneous. The retailer “others” collects the stores that do not belong to any of the retailers reported in the rest of the table. Also, retailers vary greatly in their average floorsize; only the “big four:” Asda, Morrisons, Sainsbury’s, and Tesco typically own supermarkets larger than 30000 ft$^2$. This is the floorsize threshold used in the industry to define a big-box supermarket [for instance, Tesco defines “Superstores” and “Hypermarkets” as having an area larger than 31000 and 64000 ft$^2$, respectively—see Sadun (2011)]. No retailer is present in more than 85% of the LPAs. Figure 1 conveys the same message, but graphically: different markets are served by different groups of retailers.

Discounters and “expensive” retailers lie on opposite tails of the quality (price) spectrum. The “sparse” geographic distribution is suggestive of some preference heterogeneity across Local Planning Authorities. Following Hausman & Leibtag (2007), I define an alternative $j$ as simply “small” or “big.” Small refers to supermarkets (belonging to any retailer) with floorsize smaller than 30000 ft$^2$; big to equal or larger than 30000 ft$^2$. It follows that my empirical definition of product is $(j, m) = (\text{format-size}, \text{town centre})$: big is present in a town centre only if there is, somewhere in its territory, at least one big-box supermarket. Similarly for the presence of small. Notice that, given this definition, the “switch” from $y_{big,tc} = 0$ to $y_{big,tc} = 1$ in model (2.4), only happens when town centre $tc$ passes from not having any big-box to having at least one. It does not make any difference if town centre $tc$ hosts one or ten big-boxes, in either case $y_{big,tc} = 1$.\footnote{It is important to bear this in mind while interpreting the “grants” variable (described in appendix [8.3.1] and summarized in table 1).} Using this definition, in 2002, small supermarkets were present in 100% of the 753 town centres, while big-box supermarkets were present in 58.83% of them. Somehow surprisingly,
even using such an aggregate definition of alternative, choice set heterogeneity across town centres persists. The idea is then to control for the possibility that such pattern of big-box presence correlates with market-specific demands.

Given this definition of alternative, I only have to estimate a binary probit for $j = \text{big}$, given that $j = \text{small}$ is present in 100% of the town centres. Because of assumptions (1) and (3) from section [2.3], in case of many different $j$’s, the task of finding exogenous regressors that explain entry but not preferences requires data I do not have. For instance, if I could link retailers to planning application data, then there would be some hope. Unfortunately, I only observe the aggregate number of applications per Local Planning Authority, per year. Even assuming exogeneity, it would be hard to convincingly identify the heterogeneous entry decisions of different retailers with only one market-specific regressor. Furthermore, model (2.4) is a reduced form for entry decisions. Considering chain retailers separately (as seen above, only the big four open big-boxes) would imply the necessity of thinking of strategic interactions between the various players across markets (i.e., some complex error structure for $u_{jm}$ across both $j$ and $m$). Aggregating all the retailers into one category greatly reduces issues about strategic interactions, making assumptions (1)-(3) easier to digest. Furthermore, the catalyst of the public debate around land use regulations has always been big-boxes versus all the rest. I think the first order priority in terms of consumer welfare is to evaluate big versus small. If then big is by Sainsbury’s or by Morrisons, it is second order. I do not think the extra benefits of separately identifying the behaviour of Asda from that of Tesco is worth the extra costs.

### 3.3 Household-Purchase Data.

Individual-level data about supermarket choices are from the Kantar (formerly TNS) World Panel for years 2001-2004 [see Leicester & Oldfield (2009)]. The data report information about 20708 English households. Households record in which supermarkets they shop and what they purchase. Prices are obtained from till receipts.

For each household, I aggregate over the sample period all the observed shopping expenditures at the $(j, tc)$ level, so to obtain a household-specific expenditure-ranking [see Dubois & Jodar-Rosell (2010)]. This is done for the following reasons.

The sample contains millions of shopping trips. A shopping trip is a household going shopping to a supermarket. It is infeasible to use it as a whole without some “reduction.” One strategy is to draw a random sub-sample of shopping trips, and then to estimate the model over it [see Griffith et al. (2010)]. I do not follow this strategy because many households appear to go shopping systematically both to big-
boxes and to smaller supermarkets. This behaviour is known as two-stop shopping, and it is the object of current research [see Schiraldi et al. (2011)]: on a weekly basis, households do their main shopping in a big supermarket, and then top-up in smaller ones. Given such dynamic behaviour, it would then be problematic to estimate households' preferences only on a subset of their shopping trips. On the other hand, sampling over households, rather than shopping trips (so to get all the shopping trips of a household), would leave open the questions of how to define “main” and “top-up” shopping trips and how to develop an appropriate dynamic model [see Schiraldi et al (2011)].

Given the logit assumption for the choice model, it is possible to handle rankings of choices (i.e., first-best, second-best, etc.) [see Beggs et al. (1981), BLP (2004), and Train & Winston (2007)]. Hence, I create household-specific rankings of expenditures and let the data disentangle main from top-up. The \((j, tc)\) combination where the household is observed spending the highest amount over the sample period is her first-best or main shopping destination. Similarly for the second-best destination and so on. This aggregation alleviates dynamic concerns but also aids the identification of the random coefficients. Indeed, both BLP (2004) and Train & Winston (2007) report that they were unable to identify any random coefficient unless they used data on rankings of choices (i.e., beyond the first-best).

In the computation of the contraction (see equation (8.3) in appendix [8.2]), using “sample” market-shares instead of “true” market shares introduces an additional layer of variability in the estimation procedure (on top of sampling variance and simulation variance) [see Berry et al. (2004), Berry & Pakes (2007), and Gandhi et al. (2013)]. This problem worsens the smaller is the sample of observations over which market shares are computed and the larger is the number of alternatives in the choice set. As in Golbee & Petrin (2004), I must compute market shares from my sample of households. Indeed, I am not aware of the existence of data about “true” supermarket-level market shares in England and Wales. In my application, choice sets are of the order of hundreds; but by increasing the interval of time over which I aggregate, the number of observed shopping trips to each \((j, tc)\) combination gets larger, giving a better approximation of the true market-shares.\(^{39,40}\) Therefore, a “long” aggregation over time is required to

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\(^{39}\) As discussed in section [2.1] and in appendix [8.1], I do not assume households to be stuck in the specific town centre where they live (i.e., I do not exclude from the sample those shopping trips towards different town centres). (See table 3.) Households are assumed to be free to go shopping wherever they like: for example, near their workplace, or somewhere in between their workplace and where they live. As a consequence, the sample of observations over which market shares are computed is the full sample of shopping trips towards every \((j, tc)\) combination.

\(^{40}\) An alternative option with respect to imputing full choice set to each household, would be to create household-specific choice sets based on some distance from their house town centre (e.g., say 20 Km). Even though this seems realistic, it gives rise to a practical problem within the BLP estimation procedure: how should we compute predicted market shares (for contraction (8.3) in appendix [8.2])? The set of households to be used in the computation of the predicted market share of a same \((j, tc)\) combination would have choice probabilities for \((j, tc)\) that might average out to values larger than 1, because of their heterogeneous choice sets. With full choice set, the inclusion of preferences (or disutility) for “distance from home” in the indirect utility of \((j, tc)\) rationalizes, in utility maximization terms, the fact that households seldom go shopping very far from their home town centre.
increase the reliability of my market share measures. Similar observations hold for the construction of price indexes (see appendix [8.3.2] for details about the construction of price indexes).

Table 3 reports summary statistics about household supermarket-choice behaviour. Individual households are observed to choose up to 21 different \((j, tc)\) combinations (only the first 10 are reported in table 3). There is a big gap, in terms of average expenditure, between the main shopping destination and the top-up ones. As mentioned earlier, most households seem to be systematically shopping to many different destinations: more than 50% of households divide their shopping at least among three \((j, tc)\) combinations. “Home town centre” indicates if the specific destinations chosen by the household are located in the same town centre where she lives. Around 40% of the households do their main shopping in town centres different from those where they live.

4 Empirical Strategy

In this section I describe the instruments used to separately identify entry from preferences for big-boxes and the counterfactual exploited to measure consumer benefit of big-box supermarkets.

4.1 Instruments to Entry of Big-Boxes

In the context of my application, (2.4) represents a model for entry of format-size \(j\) in town centre \(tc\). As explained in section [3.2], format-size “small” is present in each of the 753 English town centres (thus, no selection correction is needed for \(j = \text{small}\)). In the estimation of such model, as discussed in section [2.3], it is required some regressors that explain entry of big-boxes but not preferences for them. For this purpose, I exploit the 1996 reform in land use regulation: Town Centre First.

Because I need to explain only the entry of big-boxes, and not that of small supermarkets, I use the variable “grants” (i.e., number of granted applications) in the post-Town Centre First as an instrument (see description in appendix [8.3.1]). In the period covered by my analysis, if a retailer wanted to open a big-box supermarket, it had to apply to the competent Local Planning Authority, and then the request could either be granted or rejected. In case of rejection, contrary to the willingness of the retailer (likely to be correlated with town centre’s preferences for big-boxes), the big-box supermarket would not open. After rejection, if the retailer were still very motivated about entering in the specific town centre, it would
have to open a smaller supermarket. Hence, ideally, a rejection would represent an exogenous shock to entry of big-boxes. Furthermore, because the decision process is decentralized to LPAs, and LPAs are heterogeneous in their “rejection rates” (see table 1), I exploit local variation in the number of granted applications to aid the separate identification of entry from preferences. In appendix [8.4], I discuss potential sources of endogeneity of grants_{tc} and how I address them.

My town centre-specific data (i.e., the regressors w_{big,tc} in model y_{big,tc}) come from period 2001-2002, thus I cannot explain entry of big-boxes too far back in the past. Moreover, my household-purchase data are from period 2001-2004, hence I do not actually need to go too far back in the past. I restrict the original sample of 753 town centres to the 352 in which: (a) there was no entry of any big-box in the period prior to the introduction of Town Centres First and (b) there was some entry (at least a small corner shop) in the post-TCF period (i.e., 1996-2004).

In addition to my inability to explain entry prior to 1996, restrictions (a) and (b) make sure I do not compare “apples with bananas” in the entry equation. Furthermore, focusing on the post-TCF entries highlights the presence of geographical patterns in the diffusion of big-boxes (see figure 2), which in turn can be exploited to separately identify entry from preferences.

![FIGURE 2](image.png)

Intuitively, figure 2 suggests that most of the entry of big-boxes in the post-TCF period happened in the centre-north of the country. Historically, retailers started their geographical expansion from the wealthier south heading north. This can be seen more systematically from table 4.

![TABLE 4](image.png)

This pattern hints to the presence of economies of density in the retail industry, very much along the lines of the geographical expansion of Wal-Mart in the US, as documented by Holmes (2011). I exploit such a pattern in the construction of geographical instruments to entry.

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412001 was the first year in which these data were collected.
42From the reduction of the sample of town centres follows a reduction of the number of households. I need to exclude all those households who were not observed choosing any of the (j, tc)'s combinations still present in the sample. I am left with 10,083 households and preference-rankings of up to 7 (j, tc) combinations.
43(a) makes the grants_{tc} variable “binding” (i.e., before 1996 land use regulation was not as strict) and reduces the level of heterogeneity across the town centres with y_{big,tc} = 1 (i.e., in the full sample, some of the 1's are due to big-boxes developed at the beginning of the 20th century, while others are due to big-boxes developed over a hundred years later). On the other hand, (b) reduces the level of heterogeneity across the town centres with y_{big,tc} = 0: some constantly attract the interest of retailers (i.e., small supermarkets keep entering), while others do not.
Summary statistics of the final sample (of 352 town centres) over which entry model (2.4) is estimated is reported in table 5.

TABLE 5.

“Presence of big-box” is variable $y_{big(tc)}$. The remaining variables are the regressors, $w_{big(tc)}$. “Distance” is the average distance of sampled households to the specific town centre. “Dist. Big” and “Dist. Small” are the geographical instruments to entry mentioned above. I can use such variables as instruments given the focus on the specific sub-sample of the entire data: “Dist. Big” is $tc$’s distance to the closest among the $(big, tc)$’s excluded from the estimating sample, while “Dist. Small” is $tc$’s distance to the closest among the $(small, tc)$’s excluded from the estimating sample. These instruments are inspired by Holmes (2011)’s research: the geographic diffusion of chain retailers (remember that big-boxes in the UK belong only to the big four) is not random, they usually expand “gradually,” radiating from south to north (see table 4) to contiguous areas; always maintaining high store density and proximity to their logistic networks. These variables are assumed to be “cost-side” shocks to the opening of new supermarkets, and therefore exogenous to town centre’s preferences.44

4.2 Measuring Consumer Benefit: Observed Counterfactual

The primary objective of this paper is to measure the consumer benefit of big-box supermarkets. Welfare analysis typically requires the formulation of counterfactuals, in my case something like the entry of big-box supermarkets in town centres where none is present. Instead of attempting the formulation of potentially surrealistic counterfactuals, I exploit those readily available in my data: from 2002 to 2004 I observe in 17 of the 304 town centres previously with no big-boxes, the opening of new big-box supermarkets. In other words, I measure consumer welfare of big-box supermarkets by exploiting observed entry (as opposed to simulated entry) of big-boxes in town centres in which that format was previously absent.

I do so mainly for two reasons. The first was pointed out by Nevo (2011, section 6.1). The counterfactual I need, in order to measure consumer welfare, is a change to households’ choice sets (which, in my case, is a collection of (format-size, town centre) combinations). My demand model assumes logit errors, and these have been criticized as inappropriate for such a task [see Petrin (2002), Berry et al.]

44Along the same lines of these “cost-side” shocks, if I had data on the distribution networks of the big four, I could estimate a model with multiple entry equations, one for each retailer. Unfortunately, I do not have such data and must limit my analysis to a single entry equation model. 
The second reason relates to the complexity of the adjustments following the entry of new big-boxes. A model of strategic interactions among retailers would unfold over many dimensions (e.g., where to open the new store, how big it should be, what ranges of products it should offer, at which prices, etc.) and require strong and potentially unrealistic assumptions to be implemented.

As Nevo (2011) shows, even with a simple logit model (i.e., no random coefficients), it is possible to correctly evaluate the welfare effects of a changing choice set. This on the premises of having good measures of the market-shares pre and of the market-shares post-change. Usually, researchers only have one set of market-shares: either pre or post-change; and then “simulate” the remaining set with their estimated model. It is in the market-share simulation step that the logit model becomes inappropriate (the blue-bus, red-bus case is an example). Then, given wrong simulated market-shares, wrong welfare computations follow. On the other hand, if in the first place the logit model is “fed” with the correct market-shares pre and after choice set change, then the welfare calculations will be correct.

The period covered by my sample, 2001-2004, was a period of great activity from the part of retailers. Many of them were expanding. In my sample, in the period 2003-2004, 17 town centres passed from having only small supermarket formats to hosting big-boxes. What are the welfare implications of such entries for households? By observing market-shares pre-entry (i.e., 2001-2002 data) and market-shares post-entry (i.e., 2003-2004 data), I compute—following Nevo (2011)—two separate sets of δ’s: δ′02 and δ′04. Moreover, all the features of the 2003-2004 scenario are observed (e.g., where the new supermarkets are, their size, new equilibrium prices, etc.) so that the “regressors” used to describe the counterfactual do not rely on further assumptions (required in the case of simulated counterfactuals).

