A new approach to estimating state dependence in consumers' brand choices applied to 762 pharmaceutical markets

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Abstract: This article shows how state dependence effects can be estimated for many markets and with few assumptions by using data on how the shares buying specific products differ between those who bought the same product on their latest purchase occasion and other consumers. Using as instrument information regarding which product was cheapest when consumers made their last purchase, I estimate that state dependence increases the probability that consumers will buy the product they bought the last time by 8 percentage points. This effect is larger for women and the elderly than for men and younger consumers. The state dependence effect is also larger for brand-names than for generic products, but not significantly related to number of previous purchases.

Keywords: Brand choice, Consumer dynamics, Drugs, Quasi-experiment econometrics, State dependence

JEL codes: D12, D90, I11, L65

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1. Introduction

To understand the incentives facing firms in markets with repeated purchases, it is essential to have knowledge about state dependence—that is, how a consumer's current choice is affected by his/her latest choice. State dependence gives firms incentives to temporarily set prices low, potentially even below marginal cost, and to harvest the increased demand generated by this in periods with high prices. Therefore, state dependence increases price variability, and it typically also increases mean prices (MacKey and Remer, 2019).[1] State dependence can also affect firms' entry decisions because it increases the first mover advantage.

State dependence can arise from brand loyalty, habit formation, inattention, and switching and search costs (Yeo and Miller, 2018). It is also related to risk aversion because consumers often perceive that they have better knowledge of the quality of products they have used before and therefore view these products as less risky than other alternatives. State dependence can cause substantial welfare losses, but in the presence of other market failures, it might increase welfare (Handel, 2013). From a policy perspective, knowledge about state dependence is useful, for example, when considering information campaigns, choice regarding default options, or changing market rules that affect state dependence. State dependence is also important in merger analysis because long-term price elasticities can be seriously underestimated when state dependence is ignored (Osborne, 2011) and because the dynamic price effect caused by state dependence affects how mergers influence prices (MacKey and Remer, 2019).

To identify state dependence, one needs to control for both fixed and serially correlated heterogeneity in consumers' preferences (Heckman, 1981),[2] where the latter can be caused by, e.g., information or marketing just reaching part of the consumers. Roy, Chintagunta and Haldar (1996), Keane (1997), and Seetharaman (2004) are examples of prominent studies that use functional form assumptions about the

[1] As shown by Dubé, Hitsch, and Rossi (2010), Jansen (2019), and others, prices can in some market environments decrease because of state dependence.
[2] In this article, "state dependence" is used to denote what Heckman (1981) calls "structural state dependence." Heckman calls inertia arising from heterogeneity "spurious state dependence."

nature of heterogeneity to disentangle unobserved heterogeneity and state dependence. To identify state dependence, researchers have also used variation over time in available alternatives (e.g., Goldfarb, 2006) or in real or perceived attributes of the alternatives. Examples of the latter include variations caused by price changes (e.g., Dubé, Hitsch, and Rossi, 2010, and Handel, 2013) and advertising (e.g., Terui, Ban, and Allenby, 2011). In this article, I use instruments to disentangle unobserved heterogeneity and state dependence.

The main purpose of this article is to test if state dependence affects consumer choices among medically equivalent pharmaceuticals using a new estimation approach. To this end, I use register data on all exchangeable prescription pharmaceuticals bought by residents of the county of Västerbotten, Sweden, from January 2014 through April 2016. That the data cover the entire population is an advantage because selection bias, which could be a problem when participation in the data generating process is voluntary, is avoided.

At Swedish pharmacies, consumers are able to choose among different brands of products that are listed as exchangeable.[3] Pharmacies are required to inform consumers if a cheaper substitute is available and that they have to pay the full price difference if they want to buy the prescribed product. There is a lot of variation in which brands are cheapest in their exchange group, and this, together with the substitution regulation, creates massive variation in market share across time. In the estimations, I utilize this variation and that there within exchange groups in a given month is variation across consumers in which month they made their last purchase.

First, I show that having the lowest price increases the market share of a product by up to 70 percentage points. Together with consumer inflexibility in month of purchase, this creates exogenous variation in consumption experience. Then I present evidence showing that consumers are more likely to buy the

[3] A "product" is defined as a unique combination of substance, form of administration, strength, and package size sold by a specific firm. "Drug" is used as a synonym for exchange groups and includes products with the same combination of active substance, form of administration, strength, and nearly identical packet size. Because the main difference between products within an exchange group usually is the name and the identity of the marketing pharmaceutical firm, I use "brands" as a synonym for different products within exchange groups.

brand that was cheapest the last time they bought the drug, compared to other consumers facing the same choice set but having made their latest purchase of the drug during a month with different prices. This is evidence in favor of state dependence.[4] Lastly, as instrument I use information about which product was cheapest when consumers made their previous choice and estimate the causal effect of the previous choice on the current choice. The results show that state dependence, on average, increases the probability that a consumer bought the product he/she bought last time by 8 percentage points.

By using this quasi-experimental approach to identify state dependence, this article relates to Ericson (2014), who used an experiment in which plans for the Medicare Part D Low-Income Subsidy Program were randomly assigned. It also relates to Coscelli (2000) and Iizuka (2012) and more closely to Feng (2018) and Janssen (2019), who all find inertia in choices among pharmaceuticals. Coscelli (2000) used patients' switching of physicians to conclude that inertia at patient level contributed to patients repeatedly being prescribed the same drug, although therapeutically equivalent drugs exist. Iizuka (2012) also analyzes prescribers' choices, but between brand-name and generic pharmaceuticals. Feng (2018), using U.S. data, and Janssen (2019), using Swedish data, analyze how state dependence affects which pharmaceutical is bought. Feng (2018) uses variation in availability of alternative when patients started a treatment as instruments to study state dependence in choices between therapeutic alternatives and between brand-name and generic pharmaceuticals on reduced form. He shows that during the first quarters after generic entry, the fraction using any generic version was about 8 percentage points larger among those who started their treatment after the first generic entry, compared to those who started their treatment earlier. Feng also studies choices between therapeutic alternatives on structural form. Janssen (2019) uses a similar approach as Feng and instruments the first choice between any generic or any brand-name product with the first choice being made after generic entry. He then shows that having bought a generic the first time significantly increases the probability of purchasing a generic within the next three months for 21 of the 22 studied exchange groups, with the median estimate being a 52% increase in this probability. In addition,

[4] Also Dubé, Hitsch and Rossi (2013) and others have found that past prices predict current choices and interpret this as evidence for state dependence.

for one exchange group, Janssen use a structural method to analyze the choice between individual products, using income, educational level, and the choice at the first purchase occasion as controls for persistent heterogeneity across consumers. His estimates suggest that having bought a product at the latest purchase occasion increases the probability that it will be chosen again as much as a 10% to 28% price reduction would do.[5]

This article contributes to the existing literature primarily in three ways. First, I demonstrate a method to estimate state dependence effects using market shares that are separated based on the consumers' last purchase. In a market with n available product and n products bought by consumers at their previous purchase occasion, there are n^2 such market shares per time period compared to n "ordinary" market shares. The method is convenient when the purpose is to study many markets because it does not require specifying choice and consideration sets for each market and time period. Compared to methods used to study state dependence using ordinary market shares, it requires far less restrictive assumptions and can therefore also be a good alternative when individual level data cannot be accessed, e.g., for integrity reasons.

A second contribution is that the article separates state dependence for individual pharmaceutical brands from heterogeneity that cannot be captured by observables. Doing this complements the work by Feng (2018) and Janssen (2019) by, instead of producing reduced form evidence on state dependence in the dichotomous choice between the brand-name product and any generic version, estimating state dependence effects on the product level. Relative to Janssen's structural estimation, the contribution of this article

[5] Also Ching (2010), Granlund and Rudholm (2012), Granlund and Sundström (2018) and Ching, Granlund and Sundström (2019) analyze choices between medically equivalent pharmaceuticals, but none of them studies state dependence. Ching (2010) uses aggregate U.S. data to study learning, which is one possible explanation of state dependence, and reports that learning partly explains why generic market shares increase over time. The other three studies use Swedish prescription level data. Granlund and Rudholm report that consumers are more likely to pay extra to get the prescribed product if it is a brand name or branded generic. Granlund and Sundström study how consumers' welfare is affected by which brand is prescribed, and Ching, Granlund and Sundström study whether consumers who can get the cheapest product for free make different choices than other consumers who face the same price differences but must pay strictly positive amounts for all products. The two latter articles both control for previous purchases using "GL-terms," which takes higher values the more often a consumer has bought the brand and put higher weights on more recent purchases. The estimates for the GL-terms indicate that consumers obtain positive utilities by repeatedly consuming the same product.

includes that it analyzes state dependence on the product level for a large set of drugs (762 exchange groups) and uses instruments to separate state dependence from inertia caused by uncontrolled heterogeneity. Knowledge about state dependence on product level is important for understanding the incentives of generic firms, for example, the incentives for an early entry. Also, the result that state dependence exists also on the product level is important to consider when market rules and insurance policies are decided because it suggests that the utility of consumers is negatively affected by variation in which generics are available and in the prices of individual generic products.

