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Do All Your Detailing Efforts Pay Off? Dynamic Panel Data Methods Revisited

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Abstract

We estimate a sales response model to evaluate the short- and long-term value of pharmaceutical sales representatives' detailing visits to physicians of different types. By understanding the dynamic effect of sales calls across heterogeneous doctors, we provide guidance on the design of optimal call patterns for route sales. Our analyses reveal that the long-term persistence effect of detailing is more pronounced for specialist physicians; the contemporaneous marginal effect is higher for generalists. Free samples have little effect on any type of physician. We also introduce a key methodological innovation to the marketing and economics literatures. We show that moment conditions—typically used in traditional dynamic panel data methods—are vulnerable to serial correlation in the error structure. However, traditional tests to detect serial correlation have weak power and can be misleading, resulting in misuse of moment conditions and incorrect inference. We present an appropriate set of moment conditions to properly address serially correlated errors in analyzing dynamic panel data.

Key words: dynamic panel data, serial correlation, instrumental variables, sales call, detailing, pharmaceutical industry.

1. Introduction

The pharmaceutical industry plays a significant role in the world economy. According to QuintilesIMS (formerly IMS Health), the global market for prescription drugs is expected to grow from roughly $1.1 trillion in 2016 to $1.5 trillion by 2021. Despite the enormous size of this market, however, marketing to customers (that is, to physicians) largely consists of (and is typically restricted to) personal selling in the form of detailing and free samples provided during office visits. [1] Even in the United States, a nation that allows direct-to-consumer mass-media pharmaceutical advertising, personal selling remains the dominant marketing tool; some 90,000 sales representatives (1 for every 6.3 doctors) market pharmaceutical products to 567,000 U.S. physicians (Wall and Brown, 2007).

Studies of the effectiveness of personal selling at generating physician prescriptions have produced strikingly mixed findings in the literature: reported sales elasticity measures range from -14.8% (Parsons and Vanden Abeele, 1981) to 41% (Gönül et al., 2001). This extreme inconsistently appears to be attributable both to limited data on physicians' prescribing behavior and to bias arising from naive treatment of data, specifically panel data. Thus we seek to gain insights on deriving an unbiased measure of the short- and long-term value of a firm's detailing efforts. To do so, we use detailed data on both individual physicians' prescriptions and on the marketing efforts of a major multinational pharmaceutical firm. We also employ innovative dynamic panel data methods to control for unobserved physician heterogeneity while accounting for endogeneity concerns.

[1] As of 2016, direct-to-consumer advertising is allowed only in Brazil, New Zealand, and the United States, with varying restrictions on content.

To obtain a precise and unbiased value of detailing efforts turns out to be quite complicated. Physicians' prescribing behavior appears to be highly habitual, and this underlying heterogeneity is unobserved by researchers. Physicians are known to exhibit high levels of inertia (Janakiraman et al., 2008); an individual physician's past level of prescriptions is likely to persist, affecting current sales. To accommodate this dynamic process, studies in marketing have frequently adopted the advertising-as-investment framework of Nerlove and Arrow (1962), which conceptualizes a sales response model as a function of a stock of goodwill that grows in response to current marketing activities but decays over time. According to typical assumptions used in the literature, this stock of goodwill—represented by the geometric sum of marketing efforts—can be replaced by the lagged dependent variable, leading to a general dynamic panel data model specification (Balestra and Nerlove, 1966).

Identifying the causal effect of a sales call becomes challenging when this lagged dependent variable coincides with unobserved heterogeneity. Because pharmaceutical companies are likely to allocate more resources (e.g., shorter call cycles and more free samples) to physicians with higher sales-volume or growth potential, it is necessary to control for possible correlation between sales effort and potential. Moreover, an endogeneity problem arises because, by construction, the lagged dependent variable is correlated with lagged error terms through unobserved heterogeneity. Past studies have responded to this challenge by disregarding the issue (Parsons and Vanden Abeele, 1981; Manchanda et al., 2004), fixing the rate of decay (Gönül et al., 2001), or excluding dynamics (Manchanda and Chintagunta, 2004).

Dynamic panel data methods proposed by Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998) provide a practical approach to tackling the endogeneity problem while simultaneously accounting for unobserved heterogeneity. The key advantage of these methods is that they allow us to control for potential bias without relying on strictly exogenous instrumental variables, which in many empirical settings are impossible to obtain. Because of their practicality, dynamic panel data methods have been used in numerous contexts, including advertising (Clark et al., 2009; Xiong and Bharadwaj, 2013), customer-relationship management (Tuli et al., 2010), social media and online reviews (Borah and Tellis, 2016; Ludwig et al., 2013), and sales-call effectiveness (e.g., Mizik and Jacobson, 2004).

Nevertheless, under the Nerlove and Arrow (1962) advertising-as-investment framework, the use of dynamic panel data methods is afflicted by a troubling issue: namely, hinging on the geometric sum assumption, the linkage between the framework and the dynamic panel data structure, by construction, encompasses serially correlated errors. Yet the validity of traditional dynamic panel data methods relies on the assumption that the error structure does not exhibit serial correlation. If serial correlation is present, the moment conditions derived under these methods become invalid, resulting in unreliable inference.

To test for the validity of the moment conditions, and thus the model specification, past studies have relied on the Arellano-Bond test for serial correlation (Arellano and Bond, 1991). When the AR(2) test statistic is not rejected, presumably indicating an absence of serial correlation in the error structure, researchers have proceeded with the estimation without further concern. We show that the Arellano-Bond specification test is prone to weak power in detecting

serial correlation: the test statistic may fail to detect serial correlation, justifying the use of invalid moment conditions that result in biased estimates. We discuss technical shortcomings of the Arellano-Bond specification test and specify conditions where the test may fail to reject the misspecified model.