5 Estimation Results

In this section I present results from the estimation of the household-specific indirect utility model (2.1), the big-box entry model (2.4), and the market-level indirect utility model (2.7). The majority of the section is devoted to the discussion of the consequences of endogenous sample-selection due to big-box presence on the estimation of model (2.7).

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45I thank Tim Conley and David Rivers for the insightful discussions on this issue.
46I thank Rachel Griffith for addressing me in this direction.
5.1 Computation of Market-Specific Utility Shifters

The first step in the procedure outlined in section [2.3] and appendix [8.2] is to obtain an estimate of $\delta^{01-02}$ and $\delta^{03-04}$. Here I summarize the results.

The utility specification I estimate via maximum likelihood (plus contraction mapping) is:

$$U^t_{i,j,tc} = \delta^t_{j,tc} + \theta \text{distance}^t_{i,tc} + \varepsilon^t_{i,j,tc},$$

where $\text{distance}^t_{i,tc}$ is the Euclidean distance (in 100 Km) of household $i$’s home from town centre $tc$ in period $t$ (i.e., 2001-2002 or 2003-2004). It is the basis used for the computation of the average distance of households from a town centre (see table 5).

The vectors of market-specific utility shifters, $\delta^{01-02}$ and $\delta^{03-04}$, have 391 and 408 elements (i.e., the size of the choice sets), respectively, in 2001-2002 and 2003-2004. As discussed in section [4.2], my empirical strategy for measuring consumer benefit of big-boxes is to exploit actual entry of big-boxes in 2003-2004 in town centres where there was none up to 2002. Since this involves an intertemporal comparison of welfare, I address the question from the perspective of the most recent measure of preferences: I estimate $\theta$ from the 2003-2004 data and then use such an estimate, $\hat{\theta}$, to compare welfare in 2001-2002 and 2003-2004 [see Fisher & Shell (1972)].

The estimate of $\theta$ is $\hat{\theta} = -8.118$ and its standard error is 0.0634. Parameter $\theta$ may be intuitively interpreted as the disutility household $i$ gets by marginally “moving her house away” from town centre $tc$. Since other distance variables enter the specification of $\delta^t_{j,tc}$, this interpretation of parameter $\theta$ is just partial; a more complete description is presented after the estimation of the $(\alpha, \beta)$ parameters.

Figure 3 summarizes the $\delta^{01-02}$ and $\delta^{03-04}$ estimates obtained with contraction (8.3).

[FIGURE 3]

Figure 3 is useful to build up intuition about what to expect from the regressions for the estimation of the preference parameters $(\alpha, \beta)$: $\delta^t_{j,tc}$, indeed, is the “dependent variable.” Both $\delta^t_{\text{big},tc}$ and $\delta^t_{\text{small},tc}$ sensibly increased over time, with the market-specific utility of small supermarkets increasing more than that of big-boxes. For identification purposes, one of the $\delta^*_{j,tc}$’s has to be normalized to zero: the numeraire. This is equivalent to subtracting $\delta^*_{\text{numeraire}}$ from all the other $\delta^*_{j,tc}$’s (i.e. $\delta^t_{j,tc} = \delta^*_{j,tc} - \delta^*_{\text{numeraire}}$). As discussed by Nevo (2003), a positive time trend in $\delta^*_{j,tc}$
inclusion of separate time trends in the specification for $\delta^t_{j,t}$\textsuperscript{49} The comparison of various specifications with and without such time trends is examined in appendix [8.5.1].

5.2 Model of Big-Box Entry

Table 6 reports the estimates of the marginal effects of the binary probit model (2.4) estimated using the set of regressors summarized in table 5. Regressors $w_{big,tc}$ are evaluated at their means. Standard errors are bootstrapped and clustered at the Local Planning Authority level (2000 repetitions).

\[\text{TABLE 6}\].

Column (i) lists the estimates of the probit model without the regressor grants\textsubscript{tc}, column (ii) reports the results of the probit model with grants\textsubscript{tc}. In the third column the significance of the difference between the point estimates is tested. Given the worries of endogeneity of the grants\textsubscript{tc} variable (for a detailed discussion, see appendix [8.4]), the idea is to check the robustness of the results using alternative identifying assumptions. The inclusion of the grants\textsubscript{tc} variable does not significantly affect the remaining point estimates, but it increases their precision (see footnote [54] for more on this point).

The fit of the model is good (Pseudo-$R^2 = 0.267$) and most of the marginal effects have the expected, intuitive sign. For instance, a higher number of granted applications by the LPA in the previous two years is associated with a higher probability of entry of a big-box supermarket. Interestingly, distance from the closest pre-TCF big-box supermarket is less explicative than distance from the closest pre-TCF small supermarket. The more populated a town centre, the higher the chance of having a big-box supermarket in the neighbourhood; but the more densely populated, the lower the chance of big-box entry. Big-box supermarkets are more likely to be present further away from the centers where households live.

\textsuperscript{49} Given stable preferences over $t$, a positive trend in $E_{tc} \left[ \delta^t_{j,t} \right]$ can be explained in terms of a favourable change in either observable (i.e., $x^t_{j,t}$) or unobservable (i.e., $\xi^t_{j,t}$) characteristics (or both). This has important consequences for intertemporal comparisons of welfare: if all the $\delta^t_{j,t}$'s stay constant and simply $\delta^t_{numeraire,t}$ decreases, then households might actually be worse-off in 2004 with respect to 2002; despite the positive time trend in $\delta_{j,t}$. It is not possible to reject with certainty this hypothesis. On the other hand, I make a conservative choice of the numeraire so to suggest the opposite story: the positive time trend in $\delta_{j,t}$ is due to an increase in $\delta^t_{j,t}$ rather than a decrease in $\delta^t_{numeraire,t}$. I chose the numeraire to be (small, Tonsbridge): its market share was the smallest in 2002, and it actually increased by 287.21% while the average market share of “small” decreased by 5.85% (and, by construction, that of “big” increased).

49 Given stable preferences over $t$, a positive trend in $E_{tc} \left[ \delta^t_{j,t} \right]$ can be explained in terms of a favourable change in either observable (i.e., $x^t_{j,t}$) or unobservable (i.e., $\xi^t_{j,t}$) characteristics (or both). In other words, any trend in $E_{tc} \left[ \delta^t_{j,t} \right]$ can be due to either observed or unobserved changes in quality (or anything in between) [see Nevo (2003)]. In performing intertemporal comparisons of welfare, it is then important to account for the welfare effects induced by systematic changes in $\xi^t_{j,t}$ (i.e., unobserved quality). One way to capture time trends due to changes in $E_{tc} \left[ \delta^t_{j,t} \right]$ is to include in the specification for $\delta_{j,t}$ time dummies interacted with supermarket-formats.
In what follows, the estimates reported in table 6 are used to construct inverse Mills ratios so to account for possible endogenous sample-selection due to big-box presence as outlined in section [2.3].

5.3 Selection Bias due to Big-Box Presence: Part I

The specification for \( \delta_{t,j,tc} \) presented in this section does not include time trends for big-boxes and small supermarkets. In section [5.4] I present results for the alternative specification of \( \delta_{t,j,tc} \) which does include such time trends.

I estimate model (2.7) for \( \delta_{t,j,tc} \) in two steps. In the first step, I estimate the entry model (2.4) twice: once on the 2002 sample and once on the 2004 sample (i.e., results reported in table 6). Given such estimates, I compute the inverse Mills ratio \( \lambda \left( \hat{\mu}_{big}, \hat{w}_{big,tc} \right), t = 2002 \text{ and } 2004 \). The standard errors of \( \left( \hat{\alpha}, \hat{\beta}, \hat{\gamma} \right) \) are bootstrapped to account for the replacement of \( \mu_{big} = \left( \mu_{02, big}, \mu_{04, big} \right) \) with its estimate.\(^{50}\)

For each town centre \( tc \) there can be up to four observations: \( \delta^{02}_{small,tc}, \delta^{04}_{small,tc}, \delta^{02}_{big,tc} \) and \( \delta^{04}_{big,tc} \). It is likely that these four market-level utilities are somehow correlated within each town centre. I control for this possibility in two ways. First, I exploit such correlation structure to improve the efficiency of the estimator (i.e., FGLS estimator). Second, in the computation of the standard errors: within each bootstrap repetition, one “draw” from the original sample is a town centre. This is equivalent to cluster the standard errors at the town centre level. Table 7 summarizes the results.

\[ TABLE 7 \]

On the top panel of table 7, column (i) lists the estimates of \( (\alpha, \beta) \) obtained, as it is customary, without controlling for endogenous sample-selection due to big-box presence. Column (ii), on the other hand, lists the estimates of \( (\alpha, \beta, \gamma) \) obtained by controlling for the endogeneity of big-box presence to preferences. In the third column, it is tested if the point estimates from columns (i) and (ii) differ significantly.

The inverse Mills ratio has a significant positive coefficient, so there is positive correlation between the unobservable of big-box entry, \( u_{big,tc'} \), and the unobservable portion of preferences for big-boxes, \( \xi_{big,tc'} \). Big-boxes tend to be present in those places where people like them more: in those town centres where there are big-boxes, the \( \xi_{big,tc'} \)'s are relatively “higher” (i.e., the selected sample of \( \delta_{big,tc,t} \)'s has a higher average than the population). By estimating \( \delta_{j,tc,t} = \alpha p_{j,tc} + \beta x_{j,tc} + \xi_{j,tc} \) on the selected sample of town centres \( \{ \delta_{j,tc,t} \}_{j,tc,t=1}^{813} \) —without controlling for selection—, we are bound to get \( \sum_{tc,t=1}^{704} \left[ \left( \hat{\alpha} p_{big,tc} + \hat{\beta} x_{big,tc} \right) \right] \).

\(^{50}\)Bootstrap estimates are performed over 2000 repetitions (i.e., the two-step sequence of the estimation procedure, first \( p_{big} \) and then \( (\alpha, \beta, \gamma) \), is repeated 2000 times).
− \left( \alpha_{\text{big},tc} + \beta x_{\text{big},tc} \right) = \text{Bias} > 0.51 \text{ In other words, without controlling for selection we would expect estimated preferences for big-box characteristics to be more positive, for positive coefficients, and less negative, for negative coefficients (with respect to the population values).}

This is, indeed, what the third column of table 7 shows. To see how such selection bias can distort our economic inference, the bottom panel of table 7 reports averages of the marginal disutility of distance for both big-box and small supermarkets. Comparing columns (i) and (ii) confirms the expected direction of the bias: the disutility of big-box distance is estimated from a sample of households who do not mind travelling to big-boxes as much as the population at large, hence the estimate is less negative than the "true" one. Interestingly, even though all the point estimates are significantly affected by selection bias, the disutility of distance for small supermarkets is, on average, almost unaffected. Figure 4 sheds further light on these points.

\[ \text{FIGURE 4] .} \]

The selection bias on the marginal disutility of distance grows with floorsize: it is around 11.4% for 8000 \( m^2 \) (i.e., average floorsize for a small supermarket) and 49% for 40000 \( m^2 \) (i.e., average floorsize for a big-box). This makes intuitive sense. Since there is no sample-selection due to small supermarket presence (i.e., I observe \( \delta_{\text{small},tc,t}^* \) in every \( tc \) and \( t \)), one would expect the estimated preferences for small supermarkets not to be affected (at least not as much as those for big-boxes) by selection corrections due to big-box presence. Moreover, households find it more costly to go shopping to big-boxes rather than to small supermarkets.\footnote{Supermarket-format \( j \) is either big-box or small, there are 352 town centres, and two time periods (2001-2002 and 2003-2004). It follows that the population of \( \delta_{j,tc,t}^* \)'s has \( J \cdot TC \cdot T = 1408 \) elements: there is a \( \delta_{j,tc,t}^* \) for both big-box and small supermarkets for each town centre in every time period. On the other hand, the population of market-specific utility shifters for big-boxes, the \( \delta_{\text{big},tc,t}^* \)'s, has \( TC \cdot T = 704 \) elements.}

\footnote{In the current section, the marginal disutility of distance for supermarket-format \( j \) in town centre \( tc \) is loosely computed as \( MU_{j,tc}^{dist} = \frac{\partial \delta_{j,tc}^*}{\partial \text{dist}} \). More precisely, \( MU_{j,tc}^{dist} \) should be computed as \( \frac{\partial \delta_{j,tc}^*}{\partial \text{dist}} + \theta \). I ignore the \( \theta \) term, for the time being, because it is constant across observations and not affected by endogenous sample-selection due to big-box presence. The averages of \( MU_{j,tc}^{dist} \) reported in the bottom panel of table 7, for both big-box and small supermarkets, are computed across the 65 town centres where big-boxes were present by the end of 2004.}

\footnote{Big-boxes are located in remote places. On the other hand, small supermarkets are located in high streets densely populated of restaurants, bars, shops, offices, etc. Going shopping to small supermarkets can be done "in between" other activities (e.g., coffee with friends, going to work, going to the hairdresser, buying a pair of shoes, eating out, etc.) or, more in general, it takes place in a "nicer" surrounding. Going to a big-box supermarket, instead, is a "solo" task which requires to drive to some abandoned place, far from everything else. It is harder to combine big-box shopping with other pleasant activities, and the surrounding is usually saddening (e.g., parking lots, highways, gas stations, etc.). More succinctly: going to small supermarkets is a complement to doing other "nice things," while going to big-boxes is a substitute for these other "nice things." In addition, even if the household is not interested in further activities and she just needs to buy grocery, buying it in high street might be more pleasant than buying it in the middle of nowhere. The opportunity cost of travelling to small supermarkets is lower than that to big-boxes. Households seem to be willing to travel more for small supermarkets because they do other things along the way.}
Overall, these results suggest that households care about travelling away from high streets—in order to visit big-boxes—more than we would conclude without controlling for selection.

5.4 Selection Bias due to Big-Box Presence: Part II

In this section I present results for the specification of $\delta_{j,t,c}$ which includes time trends for big-box and small supermarkets. As discussed after figure 3 and in appendix [8.5.1], because my aim is the intertemporal comparison of consumer welfare, my favourite specification for $\delta_{j,t,c}$ is the one studied in the current section. Table 8 reports the estimation results.

[TABLE 8].

Column (i) refers to the estimation of model (2.3) for $\delta_{j,t,c}$ without selection correction. As observed in appendix [8.5.1], by including time trends for big-box and small supermarkets, the price index loses all its explanatory power; hence I do not perform any instrumental variable estimation. Each of the time trends (i.e., small in 2004, big in 2002, and big in 2004) is measured relatively to “small in 2002.” For example, “big in 2002” can be interpreted as $\xi_{big}^{02} - \xi_{small}^{02}$, where $\xi_{j,t,c}$ is shown in the Etc $[\xi_{j,t,c}]$. Thus, column (i) suggests that the unobserved qualities of big-boxes and of small supermarkets were approximately similar in 2001-2002 (i.e., observed characteristics explain differences in average utility between big-box and small supermarkets pretty well); while in 2003-2004 both big-box and small supermarkets improved their unobserved quality relatively to 2001-2002. Specifically, in the 2002-2004 period, big-boxes improved their unobserved quality twice as much as small supermarkets.

