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A NEW APPROACH TO ESTIMATING STATE DEPENDENCE IN CONSUMERS' BRAND CHOICES APPLIED TO 762 PHARMACEUTICAL MARKETS*

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

I. INTRODUCTION

TO UNDERSTAND THE INCENTIVES FACING FIRMS in markets with repeated purchases, it is essential to understand state dependence—that is, how a consumer's choice is affected by his/her previous choice. State dependence gives firms incentives to set temporarily low prices, 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 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 feel that they have greater knowledge of the quality of products they have used before and therefore view

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[1] As shown by Dubé, Hitsch and Rossi [2010], Janssen [2019], and others, prices can decrease in some market environments because of state dependence.

these products as less risky than other alternatives. State dependence can cause substantial welfare losses, but in the presence of other market failures, it can increase welfare (Handel [2013]). From a policy perspective, knowledge about state dependence is useful, for example, when considering information campaigns, the choice regarding default options, or changing market rules. 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 consumer preferences (Heckman [1981]),[2] where the latter can be caused by, for example, information or marketing only reaching a part of the consumer base. Roy, Chintagunta and Haldar [1996], Keane [1997], and Seetharaman [2004] are examples of prominent studies that use functional form assumptions about the nature of heterogeneity to disentangle unobserved heterogeneity and 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 to identify state dependence. 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 how 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 it avoids selection bias, which could be a problem when participating in the data generating process is voluntary.

At Swedish pharmacies, consumers can 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 the fact that

[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.'

[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 package size. Because the main difference between products within an exchange group is usually the name and the identity of the marketing pharmaceutical firm, I use 'brands' as a synonym for different products within exchange groups.

within exchange groups in a given month there 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 about 70 percentage points, which creates exogenous variation in the consumption experience. Then I present evidence showing that consumers are more likely to buy the brand that was cheapest the last time they bought the drug, compared to other consumers facing the same choice set but having made their previous 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. In the estimations, I use product-time fixed effects to control for differences in prices, availability, and demand for products. The results show that state dependence, on average, increases the probability that a consumer buys the product he/she bought the previous time by eight percentage points.

In 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], Iizuka [2012], Chan, Narasimhan and Xie, [2013], and more closely to Feng [2018] and Janssen [2019], who all found inertia in choices among pharmaceuticals. Coscelli [2000] used patients' switching of physicians to conclude that inertia at the patient level contributed to patients' being prescribed the same drug repeatedly, even though therapeutic alternatives exist. Iizuka [2012] also analyzed prescribers' choices, but between brand-name and generic pharmaceuticals, and found strong state dependence. Similarly, Chan, Narasimhan and Xie, [2013] found significant switching costs among therapeutic alternatives. Feng [2018], using U.S. data, and Janssen [2019], using Swedish data, analyzed how state dependence affects which pharmaceutical is bought. Feng used variation in the availability of alternatives when patients started treatment as an instrument to study state dependence in choices between therapeutic alternatives and between brand-name and generic pharmaceuticals on reduced form. He showed that during the first quarter after generic entry, the proportion using any generic version was about eight percentage points larger among those who started their treatment after the first generic entry, compared to those who started their treatment earlier. Feng also studied choices between therapeutic alternatives on structural form. Janssen used a similar approach and instrumented the first choice between any generic or any brand-name product, with the first choice being made after generic entry. He then showed that having bought a generic the first time significantly increased the probability

[4] Also Dubé, Hitsch and Rossi [2010] and others found that past prices predict current choices and interpret this as evidence for state dependence.

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, for one exchange group, Janssen used 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 previous purchase occasion increased the probability that it would be chosen again to the same extent as a 10% to 28% price reduction. Feng and Jansen also calucalated how changes in state dependence can affect prices. Feng's model predicts that prices would be lower in absence of state dependence but Jansen predicted that prices would increase if state dependence became less important, because this would soften competition for new consumers.[5]

This article contributes to the existing literature primarily in four 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 products 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. Also, the method is far less computationally intensive than existing methods used to estimate state dependence on micro data. This method can, for example, be used by researchers with transaction data from a supermarket to study state dependence for a large set of goods; with fixed effects for product-purchase day combinations, variation in prices and time between visits at the supermarket can be used to generate valid instruments for the previous choice.[6] Compared to the methods used to study state dependence using or-

[5] Also Ching [2010], Granlund and Rudholm [2012], Granlund and Sundström [2018], and Ching, Granlund and Sundström [2021] analyzed choices between medically equivalent pharmaceuticals, but none studied state dependence. Ching [2010] used aggregate U.S. data to study learning, which is one possible explanation for state dependence, and reported that learning partly explains why generic market shares increase over time. The other three studies used Swedish prescription level data. Granlund and Rudholm [2012] reported 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 [2018] studied how consumer welfare is affected by which brand is prescribed, and Ching, Granlund and Sundström [2021] studied whether consumers who can get the cheapest product for free make different choices than consumers who face the same price differences but who must pay strictly positive amounts for all products. The two latter articles both controlled for previous purchases using 'sGL-terms,' which take 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. See also Ching and Lim [2020] and references therein regarding how choices between therapeutic alternative drugs can be studied using learning models.

[6] Variation in the prices that are advertised to consumers outside the supermarket and hence can affect whether or not consumers visit the supermarket on a given day may not be valid instruments.

dinary market shares, this method requires far less restrictive assumptions. For example, it does not require assumptions about how unobservable quality evolves over time. Therefore, the method can also be a good alternative when individual-level data cannot be accessed, for example, for reasons of confidentiality. Firms and authorities are often reluctant to share individual-level data but are willing to share aggregate data. For example, an employer might refuse to share employee-level information on choices of health insurance plans, but agree to, for each currently available insurance plan, report the number of employees that chose each of the plans available in the past year and to do this separately by employment years. This information, together with information about the plans, will be enough to estimate the state dependence effect using this paper's approach, given that sufficiently strong instruments for the previous choice can be created by using variation in employment years and variation over time in, for example, premiums for the different plans.

A second simple contribution is that I use product-fixed effects to identify state dependence effects. These control for variation in demand and hence make variables based on previous prices into valid instruments for the previous choice, even if they are correlated with the current demand for products. This increases the possibility of using instrumental variable methods, even in mature markets for which the launch of products might not be a suitable instrument. For pharmaceuticals, this allows me to identify state dependence effects at the product level using instrument, even though all generic products of a drug were allowed to enter the market at the same time, that is, when the patent expired. Specifications with product-fixed effects and in which previous prices are used to generate instruments can also be used to estimate state dependence with microdata, but when a large number of markets are studied simultaneously, it is convenient to aggregate the data first.