To obtain unbiased estimates for identifying the causal effect of detailing, we present an adequate set of moment conditions in analyzing dynamic panel data. We provide proof that test statistics used to detect serial correlation are biased when using invalid moment conditions, and that traditional dynamic panel data methods fail once the assumption on serial correlation is eased. We formulate valid moment conditions that are robust to serial correlation. To validate our claims, we conduct Monte Carlo simulations to show that traditional methods yield biased estimates under serially correlated errors. The simulation results also reveal the weak power of the Arellano-Bond specification test using traditional moment conditions, when serial correlation is present.

We apply our method to a comprehensive panel dataset from a multinational pharmaceutical company, which includes detailed data on individual doctors' prescribing histories and on sales representatives' personal selling efforts on behalf of the firm. We postulate that when serial correlation is present, traditional methods yield biased and counterintuitive estimates implying negative effectiveness of detailing. By correcting for the misuse of invalid moment conditions, we find, on aggregate, that detailing efforts have a significant impact on physicians' prescription rates.

Subsequently, we allow for heterogeneity in the slope parameters to account for differences in the effectiveness of detailing from one medical practice area to another. We find, in general, that specialist physicians (cardiologists, diabetologists, and endocrinologists) exhibit greater persistence

in prescribing patterns and thus a long-term effect; by contrast, generalists (consulting physicians, general practitioners, and general surgeons) are more responsive to short-term detailing efforts but exhibit less persistence. In terms of magnitude, the effectiveness of sales calls tends to be highly heterogeneous across medical practice areas, ranging from insignificant to a 25% increase in prescriptions. Our simple but methodologically robust approach can help firms design optimal call patterns and sales targets to increase the overall efficiency of the sales force.

The remainder of the paper is organized as follows. Section 2 presents our methodology, which builds on traditional dynamic panel data methods and eases their assumptions on serial correlation. Section 3 addresses the conditions in which the test for serial correlation is prone to weak power; Section 4 conducts simulation tests to verify our claims. Section 5 presents our empirical application and results for inference, and Section 6 concludes.

2. Dynamic Panel Data Revisited

Dynamic panel data models have played a pivotal role in analyzing marketing and economic phenomena of a dynamic nature. A typical linear dynamic panel data model follows the form:

$$y_{it} = \lambda y_{i,t-1} + \beta' x_{it} + \gamma' z_i + u_{it} \tag{1}$$

$$u_{it} = \alpha_i + e_{it} \tag{2}$$

where $i=1,2,\ldots,N$, indexes cross-sectional units and $t=2,3,\ldots,T$, indexes time. The scalar y_{it} is the dependent variable of interest, observed at the individual level, and the recursive nature of the lagged dependent variable $y_{i,t-1}$ on the right-hand side is the source of dynamics in the model. The vector x_{it} ($k_x \times 1$ dimension) represents time-varying independent variables, and the vector z_i ($k_z \times 1$

dimension) represents time-invariant independent variables. The parameters λ, β, and γ denote the carryover effect and marginal effects with regard to x_{it} and z_i respectively. The data consist of $(y_{i1}, y_{i2},..., y_{iT})$, $(x_{i1}, x_{i2},..., x_{iT})$, and z_i for $i=1, 2,..., N$, implying a dimension of $N \times T$ observations. The focus in dynamic panel data analysis is mainly on the case where N is large and T is small—typical data available in dynamic panel data settings.

The unobservable term u_{it} consists of two components, individual unobserved heterogeneity α_i and an idiosyncratic error e_{it}. The structure of the unobservable term in **Equation (2)** raises an endogeneity problem as the time-invariant unobserved heterogeneity component α_i is correlated with the lagged dependent variable $y_{i,t-1}$. This issue can be dealt with in a relatively straightforward manner by taking the first difference of **Equation (1)** to subtract out α_i. However, the endogeneity problem with regard to the idiosyncratic error term e_{it}—that is, the lagged dependent variable $y_{i,t-1}$ being correlated with the lagged unobservables e_{is} for $s<t$—remains a concern. The dynamic panel data methods proposed by Anderson and Hsiao (1981, 1982), and further developed by Arellano and Bond (1991), Arellano and Bover (1995), and Blundell and Bond (1998), utilize lagged levels and lagged differences as instruments to deal with this endogeneity issue, but their instruments are fully valid only under the assumption that the idiosyncratic errors e_{it} are uncorrelated over time.

We show that traditional tests to detect serial correlation have weak power and can be misleading, resulting in a misuse of moment conditions that leads to incorrect inference. The revised approach that we propose utilizes a set of moment conditions that is immune to serial

correlation in the error structure, highly likely in naturally occurring data. Before presenting our methodology, we will outline the key components of the traditional dynamic panel data models that serve as building blocks. We will then present our method and check its robustness using Monte Carlo simulations.