Column (ii) refers to the estimation of model (2.3) for $\delta_{j,t,c}$ controlling for big-box presence. The estimated coefficient of the inverse Mills ratio, $\hat{\gamma}$, is significant at the 1% and has the expected sign: big-boxes tend to be located in town centres where households like them better. Comparing these results with those from table 7, it can be noticed how the time trends “absorbe” most of the selection bias (i.e., the slope coefficients are not affected in table 8). This suggests, as discussed in section [2.3], that the inverse Mills ratio is not highly correlated with big-box observed characteristics. Unfortunately, if the researcher’s aim is the computation of welfare, this is not enough to ignore selection on unobservables in demand estimation (as shown in section [6.1]).

Controlling for big-box presence, the supposed unobserved quality “premium” of big-boxes in 2003-

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54 As a robustness check, I performed the estimation excluding the grants, $t_c$ variable (which might be considered problematic, see discussion in appendix [8.4]): practically, point estimates do not change. Importantly, though, $\gamma$ is estimated with less precision (i.e., p-value=0.017).
2004 disappears: only small supermarkets experience an increase in unobserved quality from 2001-2002 to 2003-2004. This conforms with the expected upward bias in the estimation of big-box preferences discussed in section [5.3]. From an economic perspective, this story is in line with the results of Griffith & Harmgart (2008) and the changes in entry strategy of the major chain retailers discussed in footnote [35]: the fact that retailers are not “pushing” big-boxes as much as in the past, but are rather concentrating towards smaller formats, might not just be the consequence of stringent planning regulations, but actually a matter of households’ preferences.55

Similarly to the results from table 7, the results from table 8 suggest that not controlling for big-box presence leads to an upward bias in the estimation of big-box preferences. How this upward bias maps into consumer welfare is studied in the next section.

6 Welfare Analysis

In this section I evaluate the changes in households’ expected benefits of grocery shopping from 2001-2002 to 2003-2004 using the estimates from table 8. I compare the results obtained by controlling for and by ignoring endogenous sample-selection due to big-box presence. In section [6.1], I decompose the total changes in expected benefits into various components and isolate the part directly due to big-boxes. By “direct” I mean: ignoring potential spillovers that big-boxes may have, for instance, on smaller supermarkets located in their proximity.56 Differently, in section [6.2] I present some evidence of the indirect benefits potentially generated by big-boxes.

6.1 Expected Direct Benefits of Big-Boxes

Following Small & Rosen (1981), I compute household i’s expected welfare of grocery shopping in period t as:

\[
W_{el} \left( X_t^i, \hat{\text{big}}_t^i, \hat{\text{small}}_t^i, CS_t \mid \hat{\theta}, \hat{\alpha}, \hat{\beta} \right) = \ln \left[ \sum_{(j,t,c) \in CS_t} \exp \left( \hat{j} + \hat{\alpha} \hat{p}_{j,t,c}^i + \hat{\beta} \hat{x}_{j,t,c}^i + \hat{\theta} \text{distance}_{i,t,c}^i \right) \right],
\]

55Terry Leahy, CEO of Tesco between 1997 and 2010, confirms this idea: “Express stores have been introduced into every country where Tesco operates.” Hence, not only in countries with land use regulations. “Highly profitable, the convenience format is the fastest growing after e-commerce. As societies become more urban, and people the world over lead busier lives, the format is bound to expand.” [See page 225 of Leahy (2012).]

56These spillovers are studied by, for example: Gould et al. (2005), Hausman & Leibtag (2007), Matza (2011), Sadun (2011), and Schiraldi et al. (2011).
where $X_i^t = \left( p_{i,j,t}^t, x_{i,j,t}^t, d_{i,j,t}^t \right)_{(j,t,c) = 1}^{2.352.2}$, $\widehat{p}$ is the estimated time trend for either big-boxes (i.e., $\widehat{big}_t$) or small supermarkets (i.e., $\widehat{small}_t$), $C_{j}^{t}$ is the set of available $(j, tc)$ combinations in period $t$. The estimate $\hat{\theta}$ is not affected by endogenous sample-selection, therefore I omit it from $W_{i}^{t}$ for notational simplicity. The (total) change in household $i$’s expected welfare from 2001-2002 to 2003-2004 is then computed as:

$$\Delta Wel_i = Wel \left( X_{i}^{04}, \widehat{big}_{i}^{04}, \widehat{small}_{i}^{04}, C_{i}^{04} \mid \hat{a}, \hat{b} \right) - Wel \left( X_{i}^{02}, 0, 0, C_{i}^{02} \mid \hat{a}, \hat{b} \right). \quad (6.1)$$

As discussed in section [4.2], the computation of $\Delta Wel_i$ does not involve any simulation of counterfactuals: the estimated preference parameter $\hat{\theta}$ comes from the 2003-2004 period; the $\delta_{j,tc}^{02}$’s and $\delta_{j,tc}^{04}$’s come from the observed market shares of all the $(j, tc)$ combinations given $\hat{\theta}$; $X_{i}^{02}$, $X_{i}^{04}$ and $C_{i}^{02}$, $C_{i}^{04}$ are the “actually” observed characteristics and the sets of available $(j, tc)$ combinations in, respectively, 2001-2002 and 2003-2004. As argued by Nevo (2011), logit demand models require observed market outcomes—as opposed to simulated ones—in order to produce correct welfare measures.58

Not all the intertemporal difference in welfare, as measured by (6.1), can be attributed to big-boxes. I decompose (6.1) into four components: the entry of new big-boxes, the increased unobserved quality of existing big-boxes, the increased unobserved quality of small supermarkets, and the ameliorated observable characteristics. For example, the portion of $\Delta Wel_i$ due to the entry of new big-boxes is measured as:59

$$Wel \left( X_{i}^{04}, \widehat{big}_{i}^{04}, \widehat{small}_{i}^{04}, C_{i}^{04} \mid \hat{a}, \hat{b} \right) - Wel \left( X_{i}^{04}, 0, \widehat{small}_{i}^{04}, C_{i}^{02} \mid \hat{a}, \hat{b} \right), \quad (6.2)$$

while the portion of $\Delta Wel_i$ due to the increased unobserved quality of existing big-boxes is measured as:

$$Wel \left( X_{i}^{04}, \widehat{big}_{i}^{04}, \widehat{small}_{i}^{04}, C_{i}^{04} \mid \hat{a}, \hat{b} \right) - Wel \left( X_{i}^{04}, \widehat{big}_{i}^{04}, \widehat{small}_{i}^{04}, C_{i}^{02} \mid \hat{a}, \hat{b} \right), \quad (6.3)$$

and similarly for the two remaining portions of $\Delta Wel_i$. The part of $\Delta Wel_i$ I attribute to big-boxes is the sum of (6.2) and (6.3). This is what I call household $i$’s “direct” expected benefit of going shopping to

57As discussed in section [5.4], these intercepts capture the average value (across town centres) of unobserved quality of format $j$ in period $t$ relatively to that of small supermarkets in 2002. Because $\widehat{big}_{2002}$ is never significantly different from zero (see table 8), I assume $\widehat{big}_{2002} = 0$ (i.e., in 2002 big-box and small supermarkets had similar levels of unobserved quality).

58Because logit models do not usually deliver realistic estimates of substitution patterns (i.e., IIA property), researchers should not rely on them to simulate counterfactuals to be used for welfare computations.

59The only difference between $C_{i}^{02}$ and $C_{i}^{04}$ is the “inclusion” of 17 new $(big, tc)$ combinations: we observe the development of at least one big-box in 17 of the 304 town centres in which, by the end of 2002, there was none.
big-box supermarkets. Table 9 reports mean and median of the distribution of $\Delta \frac{ Wel_i }{ Wel_i }$ (first row) and its decomposition (second to fifth rows) across the 4399 households whose shopping behaviours are observed both in 2001-2002 and in 2003-2004.

The welfare computations are repeated twice: the left panel of table 9 (i.e., first two columns) uses the $(\hat{\alpha}, \hat{\beta})$ estimated without controlling for big-box presence (column (i) of table 8), while the right panel (i.e., last two columns) uses the $(\hat{\alpha}, \hat{\beta})$ estimated controlling for selection on unobservables (column (ii) of table 8). The importance of controlling for endogenous sample-selection due to big-box presence can be inferred from comparing the left and the right panels of table 9.

Without controlling for selection we would conclude that, for a randomly selected household from the population, the expected benefit of grocery shopping increased by 139% from 2001-2002 to 2003-2004. Of this increase, 53.8 percentage points are attributable to big-boxes: 45.7 are due to the opening of big-boxes in town centres where none was present up to 2002 (i.e., this is equivalent to a choice set enlargement of 4.25%), and 8.1 are due to improvements in big-boxes’ unobserved quality in those town centres where at least one big-box was present prior to 2003. The third column of table 9, on the other hand, tells quite a different story: the expected benefit of grocery shopping increased “only” by 47.8% from 2001-2002 to 2003-2004. Furthermore, the part of such increase which is attributable to big-boxes is just 7%, and it is solely due to the opening of big-boxes in town centres where none was present up to 2002. Indeed, as can be seen from column (ii) of table 8, $\hat{big}_{04}$ is not significantly different from zero when we control for endogenous selection due to big-box presence. Figure 5 plots, at a more disaggregate level, the information summarized in table 9.

As shown in sections [5.3] and [5.4], retailers locate their big-box supermarkets in those town centres

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60 This is a conservative lower bound. Indeed, also a share of the $\Delta Wel_i$ due to ameliorated observable characteristics could be included among the “direct” benefits of big-boxes (e.g., lower prices in 2004 with respect to 2002). I do not do that because, in practice—as can be seen from table 9—the characteristics I observe do not explain much of $\Delta Wel_i$ (i.e., < 4%). Most of the action is in terms of unobserved quality. This shows the challenge of characterizing with a bunch of observables the decisional process that lead households to choose among (supermarket-format, town centre)’s, and stresses the need to include the $j^t$ dummies to “extract” as much unobserved quality as possible from $\hat{\xi}^t_0$ (see discussion in appendix [8.5.1]).

61 Improvements in unobserved quality can arise for two reasons. First, if the stock of big-boxes were constant, then it could be that existing big-boxes were improved in dimensions not captured by my observable characteristics (e.g., better ranges of products, new or improved meat and fish counters, improved cleanliness, better trained and experienced staff, etc.). Second, if the composition of the stock of big-boxes changed, then it could be that old and lower quality stores were replaced by new and higher quality ones. In practice, we usually have a combination of the two.
where households like them better. The sample of revealed preferences we get from those households who actually go shopping to big-boxes is endogenously selected and, consequently, demand is estimated only from its “high end.” As table 9 and figure 5 suggest, this directly translates into inflated welfare estimates: not controlling for big-box presence mistakenly induces researchers to believe that big-boxes are valued very highly by the population at large, and not just by their most loyal customers. Furthermore, the largest part of increase in consumer welfare seems to be due to improved unobserved quality of small supermarkets.\textsuperscript{62, 63}

Table 9 reports welfare measures for a randomly selected household from the population. Table 10, instead, reports welfare measures for two separate groups of households: “near new BB” collects the 504 households living in a Local Authority for which at least one town centre with no big-boxes until 2002 experienced entry of big-boxes in 2003-2004; “far from new BB” collects the remaining 3895 households.

\textbf{[TABLE 10]}

Table 10 supports the idea that households are differentially affected by the opening of new big-boxes: the closer, the better. Households face transportation costs. Thus, in order for a household to benefit from a new big-box, it is not enough that such a big-box opens “somewhere” in the country; it must be available within the household’s reach [see Figurelli (2012)]. For those households who experience entry of new big-boxes, welfare implications drastically change when controlling for sample-selection due to big-box presence. Without controlling for big-box presence, the increase in consumer welfare due to big-boxes relative to that due to unobserved quality of small supermarkets amounts to 1.08 (i.e., \(\frac{387.8}{358.8}\)); while controlling for big-box presence, the ratio falls to 0.28 (i.e., \(\frac{55.4}{198.4}\)). Figure 6 plots, at a more disaggregate level, the information summarized in table 10.

\textbf{[FIGURE 6]}.

\textsuperscript{62}Some of the reasons behind such an increase are studied in section [6.2].
\textsuperscript{63}What I have been calling “unobserved quality,” in reality, can be a more complex object [see Nevo (2003)]. Its components are outside the model, hence the difficulty in pinning them down precisely. A positive small\textsubscript{04}, beyond an increase in unobserved quality, could be motivated by an evolution in the preferences for the unobserved characteristics (rather than by an improvement in the unobserved characteristics) or by a higher income (wealthier households might substitute away from the cheaper big-boxes towards the more expensive small supermarkets on top of any quality consideration). Any apparent change in welfare due to a change in preferences should be disregarded [see Fisher & Shell (1972)]. Because many of the households in my sample are present both in 2001-2002 and in 2003-2004 and the time interval under consideration is relatively short (i.e., two years), I assume changes in \(\xi_t\) are not due to changes in preferences. If households were sensibly wealthier in 2003-2004 with respect to 2001-2002, then the interpretation of the welfare improvements associated to a positive small\textsubscript{04} could be quite different: in the extreme case, not at all due to improvements in unobserved quality. I do not directly observe households’ income in the data, but I do have some indirect measure of wealth. The distribution of cats, dogs, televisions, cars, toilets, and gardens among households does not sensibly change over time (in my sample); hence, I rule out drastic income increases between 2002 and 2004.
Figure 7 compares the average changes in expected consumer welfare of disadvantaged households with the rest of the sample. All measures are obtained from the estimator which controls for big-box presence. Relatively to car owners, households who do not own a car obtain less benefit from the opening of new big-boxes in their Local Authority. Interestingly, this result goes beyond the benefit of big-boxes: the increase in the total welfare of households without a car is actually lower. This is important also in relation to the findings from section [6.2]: whatever the consumer benefits generated by big-boxes (direct and/or indirect), households without a car are less able to enjoy them. A similar pattern is even more pronounced for single parents and single pensioners. Crucially, preferences for big-boxes are not estimated accounting for such demographics, the only way they can affect welfare measures is by proxying for “distance from supermarkets:” disadvantaged households tend to live in deprived areas (not attractive to retailers) and have to travel further away from home for their grocery shopping.  

In addition to the comments above, the right panel of table 10 (i.e, that reporting the welfare measures estimated accounting for selection) shows another interesting pattern: households living nearby new big-boxes experience an increase in welfare, from 2001-2002 to 2003-2004, which is sensibly higher than for those living elsewhere in each of its three components, and not just in the portion directly attributable to the entry of new big-boxes. This is not an obvious finding. The changes in welfare captured by “small stores” and “observables” (from both tables 9 and 10) exclude, by construction, anything directly related to the new (big, tc) combinations.

In the next section I explore the possibility that some of the increase in welfare attributed to “small stores” and to “observables” could actually be—indirectly—due to the entry of new big-boxes.

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64 Figure 7 constitutes a lower bound on the level of welfare inequality between disadvantaged households and the rest of the sample. In fact, not only do they live in badly served areas, but their marginal transportation costs are also likely to be higher (i.e., the cost per km). This is how I interpret the results on car ownership from figure 7: among the households living nearby new big-boxes, those without car live further away from the big-boxes (captured in the figure); but not having a car makes it also harder for them to reach—the already further—big-boxes (not captured in the figure).