A third contribution of this article is that it adds to the existing limited knowledge about how state dependence varies across subpopulations. One advantage of using data on prescription pharmaceutical is that it identifies the individual consumer, not just to which household the consumer belongs. This facilitates the analyses of differences across demographic groups and also implies that “spurious variety seeking,” caused by purchases for other household members or guests, will not affect the estimates. The results show that the state dependence effect is larger among women and the elderly than among men and younger consumers and larger for brand-name products than for generics. Possible explanations of these observed differences are discussed in the results section.

The article is organized as follows. Section 2 describes the Swedish generics market, and Section 3 presents the data sources and exclusion restrictions. In Section 4, I present evidence of how much being the cheapest product affects the market share of a product as well as descriptive statistics for variables used to estimate this. Section 5 discusses the empirical methods used to estimate state dependence effects and presents related descriptive statistics, whereas the results from these analyses are presented in Section 6. Section 7 concludes the article, and robustness checks are presented and discussed in the appendix.

2. The generics market

In Sweden, physicians prescribe specific products, identifying also the pharmaceutical firm, but at the pharmacies, consumers are able to choose between products within exchange groups. The exchange groups consist of products with the same combination of active substance, form of administration, strength and packet size.[6] Thus, consumers choose between bioequivalent products/brands, but the products can include different inert ingredients and differ in color and shape.

All Swedish residents are covered by a mandatory and uniform pharmaceutical benefit scheme in which the coinsurance rate is a decreasing function of pharmaceutical cost included in the benefit that reaches zero when these costs exceed SEK 5,400 during a 12-month period. In addition to the coinsurance rate, consumers who choose a different product than the cheapest substitute, the "product of the month," have to pay extra.[7] Pharmacies are required to inform consumers if a cheaper brand is available and of the possibility to choose other substitutes than the cheapest one. The obligation is waived if the physician indicated on the prescription that substitution is not allowed for medical reasons or if the pharmacist had reason to believe that the consumer would be adversely affected , e.g., because the low-cost alternative had a package that was difficult to open for the consumer. If consumers oppose substitution and buy the prescribed product, the entire extra cost will be charged to them, and the extra cost will not be included in the accumulated pharmaceutical cost that determines the coinsurance rate. This is also the case if the pharmacy does not have the cheapest one in stock and the consumer chooses to buy another product rather than coming back when the pharmacy have received the product or going to another pharmacy (Ministry of Health and Social Affairs, 2009). However, if the consumer chooses another product than the prescribed one or the cheapest one, despite the cheapest one being available at the pharmacy, no part of the cost should be paid by the pharmaceutical benefits scheme (The Swedish Parliament, 2009). In the data used for

[6] Packet size is allowed to vary slightly; for example, substitution can be made from a 30-pill package to a package in the 28-32-pill range.

[7] The product of the month is the cheapest product within the exchange group that is guaranteed by the pharmaceutical firm to be available in Sweden throughout the month. In the first two months with generic competition, no product is declared to be product of the month.

this study, consumers choose a third alternative in less than 1% of the purchases, perhaps reflecting that consumers who prefer a specific product often ask the physician to prescribe that product and no cost is included in the pharmaceutical benefits scheme for 30% of these purchases.

Firms wanting their product to be included in the pharmaceutical benefit scheme must submit their price bids for month m to the Dental and Pharmaceutical Benefits Agency (DPBA) in month $m-2$. Firms bid in prices that are uniform across Sweden and include transport to the pharmacies. Prices not exceeding the highest price within the exchange group the previous month are always approved by the DPBA. During month $m-1$, the DPBA announces all purchase prices and the retail pharmacy prices, which are set with a simple algorithm that adds to the purchase price a margin that is continuously increasing in the pharmacy purchase price. At the same time, the DPBA also announces which products have the lowest price per pill in their exchange groups and hence should be sold without additional costs to consumers. To allow pharmacies to clear excess inventory, pharmacies are also allowed to sell the product that was cheapest the previous month during the first 15 days of the month without additional cost to the consumer (Dental and Pharmaceutical Benefits Agency, 2009).[8]

3. Data

This study is based on a panel data set obtained by merging a purchase data set, provided by the County Council in Västerbotten, with data sets compiled by the Dental and Pharmaceutical Benefits Agency that contain information about which exchange groups the products belong to. The purchase data set includes all prescribed pharmaceuticals that are dispensed to inhabitants of the county of Västerbotten from January 2014 through April 2016. The first purchase and subsequent refills are separate observations, and the total number of observations amounts to over 7.5 million.

[8] These products should then be sold at the price for month $m-1$. After day 15 in month m, pharmacies are still allowed to sell the product that was cheapest in month $m-1$ without additional cost to the consumer if the purchase price is SEK 300 or less, but then the pharmacies must sell these products at the purchase price (Dental and Pharmaceutical Benefits Agency, 2009).

First, I exclude nearly half of the purchases that are for drugs that do not belong to any exchange group, including all on-patent drugs. I also exclude drugs that can be bought without prescriptions because it is likely that I do not observe all purchases of such drugs. Then I exclude dispenses to children and adolescents below the age of 18 because their parents can collect their pharmaceuticals, implying that the identity of the individual choosing between the exchangeable products is unknown. For similar reason, 40% of the remaining dispenses that are for individualized dosage bags are excluded because a significant fraction of these might be collected by someone else, e.g., home-help staff. Individualized dosage bags are for consumers who need help making sure they take the right drug at the right time, and 73% of these bags are dispensed to consumers 75 years old or older. I also exclude 1% of the remaining dispenses that are for regular packages, but for consumers with one or more prescription for individualized dosage bags in the same exchange group. Finally, I drop 0.1% of the remaining observations that are repurchases or purchases that are repurchased the same day and 0.1% that lacks the consumer identifier. After this, 2,348,351 dispenses of 4,224 products in 1,152 exchange groups to 147,150 individuals remain.

4. The importance of the product of the month status

To estimate how being the product of the month affects the market share of a product, I aggregate the data to one observation per product and half month. Months are divided in halves, with the break after day 15, because of the possibility of selling out the cheapest product of the previous month during the first 15 days. After dropping 3% of the observations from exchange groups in which no product is declared to be the product of the month[9] and adding observations for available products with no sales,[10] the final population consists of 110,387 observations. Using these, I estimate the following reduced form equation:

[9] These excluded observations account for 1.5% of the packages sold. The reason why no product is declared to be the product of the month is that it is the first two months after generic entry or that no seller has guaranteed that their product will be available for the entire month.

[10] Products are considered available if at least one ordinary package is sold to an adult inhabitant of the county during the current month, even if no package is sold during the current half month.

$$Share_{jh} = \sum_{h=1}^{2} \left[\beta_1^h PM_{jm} + \beta_2^h PM_{j,m-1} + \beta_3^h R1_{jm} + \beta_4^h R2_{jm} + \beta_5^h Inv\_N_{em}\right] + \mu_j + \varepsilon_{jh}. \quad (1)$$

The dependent variable is the percentage market share, defined as 100 times the number of packages sold of product *j*, in half month *h*, divided by the total number of packages sold within its exchange group that half month.

The variable PM_{jm} takes the value one if product *j* is the only product of the month in its exchange group and the value zero if it lacks this status. If there is a tie, PM_{jm} takes the value one over the number of *products of the month* within the exchange group the current month. This implies that PM_{jm} is a relative measure of the "attraction" of product j, which is desired because the market share theorem states that a market share is equal to the attraction of product *j* relative to the sum of all attractions within the market (Cooper and Nakanishi, 1996). In this population, PM_{jm} equals 1 for 31% of the observations, one-half for 5%, one-sixth to one-third for less than 0.5%, and 0 for 64% of the observations. The variable $PM_{j,m-1}$ is the lag of PM_{jm}.

Note that if β_2^1 exceeds β_2^2, as is expected, β^1 must be less than β^2 for some of the other variables. Otherwise, the sum of the predicted market share cannot simultaneously sum to one for both the first and the second halves of a month. For this reason, I allow the parameters for the first five variables to take different values for the first and second halves.

The variable $R1_{jm}$ ($R2_{jm}$) takes the value one if the product is the first (second) reserve. That is, $R1_{jm}$ equals one for the first runner-up, which implies that pharmacies can sell this product without additional cost to the consumers if the product(s) of the month is no longer available in Sweden. Like PM_{jm}, $R1_{jm}$ and $R2_{jm}$ assume fractional values if the status is shared with other products in the exchange group and take the value zero for products not having this status. For 29% (50%) of the exchange groups, no product is declared to be the first (second) reserve of the current month. The most common reason for this is that all products in the exchange group are products of the month (or the first reserve).