2.1. Traditional Dynamic Panel Data Methods

For expository purposes, we assume for now that vectors x_{it} and z_i are absent. We denote ε_{it} as an idiosyncratic error term, which is assumed to be serially uncorrelated. The following model structure has been widely discussed in the economics literature (Anderson and Hsiao, 1981, 1982; Arellano and Bond, 1991; Arellano and Bover, 1995; and Blundell and Bond, 1998):

$$y_{it} = \lambda y_{i,t-1} + u_{it} \tag{3}$$

$$u_{it} = \alpha_i + \varepsilon_{it} \tag{4}$$

where all variables are independently and identically distributed across i, and $\left|\lambda\right| < 1$. The idiosyncratic error component ε_{it} in **Equation (4)** satisfies the following standard assumptions: $\mathrm{E}[\varepsilon_{it}] = 0$ for all t (mean zero), $\mathrm{E}[\varepsilon_{it}\varepsilon_{is}] = 0$ for all $t \neq s$ (no serial correlation), $\mathrm{E}[\alpha_i\varepsilon_{it}] = 0$ for all t (orthogonal to individual effects), and $\mathrm{E}[y_{i1}\varepsilon_{it}] = 0$ for all t (orthogonal to initial condition). Under these assumptions, the following linear moment conditions can be derived:

$$\mathrm{E}[y_i^{t-2}\Delta u_{it}] = 0 \tag{5}$$

where $y_i^t = (y_{i1}, y_{i2}, \ldots, y_{it})'$ and $\Delta u_{it} = u_{it} - u_{i,t-1}$.[2] As proposed by Arellano and Bond (1991), the estimator utilizing the moment conditions in **Equation (5)** is commonly referred to as the *Difference GMM (DGMM)* estimator. The key concept of DGMM is to use the lagged variables in levels as instruments for the first differenced equation. A potential downside of the DGMM estimator is that lagged levels become weak instruments for the first difference as $\lambda \approx 1$, a case when the lagged levels take a random walk and convey limited information (Staiger and Stock, 1997; Stock et al., 2002).

As a remedy, the mean stationarity assumption, $\mathrm{E}[\alpha_i \Delta y_{i2}] = 0$, can be imposed to yield the initial condition of the data. Provided the assumption holds, the first period observation of y is written as $y_{i1} = \frac{\alpha_i}{1-\lambda} + \eta_i$, where η_i has zero mean and is uncorrelated with α_i. This implies that the deviation of the first observation from the stationarity level, $\frac{\alpha_i}{1-\lambda}$, is uncorrelated with the individual effects. Combining the initial condition with the orthogonality to individual effects assumption ($\mathrm{E}[\alpha_i \varepsilon_{it}] = 0$ for all t), we can derive $\mathrm{E}[u_{i3} \Delta y_{i2}] = 0$. Hence, by iteration, the following linear moment conditions become further available:

$$\mathrm{E}[u_{it} \Delta y_{i,t-1}] = 0 . \tag{6}$$

As proposed by Arellano and Bover (1995) and Blundell and Bond (1998), the *System GMM (SGMM)* estimator creates a stacked dataset and utilizes both lagged levels to instrument for differences (**Equation (5)**) and lagged differences to instrument for levels (**Equation (6)**). Thus

[2] For purposes of brevity, superscript t is used throughout the paper to denote a vector of all observations prior to time t. For example, $y_i^t = (y_{i1}, y_{i2}, \ldots, y_{it})'$, and for the entire time horizon $y_i^T = (y_{i1}, y_{i2}, \ldots, y_{iT})'$. Also, following the standard notation in the literature, the capital Greek letter delta Δ represents a first-difference operator. For example, $\Delta u_{it} = u_{it} - u_{i,t-1}$ and $\Delta y_{it} = y_{it} - y_{i,t-1}$.

the SGMM estimator extracts more information from the data, and benefits from an increased number of moment conditions. Though the mean stationarity assumption is not necessary for identification, it contributes to the efficiency gain of the estimators by providing more linear moment conditions. In terms of the available number of moment conditions, there are $\frac{(T-1)(T-2)}{2}$ conditions in **Equation (5)** for DGMM estimators, and additional (*T-2*) conditions in **Equation (6)** for SGMM estimators.

2.2. A Motivating Example: The Advertising-as-Investment Model

Though traditional methods have played a significant role in the dynamic panel data literature, the key underlying assumption that ε_{it} is serially uncorrelated remains questionable. As a motivating example, let us consider the advertising-as-investment model of Nerlove and Arrow (1962), which has been widely applied in the marketing literature:

$$y_{it} = \tilde{\alpha}_i + \sum_{j=0}^{\infty} \lambda^j \left(\beta x_{i,t-j}\right) + \tilde{\gamma}' z_i + \nu_{it}$$

where y_{it} denotes sales and x_{it} denotes the advertisement expenditure of firm i at time t. The model captures the long-term effects of advertisement using an infinite lag distribution model. Because part of the infinite geometric sum can be replaced by $\lambda y_{i,t-1}$, this model simplifies to:

$$y_{it} = \lambda y_{i,t-1} + \beta x_{it} + \gamma' z_i + u_{it}$$

$$u_{it} = \alpha_i + \nu_{it} - \lambda \nu_{i,t-1}$$

where $\gamma = \left(1-\lambda\right)\tilde{\gamma}$ and $\alpha_i = \left(1-\lambda\right)\tilde{\alpha}_i$. Hence, the unobserved term u_{it} is shown to exhibit serial correlation caused by the error component $\nu_{i,t-1}$.

2.3. Correcting for Serial Correlation in the Error Component

Next we will consider our serial correlation corrected (hereafter, SCC) approach, which accounts for serial correlation in the error structure. The idiosyncratic error term in **Equation (2)** is now modified to exhibit serial correlation, $e_{it} = \nu_{it} - \lambda\nu_{i,t-1}$:

$$y_{it} = \lambda y_{i,t-1} + u_{it} \tag{7}$$

$$u_{it} = \alpha_i + \nu_{it} - \lambda\nu_{i,t-1} \tag{8}$$

where, again, all variables are i.i.d. across i, and $\left|\lambda\right| < 1$. Now the error term u_{it} is decomposed into time-invariant individual effects α_i and the time-varying error component ν_{it}.[3] Through the term ν_{it}, the model exhibits serial correlation that introduces additional dynamics into the model. The assumptions from **Equation (4)** are modified to accommodate the new error structure regarding ν_{it} in **Equation (8)** as follows: $\mathrm{E}[\nu_{it}] = 0$ for all t (mean zero), $\mathrm{E}[\nu_{it}\nu_{is}] = 0$ for all $t \neq s$ (no serial correlation outside the error structure), $\mathrm{E}[\alpha_i\nu_{it}] = 0$ for all t (orthogonal to individual effects), and $\mathrm{E}[y_{i1}\nu_{it}] = 0$ for all $t \geq 2$ (orthogonal to initial condition).