65 The portion of $\Delta Wel_i$ due to the increased unobserved quality of small supermarkets is measured as:

$$ Wel\left( X^{04}_{i}, 0, small^{04}, CS^{02} | \hat{\alpha}, \hat{\beta} \right) - Wel\left( X^{04}_{i}, 0, 0, CS^{02} | \hat{\alpha}, \hat{\beta} \right) , $$

while the portion of $\Delta Wel_i$ due to the ameliorated observed characteristics is measured as:

$$ Wel\left( X^{04}_{i}, 0, 0, CS^{02} | \hat{\alpha}, \hat{\beta} \right) - Wel\left( X^{02}_{i}, 0, 0, CS^{02} | \hat{\alpha}, \hat{\beta} \right) . $$

Both the measures are evaluated in $CS^{02}$, hence they refer to changes happened from 2001-2002 to 2003-2004 to the set of $(j, tc)$ combinations already available in 2001-2002.
6.2 Expected Indirect Benefits of Big-Boxes

Recent empirical studies make strong arguments that big-boxes “impact” the surrounding small supermarkets, and that the benefits associated to big-boxes might extend into improvements in the quality of the shopping experience at small supermarkets. The results reported in table 10 suggest it is worth exploring such a possibility: on the one hand, most of the increase in expected household welfare from 2001-2002 to 2003-2004 seems to be due to improvements in unobserved quality of small supermarkets, on the other, such improvements appear to be more pronounced for households living nearby places where new big-boxes opened in 2003-2004.

In order to investigate if big-boxes induce an improvement in the indirect utility households associate to nearby small supermarkets, I use a differences-in-differences approach. I start by studying outcome variable $\delta_{small,tc}^t$, the market-specific utility shifter for small supermarkets (i.e., in this section, the $\delta_{big,tc}^t$’s are not part of the analysis). I restrict the attention to the 304 town centres where there were no big-boxes until 2002, and then consider as “treated” that subset of town centres belonging to a Local Authority for which at least one town centre experienced entry of big-boxes in 2003-2004 (i.e., the same definition used in table 10). The regression used to evaluate the effect of big-boxes on the market-specific indirect utility of neighbouring small supermarkets is:

$$\delta_{small,tc}^t = \mu + \theta \text{ treatment} + \tau \text{ post} + \rho (\text{treatment} \cdot \text{post}) + \sigma \text{ controls}_{small,tc}^t + \epsilon_{small,tc}^t. \quad (6.4)$$

The pre-treatment period is $t = 2001-2002$ (i.e., post=0), while the post-treatment period is $t = 2003-2004$ (i.e., post=1). All the observable characteristics used in the estimation of the welfare measures from section [6.1] are included in the regression. This makes sure that any positive estimate of $\tau$—the time trend—can be interpreted as a generalized increase in the unobserved quality of small supermarkets (i.e., nothing to do with big-box entry), while a positive estimate of $\rho$—the treatment effect—can be interpreted as an increase in unobserved quality due to big-box entry. As it is customary in differences-in-differences regressions, the key identifying assumption relates to the “counterfactual” time trend of the treated town centres: I assume that the $\delta_{small,tc}^t$’s of the treated town centres, had they not experienced any entry of big-boxes, would have had the same time trend observed in the $\delta_{small,tc}^t$’s of the control town centres. The counterfactual

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66 Gould et al. (2005) study the positive externalities in terms of “traffic” generated by the presence of nearby stores in the US; Hausman & Leibtag (2007) the lower prices induced by big-box entry on nearby supermarkets because of the higher competitive pressure; Matsa (2011) the quality and price improvements induced by the entry of Walmart on other stores in the US; Sadun (2011) the effects of chain retailers’ entry on the unemployment rates of independent stores in the UK; Schiraldi et al. (2011) estimate the boundaries between substitutability and complementarity among big-box and small supermarkets in the UK.

67 I thank Clément de Chaisemartin for pointing me in this direction.

68 Those $\delta_{small,tc}^t$’s associated to town centres where big-boxes were present already in 2001-2002 cannot be used because I do not observe their $\delta_{small,tc}^t$’s prior to big-box entry (i.e., the household purchase data were first collected in 2001).

69 Such controls are: price index, distance, floorsize, parking lots, tills, retired, density, hamlet, working class, and people$^2$.

70 As it is customary in differences-in-differences regressions, the key identifying assumption relates to the “counterfactual” time trend of the treated town centres: I assume that the $\delta_{small,tc}^t$’s of the treated town centres, had they not experienced any entry of big-boxes, would have had the same time trend observed in the $\delta_{small,tc}^t$’s of the control town centres. The counterfactual
effect (i.e., the effect of big-boxes on the market-specific indirect utility of small supermarkets by distance groups).

**FIGURE 8**

Big-boxes have a positive and significant impact on the market-level indirect utility of nearby small supermarkets. Figure 8 supports the ideas recently proposed in the empirical literature (see footnote [66]): there seems to be, from the perspective of the households, some degree of complementarity between “neighbouring” big-box and small supermarkets.

Tables 11 reports the estimates from regression (6.4).

**TABLE 11**

The first two columns of table 11 report the results used to make figure 8. The unobserved quality of small supermarkets generally increased from 2002 to 2004 (i.e., \( \hat{\tau} > 0 \)), and for those small supermarkets in the proximity of new big-boxes the increase was even more pronounced (i.e., \( \hat{\rho} > 0 \)). Households’ willingness to travel for the increase in unobserved quality of small supermarkets due to big-box entry amounts to 9.73 Km.\(^{71}\) The right panel of table 11 reports the results from regressions similar to (6.4) in which the dependent variable is, instead of \( \delta_{small,tc}^{t} \), the price index \( p_{small,tc}^{t} \). The fifth column shows that, even though the prices of small supermarkets decreased from 2001-2002 to 2003-2004 (i.e., \( \hat{\tau} < 0 \)), there is no evidence of a further effect of big-boxes on the prices of small supermarkets (i.e., \( \hat{\rho} \approx 0 \)).

In the remaining columns of table 11, I partition the sample of 600 town centres in two subsamples: “no big four” is the subsample of town centres in which no big four supermarket (of any format) opened in the period 1997-2004 (“big four” is the complement). Does the complementarity between big-boxes and small supermarkets depend upon the presence of chain retailers?\(^{72}\) Interestingly, big-boxes do not significantly affect the indirect utility of small supermarkets in neighbouring town centres in which at least one of the big four is present in the smaller format. On the other hand, with respect to the

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\(^{71}\)This is computed as minus the ratio between \( \hat{\rho} \) and the estimated marginal utility of distance, \( \frac{\hat{\rho}}{\partial \delta_{small,t}^{t} / \partial \text{dist} + \hat{\theta}} \). It translates “utils” in terms of how much “further” from households the treated supermarkets should get (in Km) to preserve indifference with respect to 2001-2002.

\(^{72}\)For instance, it could be the case that chain retailers opened big-boxes in the proximity of their own small supermarkets so to internalize part of the positive externalities documented by Gould et al. (2005). In the UK, only the big four chain retailers (Asda, Morrisons, Sainsbury’s, and Tesco) develop big-boxes.
average treatment effect from the second column, big-boxes seem to have an even stronger impact on the indirect utility of neighbouring independent small supermarkets (i.e., the willingness to travel is almost twice as large: 18.27 as opposed to 9.73 Km). On average, the price index of small supermarkets decreased from 2001-2002 to 2003-2004, but there is no evidence of a further causal effect of big-boxes on small supermarkets’ prices. In any case, table 10 suggests that only 3-4% of the increase in consumer welfare is due to improvements in observable characteristics (such as prices).

Table 12 investigates some of the components which may contribute to the “unobserved quality” of small supermarkets (i.e., $\xi_{small,tc}$). For instance, the “quality” of the pool of small supermarkets across town centres might be heterogeneous (beyond the included controls), and this would translate into heterogeneity in $\xi_{small,tc}$ across town centres.

Interestingly, big-box entry does not seem to directly affect exit, variety, floorspace, and product range of the pool of small supermarkets (i.e., $\hat{\rho} \approx 0$), but the time trends highlight an independent (of big-box entry) improvement along these dimensions (i.e., $\hat{\tau} > 0$). The estimates of the “post” coefficients (the $\tau$’s) from table 12 show that the pool of small supermarkets is increasing: its variety (i.e., the number of different retailers owning at least one small supermarket in the town centre), its total floorspace, and the range of product categories sold.

Overall, the results of the current section suggest that small supermarkets (both big four and independents) are improving their unobserved quality and decreasing their prices, and that such quality improvements (but not price cuts) are more pronounced for independent retailers whenever a big-box enters in their neighbourhood (i.e., table 11). Some of the improvements in unobserved quality are in terms of variety of retailers, total floorspace, and the range of product categories sold. There is no evidence that such improvements are due to big-box entry (i.e., table 12). Unfortunately, because of data limitation at the supermarket-level, I cannot further explore the mechanisms by which big-box entry

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73Whereby “independent” I mean: not belonging to any of the big four retailers.
74This result, at least for the “big four” group, is not surprising. Indeed, most chain retailers in the UK adopt national pricing strategies [see Competition Commission (2000)]. Consequently, comparing the price indexes of town centres in which at least one of the big four is present should not give rise—as in the seventh column of table 11—to any significant average difference.
75The right end of table 11 suggests there is no generalized effect on the prices of small supermarkets following the entry of big-boxes. Still, as suggested by Hausman & Leibtag (2007) results, small supermarkets may respond to big-box entry by specializing into product categories in which they think to have a comparative advantage (rather than attempting to be “cheaper” in every product category). Table A4 brings two examples in support of this “specialization” story. Following the opening of a new big-box, neighbouring independents increase their revenues in ethnic food and decrease their prices on ambient meat and vegetable extracts. I estimated similar regressions for each of the 183 product categories and found similar results only for ambient salad accompaniments and total skincare products.
induces unobserved quality improvements in small supermarkets.\textsuperscript{76} Some further discussion on unobserved quality can be found in appendix [8.5.1].

7 Conclusions

7.1 Summary

Despite the many benefits, the expansion of big-box supermarkets is perceived as generating negative externalities (e.g., damaging the environment, hollowing out town centres, reducing local variety, etc.). Social planners around Europe (e.g., France, Italy, and the UK) attempted to limit such externalities by tightening land use regulations. A key policy question is whether such land use regulations are indeed desirable for society. By evaluating the consumer benefit of big-box supermarkets, my paper informs the policy debate on the efficiency of land use regulations targeted at big-boxes.

In the first part of the paper, I propose a novel framework to consistently estimate demand in the context of endogenous store entry (more in general: endogenous product availability). In the second, I exploit UK home scanner expenditure data in conjunction with the introduction of Town Centres First (i.e., a tightening in land use regulations) to separately identify preferences for big-boxes from retailers’ entry decisions. In conclusion, I evaluate the consumer welfare consequences of big-box entry. Results reveal three main deviations from conventional estimation approaches.

First, the consumer benefits from big-boxes are an order of magnitude smaller due to the implicit sample-selection bias that arises when supermarket entry locations are endogenous to local consumer preferences. Second, most of the increase in consumer welfare of grocery shopping is due to improvements in the quality of small supermarkets (e.g., variety of retailers, total floorspace, and range of product categories sold). Third, disadvantaged households (e.g., those without cars, single parents, and single pensioners) are the least able to enjoy the potential benefits generated by big-boxes: they tend to live in deprived areas (not attractive to retailers) and have to travel further away for their grocery shopping. Overall, these findings help rationalize Town Centres First, a policy frequently criticized as being anti-consumer.

\textsuperscript{76}For instance, Matsa (2011) finds that Walmart’s entry induces (by increasing the competition for households’ grocery dollars) an increase in quality (i.e., less frequent stockouts—in his case), rather than a decrease in prices, in the nearby independent stores.
7.2 Limitations and Future Directions

My household model of supermarket demand is an exploded logit along the lines of BLP (2004) and Train & Winston (2007); i.e., with approximately 800 fixed effects, one for each combination supermarket format, town centre, time period. My specification does not include any random coefficient because of lack of identification: (1) I do not observe much choice set variation in the data and (2) not enough households go shopping to more than one combination format-town centre.\footnote{The problem I face is known in the literature. BLP (1995, 2004) underline the importance of choice set variation in order to identify preference parameters. When there is not much choice set variation in the data, the exploded logit performs better than the simple logit. Given the same assumptions, the exploded logit exploits ranking data to generate “additional” choice set variation. In fact, both BLP (2004) and Train & Winston (2007) report that they were unable to identify any random coefficient unless they used data on rankings of choices (i.e., beyond the first-best). Unfortunately, in my case the ranking data are not rich enough to generate sufficient “additional” choice set variation so to identify random coefficients. However, also in my application the exploded logit model leads to practical advantages. It allows me to use all the available information about multi-stop shopping (e.g., main shopping to a big-box and top-up shopping to smaller stores) without having to deal with potentially complex dynamic considerations and having to define what “main” and “top-up” shopping trips are.}

The absence of random coefficients is not crucial for my paper. I am not after realistic substitution patterns since I do not simulate counterfactuals for the evaluation of welfare. Indeed, I evaluate welfare exploiting observed big-box entry.\footnote{The counterfactual I need, in order to measure consumer welfare, is a change in households’ choice sets. My demand model assumes logit errors, and these have been criticized as inappropriate for such a task [see Petrin (2002), Berry et al. (2004), and Berry and Pakes (2007)]. As Nevo (2011) shows, even with a simple logit model (i.e., no random coefficients), it is possible to correctly evaluate the welfare effects of a changing choice set. This on the premises of having good measures of the market-shares pre and of the market-shares post-change. Usually, researchers only have one set of market-shares: either pre or post-change; and then “simulate” the remaining set with their estimated model. It is in the market-share simulation step that the logit model becomes inappropriate (the blue-bus, red-bus case is an example). Then, given wrong simulated market-shares, wrong welfare computations follow. In contrast, if in the first place the logit model is “fed” with the correct market-shares pre and after choice set change, then the welfare calculations will be correct.}

A realistic way to improve the robustness of the demand estimates is to implement a nested logit model. This would allow the unobserved households’ preferences for supermarket format (i.e., big or small) to be correlated across town centres. (By creating a nest for big-boxes and a nest of small supermarkets.)

In the selection model I make strong distributional assumptions (i.e., probit). On the one hand, they greatly simplify the empirical implementation of the product choice model. On the other, there are margins to relax normality [see, for example, Das et al. (2003)], even though the practical payoff from doing so (balancing for simplicity) is not clear. Hence, before investing resources in relaxing distributional assumptions, I performed the analysis in the simplest possible way to see if the results were suggestive enough to motivate such an investment. In any case, from a methodological perspective, the message of the paper is not affected by distributional assumptions.