The variable Inv_N_{em} is defined as one over the number of products in exchange group e, month m. Finally, μ_j are product fixed effects. In the estimations, I use the average numbers of packages per observation within exchange groups as weights and employ two-way clustering that allows the error terms to be correlated within products and within exchange-groups half-month combinations.[11]

Table 1 presents descriptive statistics for all but the first month in the data set, which is not used in the regression because data on $PM_{j,m-1}$ are missing. The mean values for $Share_{jh}$ and PM_{jm} both reflect that there on average are four available products within an exchange group. This, together with the distribution of the number of products, gives a mean for Inv_N_{em} of 0.25. The mean of $PM_{j.m-1}$ is 0.23, whereas the mean for PM_{jm} is 0.25, because for some exchange groups, no product was the *product of the month* the previous month and the previous month's *product of the month* is not available for some other exchange groups.

Table 1 about here

That firms can prefer to set lower prices when they face less demand can bias the estimators related to the four first variables, but for two reasons, these biases are expected to be small. First, firms have incentives to randomize prices to prevent competitors from marginally undercutting their prices, and this gives large price variations even for products with stable demand.[12] Two figures that illustrate these large price variations are that 68% of generics have a different price than the preceding month and that the average absolute value of the price changes is 35%. Such frequent and large price changes would not be expected if price changes were mainly driven by changes in demand. Second, the biases are also reduced by the fact that the prices are required to be uniform across Sweden, whereas the error terms come from regression

[11] The estimations presented in this article are done using the STATA package reghdfe and ivreghdfe by Correia (2017; 2018).

[12] For firms that sell homogenous products and have the same constant marginal cost and strictly positive fixed costs, no competitive equilibrium in pure strategies exists. Instead, firms must choose prices so that they cannot be predicted by their competitors. As shown in this article, not all consumers consider products within exchange groups to be perfect substitutes, and some products, primarily brand-name products, have enough loyal consumers to choose a relative high and stable price rather than trying to become the cheapest product. Still, for most of the products that sometimes, but not always, are the product of the month—i.e., for those that identify β_1^h and β_2^h—randomization is likely the best strategy.

based on purchases by inhabitants of one county, the population of which is only 2.7% of the Swedish population. Because the biases are expected to be small, I expect the mean squared error to be much lower when these four variables are treated as exogenous variables, compared to if they are instrumented with available instruments such as prices in other markets. For this reason, I present only results obtained when treating these variables as exogenous.

As comparison to the results from fixed effect estimations of Equation 1, I also present results from a one-month difference estimation.[13] The difference estimator can be even less affected by the potential endogeneity problems than the fixed effect estimator, because firms, when they in month $m-2$ submit their prices for month m, have not observed the demand for months $m-1$ and m. Therefore, changes in the explanatory variables between two subsequent months are unlikely to be caused by changes in demand across these two months.[14]

The fixed effect and the difference estimators used in this section should be consistent if the biases just discussed are zero, but because of the covariance structure of the error term, the estimators are not the most efficient ones. Still, I use OLS rather than GLS because the latter relies on an estimated covariance matrix, which makes it less robust, and because the efficiency gains of using GLS should be small because of the large number of observations.

The results presented in Table 2 show that the fixed effect and difference estimator give similar results. Being the product of the month increases the market share by about 20 percentage points during the first half of the month and by nearly 70 percentage points during the second half. Having been the product of the month the preceding month increases the market share by about 40 percentage points during the first half of the month, but has no effect during the second half of the month. Being the first or second runner-up has small positive effects on the sales. Lastly, the estimates for Inv_N_{em} are positive as expected; this

[13] Because the time variable here is measured in half months, this is a second-difference estimation. I use this rather than a first-difference estimation because the explanatory variables change by months.

[14] Relative prices are not included in these estimations because the purpose is to estimate the total effect of the PM_{jm} and $PM_{j,m-1}$, which are later used to create the instrument. However, estimations not reported in tables show that including relative prices reduces the point estimates for PM_{jm} and $PM_{j,m-1}$ by only about 1 percentage point.

coefficient divided by the number of products in the exchange group the current month is the predicted market share for products with an intercept of zero and with all the other variables taking the value of zero.[15]

Table 2 about here

5. Empirical specification used to estimate state dependence effects

To estimate state dependence effects, I use the fact that within exchange groups, there is variation in time between purchase occasions. The mean difference between the 25th and 75th percentile of days between purchase occasions within an exchange group is 47 days, or 66% of the median within the exchange group. This variation implies that consumers who buy their current product at the same time can have bought their previous products in different months. In short, consumers facing the same choice set can have faced different choice sets upon their previous purchase occasion, and I use this to estimate state dependence effects.

As Sudhir and Yang (2014) and others noted, even when the changes in consumption sets are large, lagged consumption remains a function of unobserved preferences. That is, although choice set variation can reduce the endogeneity of lagged consumption, it can almost never make it truly exogenous. For this reason, the previous choices are predicted using the product-of-the-month status at the consumers' previous purchase occasions as the instrument. As indicated by the results of the previous section, this is a strong instrument.

[15] Considering that the sum over product within an exchange group and month is one for Inv_N_{em}, PM_{jm}, nearly one for $PM_{j,m-1}$ and the estimates for $R1_{jm}$ and $R2_{jm}$ are close to zero, one would expect $\beta_1^h + \beta_2^h + \beta_5^h$ to be close to one, for $h = 1,2$, because market share should sum to one. Using the actual values for each variable and the coefficients, the predicted market shares are found to, on average, nearly sum to one for both halves of the month. The largest discrepancy is for the first halves, in which the error term from the fixed effect specification has a weighted average of 0.015.

Because prices are uniform across pharmacies, many consumers face exactly the same choice set and have bought the same product on their latest purchase occasion. This makes it possible to aggregate the data prior to the estimations, and I do this to facilitate estimation of the average state dependence effect across all exchange groups. One advantage of this approach is that it does not require specifying choice sets for each individual market and time period. This is a great advantage in the present study, which is based on 53,465 exchange-group half-month combinations. The estimation approach also avoids the obstacle of defining the consideration set for each consumer at each purchase occasion,. On the other hand, estimating on aggregated data prevents me from estimating distributions of state dependence effects within exchange-group half-month combinations. Instead, I estimate the state dependence effect separately for subpopulations, in addition to estimating it for the full population.

Before aggregating, I drop the 27% of the purchases that are for consumers' first observed purchase within an exchange group (which first are used to create instruments) and the 1% of the purchases that are for consumers who have made multiple purchases within the same exchange group during one day. I aggregate the remaining 1.7 million purchases to one observation per combination of product (*j*), half month (*h*), and previous product bought within the exchange group (*l*). After adding observations for available products with no sales, this gives a final population of 359,671 observations.[16] For each observation, the dependent variable $Share_{jhl}$ is defined as the percentage market share of product *j* among consumers who buy a product within the exchange groups that product *j* belongs to within half month *h* and whose previous purchase within the exchange group was product *l*. For each exchange-group half-month combination, the number of observations equals the number of available products times the number of different products that consumers in that exchange group and half month had bought on their previous purchase occasion. Using these observations, an instrumental variable method is employed to compare—

[16] As in the previous section, products are considered available if at least one ordinary package is sold to an adult inhabitant of the county during the current month, even if no package is sold during the current half month or to a consumer whose previous purchase was product *l*. Outside options are not included in the estimation because buying no products within the exchange group to a large extent is driven by health characteristics that are unrelated to the choice among products within an exchange group. Also, the instruments, which are discussed below, are not defined for consumers who previously have not bought any product within the exchange group.

among consumers choosing between products in the same exchange group the same half month—how the fraction choosing a specific product differs between those who bought this product on their last purchase occasion and those who did not.