A third contribution is that the article separates state dependence for individual pharmaceutical brands from heterogeneity that cannot be captured by observables. 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. Compared to Janssen's structural estimation, the contribution of this article 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 at the product level is important for understanding the incentives of generics firms, for example, the incentives for early entry. Also, it is important to consider the result that state dependence exists also on the product level when market rules and insurance policies are decided because this 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 fourth contribution of this article is that it adds to the limited existing knowledge about how state dependence varies across subpopulations. One advantage of using data on prescription pharmaceuticals is that it identifies the individual consumer, not just to which household the consumer belongs. This facilitates the analysis 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 II describes the Swedish generics market, and Section III presents the data. In Section IV, I present descriptive evidence of the extent to which being the cheapest product affects the market share as well as descriptive statistics for the variables used to estimate this. Section V discusses the empirical methods used to estimate state dependence effects and presents related descriptive statistics, while the results from these analyses are presented in Section VI. Section VII concludes the article, while robustness checks are presented and discussed in the Appendix.

II. THE GENERICS MARKET

In Sweden, physicians prescribe specific products, identifying also the pharmaceutical firm. However, consumers are able to choose between products within exchange groups at the pharmacies. The exchange groups consist of products with the same combination of active substance, form of administration, strength, and package size.[7] Thus, consumers choose between bioequivalent products/brands, but the products can include different inert ingredients and differ in color and shape.

On average, there are 3.5 generics per exchange group, while it is rare that there is more than one brand-name product. Nevertheless, 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. In Sweden, advertising for prescription pharmaceuticals directed toward consumers is banned by law, but brand-name firms advertise products toward prescribers. 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, reaching zero when these costs exceed SEK 5,400 during a twelve-month period. In addition to the coinsurance rate, consumers who choose a different product than the 'product of the month' (PM)

[7] Package 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.

or the previous month's PM have to pay extra. The PM is the cheapest product within the exchange group that is guaranteed by the pharmaceutical firm to be available in Sweden throughout the month. In cases of ties, there can be several PM's in an exchange group.[8] To allow pharmacies to clear excess inventory, they are allowed to sell the previous month's PM without additional cost to the consumer, but if they do this after day 15 of the month they must sell it at the pharmacies' purchase price[9] (Dental and Pharmaceutical Benefits Agency [2009]). Because ties are relatively rare, pharmacies for each exchange group often have two products in stock that can be sold without additional cost to the consumer during the first half of a month. However, as pharmacies have incentives to sell out the previous month's PM during the first half of a month, they seldom have it in stock during the second half.

Pharmacies are required to inform consumers if a cheaper brand is available and of the possibility of choosing substitutes other 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, for example, if the low-cost alternative had a package that would be difficult for the consumer to open.[10] If consumers oppose substitution and buy the prescribed product, the entire extra cost is charged to them and the extra cost is not included in the accumulated pharmaceutical cost that determines the coinsurance rate. This is also the case if the pharmacy does not have the cheapest alternative in stock and the consumer chooses to buy another product, rather than returning when the pharmacy has received the product or going to another pharmacy (Ministry of Health and Social Affairs [2009]). If the consumer chooses another product than that which was prescribed or the cheapest one, despite the cheapest one's 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 this study, consumers chose 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.

Pharmaceuticals for the next 90 days can be dispensed within the benefit scheme, and if a suitable package for 90 days is missing, the closest larger

[8] In the first two months with generic competition, no product is declared to be product of the month.

[9] If the purchase price of the previous month's PM exceeds SEK 300, pharmacies are not allowed to sell it within the benefit scheme unless it is also the current months's PM or is the prescribed product. Only eight per cent of the products in the generic market have such high prices (Dental and Pharmaceutical Benefits Agency [2009]).

[10] A generic was prescribed for 73 per cent of the fillings studied in this paper but dispensed in 85 per cent of the fillings. When a generic was prescribed, the prescriber and pharmacist disallowed substitution for two and three per cent of the cases, respectively, but the corresponding figures when a brand-name product was prescribed are 14 and six per cent.

package size can be dispensed (Ministry of Health and Social Affairs [2009]). The latter means that that packages with 100 pills are often dispensed to consumers who should take one pill per day. Contraceptives are allowed to be dispensed for longer periods, but, if consumers make multiple applications for the same prescription at once, the costs are not included in the benefit scheme. A new supply within the benefit scheme can be made when two-thirds of the time that the previous one covers has elapsed. For example, a consumer for whom each supply lasts 100 days can make the second application 67 days after the first supply.

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 to the purchase price adds 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 those which should be sold without additional cost to consumers.

III. 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 DPBA that contain information about which exchange groups the products belong to. The purchase data set includes all prescribed pharmaceuticals that were dispensed to the 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.

The county of Västerbotten (population 271,736; December 31, 2019) is sparsely populated with a population density of less than five residents per square kilometer. However, nearly half of the population lives in the municipality of Umeå, which has a population density of 56. In 2018 the median income from employment or business activities was SEK 281,000 (approx. USD 32,000) for those above 20 years old. Of inhabitants 25–64 years old, 92 per cent had at least a high-school education and 30 per cent had at least three years of post-high-school education (usually university education). The median income is three per cent lower in Västerbotten than the average across Sweden, but the education level is slightly higher.

From the 7.5 million prescriptions in the data sets, I excluded nearly half that were for drugs that do not belong to any exchange group, including all patent drugs. I also excluded drugs that could be bought without prescriptions because it is likely that all purchases of such drugs were not captured by the data. Then I excluded supply to children and adolescents below the age of 18 because their parents can collect their pharmaceuticals, meaning that the identity of the individual choosing between the exchangeable products is unknown. For similar reasons, 40% of the remaining supplies that were for individualized dosage bags were excluded because a significant fraction might be collected by someone else, such as 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 aged 75 or older. I also excluded 1% of the remaining distributions that were for regular packages, but for consumers with one or more prescriptions for individualized dosage bags in the same exchange group. Finally, I dropped 0.1% of the remaining observations that were repurchases or purchases repurchased the same day and 0.1% that lacked the consumer identifier. After this, 2,348,351 distributions of 4,224 products in 1,152 exchange groups to 147,150 individuals remained.

Except for the 627,008 instances that were consumers' first supply within an exchange group observed in the data set, the number of days between subsequent distributions to a given consumer within an exchange group can be calculated, as illustrated in Figure 1. The figure shows commonly there are about 100 days between subsequent supplies and also displays a considerable drop in density at 130 days. This drop was expected because, as explained in Section II, consumers within the benefit scheme are seldom allowed to obtain a prescription that lasts more than 100 days on one occasion and can make the next prescription only when two-thirds of the time that the previous prescription should cover has elapsed. Hence, a consumer with a prescription for three supplies that each last for 100 days can make the second application 67 days after the first and then wait 67 to 133 days until making the third application. That the number of days between prescriptions is higher than this for some consumers is because not all drugs are for chronic conditions.