In my application I only have one selection equation, namely for the presence of big-boxes across town centres. A natural question is what to do in the case of multiple selection equations. In terms of
identification, there are two options. The first option requires an instrument (i.e., not correlated with preferences) that varies across markets but that is common across producers. Such market-specific characteristic should affect differentially the various producers. The second option requires an instrument that varies both across markets and across producers. In my context this could be the "proximity" to the logistic network of the retailer (e.g., the closest warehouse). One instrument is enough (i.e., we do not need as many instruments as the number of selection equations), but it is required to be heterogeneous across both markets and producers. In my case, such data are available from IGD and I am planning to use them.

My paper only addresses “half” of the efficiency question in relation to the expansion of big-boxes: the consumer welfare. Further research should consider the inclusion of a structural supply side to quantify the costs associated with big-box entry. This would allow one to better inform the policy debate about the “optimal” expansion of big-boxes and, consequently, about the design of better mechanisms to “internalize” big-boxes’ externalities.

References


As an example, imagine very high rents in central London, common to all retailers; some retailers are not able to handle them, others are. So we see Tesco and Sainsbury’s entering, but not Cost-Cutter.


8 Appendix

8.1 Individual-Level Selection

Assume individuals make choices over the set of products \((j, m)\)'s. In what follows, I investigate the conditions under which the researcher can ignore the \(m\) dimension in the description of the object of individuals' decisions and restrict individuals' choice sets to the \(j\) dimension.

Define \(CS(m) \subseteq \{1, 2, \ldots, J\}\) as the set of alternatives available in market \(m\). Also, define \(S(j) \equiv \{m | j \in CS(m)\}\) as the collection of markets where alternative \(j\) is available. The potential endogeneity of \(CS = (CS(m))_{m=1}^{M}\) with respect to individuals' preferences is one of the main themes of this paper but, for the purpose of this appendix, I assume the d.g.p. is exogenous. Moreover, I assume the indirect utility (2.1) does not include any random coefficient (i.e., \(\eta_i = \theta, \forall i\)), so the choice probability in (8.2) is a within-market multinomial logit.\(^{80}\)

The idea is simple: suppose in markets \(m_1\) and \(m_2\) are offered exactly the same sets of alternatives, \(CS(m_1) = CS(m_2)\), and that each alternative is sold both in \(m_1\) and in \(m_2\) at the same price; but that individual \(i\) is observed choosing an alternative in market \(m_1\). Before attributing this behavior to the unobserved portion of utility, \(\varepsilon_{ijm_1} - \varepsilon_{ijm_2}\), the estimator controls for other observable attributes (e.g., if the market is in a "dodgy" area and distance from \(i\)'s flat) over which the individual may have preferences that partly determine her choice: market \(m_2\) may just be an extra 10 minutes walk from \(i\)'s flat. In other words, going to market \(m_1\), and thus being "matched" to \(CS(m_1)\), may still be the outcome of a choice which does not have anything to do with the attributes of the alternatives in \(CS(m_1)\).

Given the current assumptions, choice probability (8.2) can be re-written as:

\[
Pr_{ijm}(\theta, \delta^* | CS) = Pr_i(\text{alternative } j, \text{market } m | CS, \theta, \delta^*)
\]

\[
= Pr_i(\text{market } m | \text{alternative } j, CS, \theta, \delta^*) \cdot Pr_i(\text{alternative } j | CS, \theta, \delta^*)
\]

\[
= \frac{\exp[\delta_{jm}^* + V_{jm}(p_{jm}, x_{ijm}, \theta)]}{\sum_n \sum_{g \in CS(n)} \exp[\delta_{gn}^* + V_{gn}(p_{gn}, x_{ign}, \theta)]}
\]

The first element, \(Pr_i(\text{market } m | \text{alternative } j, CS, \theta, \delta^*)\), is the probability that individual \(i\) is matched with market \(m\) given that she chose alternative \(j\). The second is the probability that individual \(i\) chooses

\(^{80}\)As it will be clearer later, with random coefficients the problem gets even worse.
alternative \( j \). The expressions for the two probabilities are:

\[
\Pr_i ( \text{alternative } j \mid \text{CS, } \theta, \delta^*) = \sum_{m \in S(j)} \Pr_i ( \text{alternative } j, \text{market } m \mid \text{CS, } \theta, \delta^*) = \frac{\sum_{m \in S(j)} \exp \left[ \delta^*_{jm} + V_{jm} (p_{jm}, x_{ijm}, \theta) \right]}{\sum_{n \in CS(n)} \sum_{g \in \text{CS}} \exp \left[ \delta^*_{gn} + V_{gn} (p_{gn}, x_{ign}, \theta) \right]}.
\]

\[
\Pr_i ( \text{market } m \mid \text{alternative } j, \text{CS, } \theta, \delta^*) = \frac{\Pr_i ( \text{alternative } j, \text{market } m \mid \text{CS, } \theta, \delta^*)}{\Pr_i ( \text{alternative } j \mid \text{CS, } \theta, \delta^*)} = \frac{\exp \left[ \delta^*_{jm} + V_{jm} (p_{jm}, x_{ijm}, \theta) \right]}{\sum_{n \in S(j)} \exp \left[ \delta^*_{jn} + V_{jn} (p_{jm}, x_{ijm}, \theta) \right]}
\]  

As it can be seen from (8.1), for \( j_1, j_2 \in \text{CS (m)} \), there is no reason to believe that \( \Pr_i (m \mid j_1, \text{CS, } \theta, \delta^*) = \Pr_i (m \mid j_2, \text{CS, } \theta, \delta^*) \). If this is the case, even in the current specification (i.e., a market-specific multinomial logit, no within-market random coefficients), the uniform conditioning property does not hold [see Bierlaire et al. (2008), Fox (2007), and McFadden (1978)], and estimating \((\theta, \delta^*)\) only via \( \Pr_i (\text{alternative } j \mid \text{market } m, \theta, \delta^*) \) leads to inconsistency (i.e., if we mistakenly assume each individual is “stuck” in a specific market for reasons beyond her preferences).\(^{81}\)

We have endogenous individual-level selection whenever \( \Pr_i (m \mid j, \text{CS, } \theta, \delta^*) \) is a function of \( j \). Again from (8.1), excluding pathological cases (e.g., all the systematic utilities are identical), this happens whenever the universal choice set of alternatives, \( \{1, 2, \ldots, J\} \), is not partitioned across the \( M \) markets (i.e., \( \exists j \in \{1, 2, \ldots, J\} \) such that \( \#S(j) > 1 \)). This will always be the case if there are more markets than alternatives, \( M > J \). Instead, if \( M \leq J \) and each market \( m \) has an exclusive set of alternatives, \( \Pr_i (m \mid j, \text{CS, } \theta, \delta^*) = 1 \) for each \( j \in \text{CS (m)} \) and \( m \) (i.e., the uniform conditioning property holds), and the researcher can safely restrict choice sets to the \( j \) dimension.

\(^{81}\)To see this, notice that we can equivalently express \( \Pr_i (\text{alternative } j \mid \text{market } m, \text{CS, } \theta, \delta^*) \) as:

\[
\Pr_i (\text{alt } j \mid \text{mkt } m, \text{CS, } \theta, \delta^*) = \frac{\exp \left\{ \delta^*_{jm} + V_{jm} (p_{jm}, x_{ijm}, \theta) + \ln [\Pr_i (\text{mkt } m \mid \text{alt } j, \text{CS, } \theta, \delta^*)] \right\}}{\sum_{g \in \text{CS}(m)} \exp \left\{ \delta^*_{gm} + V_{gm} (p_{gm}, x_{igm}, \theta) + \ln [\Pr_i (\text{mkt } m \mid \text{alt } g, \text{CS, } \theta, \delta^*)] \right\}}.
\]

If, mistakenly, the researcher does not include \( \ln [\Pr_i (\text{mkt } m \mid \text{alt } g, \text{CS, } \theta, \delta^*)] \) in her specification, there will be an omitted term correlated with the systematic utility.
Hence, the key condition for endogenous individual-level selection is that sets of available alternatives in different markets partially overlap (e.g., there are some “popular” alternatives which are sold in many different markets). This condition is easily observable from the data, so the researcher does not have to guess. Furthermore, the condition is not just statistical, it has intuitive economic meaning. The closer markets are to each other in variety space (in terms of alternatives), the fiercer price competition will be in order to attract individuals (assuming the non-price characteristics of each alternative are predetermined and only price can be altered from market to market).

In my empirical application, I have $M = 352$ and $J = 2$; furthermore, I observe individuals choosing the same $j$ from many different $m$’s (see table 3 and related discussion). Following the above argument, my application might be subject to endogenous individual-level selection, hence I define alternatives as $(j, m)$ combinations (i.e., I do not ignore the $m$ dimension). For details about my empirical definition of alternatives, see section [3.2].

8.2 Mixed Logit Model and Its Estimation

In this appendix I briefly describe the mixed logit model and how it is estimated when the market-specific utility shifters, $\delta^* = \left\{ \delta^*_{jm} \right\}_{j=1}^{J} \cdot \left\{ \delta^*_{jm} \right\}_{m=1}^{M}$, are obtained with Berry (1994)’s contraction mapping. Given the assumptions of section [2.1], the probability of product $(j, m)$ being individual $i$’s first-best is given by the mixed logit model [see McFadden & Train (2000)]:

$$
\Pr_{ijm}(\theta, \delta^*) = \frac{\exp \left[ \delta^*_{jm} + V_{jm}(p_{jm}, x_{ijm}, \eta_i) \right]}{\sum_g \sum_n \exp \left[ \delta^*_{gn} + V_{gn}(p_{gn}, x_{ign}, \eta_i) \right]} f(\eta_i | \theta) \, d\eta_i.
$$

Market-level demand is the aggregation of individual-level demands. The observed market-level demand for product $(j, m)$ is its market share, $S_{jm}$. The observed market share of alternative $j$ in market $m$ is modeled as [see BLP (2004), Golsbee & Petrin (2004), and Train & Winston (2007)]:

$$
S_{jm} = \hat{S}_{jm}[\theta, \delta^* (\theta, S)] = \frac{1}{I} \sum_{i=1}^{I} \Pr_{ijm}(\theta, \delta^* (\theta, S)),
$$

where $\delta^* = \left\{ \delta^*_{jm} \right\}_{j=1}^{J} \cdot \left\{ \delta^*_{jm} \right\}_{m=1}^{M}$ is expressed as a function of $\theta$ and $S$ [i.e., the market shares of all products $(j, m)$’s] after Berry (1994): for any value of $\theta$ there exists a unique vector $\delta^*$ such that predicted market shares, $\hat{S}$, equal observed market shares, $S$.

Following Train & Winston (2007), the estimation of $(\theta, \delta^*)$ can be performed by simulated maximum
likelihood (SML) augmented by a “contraction” algorithm. Given the observed market shares, \( S \), at each iteration of the SML search for \( \theta, \theta' \), the contraction algorithm computes the vector \( \delta^* (\theta', S) \) which enforces condition (8.3). When convergence is achieved, \( \hat{\theta} \) and \( \delta^* \left( \hat{\theta}, S \right) \) are obtained, and the estimation of \((\alpha, \beta)\) from (2.2) can be performed as outlined in section [2.3], using \( \delta^*_m \left( \hat{\theta}, S \right) \) as dependent variable.\(^{82}\)

As explained in section [3.3], I have data on rankings of \((j, m)\) combinations rather than just about the first-best. The use of preference-rankings for the estimation of a logit model [i.e., the exploded logit model, See section 7.3 of Train (2009)] appears to be relevant for the identification of random coefficients [see BLP (2004) and Train & Wiston (2007)]. More in general, it has been shown that using preference-rankings data improves the precision of the estimates when compared to first-best data [see Beggs et al. (1981)]. BLP (1995, 2004) underline the importance of choice set variation in order to identify preference parameters. When there is not much choice set variation in the data (such as in my case, choice set changes from 391 to 408 \((j, tc)\) combinations because of the entry of big-boxes in 17 town centres where none was present), the exploded logit performs better than the simple logit because, given the same assumptions, it exploits ranking data to generate “additional” choice set variation.\(^{83}\) Furthermore, as discussed in section [3.3], in my application the exploded logit model leads to practical advantages. It allows me to use all the available information about multi-stop shopping (e.g., main shopping to a big-box and top-up shoppings to smaller stores) without having to deal with potentially complex dynamic considerations and having to define what “main” and “top-up” shopping trips are. Therefore, I estimate an exploded logit model.

### 8.3 Definition of Variables

In this appendix I describe in detail the construction of the “grants” and the “price index” variables.

#### 8.3.1 “Grants” Variable

Supermarket entry data are at the detail of day, town centre; while planning application data are at the detail of year, Local Planning Authority (LPA). Unfortunately, I cannot directly link applications to town centres, but I can still exploit the timings of entry and of the bureaucratic approval process to obtain some variation across town centres within a LPA. In case an application is granted, from the moment the application is received by the competent LPA to the actual opening of the supermarket, there is a lag which varies from 4 months up to 2 years [see Sadun (2011)]. “Grants” (summarized in

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\(^{82}\)See BLP (2004) for GMM estimation of this class of models.

\(^{83}\)Data on the first-best are used as in the simple logit, but data on less preferred alternatives are included in the model by sequentially “removing” more preferred alternatives from the household’s choice set.
8.3.2 “Price Index” Variable

I construct price indexes for each \((j, tc)\) combination. In doing this, I follow Dubois & Jodar-Rosell (2010) and Schiraldi et al. (2011).

There are 183 product categories in the Kantar data (e.g., yogurt, cheese, pizza, etc.). Within each product category there are many different products. The prices of these products are recorded from till receipts by households, each time they purchase them. Call \(o(b)\) an observation in the purchase data for product \(b\) (i.e., within product category \(K\)), each household has one of such observations every time she purchases product \(b \in K\). \(\exp_{o(b),s}\) is \(o\)'s expenditure for product \(b\) in supermarket \(s\), and 

\[
\exp^K_s = \sum_{b \in K} \sum_{o(b)} \exp_{o(b),s} \quad \text{is supermarket } s\text{'s total revenue in product category } K. \quad \text{Similarly, } \exp^K_{j,tc} = \sum_{s \in (j,tc)} \exp^K_s \text{ is total revenue of format-size } j \text{ in town centre } tc. \quad \text{Then, } \exp^K_{o(b),s} = \frac{\exp_{o(b),s}}{\exp^K_s} \text{ is the share of revenues of product category } K \text{ in supermarket } s \text{ due to } o\text{’s purchase of product } b, \text{ and } \exp^K_{j,tc} = \frac{\exp^K_s}{\exp^K_{j,tc}} \text{ is the share of revenues in product category } K \text{ in format-size } j \text{ in town centre } tc \text{ of supermarket } s \in (j,tc).
\]

Define \(p_{o(b),s}\) as the unit price recorded by observation \(o\) for product \(b\) from supermarket \(s\) (i.e., \(\exp_{o(b),s}\) divided by volume purchased). \(^{85}\) Then \(p^K_s = \sum_{b \in K} \sum_{o(b)} \exp^K_{o(b),s} p_{o(b),s} \) is the unit price of product category \(K\) in supermarket \(s\), and \(p^K_{j,tc} = \sum_{s \in (j,tc)} \exp^K_{j,tc} p^K_s \) is the unit price of product category \(K\) in format-size \(j\) in town centre \(tc\). \(w^K_{j,tc} = \frac{\exp^K_{j,tc}}{\sum_{s \in (j,tc)} \exp^K_{j,tc}} \) is the revenue-weight of product category \(K\) in format-size \(j\) in town centre \(tc\). Finally, \(p_{j,tc} = \sum_{K \in K} \exp^K_{j,tc} p^K_s \) is the price index for combination \((j, tc)\).