Now let us consider the valid moment conditions under the serially correlated errors. For traditional DGMM estimators, the intuition behind the moment conditions $\mathrm{E}[y_i^{t-2}\Delta u_{it}] = 0$ in **Equation (5)** is that $\Delta u_{it} = \varepsilon_{it} - \varepsilon_{i,t-1}$ is uncorrelated with ε_{is} for any $s < t-2$, and thus is orthogonal to y_{is} for $s < t-2$. However, due to serial correlation, $\nu_{it} - \lambda\nu_{i,t-1}$, in **Equation (8)**, we have $\Delta u_{it} = \nu_{it} - (1+\lambda)\nu_{i,t-1} + \lambda\nu_{i,t-2}$, which contains an error from *t-2*. Thus in this case

[3] We also investigated the specification $e_{it} = \nu_{it} - \lambda\nu_{i,t-1} + \varepsilon_{it}$, which disentangles the serially uncorrelated shocks. The resulting proof and moment conditions are largely analogous; we omit them for brevity.

$\mathrm{E}[y_{i,t-2}\Delta u_{it}] \neq 0$ as both terms share the common $\nu_{i,t-2}$. However, the moment conditions for *t-3* and earlier remain valid. Hence, the moment conditions in **Equation (5)** are modified by the following $\frac{(T-2)(T-3)}{2}$ set of conditions for the DGMM estimator under the serially correlated error structure assumption:

$$\mathrm{E}[y_i^{t-3}\Delta u_{it}] = 0 \tag{9}$$

for *t=4,5,…,T*.

The loss of moment conditions reduces the efficiency of the estimator. The problem may be particularly significant under two circumstances often encountered in practice. The first occurs when the length of the observed time periods T is short: the shorter the observed length, the larger the proportion of invalid moment conditions becomes. The second case takes place when the weak-instrument problem is present. Blundell and Bond (1998) show that the instruments become weak when λ is close to 1. That is, $\mathrm{Cov}(y_{i,t-r}, \Delta y_{it})$ for $r \geq 2$ leans to zero as λ tends to unity. Since the covariance decreases as r increases, $y_{i,t-2}$ is a stronger (or, more precisely, less weak) instrument for Δy_{it} than $y_{i,t-r}$ for $r \geq 3$. Thus, the instruments that become invalid under the new assumptions happen to be the strongest among the weak, exacerbating the weak-instrument problem.

Analogous to the traditional approach, the mean stationarity assumption can be induced to alleviate the above issue. The initial condition for the SCC approach can be derived analogously given the mean stationarity assumption $\mathrm{E}[\alpha_i \Delta y_{i2}] = 0$. However, the expression for y_{i1} is modified to incorporate ν_{i1} by $y_{i1} = \frac{\alpha_i}{1-\lambda} + \nu_{i1} + \eta_i$, where $\mathrm{E}[\eta_i] = \mathrm{E}[\alpha_i \eta_i] = \mathrm{E}[\nu_{i1}\eta_i] = 0$. Unlike the

traditional approach in **Equation (6)**, however, in the SCC approach $\mathrm{E}[u_{i3}\Delta y_{i2}] \neq 0$ due to the presence of ν_{i2} in both u_{i3} and Δy_{i2}. Instead, we have $\mathrm{E}[u_{i4}\Delta y_{i2}] = 0$. The lagged differences should also be at least two periods to avoid the common error component. Hence, the *T-2* number of moment conditions from the traditional approach in **Equation (6)** is replaced in the SCC approach by the following *T-3* conditions:

$$\mathrm{E}\left[u_{it}\Delta y_{i,t-2}\right] = 0 \tag{10}$$

for *t=4,5,...,T*, to yield a SGMM estimator under serially correlated errors.

2.4. General Framework

We now return to the general form of the dynamic panel data model in **Equation (1)**, and discuss the moment conditions pertinent to the explanatory variables x_{it} and z_i. The error term in **Equation (2)** is altered on the basis of the SCC method to $e_{it} = \nu_{it} - \lambda\nu_{i,t-1}$:

$$y_{it} = \lambda y_{i,t-1} + \beta' x_{it} + \gamma' z_i + u_{it}$$

$$u_{it} = \alpha_i + \nu_{it} - \lambda\nu_{i,t-1}$$

for *i=1,2,...,N* and *t=2,3,...,T*. To define moment conditions with regard to regressors, one needs to comprehend their potential correlation with the individual effects. Following Hausman and Taylor (1981), we partition the vector of the independent variables as $x_{it} = (x_{1it}, x_{2it})$ and $z_{it} = (z_{1i}, z_{2i})$. x_{1it} and z_{1i} are vectors orthogonal to the individual effects, whereas x_{2it} and z_{2i} are not—that is, the latter are correlated with the individual effects. In addition to the assumptions specified in Section 2.3, the following standard assumptions with regard to the regressors hold:

$\mathrm{E}[x_{it}\nu_{is}] = 0$ for all $t<s$ (x_{it} is predetermined with respect to ν_{it}), $\mathrm{E}[z_i\nu_{it}] = 0$ for all t (z_i is orthogonal to idiosyncratic errors), $\mathrm{E}[x_{1it}\alpha_i] = \mathrm{E}[z_{1i}\alpha_i] = 0$ for all t (x_{1it} and z_{1i} are orthogonal to individual effects), and $\mathrm{E}[x_{2it}\alpha_i] = \Sigma_{x\alpha}$ for all t (correlation between x_{2it} and individual effects is constant over time).