The variation illustrated in Figure 1 consists both of variation across exchange groups and variation across consumers buying pharmaceuticals from the same exchange group. To illustrate the importance of the second source of variation, Figure 2 displays the difference between the 25th and 75th percentile of days between purchase occasions within each exchange group. The figure shows that this interquartile range is between 25 and 69 days for most observations. The mean interquartile range is 47 days. This implies that there is variation among consumers choosing among products within the same exchange group, in which half month the consumers made their previous purchase and, therefore, also in which products and prices they faced.[11] As

[11] Note that even if the interquartile range were zero there could be variation in days between subsequent filling, but then there would be no such variation for at least half of the observations within the exchange group.

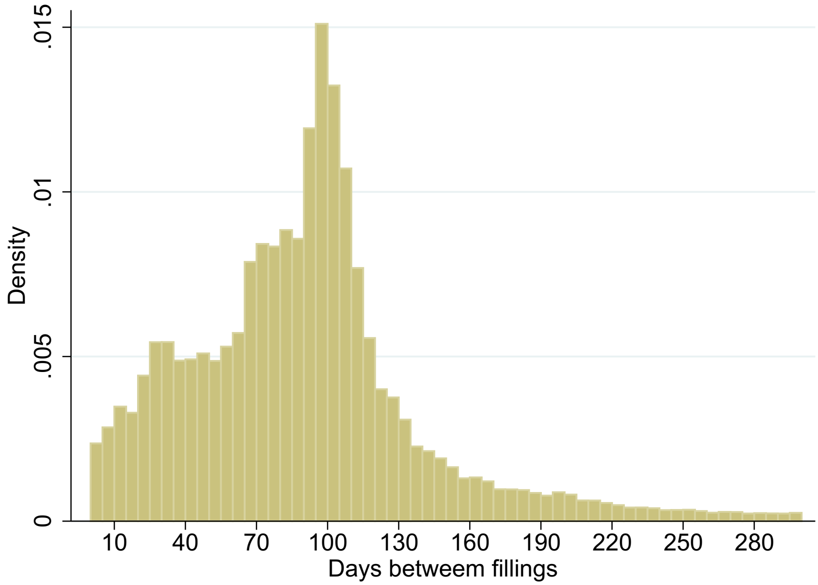

Figure 1

Distribution of Days between Subsequent Fillings of a consumer within an exchange group for 1,681,513 observations for which the number of days is 300 or less

Notes: For visual clarity, the 2.3% of the observations with days between fillings exceeding 300 are excluded. Days between supplies cannot be calculated for the 627,008 supplies that are consumers' first within an exchange group observed in the data set, so these are not included in the figure. The width of the bins is five days and the first bin displays zero to four days. [Colour figure can be viewed at *wileyonlinelibrary.com*]

explained in Section V, this is one of the variations exploited in this paper to create the instruments used to identify state dependence.

IV. DESCRIPTIVE RESULTS ABOUT THE IMPORTANCE OF THE PRODUCT-OF-THE-MONTH STATUS

The purpose of this background section is to show that the PM status is strongly related to market share, which implies that the PM status when consumers made their last purchase can be used to create strong instruments for their previous choice. Hence, this helps to motivate the instruments that are defined later and describes some of the characteristics of the markets studied in the paper. However, this section does not provide any evidence on state dependence and the estimates reported here are not needed to identify state dependence.

Specifically, this section provides estimates of the association between PM status and market shares and also a discussion on how this relates to the

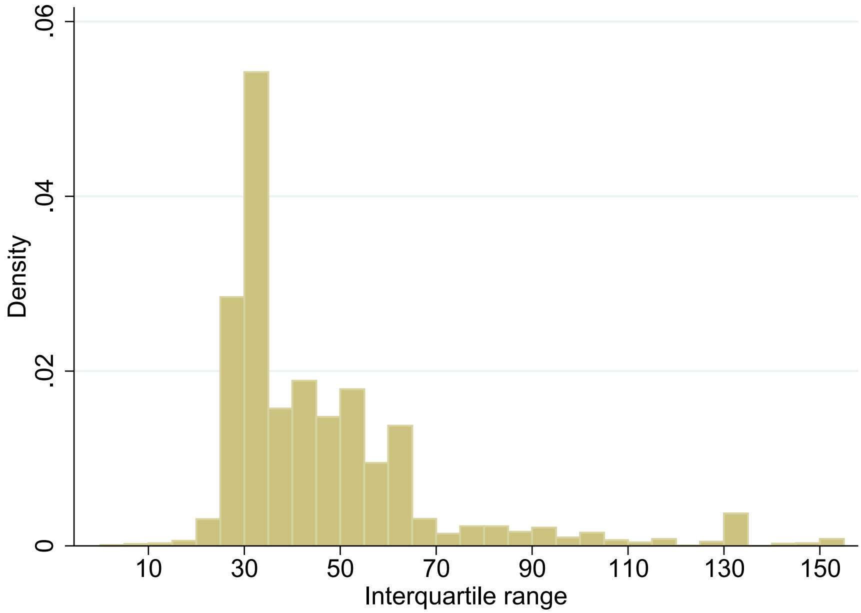

Figure 2

Distribution across Exchange Groups of Interquartile Range in Days between Subsequent Fillings of a Prescription

Notes: An observation is a prescription for which the time since the last supply can be calculated and the figure illustrates the interquartile range for 1,706,931 observations. For visual clarity, the 1% of the observations with an interquartile range exceeding 155 days are excluded. The same result can be obtained by letting each exchange group be one observation and weighting each observation with the number of supplies used to calculate the interquartile range. The width of the bins is five days and the bin with the highest density is the one for 30–34 days. [Colour figure can be viewed at *wileyonlinelibrary.com*]

causal effect of being PM on a product's current market share. To be able to estimate the associations, I aggregate the data to one observation per product and half month. Months are divided into halves, with the break after day 15, because of the incentives that pharmacies have to sell out the previous month's PM during the first 15 days. After dropping 3% of the observations from exchange groups in which no product was declared to be PM,[12] and adding observations for available products with no sales,[13] the final population consists of 110,387 observations. I estimate separately using data from

[12] These excluded observations account for 1.5% of the packages sold. The reason why no product is declared to be PM 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.
[13] 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.

the first and second half of each month, respectively, the following reduced-form equation:

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

Here, 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 in the given half month. The superindex p takes the values 1 and 2, indicating the parameters for the first and second halves of the months.