To be able to predict the last purchase, I create a variable denoted A_{jhl} which for each *jhl*-combination reflects the share that made their last purchase when the entire cost of product *j* was included in the pharmaceutical benefit scheme and product *j* were likely to be available at the pharmacies. Recall that the entire cost is included in the benefit scheme for products that are either the current or the previous month's product of the month. Also note that pharmacies that sell the previous month's product of the month in the second half of a month must sell this at the pharmacies' purchase price (Dental and Pharmaceutical Benefits Agency, 2009). Therefore, pharmacies are not likely to keep the previous month's product of the month in stock during the second half of a month. Based on this information, the instrument A_{jhl} is defined as:

$$\mathrm{A}_{jhl} = \overline{half_{t-1} = 1}_{jhl} * \left[\max\left(1, \left(\overline{PM_{J,t-1}} + \overline{PM_{J,m-1,t-1}}\right)|half_{t-1} = 1\right)\right]_{jhl} + \overline{half_{t-1} = 2}_{jhl} * \left[\left(\overline{PM_{J,t-1}}\right)|half_{t-1} = 2\right]_{jhl}.$$

Here, $\overline{half_{t-1} = 1}_{jhl}$ is the share of consumers who made their last purchase during the first half of a month. With two rare exceptions,[17] this is multiplied by the sum of the share of these consumers who made this purchase when product *j* was the product of the month and the share of these consumers who made this purchase when product *j* was the previous month's product of the month. The share of consumers who made their last purchase during the second half of a month ($\overline{half_{t-1} = 2}_{jmhl}$) is only multiplied with the share of these consumers who made this purchase when product *j* was the product of the month, since previous months' product of the month are not likely to be available during the second

[17] The first exception is that the term is not allowed to take values larger than one. This affects the value for part of the observations for which product *j* has been both the current and previous month's product of the month when consumers made their last purchase. The other exception is in the cases of ties, because PM_j and $PM_{j,m-1}$ then assume fractional values as explained in the previous section, which reduces the values of both expressions in square brackets. Using fraction values in the cases of ties makes A_{jhl} a better predictor of the previous choice since the market share of a product is increased less by being the product of the month if also substitutes have this status.

half of a month. Recall that the regression results in Table 2 show that the sum of the effects of being the product of the month of the current and previous month for the first halves of months is about as important as the effect of being the current product of the month for the second halves. Therefore, A_{jhl} can be expected to predict the previous choice about as well if the previous purchase was done during the first or second half of a month. I use this composite instrument to avoid the difficulties that otherwise could arise because different parts of the instrument have quite different explanatory power for the previous choice, depending on, among other issues, in which half of months the previous choices were made. The appendix presents estimation results obtained using different instruments, as well as from other robustness analyses.[18]

Using this instrument, I first look for reduced form of evidence for state dependence by estimating how the instrument affects $Share_{jhl}$. This is done using OLS-estimation of Equation 2:

$$Share_{jhl} = \beta_{21}\mathrm{A}_{jhl} + \mu_{jh}^{2} + \varepsilon_{jhl}^{2}. \tag{2}$$

Here, super index 2 is used to distinguish the fixed effects and error terms from corresponding terms in Equation 3. The fixed effects for product-half month combinations, μ_{jh}^{2}, control for all differences in prices, product of the month status, availability of products, and perceived quality differences at the time of the current purchase. Therefore, a positive estimate for β_{21} indicates that the choice set on the previous

[18] As in the previous section, products are considered available if at least one ordinary package is sold to an adult inhabitant of the county during the current month, even if no package is sold during the current half month or to consumers whose last purchase is product *l*. This implies that part of the observations used in the estimations represents zero sales. For these observations, $\overline{half_{t-1} = H}_{jhl}$, *H* = 1,2, is assigned the value $\overline{half_{t-1} = H}_{ehl}$, that is, the average within *ehl*-combination where *e* denotes exchange group. For these observations $\left[\overline{PM_{J,t-1}}|half_{t-1} = H\right]_{jhl}$, *H* = 1,2, is assigned the value $\left[\overline{PM_{l,t-1}}|half_{t-1} = H\right]_{ehl} * j\_is\_l_{jl}$, if the value of at least one of these variables exceeds 0.5. Similarly, $\left[\overline{PM_{J,m-1,t-1}}|half_{t-1} = 1\right]_{jhl}$ is assigned the value $\left[\overline{PM_{l,m-1,t-1}}|half_{t-1} = 1\right]_{ehl} * j\_is\_l_{jl}$, if the value of at least one of these variables exceeds 0.5. The motivation for assigning these values is that if $PM_l = 1$, and $j = l$, then $PM_j = 1$. Similarly, if $PM_l = 1$, but $j \neq l$, then $PM_j = 0$. If $j \neq l$ and $\left[\overline{PM_{l,t-1}}|half_{t-1} = H\right]_{ehl}$ ($\left[\overline{PM_{l,m-1,t-1}}|half_{t-1} = 1\right]_{ehl}$) is less than 0.5, $\left[\overline{PM_{J,t-1}}|half_{t-1} = H\right]_{jhl}$ ($\left[\overline{PM_{J,m-1,t-1}}|half_{t-1} = 1\right]_{jhl}$) is assigned the average value of this variable within the *jh*-combination. For *jh*-combination with zero sale, A_{jhl} is instead assigned the averge value of A_{jhl} within the *jm*-combination. In the appendix, I show that using the average value within *jm*-combinations for all observations representing zero sales yields similar estimates.

purchase occasion affects the current choice, which is evidence for state dependence. However, the estimate for β_{21} is no measure of the magnitude of the state dependence effect.

To derive an estimator for the state dependence effect, I depart from the following general utility function:

$$u_{ijh} = \gamma\, j\_is\_l_{jl} + \delta_{jh} + e_{ijh},$$

where u_{ijh} is the utility that consumer *i* derives from buying product *j* at half month *h*. The variable $j\_is\_l_{jl}$ takes the value 1 if the product bought now is the same as the product bought the last time, and zero otherwise. Hence, γ describes the state dependence effect in utility terms. The parameter δ_{jh} describes the average utility that consumers, in addition to the potential state dependence effect, get from buying product *j* at half month *h*. This includes both effects of persistent attributes, e.g., related to the quality of the product, and the effect of transitory attributes like the price. Lastly, e_{ijh} describes how consumer *i*'s utility of the product at half month *h* differs from that described by the two preceding terms. Hence, e_{ijh} captures both serially correlated and serially uncorrelated heterogeneity in the utility obtained from product *j*.

The state dependence effect is then estimated using Equation 3:

$$Share_{jhl} = \beta_{31}\left[1 + \frac{njl_{eh} - 1}{(nj_{eh} - 1)(nl_{eh} - 1)}\right] j\_is\_l_{jl} + \mu^3_{jh} + \varepsilon^3_{jhl}. \tag{3}$$

Here, fixed effects for product-half month combinations (μ^3_{jh}) control for the effects of δ_{jh}, i.e., the average utility that consumers, in addition to the potential state dependence effect, get from buying product *j* at half month *h*. The inclusion of product*half month fixed effects implies that the remaining parameter, β_{31}, will be determined by differences in $Share_{jhl}$ within *jh*-combinations. The indicator $j\_is\_l_{jl}$ times the square bracket (explained below) is instrumented using A_{jhl} times the square bracket. Instrumenting is required because the term e_{ijh} of the utility function can be serially correlated and therefore can affect both the previous and current choice.

The estimate of β_{31} is the estimate of the state dependence effect, showing how much more likely consumers are to buy a product because they bought it the previous time. The quotient is included to account for the fact that a market share depends on the relative, rather than the absolute, attractiveness of the product. In the quotient, nj_{eh} is the number of product available in exchange group e half month *h*; njl_{eh} is the number of these that was bought on the last purchase occasion by at least one consumer within the *eh*-combination; and nl_{eh} is the number of product that was bought on the last purchase occasion by at least one consumer within the *eh*-combination. It follows that $njl_{eh} \leq \min(nj_{eh}, nl_{eh})$.[19] The quotient accounts for the fact that the difference in shares between one observation for which j equals l and the other observations within the same *jh*-combination is not only driven by the state dependence effect on the first market share, but also by the other shares being reduced by these products being substitutes for products for which j equals l. To see this, consider an exchange group in which the same two products are sold in all months and assume that without state dependence, all shares would equal one-half. Denote the state dependence effect by *sde* and define it as the increase in $Share_{1h1}$ and $Share_{2h2}$ caused by $j = l$ for these, and note that with state dependence, $Share_{1h1} = Share_{2h2} = \frac{1}{2} + sde$. As $Share_{1hl} + Share_{2hl} = 1$, for $l = 1,2$, it follows that $Share_{2h1} = Share_{1h2} = \frac{1}{2} - sde$. Therefore, $Share_{1h1} - Share_{1h2} = Share_{2h2} - Share_{2h1} = 2 * sde$. Also note that if Equation 3 was estimated separately for this simple exchange group, $\beta_{31}\left[1 + \frac{njl_{eh}-1}{(nj_{eh}-1)(nl_{eh}-1)}\right]$ would equal $Share_{1h1} - Share_{1h2}$. In this example, $njl_{eh} = nj_{eh} = nl_{eh} = 2$, implying that $\beta_{31} * 2 = 2 * sde$. That is, β_{31} will, thanks to the quotient, equal sde. Note that within each *jh*-combination, there never is more than one observation for which $j\_is\_l_{jl} = 1$. It is for this reason that the squared bracket can be used to directly convert the differences in share within *jh*-combinations to an estimate of the state dependence effect. In the appendix, I give two examples with different numbers of products sold in different months and show that the quotient still implies that β_{31} equals *sde*.

[19] For 5% of the observation, the denominator in the squared bracket is zero. For these observations, the nominator also equals zero in 86% of the cases, whereas it equals minus one in the remaining cases, and the quotient is defined to equal zero.