By construction, price index \(p_{j,tc}\) is endogenous to sampled households’ preferences, because it is computed from the prices of those products that were actually purchased by the sampled households [see Dubois & Jodar-Rosell (2010)]. I might observe certain product categories to be sold in some town centres (where the sampled households like them) but not in others (where the sampled households do

\(^{84}\)Following Sadun (2011), I use the absolute number of granted applications rather than the acceptance rate of LPAs. Particularly strict LPAs might still have very high acceptance rates because fewer retailers applied at all, and this would underestimate the stringency of the planning regime.

\(^{85}\)All prices are deflated by the RPI rate of inflation so to be expressed in 2001 terms.
not like them) even though the product categories are actually available everywhere. Hence, the set of product categories used in the construction of the price indexes in each town centre might correlate to sampled households’ preferences. We can expect such form of endogeneity to diminish the larger is the sample of prices we observe. As discussed in the main text, I aggregate over time intervals long years. Furthermore, the same endogenous sample-selection story discussed in section [2.2] but applied to the population of product categories may hold here (i.e., supermarkets might decide not to offer a product category if households do not like it enough).\footnote{I gathered evidence (i.e., pictures) related to this hypothesis between London and Birmingham. The interested reader should contact me.} To address this possibility, I include in the set of product categories $K$ only those $K$’s which are observed to be sold in at least $\frac{2}{3}$ of the town centres, so to consider only those product categories for which households seem to have similar preferences across the country. Moreover, the estimation procedure outlined in section [2.3] is robust to price endogeneity.

### 8.4 Endogeneity of “grants” Variable

In the estimation of regressions similar to my entry model (2.4), Bertrand & Kramartz (2002), Cheshire et al. (2012), Sadun (2011), and Schivardi & Viviano (2011), question the exogeneity of $g_{tc}$ from $u_{tc}$. In my context, too, we can imagine scenarios in which $g_{tc}$ could be correlated with both $u_{tc}$ and $\xi_{big,tc}$ (so the estimation of the entry model would not be correct). In order to give some structure to the problem, I fraxe the discussion in terms of correlation between $g_{tc}$ and unobserved preferences for big-boxes, $\xi_{big,tc}$. I assume that any source of correlation between $g_{tc}$ and $u_{tc}$ is due to preferences for big-boxes—via selection on unobservables. I am not excluding the possibility that $g_{tc}$ and $u_{tc}$ are correlated; I am assuming that such a correlation, if any, stems from unobserved preferences for big-boxes, $\xi_{big,tc}$.

The $g_{tc}$ variable could be correlated with $\xi_{big,tc}$ if higher preferences for big-boxes translated into higher investments into lobbying the Local Planning Authority (LPA) to increase its acceptance rate. Similarly, in those places where big-boxes are liked better, the LPAs—being an expression of the households who live and shop there—might just be more “lenient” in judging big-box applications. Furthermore, even if the acceptance rate of the LPA were independent of $\xi_{big,tc}$, $g_{tc}$ could still be positively correlated with $\xi_{big,tc}$. This is so because a higher $\xi_{big,tc}$ might attract a larger number of applications from retailers. To address such issues, Bertrand & Kramartz (2002), Sadun (2011), and Schivardi & Viviano (2011) instrument $g_{tc}$ with the conservatives’ share of votes in the last local elections. They argue that conservatives are reluctant to approve big-box supermarkets because of their electorate made of owners of small shops and activities. I do not follow such a strategy. In my specific case, where
preferences are the focus of the analysis (the aforementioned papers focus on unemployment and other sectoral performance indicators), it would be hard to advocate that the share of votes of any political party in local elections is uncorrelated to local preferences for big-boxes; in fact such preferences are “made” of the same households who vote at the elections. I would need to exploit only those LPAs where last election’s outcomes were uncertain, but this would reduce my sample size below any acceptable threshold.\footnote{I thank Luigi Pascali for pointing this out to me.} I exploit a different strategy from instrumental variables: the “timing” of Town Centre First’s introduction and “practical” implementation.

For reasons I explain in section \[4.2\], I split the household-purchase data in two sub-samples: 2001-2002 and 2003-2004; and estimate separately the two sets of market-specific utilities, $\delta^{01-02}$ and $\delta^{03-04}$. Take the first sub-sample. Preferences are estimated on the 2001-2002 data, so $\xi_{\text{big,tc}}^{01-02}$ represents unobserved preferences for big-boxes in town centre $\text{tc}$ in the period 2001-2002. Differently, the grants$_{\text{tc}}$ variable is computed on the two years prior to the first entry observed, in the post-TCF period, in the town centre (see appendix \[8.3.1\] for a detailed description of grants$_{\text{tc}}$). Hence, only for those town centres with first entry in 2002 there could be some worry of correlation between grants$_{\text{tc}}$ and $\xi_{\text{big,tc}}^{01-02}$ (i.e., if the first new supermarket opens in 2001, then grants$_{\text{tc}}$ is computed with 1999 and 2000 data; outside of the preference interval).

\[\text{TABLE A1}.\]

As it can be seen from table A1, only 4.83% of the town centres have first new openings in 2002; furthermore, only one of these 17 town centres actually experiences the opening of their first big-box in 2002. Similarly for the period 1997-2004: only 4.83% of the town centres have first new openings in 2004 and none of them experiences the opening of their first big-box. The bulk of “first new entries” in the post-TCF (i.e., first supermarkets to open in each town centre after the introduction of Town Centre First) across the selected town centres happened between 1997 and 2001 (approximately 85% of the town centres); a period during which the grants$_{\text{tc}}$ variable—I argue in what follows—should not be correlated with $\xi_{\text{big,tc}}$. Indeed, correlation between grants$_{\text{tc}}$ and $\xi_{\text{big,tc}}$ could only arise if $\xi_{\text{big,tc}}$ were persistent over time.

If a “high” realization of $\xi_{\text{big,tc}}$ induces a “high” realization of $\xi_{\text{big,tc}}$, then there will be persistence in the unobserved portion of preferences. If this is the case, even if grants$_{\text{tc}}$ refers to previous years, it can still be correlated with current realizations of $\xi_{\text{big,tc}}$ because $\xi_{\text{big,tc}}$ conveys information about $\xi_{\text{big,tc}}$. Even though such persistence is quite plausible over consecutive years—and I do control for it in
estimation—, there is evidence that, on the one hand, big four retailers were “updating” their “estimates” of town centres’ preferences only every 10 years and that, on the other, preferences in the grocery sector were evolving at a faster pace. If retailers used “old” estimates of preferences and actual preferences evolved fast, then worries about grants’s endogeneity would greatly reduce. Evidences of these facts were recently disclosed by Terry Leahy, CEO of Tesco between 1997 and 2010:

“We had always relied on the UK’s National Census to help us decide where a store should be located, how it should be designed and what ranges it should stock. The problem was that the Census is only conducted once every ten years [...]. Moreover, the cultural makeup of the UK has been changing fast in recent years, with people coming into the country from many parts of the world. Our data were simply not reflecting this. This problem was thrown into sharp relief when we decided to rebuild our store in Slough, just west of London, in 2005. Just walking round the neighbourhood at the time revealed how much the community had clearly changed since the time of the 2001 Census. [...] Our existing store wasn’t serving our customers; our plans for a new store wouldn’t work either. So the entire company put in a massive effort to understand the community, [...] to find out the truth about what customers in Slough wanted. [...] This was something we had never done before. None of the changes we made would have occurred to us had we relied on the Census. [...] [T]he success of the Slough venture completely changed how we did business, not just there but across the UK.” Leahy (2012, pages 34-35).

Entry and lobbying decisions up until 2001 were based on the 1991 Census (the next wave of the Census was conducted the 29th of April 2001). The example of Tesco in Slough shows how estimated preferences from the 2001 Census were already outdated in 2005. Thus, it seems reasonable to assume that estimated preferences from the 1991 Census, used by retailers to make entry and lobbying decisions up until 2001, were outdated and not correlated with the 2001-2004 preferences used in my analysis. Despite the efforts to minimize the risk of endogeneity between grants, because no direct testing procedure is possible, doubts may remain. Consequently, as a robustness check, I estimate the full model with and without the grants, variable (i.e., only using the geographical instruments to entry, see section [4]). Qualitatively, point estimates do not differ; they are though more precise in the specification which includes grants.
8.5 Supplementary Estimation Results

8.5.1 Price Endogeneity and Format-Specific Time Trends

In this section I compare different specifications for model $\delta_{j,tc}$ in (2.3) in order to choose whether to include in the analysis separate time trends for big-boxes and for small supermarkets. Such a decision is interwoven with the issue of price endogeneity.

As detailed in appendix [8.3.2], the price index is expected to be endogenous with respect to unobserved preferences. Thus, it is required to find instruments to correct for such endogeneity. In the UK, chain retailers adopt a national pricing strategy [see Competition Commission (2000)]. In broad terms, whenever a retailer offers a specific product in any of its stores, it will be sold at the same price everywhere in the UK. Within the same retailer, price indexes still vary across stores because different stores, located in different town centres, offer different ranges of products (and different pack-sizes for given product). A direct implication of national pricing is that cost-shifters, a “classic” choice for instruments, are not expected to explain any price variation. Indeed, even though different stores belonging to the same retailer do face different marginal costs, such heterogeneity is simply averaged-out by the retailer across the whole chain. In other words, most of the variation in price indexes comes from heterogeneity in preferences across town centres rather than from cost-side considerations.

Other candidate instruments, first proposed by Hausman (1996), would be the price indexes of the same supermarket-format $j$ but from different town centres. Following the discussion in Nevo (2001), this also does not look like a good idea in my application. Town centres are relatively small areas (compared, for instance, to US cities) and households, as can be seen from table 3, tend to “go around” for shopping. Consequently, it is likely that price $p_{j,tc}$ (i.e., the range of offered products) is set taking into consideration the $\xi_{j,tc}$’s of all the neighboring town centres, and not just its own.

It is then hard to imagine any source of exogenous (with respect to unobserved preferences) variation in the price indexes beyond the “location” of combination ($j, tc$) in characteristics space [i.e., the IV strategy proposed by BLP (1995)]. As said above, heterogeneity in the price index is mainly due to heterogeneity in the ranges of products offered. Unfortunately, I do not have such an information in my data. Floorsize seems to convey some related information, in the sense of being negatively correlated with the price index (see table A2), but unfortunately—after extensive experimentation—it does not appear to work well as a BLP-kind of instrument. The same is true for the remaining observed characteristics. Following Nevo (2001), there is less structural version of the same BLP-kind of instruments: the format-specific time trends. The idea is to instrument price with the average unobserved characteristics of supermarket-format $j$ across town centres.
Table A3 reports four alternative specifications for the $\delta_{jt,c}$ regression (without accounting for sample-selection) in model (2.3). The number of observations is 813: all the $(j,tc)$ combinations in periods 2001-2002 and 2003-2004.

The first column of table A3 lists the results from OLS regression of $\delta_{jt,c}$ on observable characteristics of both $j$ and $tc$. The coefficient on the price index is negative and significant. Also the coefficients on distance and floorsize are significant and have the expected sign. By construction, the price index is endogenous because it is computed only on the disaggregated prices of those products actually purchased by households. In the second and third column I compare the validity of the different instruments discussed earlier. In column (ii) I use Hausman-kind of instruments, while in column (iii)—following Nevo (2001)—I use BLP-kind of instruments. As customary in the literature, the magnitude of the estimated price coefficient greatly increases after controlling for potential endogeneity. In line with the discussion above, BLP-kind of instruments seem to work better than Hausman-kind of instruments. Interestingly, controlling for price endogeneity affects (to a lesser extent, but still significantly) also the floorsize coefficient. This makes economic sense. Table A3 confirms the intuitive idea that a higher unobserved utility is associated with higher prices. Table A2, on the other hand, suggests a negative correlation between prices and floorsize. In other words, households avoid the higher-priced supermarkets less than they would if the higher prices occurred without any compensation in terms of unobserved utility. But, avoiding higher-priced supermarkets less, means going more often to smaller supermarkets than they would if the higher prices occurred without any compensation in terms of unobserved utility. In column (iv) of table A3, I perform again an OLS regression, but this time add three fixed effects to the specification: big-box in 2002, big-box in 2004, and small in 2004. These are the time trends for big-boxes and for small supermarkets used in column (iii) as instruments for the price index. As soon as we add such variables, the price index loses all its explanatory power. On the other hand, the same is not true for the remaining characteristics. As noted by Nevo (2001), the inclusion of such intercepts “absorbs” the effect of any characteristic which does not vary “much” within the $j$ dimension (i.e., big and small) but only across $j$’s. Hence, the price index does not seem to rationalize enough variation within each $j$ (at least not after having controlled for the other characteristics) and loses all its explanatory power once we include alternative specific constants. A possible explanation for this might be the national pricing

\[ \text{For price index } p_{jt,c} \text{ I use instruments } \bar{p}_{jt,c}^1 = \frac{1}{812} \sum_{m \neq c} p_{jt,c} \text{ and } \bar{p}_{jt,c}^2 = \frac{1}{812} \sum_{m \neq c} (p_{jt,c} - \bar{p}_{jt,c})^2. \]
strategy discussed earlier.

The only instruments that seem to be economically and practically convincing are the time trends for big-boxes and for small supermarkets used in column (iii). Unfortunately, using them as instruments prevent their inclusion as regressors. But, in my application, this could be a serious limitation. Indeed, my observed attributes (i.e., price, floorsize, parking lots and tills) are quite “modest” with respect to the complexity of the “objects” they are supposed to characterize. Using Matsa (2011)’s words: “[i]n the retail sector, a firm’s “product” is the shopping experience it provides its customers. Like the quality of physical products, the quality of the shopping experience has many dimensions, including the store’s location, cleanliness, checkout speed, the courteousness of its staff, the depth of its product assortment, and the availability of ancillary services such as parking and bagging.” Households might be making their shopping choices largely on the basis of unobserved (to the researcher) characteristics, and this strongly warrants the use of alternative-specific constants (such as time trends for big-boxes and for small supermarkets) in order to “pull out” from the $\xi_{jt,tc}$’s any systematic pattern [see Nevo (2001)]. This is particularly relevant in my application. Indeed, as discussed in footnote [49], in performing intertemporal comparisons of welfare it is important to account for any observed and unobserved quality change over time [see Nevo (2003)].

In addition, as seen in column (iv), the time trends for big-boxes and for small supermarkets subsume all the information carried by the price index, relieving me from the non simple task of finding convincing intruments for it. For these reasons, I favour the inclusion of the time trends for big-boxes and for small supermarkets in the specification for $\delta_{jt,tc}$. 