Predetermined variables, as the term implies, are variables whose values are determined by observations prior to the current period. Thus current-period errors are uncorrelated with current and lagged values, but not necessarily with future values, of predetermined variables. Utilizing predetermined variables in panel data models has been discussed in Hausman and Taylor (1981), Amemiya and MaCurdy (1986), and Breusch et al. (1989). Under the above assumptions, and accounting for serial correlation in the error structure, the following moment conditions are available for estimation:

$$\begin{aligned}
&\mathrm{E}[x_{1it}u_{is}] = 0 \quad \text{for } t \leq s-1 \\
&\mathrm{E}[x_{2it}\Delta u_{is}] = 0 \quad \text{for } t \leq s-2 \\
&\mathrm{E}[\Delta x_{2i,t-2}u_{it}] = 0 \quad \text{for } t = 4,5,...,T \\
&\mathrm{E}[z_{1i}u_{it}] = 0 \quad \text{for } t = 1,2,...,T \\
&\mathrm{E}[z_{2i}\Delta u_{it}] = 0 \quad \text{for } t = 1,2,...,T\,.
\end{aligned} \tag{11}$$

3. Failure of Tests for Serial Correlation

It is crucial to check for the presence of serially correlated errors, which determines the construction of the moment conditions used for estimation. The Arellano and Bond (1991)

specification test—specifically, the AR(2) test—has been widely employed in empirical applications to check for serial correlation in idiosyncratic errors.

However, the AR(2) test may generate unreliable results when test statistics are constructed using biased estimators obtained from invalid moment conditions (Bowsher (2002), for example, finds that AR tests have extremely low power in finite samples). For expository purposes, let us suppose that there exists serial correlation in the errors. The previous section showed that estimators from traditional methods can be biased because they are obtained using invalid moment conditions. Nevertheless, in the test procedure these biased estimators are recursively utilized to calculate the error components in the model—which are also likely to become biased. As the basis of the test statistic becomes biased, the AR(2) test exhibits weak power and may fail to reject the null hypothesis of no serial correlation.

We now elaborate on this argument. Let us suppose that the true model is represented by **Equations (7)** and **(8)**, where the error structure is serially correlated. However, without knowing whether serial correlation is present, suppose we estimate λ in **Equation (3)** using the traditional moment conditions given by **Equation (5)**. Here we are particularly interested in testing the null hypothesis of no serial correlation $\mathrm{E}[e_{it}e_{i,t-1}] = 0$ against its negation. Because there exists first-order serial correlation in levels, we have $\mathrm{E}[e_{it}e_{i,t-1}] = -\lambda\mathrm{E}(\nu_{i,t-1}^2) \neq 0$ for all t. Thus, the moment conditions in **Equation (5)** become invalid because $\mathrm{E}[y_{i,t-2}\Delta u_{it}] = \lambda\mathrm{E}(\nu_{i,t-2}^2) \neq 0$. In this case the GMM estimator $\hat{\lambda}$ is expected to be downward-

biased because the sign of the correlation between $\Delta y_{i,t-1}$ and Δu_{it} is negative (assuming $\lambda > 0$); thus the misspecification bias would also be negative. For brevity, let $B = E(\hat{\lambda} - \lambda)$ be the bias.

To check for first-order serial correlation in levels, the Arellano-Bond AR(2) test looks at the second-order correlation in differences. In other words, $\mathrm{E}[\Delta u_{it}\Delta u_{i,t-2}] = 0$ only when the null hypothesis is true. Let $\widehat{\Delta u_{it}} = \Delta y_{it} - \hat{\lambda}\Delta y_{i,t-1}$ be the sample estimate of Δu_{it}. Due to the bias in $\hat{\lambda}$, the estimate $\widehat{\Delta u_{it}} \approx \Delta u_{it} - B\Delta y_{i,t-1}$ is also contaminated. By replacing this expectation with the sample counterparts, we have:

$$\mathrm{E}[\widehat{\Delta u_{it}}\,\widehat{\Delta u_{i,t-2}}] \approx \mathrm{E}[\Delta u_{it}\Delta u_{i,t-2}] - B\mathrm{E}[\Delta y_{i,t-1}\Delta u_{i,t-2}] - B\mathrm{E}[\Delta y_{i,t-3}\Delta u_{it}] + B^2\mathrm{E}[\Delta y_{i,t-1}\Delta y_{i,t-3}]\,.$$

For illustrative purposes, let us further assume homoskedasticity in differences (i.e., $E(\nu_{it}^2) = \sigma_\nu^2$ for all t). Substituting the components in **Equations (7)** and **(8)** causes the above terms to become:

$$\mathrm{E}[\Delta u_{it}\Delta u_{i,t-2}] = \lambda\sigma_\nu^2 > 0$$

$$\mathrm{E}[\Delta y_{i,t-1}\Delta u_{i,t-2}] = -\sigma_\nu^2$$

$$\mathrm{E}[\Delta y_{i,t-3}\Delta u_{it}] = \mathrm{E}[\Delta y_{i,t-1}\Delta y_{i,t-3}] = 0$$