The variable PM_{jm} takes the value 1 if product j is the only PM in its exchange group and the value of zero if it lacks this status. If there is a tie, PM_{jm} takes the value one over the number of PM's within the exchange group in 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 because pharmacies have incentives to sell out the previous month's PM during the first half of a month (see Section II), β^1 is expected to be less than β^2 for some of the other variables. 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 1 if the product is the first (second) reserve. That is, $R1_{jm}$ equals 1 for the first runner-up, which means that pharmacies can sell this product without additional cost to the consumers if the PM 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 of zero for products not having this status. For 29% (50%) of the exchange groups, no product was 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 PM's (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-group half-month combinations.[14]

[14] The estimations presented in this article were conducted using the STATA package *reghdfe* and *ivreghdfe* by Correia [2017; 2018].

TABLE I
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

Notes: 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 I 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 are four available products on average 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 PM the previous month and the previous month's PM is not available for some other exchange groups.

That price can be a function of expected demand implies that the estimators related to the four first variables can be biased estimators of the causal effects of these variables. For example, it is reasonable to suspect that high demand during a few months reduces the value PM_{jm} and $PM_{j,m-1}$, while increasing $Share_{jh}$. Hence, the bias likely results in an underestimation of the effect of these two variables on the market share. Low demand can reduce the expected values of $R1_{jm}$ and $R2_{jm}$ by making it more likely that the product becomes the cheapest, but can also increase these values by increasing the likelihood that a product is the second or third cheapest instead of being more expensive. Hence, the signs of the biases affecting the estimators related to $R1_{jm}$ and $R2_{jm}$ are uncertain.

There are numerous examples that can explain why firms can predict the demand for month m when they set the prices for it in month $m-2$. Here, I describe one that relates to state dependence. Consider a firm that sold unusually many of its three-month package in January. This firm can expect an unusually high demand for this package when many of the January consumers make another filling of their prescriptions in April, given that some of these prefer to buy the same brand they bought the previous time (i.e., given that a state dependence effect exists). For Equation 1 this implies that ε_{jh}, and hence $Share_{jh}$, can be expected to be high in April. In addition, the firm can be expected to set a higher price than average for April in order to harvest profits from state dependence. This reduces the expected value of PM_{jm} for

April which, together with the high value of ε_{jh}, results in a negative bias for the estimators of β_1^p.

These biases are expected to be small for two reasons. First, firms have incentives to randomize prices to prevent competitors from marginally undercutting their prices, which results in large price variations even for products with stable demand.[15] 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 regressions based on purchases by inhabitants of one county containing only 2.7% of the Swedish population.[16]

Because the biases are expected to be small, I expect the mean squared error to be lower when these four variables are treated as exogenous, compared to the situation if they were 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 a comparison to the results from the fixed effect estimations of Equation 1, I also present results from one-month difference estimations.[17] The difference estimators can be affected even less by some sources of bias than the fixed effect estimators.[18]

The fixed effect and the difference estimators used in this section would be consistent if the biases discussed above were zero. However, because of the covariance structure of the error term, these estimators would not be 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.

[15] 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 be able 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.

[16] This reduces the bias, given that the correlation between ε_{jh} and the corresponding error terms that would be obtained by estimating Equation 1 on national data is less than one. However, it cannot eliminate the bias unless this correlation is zero.

[17] 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.

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

TABLE II
ESTIMATION RESULTS FOR $Share_{jh}$ AND $\Delta Share_{jh}$ USING EQUATION 1

	First half		Second	
	Fixed effects	One-month differences	Fixed effects	One-month differences
PM_{jm}	22.17***	22.38***	70.24***	72.15***
	(0.93)	(0.71)	(1.39)	(1.03)
$PM_{j,m-1}$	42.24***	43.99***	0.01	1.29***
	(0.97)	(0.78)	(0.63)	(0.40)
$R1_{jm}$	1.46**	0.45	3.36***	3.82***
	(0.71)	(0.41)	(0.84)	(0.55)
$R2_{jm}$	0.58	0.54	0.03	0.67
	(0.71)	(0.43)	(0.59)	(0.48)
Inv_N_{em}	17.99***	20.09***	3.67***	10.00**
	(1.47)	(2.61)	(0.15)	(3.98)
r2_w	0.83	0.63	0.89	0.86
# *jh* (Observations)	53,029	43,047	52,999	42,777
# *eh*	17,706	15,407	17,780	15,297
# *j* (Products)	3,669	3,478	3,675	3,457
# *e* (Exchange groups)	982	942	988	932
# Purchases	1,081,596	1,004,709	1,134,783	1,019,578

Notes: # *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 and half months and within products, are given in parentheses. *** and ** indicate that the coefficient is statistically significantly different from zero on the 1% and 5% significance levels, respectively.

The results presented in Table II show that the fixed effect and difference estimators give similar results for most variables. One notable difference for the second halves is that the estimate for $PM_{j,m-1}$ is significantly different from zero (but only 1.29) when the difference estimator is used, but not when the fixed effect estimator is used. Also, the estimates for Inv_N_{em} are larger when the difference estimators are used as compared to when the fixed effect estimators are used.

According to the point estimates, being the PM increases the market share by 22 percentage points during the first half of the month and by 70–72 percentage points during the second half. Having been the PM the preceding month increases the market share by 42–43 percentage points during the first half of the month, but has no or only a small effect during the second half of the month. Also, the sum of the coefficients for PM_{jm} and $PM_{j,m-1}$ are about as high for the first and second halves of the month. The estimates reflect that the sale to consumers that do not pay extra to get a more expensive version is distributed between the PM and the previous month's PM during the first half of the month, whereas the current PM receives all, or nearly all, of this sale during the second half. The reason for the latter is that pharmacies must sell the previous month's PM at their purchase price if they sell it after the 15th day of a month.

Being the first or second runner-up has small positive effects on sales. Lastly, the estimates for Inv_N_{em} are positive, as expected; this coefficient divided by the number of products in the exchange group in the current month is the predicted market share for products with an intercept of 0, with all the other variables taking the value of 0.[19]

V. EMPIRICAL SPECIFICATION USED TO ESTIMATE STATE DEPENDENCE EFFECTS

To estimate state dependence effects, I use the fact that among consumers buying products from the same exchange group in the same half month, there is variation in which half month they made their previous purchase. This variation suggests that consumers facing the same choice set could have faced different choice sets on their previous purchase occasion. One source of this variation is the variation in time between purchase occasions, described in Section III. Another source is variation in the date when the prescription was written. To see this, consider two patients who are prescribed two supplies of 100-tablet packages and instructed to take one pill per day. Assume that both collected their first package the day the prescription was written and the second one 100 days after, and that one consumer got the prescription on March 28th and the other on April 4th. Then, both can collect their second package during the first half of July (7th and 14th, respectively). Thus, among consumers making their second collection in the same half month, there will be variation in which half month the first one was made and, therefore, likely in the choice set they would then face.

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 rarely make it truly exogenous. For this reason, the previous choices are predicted using the PM status at the consumers' previous purchase occasions as the instrument. As indicated by the results of the previous section, this is a strong instrument.