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### TABLE 1 — Town Centre Data (2001-2002)

<table>
<thead>
<tr>
<th>Variables</th>
<th>Obs.</th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>People (100,000)</td>
<td>753</td>
<td>5.75</td>
<td>3.96</td>
<td>.615</td>
<td>40</td>
</tr>
<tr>
<td>Density</td>
<td>753</td>
<td>48.40</td>
<td>33.12</td>
<td>6.03</td>
<td>243.51</td>
</tr>
<tr>
<td>Hamlet (%)</td>
<td>753</td>
<td>.044</td>
<td>.062</td>
<td>0</td>
<td>.382</td>
</tr>
<tr>
<td>Working Class (%)</td>
<td>753</td>
<td>.274</td>
<td>.069</td>
<td>.09</td>
<td>.465</td>
</tr>
<tr>
<td>Retired (%)</td>
<td>753</td>
<td>.140</td>
<td>.033</td>
<td>.059</td>
<td>.278</td>
</tr>
<tr>
<td>Area (hectares)</td>
<td>753</td>
<td>29.73</td>
<td>122.25</td>
<td>4</td>
<td>3256.5</td>
</tr>
<tr>
<td>Closest (10 Km)</td>
<td>753</td>
<td>.615</td>
<td>.506</td>
<td>.053</td>
<td>2.81</td>
</tr>
<tr>
<td>Grants*</td>
<td>753</td>
<td>3.74</td>
<td>4.03</td>
<td>0</td>
<td>25</td>
</tr>
<tr>
<td>Local Authority</td>
<td>320</td>
<td>2.69</td>
<td>2.02</td>
<td>1</td>
<td>16</td>
</tr>
</tbody>
</table>


### TABLE 2 — Supermarket Presence (EW, 2002)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Asda</td>
<td>0.44</td>
<td>277</td>
<td>47820</td>
</tr>
<tr>
<td>Morrisons</td>
<td>0.57</td>
<td>362</td>
<td>29801</td>
</tr>
<tr>
<td>Sainsbury's</td>
<td>0.62</td>
<td>508</td>
<td>31172</td>
</tr>
<tr>
<td>Tesco</td>
<td>0.85</td>
<td>816</td>
<td>27666</td>
</tr>
<tr>
<td>Discouter*</td>
<td>0.56</td>
<td>509</td>
<td>8112</td>
</tr>
<tr>
<td>M&amp;S</td>
<td>0.59</td>
<td>306</td>
<td>17018</td>
</tr>
<tr>
<td>Waitrose</td>
<td>0.24</td>
<td>141</td>
<td>19103</td>
</tr>
<tr>
<td>Iceland</td>
<td>0.82</td>
<td>787</td>
<td>4837</td>
</tr>
<tr>
<td>Co-op</td>
<td>0.83</td>
<td>1834</td>
<td>4797</td>
</tr>
<tr>
<td>Somerfield</td>
<td>0.76</td>
<td>779</td>
<td>8469</td>
</tr>
<tr>
<td>Others</td>
<td>0.95</td>
<td>2652</td>
<td>5253</td>
</tr>
</tbody>
</table>

FIGURE 1 — Supermarket Presence (England & Wales, 2002)

### TABLE 3 — Households’ Rankings of Choices (2003-2004)

<table>
<thead>
<tr>
<th>Ranking Positions</th>
<th>Obs.</th>
<th>Total Expenditure (£)</th>
<th>Home Town Centre (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Mean</td>
<td>SD</td>
</tr>
<tr>
<td>1&lt;sup&gt;st&lt;/sup&gt;</td>
<td>20708</td>
<td>1526.05</td>
<td>1602</td>
</tr>
<tr>
<td>2&lt;sup&gt;nd&lt;/sup&gt;</td>
<td>17386</td>
<td>313.27</td>
<td>429.19</td>
</tr>
<tr>
<td>3&lt;sup&gt;rd&lt;/sup&gt;</td>
<td>12095</td>
<td>119.78</td>
<td>181</td>
</tr>
<tr>
<td>4&lt;sup&gt;th&lt;/sup&gt;</td>
<td>7892</td>
<td>62.07</td>
<td>89.74</td>
</tr>
<tr>
<td>5&lt;sup&gt;th&lt;/sup&gt;</td>
<td>4899</td>
<td>38.32</td>
<td>54.95</td>
</tr>
<tr>
<td>6&lt;sup&gt;th&lt;/sup&gt;</td>
<td>2863</td>
<td>26.59</td>
<td>36.28</td>
</tr>
<tr>
<td>7&lt;sup&gt;th&lt;/sup&gt;</td>
<td>1612</td>
<td>20.24</td>
<td>26.36</td>
</tr>
<tr>
<td>8&lt;sup&gt;th&lt;/sup&gt;</td>
<td>871</td>
<td>14.59</td>
<td>16.86</td>
</tr>
<tr>
<td>9&lt;sup&gt;th&lt;/sup&gt;</td>
<td>439</td>
<td>11.45</td>
<td>13.67</td>
</tr>
<tr>
<td>10&lt;sup&gt;th&lt;/sup&gt;</td>
<td>205</td>
<td>9.77</td>
<td>10.95</td>
</tr>
</tbody>
</table>

Notes: Descriptive statistics about the rankings of supermarket choices made by households in the UK, 2003-2004. “Home Town Centre” lists the likelihoods of the specific destinations chosen to be in the same town centres where the households live. Source: Kantar (former TNS) data, United Kingdom.

### TABLE 4 — Timing of Store Entry (1901-2004)

<table>
<thead>
<tr>
<th>Geographic Area</th>
<th>Big-Box Stores (1179 Obs.)</th>
<th>Small Stores (7520 Obs.)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean (year)</td>
<td>75&lt;sup&gt;th&lt;/sup&gt; perc. (year)</td>
</tr>
<tr>
<td>South</td>
<td>1990.9</td>
<td>1997</td>
</tr>
</tbody>
</table>

Notes: Average and 3<sup>rd</sup> quartile of year of supermarket entry computed within each of three UK geographic areas (north, centre, and south), separately for big-boxes and for small supermarkets. The total number of supermarkets is 8699. The three geographic areas have equal length in terms of vertical coordinates. Source: IGD data.
FIGURE 2 — Big-Box Presence (England & Wales, 2002)

Notes: In the “full sample” there are the 753 town centres for which I have complete data. The “restricted sample,” 352 town centres, excludes the town centres in which: (a) big-boxes entered before 1996 and (b) there was not any entry after 1996. Source: IGD data.
TABLE 5 — Entry of Big-Box Data (2004)

<table>
<thead>
<tr>
<th>Variables</th>
<th>Obs.</th>
<th>Mean</th>
<th>SD</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Presence of Big-Box</td>
<td>352</td>
<td>.184</td>
<td>.388</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>Grants*</td>
<td>352</td>
<td>3.45</td>
<td>3.77</td>
<td>0</td>
<td>23.5</td>
</tr>
<tr>
<td>Dist. Big (100 Km)</td>
<td>352</td>
<td>.088</td>
<td>.072</td>
<td>.006</td>
<td>.342</td>
</tr>
<tr>
<td>Dist. Small (100 Km)</td>
<td>352</td>
<td>.113</td>
<td>.083</td>
<td>.009</td>
<td>.487</td>
</tr>
<tr>
<td>Distance (100 Km)</td>
<td>352</td>
<td>1.99</td>
<td>.471</td>
<td>1.476</td>
<td>4.12</td>
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<tr>
<td>People$^2$ (100,000)$^2$</td>
<td>352</td>
<td>19.39</td>
<td>24.8</td>
<td>.669</td>
<td>218.38</td>
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<tr>
<td>Density</td>
<td>352</td>
<td>43.82</td>
<td>34.57</td>
<td>6.03</td>
<td>230.73</td>
</tr>
<tr>
<td>Retired (%)</td>
<td>352</td>
<td>.146</td>
<td>.037</td>
<td>.067</td>
<td>.278</td>
</tr>
<tr>
<td>Hamlet (%)</td>
<td>352</td>
<td>.062</td>
<td>.076</td>
<td>0</td>
<td>.382</td>
</tr>
<tr>
<td>Working Class (%)</td>
<td>352</td>
<td>.268</td>
<td>.071</td>
<td>.09</td>
<td>.419</td>
</tr>
</tbody>
</table>

Notes: Descriptive statistics about the data used to estimate the model of entry of big-boxes. See text for descriptions of variables. Source: IGD, Kantar (former TNS), ODPM, and ONS data.*Calculated from ODPM data, period 1995-2003.

FIGURE 3 — Averages of Market-Specific Utility Shifters

Notes: 2001-2002 and 2003-2004 averages, across town centres, of the estimated market-specific utility shifters for both big-boxes and small supermarkets.
### TABLE 6 — Model of Big-Box Entry (1997-2004)

<table>
<thead>
<tr>
<th>Variables</th>
<th>(i): ML</th>
<th>(ii): ML</th>
<th>(ii)-(i)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Est. (SE)</td>
<td>Est. (SE)</td>
<td>P-Value</td>
</tr>
<tr>
<td>Grants</td>
<td>—</td>
<td>.303**</td>
<td>—</td>
</tr>
<tr>
<td>Dist. Big</td>
<td>-.615 (.457)</td>
<td>-.549 (.44)</td>
<td>.449</td>
</tr>
<tr>
<td>Dist. Small</td>
<td>.854**</td>
<td>.888**</td>
<td>.604</td>
</tr>
<tr>
<td>Distance</td>
<td>1.551 (1.064)</td>
<td>1.682 (1.053)</td>
<td>.472</td>
</tr>
<tr>
<td>People²</td>
<td>.609***</td>
<td>.576***</td>
<td>.28</td>
</tr>
<tr>
<td>Density</td>
<td>-.548 (.342)</td>
<td>-.513 (.359)</td>
<td>.591</td>
</tr>
<tr>
<td>Retired</td>
<td>-2.899***</td>
<td>-2.774***</td>
<td>.62</td>
</tr>
<tr>
<td>Hamlet</td>
<td>-1.079*</td>
<td>-1.077**</td>
<td>.922</td>
</tr>
<tr>
<td>Working Class</td>
<td>1.154 (.745)</td>
<td>1.043 (.791)</td>
<td>.429</td>
</tr>
<tr>
<td>Hausman Test</td>
<td>—</td>
<td>—</td>
<td>.8</td>
</tr>
</tbody>
</table>

Notes: Marginal effects of probit model for entry of big-box supermarkets. Index evaluated at regressors’ means. Number of town centres (observations): 352. SEs and Hausman statistic’s variance are bootstrapped over Local Planning Authorities (206 of them), 2000 repetitions. Both specifications include a constant. Pseudo-R²: (i) 0.2515 and (ii) 0.2675. ***: 1% significance. **: 5% significance. *: 10% significance.

### TABLE 7 — Selection Bias, Part I

<table>
<thead>
<tr>
<th>Variables</th>
<th>(i): FGLS</th>
<th>(ii): FGLS</th>
<th>(ii)-(i)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Est. (SE)</td>
<td>Est. (SE)</td>
<td>Est. (SE)</td>
</tr>
<tr>
<td>Distance (100 Km)</td>
<td>-2.19***</td>
<td>-2.28***</td>
<td>-4%**</td>
</tr>
<tr>
<td>Floorsize (100 m²)</td>
<td>-.034*</td>
<td>-.054**</td>
<td>-56%**</td>
</tr>
<tr>
<td>Floorsize·Distance</td>
<td>.119***</td>
<td>.139***</td>
<td>+16%**</td>
</tr>
<tr>
<td>Floorsize²·Distance</td>
<td>-.004**</td>
<td>-.005***</td>
<td>-25%**</td>
</tr>
<tr>
<td>Controls</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Inverse Mills Ratio</td>
<td>—</td>
<td>.479**</td>
<td>—</td>
</tr>
</tbody>
</table>

\[ E_{it}[MU_{dist|small}] \]

| \[ E_{it}[MU_{dist|big}] \]

Notes: Regression of market-level utility model without [column (i)] and with [column (ii)] selection correction. Number of observations: 813. SEs bootstrapped over town centres, 2000 repetitions. FGLS accounts for correlation across error terms within the same town centre. Controls: Price Index, Floorsize²·Distance, Floorsize·Distance, Retired, Working Class, and People². †Here I loosely refer to \( MU_{dist} \) as to \( \bar{\theta} \). More precisely, \( MU_{dist} = \bar{\theta} / \theta \). The averages of \( MU_{dist} \) are computed across the 65 town centres where big-box supermarkets are present in 2004. ***: 1% significance. **: 5% significance. *: 10% significance.
FIGURE 4 — Selection Bias in Marginal Utility of Distance

Notes: Estimated marginal utility of distance with (solid line) and without (dashed line) controlling for big-box presence. Author’s re-elaboration of estimates reported in top panel of table 7.

<table>
<thead>
<tr>
<th>Variables</th>
<th>(i): FGLS Estimate (SE)</th>
<th>(ii): FGLS Estimate (SE)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Big 2002</td>
<td>.544(.598)</td>
<td>-.272(.661)</td>
</tr>
<tr>
<td>Big 2004</td>
<td>1.518***</td>
<td>.856(.651)</td>
</tr>
<tr>
<td>Small 2004</td>
<td>.792***</td>
<td>.787***</td>
</tr>
<tr>
<td>Price Index</td>
<td>-.011 (.012)</td>
<td>-.011 (.012)</td>
</tr>
<tr>
<td>Distance (100 Km)</td>
<td>-.2***</td>
<td>-.2.026***</td>
</tr>
<tr>
<td>Floor size (100 m²)</td>
<td>.057*</td>
<td>.057*</td>
</tr>
<tr>
<td>Parking Lots</td>
<td>.0009**</td>
<td>.0009**</td>
</tr>
<tr>
<td>Controls</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Inverse Mills Ratio</td>
<td>—</td>
<td>0.7***</td>
</tr>
</tbody>
</table>

Notes: Regression of market-level utility model without [column (i)] and with [column (ii)] selection correction. Number of observations: 813. SEs bootstrapped over town centres, 2000 repetitions. FGLS accounts for correlation across error terms within the same town centre. Controls: Tills, Floorsize·Tills, Distance·Tills, Floorsize²·Distance, Floorsize³·Distance, Floorsize⁴·Distance, Retired, Working Class, and People². ***: 1% significance; **: 5% significance; *: 10% significance.
TABLE 9 — $\Delta$ Consumer Benefit (2001-2004), Overall

<table>
<thead>
<tr>
<th>Benefit Decomposition</th>
<th>NO Selection Correction</th>
<th>WITH Selection Correction</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean (%)</td>
<td>Median (%)</td>
</tr>
<tr>
<td><strong>TOTAL $\Delta$ BENEFIT</strong></td>
<td>+139</td>
<td>+43</td>
</tr>
<tr>
<td>New Big-Boxes (+4.25%)</td>
<td>45.7</td>
<td>1.26</td>
</tr>
<tr>
<td>Old Big-Boxes ($\Delta \xi_{big}$)</td>
<td>8.1</td>
<td>5.01</td>
</tr>
<tr>
<td>Small Stores ($\Delta \xi_{smal}$)</td>
<td>81.1</td>
<td>29.25</td>
</tr>
<tr>
<td>Observables ($\Delta \xi$)</td>
<td>4.1</td>
<td>1.48</td>
</tr>
</tbody>
</table>

Notes: Changes in expected consumer welfare from 2001-2002 to 2003-2004 for a randomly selected household. Measures of welfare are derived from the estimates reported in table 8; “no sample correction” from column (i) and “with sample correction” from column (ii). Intertemporal comparisons of welfare are performed at the household level (in % with respect to the 2001-2002 value), across the 4399 households whose shopping behaviours are observed both in 2001-2002 and in 2003-2004. “Mean” and “Median” are computed across the % measures of the 4399 households. Total changes in welfare are decomposed into their components (e.g., “new big-boxes” is the portion of total change in welfare due to the development of new big-boxes in 2003-2004 in those town centres where there were none in 2002: 17 out of 304).