Note that, under absence of bias ($B=0$), $\mathrm{E}[\widehat{\Delta u_{it}}\,\widehat{\Delta u_{i,t-2}}]$ would converge to $\lambda\sigma_\nu^2$, and the degree of serial correlation captured by λ and σ_ν^2 would jointly determine the test statistic. However, when $\hat{\lambda}$ becomes biased, $\mathrm{E}[\widehat{\Delta u_{it}}\,\widehat{\Delta u_{i,t-2}}]$ would converge to $(\lambda + B)\sigma_\nu^2$, and, because B is likely negative, $\mathrm{E}[\widehat{\Delta u_{it}}\,\widehat{\Delta u_{i,t-2}}]$ would become downward-biased. Thus, depending on the degree of bias in

$\hat{\lambda}$, the test statistic based on $\mathrm{E}[\widehat{\Delta u_{it}}\widehat{\Delta u_{i,t-2}}]$ may falsely infer that $\mathrm{E}[\widehat{\Delta u_{it}}\widehat{\Delta u_{i,t-2}}] = 0$ and fail to reject the null hypothesis, conferring validity on incorrect inference.

The essence of this problem is a trade-off between efficiency and robustness. Although the test statistic utilizing all moment conditions in **Equation (5)** is expected to be more efficient, this efficiency gain does not materialize due to some invalid moment conditions. Rather, in this case, the cost of the misspecification bias dominates the benefit of using more moment conditions, particularly in finite samples. In Section 4, we verify these assertions using simulation studies, and demonstrate the poor performance of the test statistic under traditional methods when serial correlation is present.

4. Simulation Study

To compare and evaluate the performance of different estimators and of the Arellano-Bond specification test, we run Monte Carlo experiments using simulated data. We set the data-generating process to follow a simple form of the model with one predetermined variable (k_x=1):

$$y_{it} = \lambda y_{i,t-1} + \beta x_{it} + u_{it}$$

where u_{it} does not exhibit serial correlation ($u_{it} = \alpha_i + \varepsilon_{it}$) in *Case 1* and does so ($u_{it} = \alpha_i + \nu_{it} - \lambda\nu_{i,t-1}$) in *Case 2*. Utilizing the typical structure of dynamic panel data in a real-world setting, where N is large and T is small, we simulate data for N=1,000 and T=10. For each setting, we run 200 Monte Carlo iterations and report the mean values of the estimates. In interpreting the results, the terms *traditional approach* and *SCC approach* are used to designate the estimators utilizing the moment conditions derived in Sections 2.1 and 2.3 respectively.

4.1. Case 1: Without serial correlation

For Case 1, we set the error structure in the data-generating process to follow **Equation (4)**, which does not exhibit serial correlation:

$$u_{it} = \alpha_i + \varepsilon_{it}\,.$$

As discussed in Section 2.1, the corresponding initial condition of the data generating process is $y_{i0} = \frac{\alpha_i}{1-\lambda} + \eta_i$, where $\alpha_i \sim^{i.i.d.} U(0,1)$ and $\eta_i \sim^{i.i.d.} N(0,1)$. We allow for heteroskedasticity in the error term, namely $\varepsilon_{it} \sim N(0, \sigma_\varepsilon^2)$, where $\sigma_{\varepsilon,it}^2 = \theta_0 + \theta_1 \cdot x_{it}^2$. We set $\theta_0 = 0.8$ and $\theta_1 = 0.2$ for the remainder of the experiment.

The simulation results appear in **Table 1**. The upper portion of the table presents mean estimates across iterations; the lower portion reports the rejection frequency of the Arellano-Bond specification test. As the mean estimates demonstrate, all four methods perform well at recovering the model primitives. A slight exception occurs for DGMM estimators, for both traditional and SCC methods, as λ approaches 1, where the carryover-rate estimates become slightly downward-biased. As discussed, this phenomenon embodies the weak-instruments problem, where lagged levels lose information as $\lambda \approx 1$ and become poor instruments for the first differences.

As for the test statistics, the null hypotheses for the Arellano-Bond AR(1) and AR(2) tests are that there exists no first- and second-order serial correlation, respectively, in the differenced error structure. The results show that AR(1) tests are rejected 100% of the time across all models, implying that a first-order serial correlation exists among the differences; this is to be expected, as Δu_{it} and $\Delta u_{i,t-1}$ are correlated through the shared $\varepsilon_{i,t-1}$ term by construction. By contrast, the

AR(2) tests are not rejected; rejection frequency hovers around 5%, outside of the confidence interval. Therefore, the power of the test is shown to be reliable in the absence of serial correlation.

4.2. Case 2: With serial correlation

Next we impose serial correlation on the error structure for data generation. We set the error structure according to **Equation (8)**:

$$u_{it} = \alpha_i + \nu_{it} - \lambda \nu_{i,t-1} .$$

The corresponding initial condition for y is replaced by $y_{i0} = \frac{\alpha_i}{1-\lambda} + \nu_{i0} + \eta_i$, where $\alpha_i \sim^{i.i.d.} U(0,1)$ and $\eta_i \sim^{i.i.d.} N(0,1)$. As in Case 1, we allow for heteroskedasticity in the serially correlated error term, $\nu_{it} \sim N(0, \sigma_\nu^2)$, where $\sigma_{\nu,it}^2 = \theta_0 + \theta_1 \cdot x_{it}^2$.

The simulation results are reported in **Table 2**. As the mean estimates show, traditional methods' estimates exhibit a strong bias. Most of the λ estimates in DGMM and SGMM fail to recover their true values; as a result, β estimates also tend to become downward-biased. By contrast, the SCC method remains robust. A slight exception appears in the DGMM estimates in cases where the true value of λ approaches unity, indicating the weak-instrument problem discussed earlier. The SGMM estimates remain robust within the true parameter values across all values of λ.