One advantage of including the fixed effects (μ^3_{jh}) in Equation 3 is that these control for variation in demand across products and time. Therefore, even if the value of the instrument is related to the demand for the product, this will not bias the estimator for β_{31}. Therefore, Equation 3 will provide unbiased estimates of the state dependence effects assuming that the timing of consumers' last purchase is not driven by which brand they prefer. In the appendix, I show that the main result is robust to instead using the product-of-the-month status when the prescription for the previous purchase was written, rather than dispensed, when generating the instrument. This indicates that the result is not driven by consumers choosing when to buy a drug based on which products that are cheapest in different half months.

Equation 3 is estimated for the full population and also separately for different subpopulations, e.g., based on sex and age of the consumer and on how many purchases the consumer has made before within the exchange group. This is explained further in connection to the presentation of the estimation results in section 6.

The estimation approach used in this article differs substantially from that of Yeo and Miller (2018), MacKay and Remer (2019), and others who use aggregated market shares to estimate state dependence. Perhaps the most important difference is that having information on the individual's last purchases makes it possible to calculate separate market shares depending on which product the consumer bought the last time. This implies that assumptions regarding how unobservable quality evolves over time are not required for identification.

One advantage of Equation 3 is that it provides a direct and easily interpretable measure of the average state dependence effect, i.e., as a change in a market share that also equals the effect of state dependence on the probability that a consumer chooses the product he/she bought the last time. Another advantage is that the product*half month-fixed effect (μ^3_{jh}) controls for prices, which is a potentially endogenous variable. The disadvantage is that Equation 3 does not give a direct measure of the price equivalence of the state dependence effect, which makes it hard to compare the estimate of state dependence effect with previous studies that have not reported how state dependence affects the probability that a consumer chose the product he/she bought the last time.

When estimating Equations 2 and 3, I use two-way clustering that allows the error terms to be correlated within products and *ehl*-combinations. For each population, I estimate two alternative specifications of Equation 3, which differ in the weights used and in the observations included. In the a-specifications, observations are weighted with the average numbers of packages per observation within product half-month combinations (called *jh_weights*), and all observations are included. The purpose of the a-specifications is to estimate the average state dependence effect over all purchases within a population. The b-specifications are instead designed to avoid that comparisons of the state dependence effect across populations are affected by in which exchange groups and half months the compared populations buy drugs. Hence, in the b-specifications, the populations are restricted to exchange group and half month with variation in the explanatory variable $j\_is\_l_{jl}$ for all populations being compared. Importantly, this excludes exchange groups and half months with no sales in any of the populations being compared. Also, the weights used in the b-specifications (called *eh_weights*) are defined so that the total weight within an exchange-group half-month combination is the same for all populations being compared. For example, when comparing the state dependence effect across generics and brand-name products, this means that the weight for generics (brand names) assigned to each observation equals the total number of purchases within the exchange group *e* and half month *h* divided by the number of generic (-name) observations with the *eh*-combination. When the a- and b-specifications give similar results, only the results of the a-specification are reported in tables in the text, whereas results from the b-specification are presented in the appendix. For Equation 2, only estimations using *jh-weights* are reported. Table 3 presents descriptive statistics for the variables I use to estimate Equations 2 and 3.

Table 3 about here

6. Estimation results, state dependence

Table 4 presents the main estimation results, whereas Tables 5 and 6 present results for subpopulations. In Table 4, column 1 presents results for Equation 2, whereas instrumental method results for Equation 3 are presented in columns 2 and 4-6. Column 3 presents results obtained by estimating Equation 3 without instrumenting, i.e., treating $j\_is\_l_{jhl}$ as an exogenous variable. For columns 1-3, all 1.7 million purchases are used to define the observations. In column 4, the 7% of the purchases for which the prescriber or the pharmacy has vetoed substitution are excluded. Columns 5 and 6 present separate estimates for generics and brand-name products.

Column 1 of Table 4 shows a statistically significant effect of the instrument A_{jhl} on $Share_{jhl}$. The estimate reflects the product of two processes: the effect of A_{jhl} on the choices on the previous purchase occasion and the state dependence effect. Because the first of these effects should be positive according to the results of section 4, the positive estimate for β_{21} in specification 1 is evidence in favor of state dependence. However, because it reflects two processes, the size of the estimate does not, by itself, reveal the size of the state dependence effect. Instead, it reveals an additional benefit of getting product-of-the-month status. In section 4, this status was estimated to be associated with an increase in the market share by 23 percentage points in the first half month, 70 in the second half, and 43 percentage points in the first half of the coming month. The estimate of 10.08 reveals that the product-of-the-month status also increases the market share by about 10 percentage points when the consumers make their next purchases within the exchange group. Of course, this effect is usually distributed over several months, because the time between purchases differs across consumers. The r2-statistic reveals that the instrument explains only 1% of the variation in $Share_{jhl}$ within jh-combinations, whereas 13% are explained in the instrumental variable regressions on the full population.

Table 4 about here

The results for β_{31} reported in column 2 show that the causal state dependence effect is an 8 percentage point increase in the probability of choosing the same product as the last time. The effect is statistically significant at the 1% level, but compared to the effects of being the product of the month, the effect is small. That the estimate is smaller than the estimate for β_{21} reported in the column 1 is mainly caused by the square bracket being included in Equation 3, but not in Equation 2. The estimate for β_{21} reflects the increase in market share caused by the product-of-the-month status on the previous purchase occasions compared to if a competitor instead had this status. That is, it reflects the influence of the instrument of the attractiveness of both product *j* and some of its substitutes. On the other hand, the inclusion of the square bracket in Equation 3 means that the estimates for β_{31} show the state dependence effect, not the effect of product *j* benefiting from state dependence instead of some substitute benefiting from it. Results not shown in the tables reveal that if the square bracket was dropped, the estimate for β_{31} (which then should be interpreted as the effect of randomly assigning a product on the probability that the consumers will buy the same product next time) would become 10.17 (std. err., 0.93), that is, very similar to the estimate of β_{21}.[20] The relative size of the estimates for β_{21} and β_{31} also depends on the choice of instruments. The appendix includes results from estimations using different instruments that all give similar estimates for β_{31}, but different estimates for β_{21}.

The estimated state dependence effect of 8 percentage points can also be compared to the OLS estimate presented in column 3, which shows that the market share of product *j* is 21 percentage points higher among those who bought this product the previous time, compared to what would have been expected without heterogeneity or state dependence. Together, the results of columns 2 and 3 suggest that nearly two-thirds of the observed persistence is due to heterogeneity.

[20] This estimate can in turn be compared to the estimate of Feng (2018), who estimate that being assigned a molecule increases the probability of the same molecule being chosen three-quarters later with 54% to 69%. That I achieve much lower estimates is expected as I study the choice between bioequivalent products, which the patients can choose among themselves at the pharmacy, while Feng's estimates concern choices between therapeutic alternatives containing different ingredients. That this estimate of Feng's concerns choices three-quarters later rather than at the next prescription/purchase occasion should, however, reduce the difference between our estimates.

Column 4 reports a state dependence effect of 6 percentage points for purchases for which neither the prescriber nor the pharmacist has vetoed substitution. That this estimate is smaller than that for the full population suggests that the state dependence effect is many times larger for the 7% of the purchases for which the prescriber or the pharmacy has vetoed substitution. Further analyses presented in the appendix reveal that 80% of the differences in state dependence estimate across specifications 2 and 4 are caused by the exclusion of purchase for which the prescriber has vetoed substitution. That is, the results suggest that 1.55 of the total state dependence effect of 8.09 is caused by the doctor vetoing substitution and prescribing the product the consumer bought last time. The doctors might do this on their own incentive because they fear that the patient would mix up different drugs or not follow the ordination if he/she received a new brand, but doctors might also be asked by their patients to do this as patients can avoid out-of-pocket costs if the doctor, instead of the patient, opposes substitution.

The results in the last two columns of Table 4 show that the state dependence effect is stronger for brand-name products than for generics. Also, the statistics # *j* and # e together show that on average, there are 3.5 generics per exchange group, whereas it is rare with more than one brand-name product.[21] One possible explanation of the different estimates relates to the names of the products. Whereas brand names are sold under their own protected names, generics are usually sold under the substance name followed by the company name. Hence, the difference in name is usually smaller between two generic substitutes than between a generic and a brand-name product, and this can affect the state dependence effects. For example, Olsson et al. (2015) report that 41% of nearly 300 consumers interviewed at Swedish pharmacies consider that changes in name complicate adherence. It is also possible that some consumers view brand names as less close substitutes to other products for other reasons, for example, because they believe that brand names have superior quality.[22]

[21] Sometimes, two brand-name products are sold within the same exchange group, for example, because both a 98-pill package and a 100-pill package are sold or because the brand-name firm sells both blister packs and tins.