FIGURE 5 — Distribution $\Delta$ Consumer Benefit (2001-2004), Part I

Notes: Distribution of changes in expected consumer welfare from 2001-2002 to 2003-2004 across all households. All figures illustrate the results reported in table 9. See notes at the bottom of table 9 for further details.
### TABLE 10 — Δ Consumer Benefit (2001-2004), by Distance Group

<table>
<thead>
<tr>
<th>Household’s Group</th>
<th>Benefit Decomposition</th>
<th>NO Selection Correction</th>
<th>WITH Selection Correction</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Near New BB</td>
<td>Far from New BB</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Mean%</td>
<td>Median%</td>
</tr>
<tr>
<td>TOTAL Δ BENEFIT</td>
<td></td>
<td>+760</td>
<td>+48.2</td>
</tr>
<tr>
<td>New Big-Boxes (+4.25%)</td>
<td></td>
<td>373.5</td>
<td>9.4</td>
</tr>
<tr>
<td>Old Big-Boxes (Δξ_{big})</td>
<td></td>
<td>14.3</td>
<td>4</td>
</tr>
<tr>
<td>Small Stores (Δξ_{small})</td>
<td></td>
<td>358.8</td>
<td>34.3</td>
</tr>
<tr>
<td>Observables (Δx)</td>
<td></td>
<td>13.4</td>
<td>1.51</td>
</tr>
<tr>
<td>Households</td>
<td></td>
<td>504</td>
<td>504</td>
</tr>
</tbody>
</table>

Notes: Changes in expected consumer welfare from 2001-2002 to 2003-2004 for households living “nearby” or “far” from newly developed big-boxes. Measures of welfare are derived from the estimates reported in table 8: “no sample correction” from column (i) and “with sample correction” from column (ii). Intertemporal comparisons of welfare are performed at the household level (in % with respect to the 2001-2002 value), across the 4399 households whose shopping behaviours are observed both in 2001-2002 and in 2003-2004. “Near new BB” collects the 504 households living in a Local Authority for which at least one town centre with no big-boxes until 2002 experienced entry of big-boxes in 2003-2004. “Far from new BB” collects the remaining 3895 households. “Mean” and “Median” are computed across the % measures of the households in each distance group. Total changes in welfare are decomposed into their components (e.g., “new big-boxes” is the portion of total change in welfare due to the development of new big-boxes in 2003-2004 in those town centres where there were none in 2002: 17 out of 304).
Notes: Distribution of changes in expected consumer welfare from 2001-2002 to 2003-2004 as a function of households' distance from new big-boxes. All figures illustrate the results reported in table 10. See notes at the bottom of table 10 for further details.
FIGURE 7 — Distribution $\Delta$ Consumer Benefit (2001-2004), Part III

Notes: Distribution of changes in expected consumer welfare from 2001-2002 to 2003-2004. The first column separates households in terms of car ownership. The second column highlights the status of single parents and single pensioners as opposed to everybody else. The first row reports overall means (i.e., means across the whole sample, 4399 households). The second row reports means across the 504 households living “near new big-boxes.” The third row reports means across the 3895 households living “far from new big-boxes.” Welfare measures are obtained from the estimator which controls for big-box presence. All figures illustrate the results reported in table 10. See notes at the bottom of table 10 for further details.
FIGURE 8 — Big-Box Effect on Utility of Small Supermarkets

Notes: Effect of the entry of big-boxes on the market-specific indirect utility of small supermarkets by distance groups. The total sample is the group of 600 town centres which in 2002 did not have any big-box supermarket. “Treatment” is the group of town centres which experienced entry of big-boxes, within their Local Authority, in the period 2003-2004. Results are excerpted from table 11, see notes at the bottom of table 11 for further details.
### TABLE 11 — Diff.-in-Diff.: Effect of Big-Box Entry on Small, Part I

<table>
<thead>
<tr>
<th>Outcome</th>
<th>$E_t[U_{i.small tc}] = \delta_{small tc}$</th>
<th>price$_{small tc}$</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Markets</td>
<td>All Markets</td>
</tr>
<tr>
<td>Post</td>
<td>.651***</td>
<td>.628***</td>
</tr>
<tr>
<td>Treatment</td>
<td>-.964***</td>
<td>-.389***</td>
</tr>
<tr>
<td>Treat·Post (TE)</td>
<td>.929**</td>
<td>.912***</td>
</tr>
<tr>
<td>TE in Km Closer†</td>
<td>—</td>
<td>+9.73</td>
</tr>
<tr>
<td>Controls</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Observations</td>
<td>600</td>
<td>600</td>
</tr>
</tbody>
</table>

Notes: Diff-in-Diff regressions for the effect on the market-specific indirect utility (columns 2 to 5) and on the price index (columns 6 to 8) of small supermarkets of the entry of big-boxes. “All markets” is the sample of town centres which in 2002 did not have any big-box supermarket. “Treatment” is the group of town centres which experienced entry of big-boxes, within their Local Authority, in the period 2003-2004. “NO Big 4” is the subsample of town centres where no big-four supermarket (of any supermarket-format) opened in the period 1997-2004 (“Big 4” is the complement). †This is minus the ratio between TE and the estimated MU$_{dist} = \partial/\partial dist + \theta$; it translates “utils” in terms how much “farther” from home the treated supermarkets should get (in Km) to preserve indifference with respect to 2001-2002. All regressions include a constant. Controls: Price Index, Distance, Floorsize, Parking Lots, Tills, Retired, Density, Hamlet, Working Class, and People$^2$. ***: 1% significance. **: 5% significance. *: 10% significance.

### TABLE 12 — Diff.-in-Diff.: Effect of Big-Box Entry on Small, Part II

<table>
<thead>
<tr>
<th></th>
<th>Store Entry</th>
<th>Store Exit</th>
<th>Store Variety</th>
<th>Floor-space</th>
<th>Product Categories</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post</td>
<td>-.255**</td>
<td>.081**</td>
<td>.343**</td>
<td>4.98***</td>
<td>4.97**</td>
</tr>
<tr>
<td>Treatment</td>
<td>.173 (.167)</td>
<td>.04 (.071)</td>
<td>-.404 (.292)</td>
<td>-1.59 (3.48)</td>
<td>5.24 (4.51)</td>
</tr>
<tr>
<td>Treat·Post</td>
<td>-.399*</td>
<td>-.064 (.097)</td>
<td>-.057 (.397)</td>
<td>-1.75 (4.73)</td>
<td>-5.29 (6.12)</td>
</tr>
<tr>
<td>Controls</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Mean(Outcome)</td>
<td>.78 [n° stores]</td>
<td>.14 [n° stores]</td>
<td>4 [n° diff. stores]</td>
<td>44.36 [1000 ft$^2$]</td>
<td>104.7 [n° categ.]</td>
</tr>
<tr>
<td>Observations</td>
<td>600</td>
<td>600</td>
<td>600</td>
<td>600</td>
<td>599</td>
</tr>
</tbody>
</table>

Notes: Diff-in-Diff regressions for the effect of big-box entry on the market-specific outcomes of small supermarkets. Estimation is performed on the sample of town centres which in 2002 did not have any big-box supermarket. “Treatment” is the group of town centres which experienced entry of big-boxes, within their Local Authority, in the period 2003-2004. “Store entry” is the number of new small stores developed in the previous two years. “Store exit” is the number of small stores which closed down in the previous two years. “Store Variety” is the number of small stores with different names. “Floor-space” is the sum of the floorsizes of all small stores. “Product Categories” is the number of different product categories purchased from small stores. All regressions include a constant. Controls: Price Index, Distance, Floorsize, Parking Lots, Tills, Retired, Density, Hamlet, Working Class, and People$^2$. ***: 1% significance. **: 5% significance. *: 10% significance.
### TABLE A1 — Timing of Supermarket Entry (post-TCF)

<table>
<thead>
<tr>
<th>Year of First Entry</th>
<th>( y_{jtc}^{2002} = 0 )</th>
<th>( y_{jtc}^{2002} = 1 )</th>
<th>( y_{jtc}^{2004} = 0 )</th>
<th>( y_{jtc}^{2004} = 1 )</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>1997</td>
<td>33</td>
<td>10</td>
<td>30</td>
<td>13</td>
<td>12.22%</td>
</tr>
<tr>
<td>1998</td>
<td>39</td>
<td>13</td>
<td>37</td>
<td>15</td>
<td>14.77%</td>
</tr>
<tr>
<td>1999</td>
<td>32</td>
<td>9</td>
<td>31</td>
<td>10</td>
<td>11.65%</td>
</tr>
<tr>
<td>2000</td>
<td>76</td>
<td>11</td>
<td>71</td>
<td>16</td>
<td>24.72%</td>
</tr>
<tr>
<td>2001</td>
<td>75</td>
<td>6</td>
<td>73</td>
<td>8</td>
<td>23.01%</td>
</tr>
<tr>
<td>2002</td>
<td>16</td>
<td>1</td>
<td>16</td>
<td>1</td>
<td>4.83%</td>
</tr>
<tr>
<td>2003</td>
<td>14</td>
<td>0</td>
<td>12</td>
<td>2</td>
<td>3.98%</td>
</tr>
<tr>
<td>2004</td>
<td>17</td>
<td>0</td>
<td>17</td>
<td>0</td>
<td>4.83%</td>
</tr>
</tbody>
</table>

Notes: IGD data. Each of the 352 town centres is associated to the year in which, in the post Town Centre First era, the first supermarket (any floorsize) was observed to enter. The second (fourth) and third (fifth) columns divide town centres among those which were observed to have big-boxes by the end of 2002 (2004) \([i.e., y_{jtc}^{2002} = 1]\) and those that were not \([i.e., y_{jtc}^{2002} = 0]\). The sixth column is computed over the sum of columns two and three (or, equivalently, four and five).

### TABLE A2 — Supermarket Characteristics (2003-2004)

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>Big-Box Stores (65 Obs.)</th>
<th>Small Stores (352 Obs.)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean</td>
<td>SD</td>
</tr>
<tr>
<td><strong>Price Index</strong></td>
<td>4.41</td>
<td>.813</td>
</tr>
<tr>
<td><strong>Distance (100s Km)</strong></td>
<td>1.96</td>
<td>.484</td>
</tr>
<tr>
<td><strong>Floorsize (100 m^2)</strong></td>
<td>40.08</td>
<td>10.48</td>
</tr>
<tr>
<td><strong>Parking Lots</strong></td>
<td>500.24</td>
<td>176.43</td>
</tr>
<tr>
<td><strong>Tills</strong></td>
<td>25.87</td>
<td>5.35</td>
</tr>
</tbody>
</table>

Notes: Descriptive statistics about supermarket characteristics, separately for big-boxes and for small supermarkets.
Source: IGD, ODPM, and ONS data.

<table>
<thead>
<tr>
<th>Variables</th>
<th>(i) OLS</th>
<th>(ii) 2SLS</th>
<th>(iii) 2SLS</th>
<th>(iv) OLS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Est. (SE)</td>
<td>Est. (SE)</td>
<td>Est. (SE)</td>
<td>Est. (SE)</td>
</tr>
<tr>
<td>Big 2002</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>.654 (.516)</td>
</tr>
<tr>
<td>Big 2004</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>1.703***</td>
</tr>
<tr>
<td>Small 2004</td>
<td>—</td>
<td>—</td>
<td>—</td>
<td>.783***</td>
</tr>
<tr>
<td>Price Index</td>
<td>-.031***</td>
<td>-2.3***</td>
<td>-.348***</td>
<td>-.01 (.008)</td>
</tr>
<tr>
<td>Distance (100 Km)</td>
<td>-2.04***</td>
<td>-2.06***</td>
<td>-2.08***</td>
<td>-1.994***</td>
</tr>
<tr>
<td>Floorsize (100 m²)</td>
<td>.096***</td>
<td>.122***</td>
<td>.145***</td>
<td>.063**</td>
</tr>
<tr>
<td>Controls</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
</tbody>
</table>

Notes: Regression of market-level utility model without selection-correction. Column (i) is the OLS regression of δ_{f,t} on observable characteristics of both supermarket-format and town centre. Columns (ii) and (iii) are the 2SLS regressions of δ_{f,t} on observable characteristics in which the instrumented variable is Price Index. Column (iv) is the OLS regression of δ_{f,t} on observable characteristics and the dummies: Big 2002, Big 2004, and Small 2004. Number of observations: 813. Instruments in (ii) [à la Hausman (1996)]: Average and SD of Price Index of same format (i.e., big-box or small) across town centres. Instruments in (iii) [à la BLP (1995)]: Big 2002, Big 2004, and Small 2004. Controls: Parking Lots, Tills, Floorsize·Tills, Distance·Tills, Floorsize²·Distance, Floorsize³·Distance, Retired, Working Class, and People². ***: 1% significance. **: 5% significance. *: 10% significance.

## TABLE A4 — Diff.-in-Diff.: Effect of Big-Box Entry on Small

<table>
<thead>
<tr>
<th>Outcome</th>
<th>Ethnic Food Expenditure</th>
<th>Ambient Meat and Veg. Extracts Price</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Markets</td>
<td>NO Big 4</td>
</tr>
<tr>
<td>Post</td>
<td>6.57***</td>
<td>6.28*</td>
</tr>
<tr>
<td>Treatment</td>
<td>.219 (4.07)</td>
<td>5.95 (6.52)</td>
</tr>
<tr>
<td>Treat·Post</td>
<td>12.64**</td>
<td>22.51**</td>
</tr>
<tr>
<td>Controls</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Mean(Outcome, £)</td>
<td>16.39</td>
<td>16.12</td>
</tr>
<tr>
<td>Observations</td>
<td>464</td>
<td>180</td>
</tr>
</tbody>
</table>

Notes: Diff-in-Diff. regressions for the effect of big-box entry on the market-specific expenditure on ethnic food (columns 2 to 4) and on the price of ambient meat and vegetable extracts (columns 5 to 7) in small supermarkets. “Ethnic food” is, for example: Chinese, Ethiopian, Indian, Mexican, Polish, and Thai. “Ambient meat” is processed meat that can be stored without refrigeration for relatively longer periods. “Vegetable extracts” are highly concentrated forms of mixed vegetables rich of antioxidants. “All markets” is the sample of town centres which in 2002 did not have any big-box supermarket. “Treatment” is the group of town centres which experienced entry of big-boxes, within their Local Authority, in the period 2003-2004. “NO Big 4” is the subsample of town centres where no big four supermarket (of any supermarket-format) opened in the period 1997-2004 (“Big 4” is the complement). All regressions include a constant. Controls: Price Index, Distance, Floorsize, Parking Lots, Tills, Retired, Density, Hamlet, Working Class, and People². ***: 1% significance. **: 5% significance. *: 10% significance.