With regard to the power of tests, the AR(1) test precisely detects first-order serial correlation in differences across all methods except DGMM when $\lambda = 0.9$. For AR(2) tests, however, some noteworthy patterns are evident. First, regardless of approach (traditional or SCC), the power of tests based on DGMM decreases significantly as $\lambda \to 1$ due to bias in the estimates arising from

the weak-instrument problem. In contrast, the tests based on SGMM remain robust to the weak-instrument problem, where rejection frequency remains steady over different values of λ. Second, the tests based on the SCC approach correctly reject the null because the estimates are unbiased; by contrast, the power of the test using traditional methods decreases due to bias in the estimates, as discussed in Section 3. In the current setting, the SGMM under the traditional approach rejects the null on average in only 70% of cases, implying that for the remaining 30% the underlying serial correlation could go undetected.[4] Third, the low power of the test across both approaches for λ=0.1 to 0.2 reflects the actual decrease in the degree of serial correlation.

Overall, the simulation results reveal that, with the existence of serial correlation in the idiosyncratic shocks, applying traditional methods can inadvertently yield a severe downward bias in the estimates. In practice, this bias may lead to misinterpreting or undermining of the marginal effects of the covariates. In contrast, the revised SCC approach is robust in detecting plausible serial correlation and recovering the model primitives.

5. Empirical Analysis

This section applies our proposed method to real-world data to properly examine the short- and long-term effects of detailing visits in the pharmaceutical industry. Following an institutional description of, and data on, the focal firm, we develop our empirical model and discuss our findings. Finally, we present a representative case in which traditional methods may fail to detect

[4] The power of the test under traditional SGMM decreases monotonically as the variance of serially correlated error increases, whereas that of SCC SGMM remain robust. Simulation results for different values of θ_0 and θ_1 are available from the authors on request.

underlying serial correlation and thus falsely justify the use of biased outcomes, leading to improper inference.

5.1. Data and Institutional Details

Our focal firm is a highly regarded Fortune 500 company that operates in over 150 countries. It offers a broad range of branded generic pharmaceuticals, medical devices, diagnostics, and nutritional products. Our empirical analysis utilizes data from the chronic-care sales division of the firm's business operations in India. The data consist of a detailed record of prescriptions written by 45,025 physicians over the six-month period from January through June 2016. For each physician, we observe the number of prescriptions written, the number of visits by a sales representative, and the number of free samples provided to the physician.

The firm organizes its sales activity by route call sales: each sales representative is assigned a series of scheduled visits, within a given time period, to physicians along a set route. Our data are unique in that they include the full range of the firm's brands. Previous studies of detailing effectiveness (Parsons and Vanden Abeele, 1981; Manchanda et al., 2004; Mizik and Jacobson, 2004) have been limited to data on a single brand or a few brands; their results neglect possible spillover effects among brands, and are thus apt to underestimate the overall effectiveness of detailing. Because firms and managers are likely to be most interested in the impact of sales calls (and free samples) on overall performance, we believe that our dataset provides a better measure for evaluating the true effect of detailing efforts.[5]

[5] Because the firm does not track the specific brands detailed during each call, we aggregate prescription quantities across the firm's brands to obtain the total number of prescriptions written per month. Though the effectiveness of

To fully exploit the nature of a dynamic panel data model, we restrict our attention to physicians whose interactions with the sales force are ongoing, and for whom the data include no intermissions in prescription history. To explore differences in the effectiveness of sales calls across physician specialties, we focus exclusively on the six medical practice areas that account for approximately 90% of the active physicians in our data: cardiologists, diabetologists, endocrinologists, consulting physicians, general practitioners, and general surgeons. For expository purposes, we will refer to the first three groups as *specialists* and the latter three as *generalists*. These restrictions lead us to focus our attention on N=9,595 physicians over T=6 month horizon.

Figure 1 depicts the empirical distribution of prescriptions and detailing calls. Figure 1a shows the number of prescriptions per month to be highly heterogeneous and right-skewed across doctors, implying significant unobserved physician heterogeneity. The number of calls per month, illustrated in Figure 1b, shows heterogeneity similar in shape to Figure 1b but also discreteness: the majority of observations fall between 1 and 7 visits, in keeping with the firm's route sales procedure (and monthly quotas).

Tables 3–5 report descriptive statistics by medical practice area for numbers of prescriptions, detailing calls, and free samples respectively. Most striking is the magnitude of between-group difference: both sales-force efforts and outcomes are, on average, greater for the specialists. Heterogeneity in both prescriptions and sales calls within specialists is also noteworthy: although the diabetologists write the most prescriptions, sales-force efforts are more intensively targeted at the endocrinologists. Among generalists, however, within-group heterogeneity is only modest:

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detailing may vary from brand to brand, analysis at the aggregate level offers generalizable insights: the firm's 81 brands represent a comprehensive cross-section of drugs in the marketplace.

consulting physicians generate slightly more attention and sales. The preceding section accounts for heterogeneity while addressing the endogeneity issues inherent in using lagged variables to represent the dynamics of detailing efforts.