[19] Considering that the sum over product within an exchange group and month is 1 for Inv_N_{em}, PM_{jm}, nearly 1 for $PM_{j,m-1}$ and the estimates for $R1_{jm}$ and $R2_{jm}$ are close to 0, one would expect $\beta_1^p + \beta_2^p + \beta_5^p$ to be close to 1, for $p = 1, 2$, because market share should sum to 1. Using the actual values for each variable and the coefficients, the predicted market shares are found to, on average, nearly sum to 1 for both halves of the month. The largest discrepancy is for the first half, 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 the same choice set and bought the same product on their most recent purchase occasion. This makes it possible to aggregate the data before the estimations, and I do this to facilitate estimation of the average state dependence effect across all 53,465 exchange-group half-month combinations. However, estimating from 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 are first used to create the 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.[20] 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 bought on their previous purchase occasion. Using these observations, an instrumental variable method is employed to compare—among consumers choosing between products in the same exchange group in 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. Product-half month fixed effects are used to identify the state variation among consumers choosing between products in the same exchange group in the same half month. One advantage of using these fixed effects 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.

[20] 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 is driven to a large extent by health characteristics that are unrelated to the choice among products within an exchange group. Also, the instruments discussed below are not defined for consumers who have not previously bought any product within the exchange group.

To be able to predict the last purchase, I created 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* was 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 PM. Also note that pharmacies that sell the previous month's PM 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 PM in stock during the second half of a month.[21] Based on this information, the instrument A_{jhl} is defined as:

$$A_{jhl} = \overline{half_{t-1} = 1}_{jhl} * \left[max\left(1, \left(\overline{PM}_{j,t-1} + \overline{PM}_{j,m-1,t-1}\right) | half_{t-1} = 1\right)\right]_{jhl}$$
$$+ \overline{half_{t-1} = 2}_{jhl} * \left[\left(\overline{PM}_{j,t-1}\right) | half_{t-1} = 2\right]_{jhl}.$$

Here, $t-1$ indicates the previous purchase occasion within the exchange group of a consumer. The share of consumers who made their last purchase during the first half of a month is denoted $\overline{half_{t-1} = 1}_{jhl}$. With one rare exception,[22] this is multiplied by the sum of $\overline{PM}_{j,t-1}$ and $\overline{PM}_{j,m-1,t-1}$. When conditioned on $half_{t-1} = 1$, the variables $\overline{PM}_{j,t-1}$ ($\overline{PM}_{j,m-1,t-1}$) are defined as the average value of PM_{jm} ($PM_{j,m-1}$) among consumers: i) whose current purchase is product *j*, bought in half month *h*, and who bought product *l* last time, and ii) made their last purchase during the first half of a month. Similarly, when conditioned on $half_{t-1} = 2$, the variables $\overline{PM}_{j,t-1}$ are defined as the average value of PM_{jm} among consumers: i) whose current purchase is product *j*, bought in half month *h*, and who bought product *l* last time, and ii) made their last purchase during the second half of a month. The share of consumers who made their last purchase during the second half of a month ($\overline{half_{t-1} = 2}_{jhl}$) is only multiplied with the share of these consumers who made this purchase when product *j* was the current PM, because previous month's PM are not likely to be available during the second half of a month.

Recall that the regression results in Table II show that the sum of the effects of being the PM of the current and previous month for the first half of the month is about as important as the effect of being the current PM for the second half. Therefore, A_{jhl} can also be expected to predict the previous choice if the previous purchase was made during the first or second half of a month. I use this composite instrument to avoid the difficulties that could

[21] Because of this, I have also made estimations excluding products that were PM the previous, but not the current, month from the second half of the months. This reduced the estimated state dependence effect with less than a half of standard error.

[22] The 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* was both the current and previous month's PM when consumers made their last purchase.

otherwise 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 the month the previous choices were made. The appendix presents estimation results obtained using different instruments, as well as from other robustness analyses.[23]

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

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

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^2_{jh}, control for all differences in prices, PM 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 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:

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

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, such as those related to the quality of

[23] 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, $half_{t-1} = H_{jhl}$, $H = 1,2$, is assigned the value $half_{t-1} = H_{ehl}$, 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-combinations with zero sales, 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.

the product, and the effect of transitory attributes like the price. Lastly, κ_{ijh} describes how consumer i's utility of the product at half month h differs from that described by the two preceding terms. Hence, κ_{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:

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

Here, fixed effects for product-half month combinations (μ^3_{jh}) control for the effects of δ_{jh}, that is, 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 κ_{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 the market share depends on the relative, rather than the absolute, attractiveness of the product. In the quotient, nj_{eh} is the number of products available in exchange group e in half month h, njl_{eh} is the number of products available in exchange group e in half month h that was bought on the last purchase occasion by at least one consumer within the eh-combination, and nl_{eh} is the number of products bought on the last purchase occasion by at least one consumer within the eh-combination. It follows that $njl_{eh} \leq \min\left(nj_{eh}, nl_{eh}\right)$.[24] 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 driven not only 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,

[24] 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.

$Share_{1h1} - Share_{1h2} = Share_{2h2} - Share_{2h1} = 2 * sde$. Also, note that if Equation 3 were 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 is never more than one observation for which $j\_is\_l_{jl} = 1$. It is for this reason that the squared bracket can be used to convert directly 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*.

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 provides unbiased estimates of the state dependence effect 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 using the PM 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 are cheapest in different half months.

Equation 3 is estimated for the full population and also separately for different subpopulations, for example, based on the sex and age of the consumer and on how many purchases they have made before within the exchange group. This is explained further in connection with the presentation of the estimation results in Section VI.

The estimation approach used in this article differs substantially from that of Yeo and Miller [2018], MacKay and Remer [2019], and others who used 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 previous time. This means 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, that is, as a change in a market share that also equals the effect of state dependence on the probability that a consumer will choose 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 the state dependence effect with previous studies that have not reported how state dependence affects the probability that a consumer will choose the product he/she bought the previous time.

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 across all purchases within a population. The b-specifications are designed to avoid the comparisons of the state dependence effect across populations being affected by the question of in which exchange of 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 (brand-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 Equations 2 and 4 (describe below), only estimations using *jh-weights* are reported.