[22] The numbers for the Kleibergen-Paap rk LM test of weak instruments reveal that the instrument is less strong for brand-name products, which is explained by brand-name products seldom are products of the month. Of the 547 brand-name products, 330 (representing 34% of brand-name packages sold) were never the products of the month

Because it is rare for there to be more than one brand-name product per exchange group, it is possible to make a rough comparison between the estimated state dependence effect for brand-names and the reduced form evidence reported by Feng (2018) concerning the choice between any brand-name product and any generic product. He finds that during the first quarters after generic entry, the fraction using any brand-name product was about 8 percentage points larger among those who started their treatment before the first generic entry, compared to those who started their treatment later. One possible explanation of Feng's lower estimate is that some who started their treatment before generic entry had made multiple purchases during the first quarter after generic entry. This should affect the estimates because persistence is not complete, implying that the influence of a previous purchase should fade away the more purchases that are made after that.

As reported in Table 4, only 428 of the exchange groups are used in the estimation for brand names, as compared to 762 for the full population and 754 for generics. This raises the question of whether the difference in estimates between brand names and generics is caused by brand-name products being present only in markets in which state dependence is stronger for all products. However, results from the b-specification, presented in the appendix, show that the differences in state dependence effect are similar when the populations are restricted to exchange group and half months in which both generics and brand names are sold.

Table 5 presents separate estimates when purchases by only women or men, respectively, or when only consumers in specific age groups are used to define the market shares. The state dependence effect is about 1.8 percentage points larger for women, which is a statistically significant difference. As reported in the appendix, similar results are obtained using the b-specifications. As Erdem and Keane (1996), Crawford and Shum (2005) and others noted, state dependence can partly be explained by risk aversion in combination with less good knowledge about brands that the consumer has not previously used. Also,

during the study period, whereas for generics, only 215 (representing 1% of generic packages sold) out of 3,080 products were never the products of the month.

numerous studies[23] report that women tend to be more risk averse than men, and this can therefore be one possible explanation for the higher state dependence effect for women.

Table 5 about here

Turning to the age groups, the point estimates indicate that the average state dependence effect is largest among the oldest third of the consumers. The differences are not statistically significant across the a-specifications, but the results for the b-specifications show positive and significant associations between the state-dependence effect and the age of the consumers. [24] Again, risk preferences can be one possible explanation. Whereas some experimental studies have found mixed results regarding the associations between age and the choice among different risky alternatives, Mather et al. (2012) report that 64–89 years old, to a larger extent than younger adults, prefer a small certain gain over a chance of a larger gain. It is possible that consumers view consuming the same product they have used before as giving a certain gain.

In Table 6, separate results are presented for the second, the third, the fourth or fifth, and the sixth or later purchase occasion of a consumer. I define a purchase as the first purchase if it is the first purchase in the data set by a consumer within a specific exchange group and if the data show that the consumer has not bought anything from the exchange group in the last six months. This implies that all purchases by consumers who made their first purchase recorded in the data set during the first half year it covers are excluded. Also, all purchases for consumers with an observed time span between two subsequent purchases exceeding six months are excluded.

Table 6 about here

[23] See, e.g., Byrnes, Miller and Schafer (1999) and references therein, and Sapienza, Zingales and Maestripieri (2009).

[24] The results are consistent with Chen and Hitt (2002) and Wang (2017), who report negative (but not statistically significant) associations between number of switches and consumer age and household age, respectively. Number of switches is in turn negatively correlated with state dependence. Chen and Hitt also report a non-significant positive association between number of switches and being female.

The results from the a-specifications show that the average state dependence effect is largest among consumers making their sixth or later purchase within an exchange group. However, results from the b-specifications show that this is, at least to a large part, caused by differences across exchange groups. That is, when observations from the same exchange groups and months are used to identify state dependence for all populations, no statistically significant differences between the estimates are found.

7. Conclusion

This article introduces a new approach to estimating state dependence using data on how the share buying a specific product differs between those who bought this product on their latest purchase occasion and other consumers. The approach is convenient to use when studying many markets because it does not require the researcher to specify choice and consideration sets for each market and time period. Compared to methods used to study state dependence using ordinary market shares, it builds on far less restrictive assumptions and can therefore also be a good alternative when individual level data cannot be accessed, e.g., for integrity reasons.

The results show that state dependence causally increases the likelihood that a consumer buys the product he/she bought the last time by 8 percentage points. This state dependence effect is larger among women and the elderly than among men and younger consumers, which might be explained by higher degrees of risk aversion among women and the elderly.

The state dependence effect is also found to be larger for brand-name products than for generic products. Here, one possible explanation is that state dependence relates to name recognition and therefore is lower among generics because it is more likely that these products have substitutes with similar names. If so, one way to reduce the welfare cost caused by state dependence could be to introduce generic prescribing, meaning that physicians prescribe substance-strength-form combinations instead of specific products. This could shift consumers' focus from product names to substance names and therefore reduce state dependence, especially for brand-name products. Generic prescribing is currently not used in Sweden but is common in, e.g., Great Britain.

The existence of state dependence in choices among substitutes implies that optimal pricing is not static. As Osborne (2011), MacKey and Remer (2019) and others explain, this is important to consider when analyzing competition in markets, for example, when performing merger analysis, and doing this requires accurate estimates of the state dependence effect. Therefore, I hope that others will use approaches similar to the one introduced in this article to study state dependence effects in markets for which the method previously used gives imprecise estimates or rests on many restrictive assumptions and in markets for which state dependence has not yet been studied. Also, more research is needed on differences in state dependence across consumer and product groups. Here, one suggestion for future research is to investigate the causes of observed differences. One way to do this could be to combine studies of state dependence effects using observed purchases with surveys of consumers regarding the perceived disadvantages of switching to a new brand.

Appendix

Results using different instruments and other robustness analyses

Table A1 presents results from robustness analyses. To facilitate comparison, results from the preferred specification estimated on the entire population are also presented here, more precisely in column 1 of Table A1.

Column 2 presents results obtained when the following variable:

$$A^2_{jhl} = \overline{half_{t-1} = 1}_{jhl} * \left[\left(0.2290\overline{PM_{J,t-1}} + 0.4261\,\overline{PM_{J,m-1,t-1}}\right)|half_{t-1} = 1\right]_{jhl} + \overline{half_{t-1} = 2}_{jhl} * \left[0.6952\left(\overline{PM_{J,t-1}}\right)|half_{t-1} = 2\right]_{jhl}$$

times the square bracket of Equation 3 is used as instrument for $j\_is\_l_{jl}$ times the square bracket.[25] The numbers used to define A^2_{jhl} are coefficient estimates reported in Table 2, and one could therefore expect that using A^2_{jhl} instead of A_{jhl} would give a stronger instrument. However, using A^2_{jhl} actually results in a weaker instrument according to the Kleibergen-Paap rk LM statistic. Table A1 also shows that the change of instrument has a very small effect on the estimated state dependence effect (i.e., parameter β_{31}). The specification using A_{jhl} is the preferred specification because A^2_{jhl} can be considered endogenous because its exact value can depend on individual consumption choices. However, the small difference between the estimates in columns 1 and 2 of Table A1 indicates that any bias caused by this is very small.

Column 3 of Table A1 presents results obtained when instead of A_{jhl}, A_jm_{jhl} is used, which differs from A_{jhl} by being assigned the average value of A_{jhl} within *jm*-combinations for all observations representing zero sales.[26] Compared to column 1, this reduces the strength of the instrument according to the Kleibergen-Paap rk LM statistic, and this is the reason why this version of the instrument is not used in the

[25] For the 9,769 observations where values for A^2_{jhl} is originally missing, A^2_{jhl} is assigned the average value of A^2_{jhl} within the *jm*-combination.

[26] Footnote 17 describes which values that are assigned to A_{jhl} for observations representing zero sales.

preferred specification. Also, the estimate of the state dependence effect as well as the r^2-value becomes lower when using this version of the instrument. Using A^2_{jhl} and A_jm_{jhl}, respectively, as regressors in Equation 2 instead of A_{jhl} gives estimates for β_{21} of 19.58 (std.err. 1.83) and 8.18 (std.err. 1.03). These results are not presented in tables.

A requirement for the instrument A_{jmhl}, and the versions of it just described, to be valid is that consumers do not choose which months to fill their prescriptions based on the product-of-the-month status. Otherwise, the estimator of the state dependence effect will suffer from a positive bias because the instrument would then partly reflect consumers' preferences. If some consumers choose which months to fill their prescriptions based on the product-of-the-month status, I expect this to be most common for prescriptions written a few days before the product-of-the-month status changes, because it is then that the purchases must be advanced or delayed least to get the preferred product at lowest cost. However, I find no evidence for such behavior when analyzing how the number of days before the prescription is filled depends on the day in the month when it was written. Still, this does not rule out that some consumers choose the months to fill their prescription based on which products are the products of the month. Therefore, I investigate the validness of the instrument by studying if similar results are obtained when using another instrument that cannot be affected by consumers advancing or delaying when they fill a prescription. This alternative instrument differs from the baseline instrument by being defined based on the product-of-the-month status when the prescription for the previous purchase was written, instead of when it was dispensed.

