Comments on “Private Labels and Retailer Profitability: Bilateral Bargaining in the Grocery Channel” by Paul Ellickson, Pianpian Kong, and Mitch Lovett

Over the past decades, the question whether a power shift has occurred in the retail channel has been central to both the academic literature and managerial practice (Messinger and Narasimhan 1995; Ailawadi 2001). The consolidation of grocery chains and the associated rise of store brands (private labels), in particular, has been blamed for the increasing dominance of retailers vis-à-vis manufacturers. Retailers earn much higher margins on private labels, they can potentially expand a category by appealing to more price-sensitive consumers, and in addition, they can build up brand equity and become an effective competitor to nationally branded products. This latter aspect of private labels has the potential to influence a retailer’s ability to bargain with manufacturers over the prices of their products and extract higher margins. Previous research has explored some of these possibilities. Chintagunta, Bonfer, and Song (2002) find that manufacturers are more “accommodating” after a store brand entry. Pauwels and Srinivasan (2004) find higher unit margins for both store brands and national brands but no evidence of category expansion or store traffic increase. Meza and Sudhir (2010) show that wholesale prices of imitated national brands decrease, thus affording more power to the retailers relative to the manufacturers of these brands.

Ellickson, Kong and Lovett (2017) add to our understanding of the role of private labels in the channel by developing and estimating a structural bargaining model for the single-serve cup coffee category. The framework builds on a string of recent papers that integrate bargaining into a demand-and-supply model of the retail channel interactions (e.g., Misra and Mohanty 2006; Draganska, Klapper, and Villas-Boas 2010). Manufacturers conduct bilateral bargains with retailers over wholesale prices, taking retail prices as given. Retailers, assumed to be local
monopolists, set the retail prices, and finally consumers make their purchase decisions. In what follows, I highlight aspects of the paper that readers and future researchers should take note of, while also providing some suggestions and discussion about some specific features of the model and estimation strategy.

The true power of the bargaining model used by the authors is its ability to decompose the profit gain that retailers experience from introducing a private label. The model accounts for the potential direct profitability increase through shifting sales from lower-margin national brands to the higher-margin private label. Additionally, it allows the authors to determine whether a private label entry shifts power in the retail channel. A private label could strengthen the retailer’s ability to bargain with manufacturers or it can affect its bargaining position vis-a-vis manufacturers by improving the retailer's disagreement profit – after all, if the negotiations fail and then the retailer does not carry the national brand, then consumers may substitute to the private label. The estimates show that the latter is precisely what happens.

Although the nature of interaction in the channel and its implications for pricing and assortment decisions is of utmost interest, empirical studies have been hindered by the lack of data. Ideally, we would want to have access to the actual vertical contracts, specifying wholesale prices along with any discounts, allowances, etc. Unfortunately, this is very rarely the case (some notable exceptions are Sudhir and Rao 2006; Conlon and Mortimer 2014). Absent such data, we need to rely on the structure provided by a model to recover wholesale prices (assuming away discounts, allowances and other more complex contracts) and to identify parameters of interest from data that is typically available, namely information on demand, marketing mix, and some cost shifters.
Because relatively little is known about how bargaining between manufacturers and retailers actually takes place, there are multiple possible models that one might write down and take to data. For example, one alternative to Ellickson, Kong, and Lovett (2017) is Draganska, Klapper, and Villas-Boas (2010), which assumes that retail and wholesale prices are set simultaneously and then shows how this formulation nests two other specific models—the Vertical Nash model and the Manufacturer Stackelberg models that are widely used in the marketing literature. The current paper, in contrast, assumes that the retailer has the ability to set prices after negotiations with the manufacturer. Furthermore, following Grennan (2013) the econometric error is assumed to stem from bargaining as opposed to the manufacturer’s cost as in Draganska, Klapper, and Villas-Boas (2010). Exploring how these modeling choices affect the results may be a fruitful avenue for future research.

Single-serve cup coffee was originally developed by Keurig and protected by a patent until 2012, thus making the entry of private labels impossible prior to patent expiration. A particularly nice feature of the paper is that the authors’ data covers both the periods before and after the private label introduction. This provides a useful source of variation to investigate the impact of entry on retail profitability and bargaining power. Treating this variation as a quasi-experimental design requires some unconfoundedness conditions. In this case, the assumption is that retailers do not have beliefs about the timing and/or expected gains from patent expiration which might alter their pre-expiration strategy. Evaluating these pre-expiration strategies (e.g., pricing) could help to alleviate these concerns. Since patent expirations are not sudden and their date is well known to market participants, those retailers who expect to gain most might introduce private labels to the market earlier.
One test the authors provide evaluates whether changes due to private label introductions are arising from the retailers’ bargaining ability or the retailers’ bargaining leverage relative to those of the manufacturers’. Estimates of the model that allow the bargaining ability (also referred to as the bargaining power parameter) to change with patent expiration reveal no significant differences. That is, there is no evidence of a structural break associated with the patent expiration in 2012. This is a nice sanity check, as the authors point out, because although we observe an improved bargaining position of the retailers in the presence of a private label, we would not expect a sudden improvement in their bargaining ability. Because the bargaining position is a complex function of demand primitives and observable variables, this provides further evidence for why the application is not well-suited to a descriptive “discontinuity” approach to estimate the effect of private label entry on bargaining outcomes.