To describe the differences in state dependence effect across exchange-group half-month combinations, I also estimate Equation 4:

$$\text{(4)} \quad \begin{aligned} Share_{jhl} = {} & \beta_{41}\,[.\,]\,j\_is\_l_{jl} + \beta_{42}\,[.\,]\,j\_is\_l_{jl} * days_e + \beta_{43}\,[.\,]\,j\_is\_l_{jl} * gap_e \\ & + \beta_{44}\,[.\,]\,j\_is\_l_{jl} * nj_{eh} + \beta_{45}\,[.\,]\,j\_is\_l_{jl} * gen_j + \mu^4_{jh} + \varepsilon^4_{jhl}, \end{aligned}$$

where $[.\,]$ is short for $\left[1 + \frac{njl_{eh} - 1}{(nj_{eh} - 1)(nl_{eh} - 1)}\right]$. Hence, Equation 4 differs from Equation 3 by including interactions between $\left[1 + \frac{njl_{eh} - 1}{(nj_{eh} - 1)(nl_{eh} - 1)}\right] j\_is\_l_{jl}$ and four variables. The first variable, $days_e$, is defined as the average number of days between subsequent fillings made by the same consumer within exchange group e. The second variable, gap_e, is the approximate average number of days of gap in usage in exchange group e. More exactly, gap_e is defined as the average within the exchange group of $gap_{iet} = days\ since\ previous\ filling_{iet} - last_{ie,t-1}$. Here, $last_{ie,t-1}$ denotes how long the previous filling of consumer i in exchange group e can be expected to last and it is defined as the minimum of the number of defined daily dosage of the previous filling ($DDD_{ie,t-1}$) and the number of tablets, capsules, or equivalent ($pills_{ie,t-1}$). Daily doses are the assumed average maintenance dose per day for a drug used for its main indication in adults. Because of the assumptions made when defining daily doses (World Health Organization [2018]) the minimum of $DDD_{ie,t-1}$ and $pills_{ie,t-1}$ are used to define $last_{ie,t-1}$.

Interactions with $days_e$ and gap_e are included to study if the state dependence effect decreases in the time between fillings and if there is an additional effect of gaps between usage. To study if the state dependence effect depends on the number of products available in exchange group e in half month h, an interaction with nj_{eh} is included. Lastly, an interaction with the indicator variable gen_j, which takes the value of one if product j is a generic and zero otherwise, is added to avoid the estimates for the other interaction variables being affected by the share of generics and brand-name products within exchange-group half-month combinations. The five explanatory variables of Equation 4 are instrumented with $\left[1+\frac{njl_{eh}-1}{(nj_{eh}-1)(nl_{eh}-1)}\right]\mathrm{A}_{jhl}$ and this variable interacted with $days_e$, gap_e, nj_{eh}, and gen_j, respectively.

When estimating Equations 2, 3, and 4, I use two-way clustering that allows the error terms to be correlated within products and *ehl*-combinations. Table III presents descriptive statistics for the variables used in the estimations. The variable gap_e has negative values for nearly nine per cent of the

TABLE III
DESCRIPTIVE STATISTICS FOR THE OBSERVATIONS USED TO ESTIMATE EQUATIONS 2, 3, AND 4

	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
$days_e$	95.19	22.94			1	397
gap_e	33.54	31.25			-65	379.5
nj_{eh}	5.21	2.55			1	14
gen_j	0.84	0.37			0	1

Notes: The *jh_weights* (*eh_weights*) equal the average number 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 at least one choice is made among products, at least one product has positive sales, implying that the *eh_weight* is never zero. The values for the variable gap_e are missing for 5,490 of the observations.

observations, which can be caused by consumers on average advancing fillings in some exchange groups, or by the drugs being used for an indication that requires higher dozes than the drugs main indication. Nearly identical results as those presented in the next section are obtained if these negative values are replaced with zeros.

VI. ESTIMATION RESULTS, STATE DEPENDENCE

Table IV presents the main estimation results, whereas Tables V and VI present the results for subpopulations. In Table IV, 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, that is, 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, 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 and column 7 presents the results for Equation 4.

Column 1 of Table IV 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 IV, 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 PM status. In Section IV, this status was estimated to be associated with an increase in the market share by 22 percentage points in the first half month, 70–72 in the second half, and 42–43 percentage points in the first half of the coming month. The estimate of 10.08 reveals that the PM status also increases the market share by about ten 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% is explained in the instrumental variable regressions on the full population.

The results for β_{31} (the parameter for $\left[1+\frac{njl_{eh}-1}{(nj_{eh}-1)(nl_{eh}-1)}\right] j\_is\_l_{jl}$) reported in column 2 show that the causal state dependence effect is an eight 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 PM, the effect is small. That the estimate is smaller than the estimate for β_{21} (the parameter for A_{jhl}) reported in column 1 is mainly caused by the square bracket's being included in Equation 3, but not in

TABLE IV
ESTIMATION RESULTS FOR $Share_{jhl}$ USING EQUATION 2 AND A-SPECIFICATIONS OF EQUATIONS 3 AND 4

Specification, population	1. All	2. All	3. All	4. Allowed	5. Generics	6. Brand names	7. Interactions
Equation	2	3	3-OLS	3	3	3	4
A_{jhl} or							
$[.]\, j\_is\_l_{jl}$	10.08***	8.09***	20.86***	6.15***	7.49***	12.62***	19.97***
	(1.01)	(0.65)	(0.72)	(0.58)	(0.68)	(1.66)	(2.32)
$[.]\, j\_is\_l_{jl} * days_e$							-0.07***
							(0.02)
$[.]\, j\_is\_l_{jl} * gap_e$							-0.03
							(0.02)
$[.]\, j\_is\_l_{jl} * nj_{eh}$							-0.11
							(0.23)
$[.]\, j\_is\_l_{jl} * gen_j$							-4.19***
							(1.55)
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	0.14
# *jhl*	297,944	297,944	297,944	242,725	247,443	44,811	288,293
# *ehl*	85,149	85,149	85,149	79,401	83,534	41,628	83,095
# *jh*	70,905	70,905	70,905	60,429	56,682	12,560	67,775
# *j*	3,362	3,362	3,362	3,217	2,666	461	3,039
# e	762	762	762	753	754	428	731
K-P rk LM		281		285	300	13	12
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	1,543,952	1,407,284	253,799	1,648,124

Notes: $[.]\, j\_is\_l_{jl}$ is short for $\left[1 + \frac{njl_{eh} - 1}{(nj_{eh} - 1)(nl_{eh} - 1)}\right] j\_is\_l_{jl}$. *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. *** indicate that the coefficient is statistically significantly different from zero on the 1%, 5%, and 10% significance levels, respectively. Two per cent of the observations are for products classified as parallel imports, and these are not included in separate estimations for generics and brand-name products and in the estimation with interactions. In addition, 3,961 observation with missing values on gap_e are not used in the latter estimation.

TABLE V
ESTIMATION RESULTS FOR $Share_{jhl}$ USING EQUATION 3 ON SUBPOPULATIONS

	A-specificationss					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

Notes: 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 IV.

TABLE VI
ESTIMATION RESULTS FOR $Share_{jhl}$ USING EQUATION 3, SEPARATELY BY NUMBER OF PREVIOUS PURCHASES

Order of purchase	2nd	3rd	4th or 5th	6th or later	2nd	3rd	4th or 5th	6th 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:nds	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

Notes: See notes to Tables IV and V.