The downside is that this instrument is expected to be weak when the previous purchase is a refill. To see this, consider patients who in January get prescriptions that can be filled with tablets for 90 days at the time, four times during the coming year. Patients are not allowed to buy drugs for more than about three months ahead if they want the costs to be covered by the pharmaceutical benefit scheme. Therefore, the patients are expected to make purchases with about three-month intervals, e.g., in January, April, July, and October. Even if not all consumers make their first purchase the same month the prescription is written, we can expect the product-of-the-month status in January to be a quite strong for the first choice of product, meaning that this can be used to estimate the effect of state dependence on the choice in April. However, because not all consumers always buy the same product as they bought last time, the instrument should be a weaker predictor for the choices made at the latter purchase occasions. In fact, I expect the instrument to be strong enough only for the first filling of a prescription and therefore use this instrument only for these purchases. The results of this analysis are presented in column 4 of Table A1. As a comparison, the results obtained using the baseline instrument on the same subpopulation are presented in column 5.

Table A1 about here

The point estimates for the state dependence effect reported in column 4 are about 0.8 percentage point or one standard error smaller than the point estimate reported in column 5. The small difference indicates that if the estimator used in column 5 suffers from a positive bias, this bias is not large, but of course, the results do not prove that no bias exists. The Kleibergen-Paap rk LM statistics show that the instruments based on prescription month are less strong than the instruments based on the dispensing month even though the population is restricted to the first purchase for each prescription. This is likely explained by that 12% make their first filling in a different month than when the prescription is written. Not surprisingly, this is most common for prescriptions written at the end of a month.

The quotient in Equation 3

Consider an exchange group in which products 1 and 2 are available in half month h, and both these products and also product 3 were bought by some consumers on their previous purchase occasion. Assume for simplicity that all shares would equal one-half if it were not for the state dependence effect. Then we get the following shares: $Share_{1h1} = Share_{2h2} = \frac{1}{2} + sde$. Because $Share_{1hl} + Share_{2hl} = 1$, for

$l = 1,2$, we also get $Share_{2h1} = Share_{1h2} = \frac{1}{2} - sde$. We also have $Share_{1h3} = Share_{2h3} = \frac{1}{2}$. Estimation of Equation 3 on these observations would yield

$$\beta_{31}\left[1 + \frac{njl_{eh} - 1}{(nj_{eh} - 1)(nl_{eh} - 1)}\right] = Share_{1h1} - \frac{Share_{1h2} + Share_{1h3}}{2}$$

$$= Share_{2h2} - \frac{Share_{2h1} + Share_{2h3}}{2} = sde + sde/2.$$

Here, $njl_{eh} = nj_{eh} = 2$ and $nl_{eh} = 3$, implying that $\beta_{31}[1 + 1/2] = 1.5sde$, i.e., $\beta_{31} = sde$ as desired.

Also, consider the case with $njl_{eh} = nl_{eh} = 2$ and $nj_{eh} = 3$. To keep it simple, but to be more general than in the previous examples, assume that without state dependence, $Share_{jhl} = S_{jh}$. With state dependence, we get $Share_{1h1} = S_{1h} + sde$ and $Share_{2h2} = S_{2h} + sde$. Because the sum of shares over *j* must equal one, we also get that $Share_{2h1} + Share_{3h1} = S_{2h} + S_{3h} - sde$ and $Share_{1h2} + Share_{3h2} = S_{1h} + S_{3h} - sde$. Estimation of Equation 3 on these observations would yield

$$\beta_{31}\left[1 + \frac{njl_{eh} - 1}{(nj_{eh} - 1)(nl_{eh} - 1)}\right] = \frac{Share_{1h1} + Share_{2h2} - Share_{1h2} - Share_{2h1}}{njl_{eh}}.$$

Assuming that all products, on average, are affected equally by being substitutes for products that gain by state dependence implies that $Share_{2h1} + Share_{1h2} = S_{1h} + S_{2h} - sde$ and the right-hand side then equals $1.5sde$. The left-hand side equals $\beta_{31}\left[1 + \frac{2-1}{(3-1)(2-1)}\right] = 1.5\beta_{31}$, so again $\beta_{31} = sde$.

Results from b-specifications and some additional results

Results from b-specifications that are not presented in the text are presented in Table A2.

Table A2 about here

In the result section, I write that 80% of the difference in estimates for β_{31} across specification 2 and 4 is explained by the exclusion of purchases for which the doctor has opposed substitution. This figure is calculated as (8.09-6.54)/(8.09-6.15)=0.80, where the numbers come from specification 2, specification "Exclude disallowed by prescriber" presented in Table A3, and specification 4. A similar calculation using specification "Exclude disallowed by pharmacy" suggests that 14% of the difference in estimate for β_{31} across specifications 2 and 4 is explained by the exclusion of purchases for which the pharmacy has opposed substitution. The point estimate for the third specification in Table A3 indicates that the state dependence effect in the prescribers' choices is as high as 55.24. The strength of the instrument is weaker for this population compared to the full population, which can be explained because the prescriber had vetoed substitution also at the previous prescription for a high share of the consumer for which substitution is currently vetoed. Still, the product-of-the-month status on the previous purchase occasion was relevant enough for identification.

Table A3 about here

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Table 1. Descriptive statistics for the observations used to estimate Equation 1

	Mean	Std.dev.	Min	Max
$Share_{jh}$	24.98	30.97	0	100
PM_{jm}	0.25	0.42	0	1
$PM_{j.m-1}$	0.23	0.41	0	1
$R1_{jm}$	0.18	0.37	0	1
$R2_{jm}$	0.11	0.31	0	1
Inv_N_{em}	0.25	0.15	0.07	1

Note: The descriptive statistics do not include the first month in the data set, because $PM_{j.m-1}$ is missing for this month. The number of observations is 106,611 for all variables. The weights used are the average number of packages per observation within exchange groups.

Table 2. Estimation results for $Share_{jh}$ and $\Delta Share_{jh}$ using Equation 1

Variable	Parameter	1. Fixed effects	2. One-month difference
PM_{jm}	β_1^1	22.90***	23.35***
		(0.93)	(0.56)
PM_{jm}	β_1^2	69.52***	67.64***
		(1.35)	(1.19)
$PM_{j,m-1}$	β_2^1	42.61***	42.39***
		(0.96)	(0.96)
$PM_{j,m-1}$	β_2^2	-0.38	-0.11
		(0.57)	(0.44)
$R1_{jm}$	β_3^1	1.79**	2.11***
		(0.71)	(0.34)
$R1_{jm}$	β_3^2	3.02***	1.65***
		(0.80)	(0.37)
$R2_{jm}$	β_4^1	0.59	1.37***
		(0.69)	(0.33)
$R2_{jm}$	β_4^2	0.03	0.45
		(0.58)	(0.34)
Inv_N_{em}	β_5^1	14.36***	10.97***
		(2.03)	(2.11)
Inv_N_{em}	β_5^2	7.22***	8.75***
		(1.91)	(2.09)
r2_w		0.75	0.65
# *jh* (Observations)		106,611	83,161
# *eh*		35,626	28,838
# *j* (Products)		3,979	3,339
# *e* (Exchange groups)		1,041	873
# Purchases		2,219,269	2,010,311

Note: # *jh* is the number of observations used in the estimations, whereas # *eh* and # *j* denote the number of clusters. # *j* is also the number of fixed effects used in the first specification. # *e* is the number of exchange groups, and # Purchases is the number of purchases used to generate the number of observations used. Standard errors, robust to correlations within exchange groups, are given in parentheses. ***, **, and * indicate that the coefficient is statistically significant different from zero on the 1%, 5% and 10% significance levels, respectively.

Table 3. Descriptive statistics for the observations used to estimate Equations 2 and 3

	jh_weights		*eh_weights*			
	Mean	Std.dev.	Mean	Std.dev.	Min	Max
$Share_{jhl}$	54.33	36.67	25.83	35.19	0	100
A_{jhl}	0.34	0.37	0.28	0.37	0	1
$j\_is\_l_{jl}$	0.24	0.43	0.25	0.43	0	1
$\left[1+\frac{njl_{emh}-1}{(nj_{emh}-1)(nl_{emh}-1)}\right]\mathrm{A}_{jhl}$	0.48	0.56	0.39	0.56	0	2
$\left[1+\frac{njl_{emh}-1}{(nj_{emh}-1)(nl_{emh}-1)}\right]j\_is\_l_{jl}$	0.35	0.63	0.35	0.64	0	2

Note: The *jh_weights* (*eh_weights*) equal the average numbers of packages per observation within each product-half-month combination (exchange-group half-month combination). Because the *jh_weights* are larger for products that have a large sale in the current half month, the mean of $Share_{jmhl}$ is expected to be larger when *jh_weights* are used. Of the 185,831 observations representing zero sales, 49,640 have a *jh_weight* of zero, yielding an effective population of 310,031 when *jh_weights* are used. However, in all exchange group and half months in which a least one choice is made among products, at least one product has positive sales, implying that the *eh_weight* is never zero.