As with any structural model, one of the fundamental modeling decisions is how much institutional detail to incorporate in order to capture all essential features of the data, while keeping the model parsimonious. The setting the authors deal with is complex--consumers in the single-cup coffee market need to own the Keurig brewer (there is a hardware requirement). Moreover, Keurig did not launch nationally at the same time but introduced brands gradually, so there was variation in the availability of brewers, and hence demand for single-serve capsules, across RMAs. Single-serve cups are a disruptive innovation and it is not entirely clear what products they are replacing (if any).

The authors decided to model demand for the entire coffee category because of concerns that there might be substitution between single-serve cup coffee and other in-home brewed coffee. I am wondering whether there is evidence that single-serve cups were indeed viewed as a substitute for other in-home brewed coffee. It is entirely possible that it was more of an add-on
purchase. The authors find that substitution is not very meaningful between the two. As a result, choosing a narrower market definition may allow the authors to reduce the computational burden and make fewer assumptions in other parts of the framework.

One area where the study could benefit from a narrower definition is related to the definition of prices. Like most studies in marketing, prices are set to a common standard, the price per cup of coffee. While this pricing matches closely to pricing in the single-cup subcategory, in the other coffee segments (e.g., instant) units are not as closely connected to a per cup price, which might cause problems for estimating price elasticities for these other segments.

Next, the authors consider the installed base and availability issues that may affect their demand estimation. They have excellent data on distribution (ACV) across RMAs, which allows them to apply the approach of Bruno and Vilcassim (2008) and account for the differential availability of single-serve cups. However, they have comparatively less data on the sales and installed base of Keurig brewers. This is a classic initial conditions problem that plagues these types of markets. For example, when Hartmann and Nair (2010) looked at razors and blades, they assumed anyone observed to purchase a blade prior to a razor already had a razor. This paper takes this problem more seriously by imputing the missing information and estimating the installed base. However, to do so requires some additional calibration and assumptions. In particular, the authors fit a Bass diffusion model using four years of data, which is then adjusted upwards by 3 percentage points to account for the fact that some brewer owners may not purchase from a given retailer. Finally, the estimated annual installed base is interpolated to obtain quarterly, and then weekly levels using sales of brewers. All assumptions made by the authors are plausible, however, I do wonder whether the additional benefit from considering the installed base merits the cost (and the potential error introduced with every set of computations). Since Keurig was the sole
manufacturer of the brewer and single-serve coffee cups prior to 2012, I would expect that they would adjust distribution to different RMAs based on their knowledge of brewer penetration in the region. ACV and installed base should be thus highly correlated, which can be observed directly in their data. Examining this correlation could give insight about such behavior and might motivate another specification check.

Given the data limitations, the authors probably go as far as they can in recognizing the institutional features of the single-serve coffee market. Keurig is special amongst the manufacturers included in the model in that it produces both the machines and the cups. The authors carefully account for the licensing agreements between Keurig and other manufacturers but not for the profits that Keurig makes from selling the machines. This seems warranted if Keurig adhered to a razor-and-razor blades model, with thin margins on the machines. However, this may change with the advent of third-party cups. The authors provide evidence that Keurig's pricing of the machines remained unchanged over the sample period to alleviate such concerns.

On the other hand, Keurig introduced a variety of new brewers in subsequent years, possibly in an attempt to cash in on the richer offerings of single-serve coffee cups (and an attempt to channel demand to licensed cups only). But if Keurig gained from private label entry through additional sales of brewers, this changes its payoffs in the bargaining process. While beyond the scope of the current research question, future research might consider expanding the inquiry to consider the potential feedback loop in which additional cup sales lead to the more widespread adoption of brewers.

The authors assume monopolistic retailers because of a lack of data. Although they allow the brand intercepts to change every six months, which in principle can account for a significant amount of competitive effects, the monopolistic retailer assumption likely leads to an
overstatement of retail markups and a commensurate understatement of wholesale prices. The bargaining model aims to rationalize these implied wholesale prices. Given the nonlinearity of the model, it seems unlikely that the parameter estimates are invariant to a level shift in wholesale prices. Future work should therefore seek to explore to what extent the shift in bargaining position will occur in the presence of retail competition.

In summary, this paper highlights the role of private label introductions in determining the bargaining power split between retailers and manufacturers. The chosen application has the advantage of a sharper change in private label entry relative to settings where patent protections do not exist. The findings suggest that accounting for bargaining is important, that the bargaining position, not the bargaining power shifts in response to private label entry, that the bargaining benefits are meaningful, reaching 20% of the overall benefit of private label entry, and that the gains from private labels vary widely between retailers. Hopefully, this paper and the comments herein motivate more people to work on the role of private labels in bargaining power and push the research frontier by developing models and relaxing assumptions to reduce the distance between modeling and practice.

References