Equation 2. The estimate for β_{21} reflects the increase in market share caused by the PM status on the previous purchase occasions compared to if a competitor instead had this status. That is, it reflects the influence of the instrument on 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}.[25] 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 eight 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.

Column 4 reports a state dependence effect of six 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 estimates across specifications 2 and 4 are caused by the exclusion of purchases 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's vetoing substitution and prescribing the product the consumer bought the previous time. The doctors might do this because they fear that the patient would mix up different drugs or not follow the prescription if he/she received a new brand, but doctors might also

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

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 columns 5 and 6 of Table IV 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, while it is rare for an exchange group to contain more than one brand-name product. One possible explanation for 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 effect. For example, Olsson *et al*. [2015] reported 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.[26]

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 found that during the first quarter after generic entry, the fraction using any brand-name product was about eight 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 for Feng's lower estimate is that some who started their treatment before generic entry made multiple purchases during the first quarter after generic entry. This could 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 IV, 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 the exchange groups and half months in which

[26] 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 the fact that brand-name products are seldom PM's. Of the 547 brand-name products, 330 (representing 34% of brand-name packages sold) were never the PM during the study period, whereas for generics, only 215 (representing 1% of generic packages sold) out of 3,080 products were never the PM.

both generics and brand names are sold. Also the results from the specification with interactions confirm that the state dependence effect is weaker for generics.

The point estimates for the first two interaction variables, presented in column 7 of Table IV, together indicate that the state dependence effect is reduced by one percentage point if the average number of days between fillings in the exchange group is increased by ten days. One possible explanation for this result is that more consumers remember the name of a product they bought recently. A result not presented in the tables shows that this joint effect is significantly different from zero at the one per cent significance level, but it is only the estimate for the first of these interaction variables that is significant on its own. The negative, but insignificant, point estimate for the second of these interaction variables, $\left[1+\frac{njl_{eh}-1}{(nj_{eh}-1)(nl_{eh}-1)}\right] j\_is\_l_{jl} * gap_e$, is consistent with more days between fillings having a larger effect if it is caused by gaps in usage rather than large quantities being dispensed at once. The lack of significance might be caused by a measurement error in gap_e, but it prevents us from concluding if more days between fillings having a larger effect when this is because of gaps in usage.

The point estimate for the third interaction variable, $\left[1+\frac{njl_{eh}-1}{(nj_{eh}-1)(nl_{eh}-1)}\right] j\_is\_l_{jl} * nj_{eh}$, is not statistically different from zero and also close to zero in an economic meaning. According to the point estimate, an increase in the number of products available by five (which equals two standard deviations) would only reduce the state dependence effect by 0.5 percentage points.[27] A calculation not presented in the table reveals that the predicted state dependence effect is 12.83 (std.err. 1.40) for a brand-name product for which the values of $days_e$, gap_e, and nj_{eh} equals the weighted average among observations with $j\_is\_l_{jl} = 1$. Hence, the results of this specification are in line with the result for brand-name products presented in column 6 of Table IV.

Table V presents separate estimates when purchases are made only by women or men, 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 Table AII in the appendix, similar results are obtained using the b-specifications. As Erdem and Keane [1996] and Crawford and Shum [2005] noted, state dependence can partly be explained by risk aversion in combination with less knowledge about brands that the consumer has not previously

[27] Even if nj_{eh} does not influence the state dependence effect, and an increase in nj_{eh} reduces the value of the square bracket $\left[1+\frac{njl_{eh}-1}{(nj_{eh}-1)(nl_{eh}-1)}\right]$ for given values of njl_{eh} and nl_{eh} and hence reduces the effect of $j\_is\_l_{jl}$ on $Share_{jhl}$ for a given value of β_{31}.

used. Also, numerous studies[28] 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.

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.[29] 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 year olds, to a larger extent than younger adults, prefer a small certain gain over the chance of a larger gain. Consumers may view consuming a product they have used before as providing a certain gain.

The results presented in Table V can be compared to those of Feng [2018]. He reported that women and older individuals were more likely to switch substances than men and younger individuals but, when a health proxy, age, and gender were included in the same regression, he found a weak positive effect of age and being female on the probability of using the same substance as four years earlier. That the latter set of results are in line with those obtained using b-specifications is expected, because the results from the b-specifications describe differences across consumers buying products from the same exchange groups and hence sharing some health characteristics. There are several possible explanations for the differences between the results from the a-specifications, which are affected by differences in health across consumers buying products from different exchange groups, and the ones Feng obtained without the health proxy. One relates to the choices between substances being made by prescribers, whereas choices between products within exchange groups primarily are made by consumers. Thus, Feng's results do not rule out the possibility that women and the elderly, on average, have stronger preferences for using the substances they have used before, but that this is dominated by them, to a lesser extent than men and younger individuals, persuade the doctors to prescribe the substances they prefer.

In Table VI, separate results are presented for the second, third, 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

[28] See, for example, Byrnes, Miller and Schafer [1999] and references therein, and Sapienza, Zingales and Maestripieri [2009].

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

consumer has not bought anything from the exchange group in the previous 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 period between two subsequent purchases exceeding six months are excluded.

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.

Lastly, I investigate whether prices of generics are higher in months when firms can harvest the increased demand generated by state dependence and large sales in previous months. To do this, I define $harvest_{jm}^{K} = \sum_{k=2}^{K} S_{ke} PM_{j,m-k}$, where K is either 3, 4 or 6, and S_{ke} is the share of purchases within exchange group e that are done k months after the consumer's previous purchase. Of the repeated purchases, 75% occurred within three months, 88% within four months, and 95% within six months. One of the $harvest_{jm}^{K}$-variables is used as an explanatory variable in each regression. The variables take values between 0 and 1, and the higher values they take, the higher is the expected demand and price for the product. As a dependent variable I use either the natural logarithm of the price, lnP_{jm}, or $RelativeP_{jm}$, which is defined as P_{jm} devided by the the average price of generics in month m in the exchange group that product j belongs to. I control for K lags of PM_{jm} and product fixed effects.

The results, presented in Table VII, show that all six estimates of $harvest_{jm}^{K}$, $K = 3, 4, 6$ are positive and the estimates are significantly different from zero at the 5% level when $RelativeP_{jm}$ is used as a dependent variable. With lnP_{jm} as the dependent variable, the estimates for $harvest_{jm}^{4}$ and $harvest_{jm}^{6}$ are significant at the 10% level, while the estimate for $harvest_{jm}^{3}$ is not significant at any convential level. The positive estimates for $harvest_{jm}^{K}$ are consistent with large sales in previous months, because of state dependence, generates large demand in the current month and firms set higher prices the larger the demand is.