Table 4. Estimation results for $Share_{jhl}$ using Equation 2 and a-specifications of Equation 3

Specification, population	1, All	2, All	3, All	4, Allowed	5, Generics	6, Brand names
Equation	2	3	3-OLS	3	3	3
β_{21} /β_{31}	10.08***	8.09***	20.86***	6.15***	7.49***	12.62***
	(1.01)	(0.65)	(0.72)	(0.58)	(0.68)	(1.66)
Test vs. s.,pop.				2, All		5, Generics
p-value				0.000		0.004
R2-within	0.01	0.13	0.20	0.10	0.09	0.41
# *jhl*	297,944	297,944	297,944	242,725	247,443	44,811
# *ehl*	85,149	85,149	85,149	79,401	83,534	41,628
# *jh*	70,905	70,905	70,905	60,429	56,682	12,560
# *j*	3,362	3,362	3,362	3,217	2,666	461
# e	762	762	762	753	754	428
K-P rk LM		281		285	300	13
K-P rk LM, p.		0.000		0.000	0.000	0.000
#Purchases	1,670,392	1,670,392	1,670,392	1,543,952	1,407,284	253,799

Note: *Test vs. s.,pop.* and *p-value* report w.r.t. for which specification and population (if any) a test of equality of estimates for β_{31} is performed and the p-value from this test. # *jhl* and # *jh* are the numbers of observations and number of fixed effects used in the estimations, respectively, whereas # *j* and # *ehl* denote the number of clusters. The number of observations is less than the 310,031 mentioned in the previous section also in the first three specifications, because 12,087 observations belong to singleton *jh*-groups. K-P rk LM is short for the Kleibergen-Paap rk LM statistic, which indicates the strength of the instruments. K-P rk LM, p. reports the p-value for the Kleibergen-Paap test, for which the null hypothesis is that the model is under-identified. #Purchases is the number of purchases used to generate the dependent variable for the observations used in the regression. Standard errors, robust to correlations within the clusters, are given in parentheses. ***, **, and * indicate that the coefficient is statistically significant different from zero on the 1%, 5% and 10% significance levels, respectively. Two percent of the observations are for products classified as parallel imports, and these are not included in estimations for generics and brand-name products.

Table 5. Estimation results for $Share_{jhl}$ using Equation 3 on subpopulations

	A-specifications					B-specifications		
Population	Women	Men	Age <60	Age 60-72	Age >72	Age <60	Age 60-72	Age >72
β_{31}	8.31***	6.54***	6.92***	6.83***	7.53***	5.23***	6.05***	6.88***
	(0.64)	(0.49)	(0.54)	(0.57)	(0.82)	(0.40)	(0.44)	(0.52)
Test vs. pop.		Women		Age <60	Age <60		Age <60	Age <60
p-value		0.000		0.805	0.267		0.003	0.000
Test vs. pop.					Age 60-72			Age 60-72
p-value					0.332			0.012
R2-within	0.14	0.09	0.12	0.09	0.10	0.11	0.13	0.14
# *jhl*	227,320	198,208	177,626	165,378	160,228	152,405	165,601	163,398
# *ehl*	68,562	61,776	60,155	51,685	47,967	36,627	37,788	37,171
# *jh*	55,917	48,926	46,926	40,518	38,231	34,229	35,633	35,522
# *j*	2,987	2,877	2,828	2,523	2,367	1,709	1,720	1,687
# *e*	687	658	660	598	566	376	376	376
K-P rk LM	260	271	304	233	200	205	202	204
K-P rk LM, p.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
#Purchases	896,001	759,235	561,269	528,305	553,772	457,572	498,847	524,748

Note: Only purchases by consumers in the specified demographic group are used to define the dependent variable. This means, for example, that $Share_{jhl}$ for the female population is defined as the percentage market share of product *j* among female consumers. A-specifications are tested against other a-specifications, and likewise for b-specifications. The number of observations (# *jhl*) for two age groups is lower in a- than in b-specifications as a consequence of the *jh-weights* equaling zero for some products that are currently chosen by no consumer. See also the note to Table 4.

Table 6. Estimation results for $Share_{jhl}$ using Equation 3: separately by number of previous purchases

Order of purchase	2:nd	3:rd	4:th or 5:th	6:th or later	2:nd	3:rd	4:th or 5:th	6:th or later
	A-specifications				B-specifications			
β_{31}	7.27***	6.17***	6.35***	9.11***	7.29***	7.02***	6.72***	7.41***
	(0.52)	(0.72)	(0.62)	(1.06)	(0.77)	(0.92)	(0.77)	(0.85)
Test vs. pop.		2:nd	2:nd	2:nd		2:nd	2:nd	2:nd
p-value		0.055	0.117	0.045		0.558	0.323	0.983
Test vs. pop.			3:rd	3:rd			3:rd	3:rd
p-value			0.696	0.001			0.759	0.597
Test vs. pop.				4:th or 5:th				4:th or 5:th
p-value				0.003				0.380
R2-within	0.09	0.07	0.08	0.15	0.13	0.12	0.13	0.13
# *jhl*	57,081	37,579	41,083	28,776	25,453	23,365	28,245	23,420
# *ehl*	25,095	16,535	16,964	12,284	7,247	6,847	7,465	6,802
# *jh*	18,642	12,291	12,508	9,432	6,784	6,588	7,167	6,574
# *j*	2,189	1714	1747	1532	839	829	853	828
# *e*	556	447	470	421	186	186	186	186
K-P rk LM	304	234	234	134	206	181	191	179
K-P rk LM, p.	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
#Purchases	96,926	56,262	68,139	47,444	40,208	29,749	44,863	35,675

Note: See notes to Tables 4 and 5.

Table A1. Estimation results for $Share_{jhl}$ using a-specifications for Equation 3

Specification	Preferred	A_{jhl}^2	A_{jhl} average	Prescription time instrument	A_{jhl} on prescription time instrument pop
β_{31}	8.09***	8.04***	6.67***	6.33***	7.11***
	(0.65)	(0.58)	(0.67)	(0.75)	(0.59)
R2-within	0.13	0.13	0.11	0.10	0.11
# *jhl*	297,944	297,944	297,944	222,470	222,470
# *ehl*	85,149	85,149	85,149	70,066	70,066
# *jh*	70,905	70,905	70,905	55,637	55,637
# *j*	3,362	3,362	3,362	3,169	3,169
# *e*	762	762	762	729	729
K-P rk LM	281	246	260	261	284
K-P rk LM, p.	0.000	0.000	0.000	0.000	0.000
#Purchases	1,670,392	1,670,392	1,670,392	809,786	809,786

See notes to Table 4.

Table A2. Estimation results for $Share_{jhl}$ using b-specifications for Equation 3

Population	All	Allowed	Generics	Brand names	Women	Men
β_{31}	6.93***	5.59***	7.46***	14.13***	7.08***	5.67***
	(0.41)	(0.38)	(0.74)	(1.68)	(0.45)	(0.40)
Test vs. pop.		All		Generics		Women
p-value		0.000		0.000		0.000
R2-within	0.17	0.14	0.18	0.37	0.17	0.13
# *jhl*	336,212	286,764	125,748	43,034	232,367	214,980
# *ehl*	81,988	78,533	40,079	40,079	55,054	53,479
# *jh*	79,278	71,496	26,563	11,674	53,121	50,882
# *j*	3,307	3,216	1,238	427	2,485	2,430
# *e*	750	750	399	399	543	543
K-P rk LM	249	276	71	15	228	246
K-P rk LM, p.	0.000	0.000	0.000	0.000	0.000	0.000
#Purchases	1,664,477	1,542,543	806,937	245,338	842,359	722,694

Note: For some populations, the number of observations (# *jhl*) is lower in the a- than b-specification as a consequence of the *jh-weights* equaling zero for some products that are currently chosen by no consumer. Also, see notes to Tables 4 and 5.

Table A3. Estimation results for $Share_{jhl}$ using Equation 3

Specification /Population	Exclude disallowed by pharmacy	Exclude disallowed by prescriber	Include only disallowed by prescriber
β_{31}	7.81***	6.54***	55.24***
	(0.65)	(0.58)	(1.35)
R2-within	0.12	0.10	0.68
# *jhl*	278,090	266,672	61,261
# *ehl*	83,428	81,436	17,043
# *jh*	66,332	65,943	16,000
# *j*	3,303	3,300	1,232
# *e*	758	760	328
K-P rk LM	278	288	76
K-P rk LM, p.	0.000	0.000	0.000
#Purchases	1,620,413	1,594,173	60,094

Note: See notes to Table 4.