The estimates for $harvest_{jm}^{K}$ range from 0.06 to 0.10 with a mean of 0.08. An estimate of 0.08 suggests that the price of a product, k months after it was the PM, is 4% higher if $S_{ke} = 0.5$ (i.e., 50% of purchases are made by consumers who bought a product from the exchange group k months earlier) than it would have been if $S_{ke} = 0$. If the market share of a product is 50 percentage points higher when it is the PM[30] and $S_{3e} = 0.5$, and the state dependence

[30] The point estimates presented in Table II suggest that the markets shares are 46-47 percentage points higher when products are the PM, if 50% of sales are from each half of the month. As discussed, because of negative bias this can be an underestimation of the effect of being the PM on the market share.

TABLE VII
ESTIMATION RESULTS FOR lnP_{jm} AND $RelativeP_{jm}$

K; # lags of P_{jm}	3	4	6	3	4	6
Dependent variable		lnP_{jm}			$RelativeP_{jm}$	
$harvest^K_{jm}$	0.06	0.09*	0.08*	0.07**	0.08**	0.10**
	(0.04)	(0.05)	(0.05)	(0.03)	(0.04)	(0.04)
$P_{j,m-1}$	0.25***	0.24***	0.24***	0.22***	0.22***	0.21***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
$P_{j,m-2}$	0.08***	0.09***	0.09***	0.06***	0.06***	0.07***
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
$P_{j,m-3}$	0.00	-0.00	0.00	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
$P_{j,m-4}$		-0.00	0.01		-0.01	-0.00
		(0.01)	(0.01)		(0.01)	(0.01)
$P_{j,m-5}$			0.01			0.01
			(0.01)			(0.01)
$P_{j,m-6}$			0.02*			0.01
			(0.01)			(0.01)
r2_w	0.83	0.83	0.84	0.37	0.39	0.41
# *jh* (Observations)	22,403	19,427	15,268	22,403	19,427	15,268
# *eh*	11,649	10,288	8,344	11,649	10,288	8,344
# *j* (Products)	1,876	1,620	1,286	1,876	1,620	1,286
# *e* (Exchange groups)	713	659	572	713	659	572
# Purchases	1,162,514	1,075,156	918,100	1,162,514	1,075,156	918,100

Notes: # *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. # *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 and half months and within products, are given in parentheses. ***, **, and * indicate that the coefficient is statistically significantly different from zero on the 1%, 5%, and 10% significance levels, respectively.

effect is eight percentage points, a product's market share will be will be two percentage points (0.5*0.5*0.08 = 0.02), or about 8%, higher three months after it has been the PM compared to the case when either S_{3e} or the state dependence effect equals zero. Hence, the estimates are consistent with the proposition that an 8% increase in demand increases the price of the product by 4%.

Table VII also shows that $PM_{j,m-1}$ and $PM_{j,m-2}$ have significantly positive effects on the price in month m. One possible explanation for this is that firms have too little in stock to be able to meet the demand if its product becomes the PM again after just having been the PM and therefore they set higher prices.

VII. CONCLUSION

This article introduces a new approach to estimating state dependence using data on how the people buying a specific product differ between those who bought this product on their most recent purchase occasion and those who did not. 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 the 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, for example, for confidentiality reasons.

The results show that state dependence increases the likelihood that a consumer will buy the product he/she bought the last time by eight percentage points. This state dependence effect is larger in exchange groups with a short average time between fillings, perhaps because more consumers remember the name of a product they bought recently. It is also larger among women and the elderly than among men and younger consumers, which might be explained by higher degrees of risk aversion.

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 is, therefore, 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, for example, Great Britain.

The existence of state dependence in choices among substitutes implies that optimal pricing is not static. As Osborne [2011] and MacKey and Remer [2019] 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 methods previously used give imprecise estimates or rest 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 A

A1. *Results Using Different Instruments and Other Robustness Analyses*

Table AI 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 AI.

Column 2 presents results obtained when the following variable:

$$A^B_{jhl} = \overline{half_{t-1}=1}_{jhl} * \left[\left(0.22\overline{PM}_{j,t-1} + 0.42\overline{PM}_{j,m-1,t-1}\right) | half_{t-1}=1\right]_{jhl} + \overline{half_{t-1}=2}_{jhl} * \left[0.70\left(\overline{PM}_{j,t-1}\right) | half_{t-1}=2\right]_{jhl}$$

times the square bracket of Equation 3 is used as instrument for $j\_is\_l_{jl}$ times the square bracket.[31] The numbers used to define A^B_{jhl} are coefficient estimates reported in Table II, and one could therefore expect that using A^B_{jhl} instead of A_{jhl} would give a stronger instrument. However, using A^B_{jhl} actually results in a weaker instrument according to the Kleibergen-Paap rk LM statistic. Table AI 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^B_{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 AI indicates that any bias caused by this is very small.

Column 3 of Table AI 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.[32] 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 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^B_{jhl} and A_jm_{jhl}, respectively, as regressors in Equation 2 instead of A_{jhl} gives estimates for β_{21} of 19.54 (std.err. 1.82) 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 PM status. Otherwise, the estimator of the state dependence effect will suffer from a positive bias because the instrument would then partly reflect consumers'

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

[32] Footnote 23 describes which values are assigned to A_{jhl} for observations representing zero sales.

TABLE AI
ESTIMATION RESULTS FOR $Share_{jhl}$ USING A-SPECIFICATIONS FOR EQUATION 3

Specification	Preferred	A^B_{jhl}	A_{jhl} average	Prescription time instrument	A_{jhl} on prescription time instrument pop
β_{31}	8.09***	8.02***	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

Notes: See notes to Table IV.

preferences. If some consumers choose which months to fill their prescriptions based on the PM status, I expect this to be most common for prescriptions written a few days before the PM 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 may choose the months to fill their prescription based on which products are the products of the month. Therefore, I investigate the validity of the instrument by studying whether 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 PM 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. Because of the rules for the pharmaceutical benefit scheme described in Section II, these patients are expected to make purchases at 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 PM status in January to be 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 renewal of a prescription and therefore use this instrument only for these purchases. The results of this analysis are presented in column 4 of Table AI. As a comparison, the results obtained using the baseline instrument on the same subpopulation are presented in column 5.

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 the fact 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.

A2. *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

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

Here, $njl_{eh} = nj_{eh} = 2$ and $nl_{eh} = 3$, implying that $\beta_{31}\left[1 + 1/2\right] = 1.5sde$, 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

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

Assuming that all products, on average, are affected equally by being substitutes for products that gain by state dendence 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 $\beta_{31} = sde$.

TABLE AII
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

Notes: 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 IV and V.

TABLE AIII
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

Notes: See notes to Table IV.

A3. *Results from b-Specifications and Some Additional Results*

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

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 AIII, 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 AIII 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 PM status on the previous purchase occasion was relevant enough for identification.

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