5.2. The Empirical Model

We model doctor *i*'s prescriptions of the focal firm's pharmaceutical drugs at time *t*, S_{it}, as a function of an unobserved doctor-specific effect $\tilde{\alpha}_i$ constant over time, a stock of goodwill G_{it} (created by the firm's sales force), a time-specific effect (reflecting seasonality) $\tilde{\delta}_t$ common to all physicians, and an idiosyncratic unobserved component ν_{it} such that:

$$S_{it} = \exp\left(\tilde{\alpha}_i + G_{it} + \tilde{\delta}_t + \nu_{it}\right).$$

We use the multiplicative form to prevent overweighting of high-volume prescribers. The stock of goodwill G_{it} is assumed to increase with current-period sales efforts x_{it}, but to decay over time, taking the geometric decay form of

$$G_{it} = \beta' x_{it} + \lambda\beta' x_{i,t-1} + \lambda^2\beta' x_{i,t-2} + ...$$

where λ is the carryover rate ($1-\lambda$ would be the decay rate).[6]

Our empirical application uses the total number of detailing calls and free samples provided to physician i during month t to create the detailing-effort vector x_{it}. As shown in Section 2.2 and typically assumed in the marketing literature as the data-generating process of advertising (Lilien et al., 1992: 290), the geometric sum can be replaced by the lagged dependent variable $s_{i,t-1}$. Hence our model specification simplifies to

[6] Because our model is multiplicative, the parameter λ represents elasticity in the current setting.

$$s_{it} = \lambda s_{i,t-1} + \beta' x_{it} + \delta_t + u_{it} \tag{12}$$

$$u_{it} = \alpha_i + \nu_{it} - \lambda \nu_{i,t-1}$$

where $s_{it} = \log(S_{it})$, $\alpha_i = (1-\lambda)\tilde{\alpha}_i$, and $\delta_t = \tilde{\delta}_t - \lambda\tilde{\delta}_{t-1}$.[7] Notice that by the geometric sum assumption of the stock of goodwill, the error structure, by construction, posits serial correlation.

For our empirical estimation, we construct moment conditions for **Equation (12)** by two different assumptions with regard to serial correlation, as discussed in Section 2. First, in the traditional approach, we utilize levels of time lags *t-2* and earlier to instrument for the first difference of the lagged dependent variables as in **Equation (5)**—DGMM, and, in addition, the differences of time lags *t-1* to instrument for the levels as in **Equations (5)** and **(6)**—SGMM. In our modified SCC approach, we restrict the use of time lags to *t-3* and earlier in DGMM, as in **Equation (9)**, and to *t-2* in SGMM as in **Equations (9)** and **(10)**.

In addition to the endogeneity issue associated with using lagged dependent variables, detailing efforts may possess an endogeneity problem because the error component in **Equation (12)** encapsulates any other unobservable factors that affect physicians' prescriptions. By using an extensive set of physician fixed effects, we mitigate this concern. Furthermore, because sales visits are scheduled in advance, and as a matter of company policy rarely rescheduled, we believe that by treating detailing efforts as predetermined we avoid this endogeneity issue. We limit the total number of moment conditions for our predetermined variables, detailing calls and free samples, by using only the most recent lag available for the differenced equation to prevent a potential

[7] We tested for diminishing returns to detailing efforts by including quadratic terms, as in Manchanda and Chintagunta (2004). However, all coefficients for the quadratic terms are found to be insignificant. Thus we exclude them from the analysis.

overfitting problem.[8] Thus, in our SCC approach we utilize $E[x_{2i,t-2}\Delta u_{it}] = 0$ for DGMM, and both $E[x_{2i,t-2}\Delta u_{it}] = 0$ and $E[\Delta x_{2i,t-2}u_{it}] = 0$ for SGMM, among the conditions given in **Equation (11)**; in the traditional approach, we use lags of *t-1* in an analogous manner.

5.2.1. Results: A Homogeneous Model

Table 6 reports parameter estimates of the model given in **Equation (12)**. We first turn our attention to the specification test results. The Arellano-Bond test for serial correlation shows that both AR(1) and AR(2) are rejected across all specifications. This result implies the existence of both first- and second-order serial correlation in the differenced error structure, providing a strong rationale for restricted use of the instruments as in our proposed methodology.

Because serial correlation exists in the unobserved components of the data, the key assumption under the traditional methods is not satisfied. Hence the estimates obtained using the improper moment conditions of traditional methods, in the first and second columns of Table 6, are biased. This is evident in the counterintuitive results, which attribute negative effectiveness to detailing. Also, the carryover elasticity estimates become downward-biased, as shown in Section 4.2.

In the third column of **Table 6**, the DGMM estimates under the SCC approach, which imposes restricted moment conditions, show recovered carryover elasticity measures. However, the model suffers from the weak-instruments problem associated with the sole use of levels as instruments for differences, and the slope parameters representing the effectiveness of sales efforts remain insignificant. Thus, for model inference, we turn our attention to the SGMM estimates

[8] We also tested exploiting the full set of moment conditions for the predetermined variables (all available lags of levels, given by the second row of Equation (11)); the results were qualitatively similar.

under the SCC approach, which impose proper moment conditions while extracting more information from the data to correct for the weak-instrument problem. In the rightmost column of **Table 6** we find that, on aggregate, the long-term effect—specifically, the carryover effect—is 0.545, and that in the short term a unit increase in detailing calls elicits a 10.1% increase in prescriptions by the physician.[9]

5.2.2. Heterogeneity in Detailing Effectiveness across Specialties

The preceding section accounts only for permanent heterogeneity via physician fixed effects. In reality, firms care about the effectiveness of detailing across different medical specialties. To investigate differences in the value of sales efforts across specialties, we allow for different slope parameters for each specialty such that

$$s_{it} = \sum_{d} I_{(i \in S_d)} (\lambda_d s_{i,t-1} + \beta_d' x_{it}) + \delta_t + u_{it} \qquad (13)$$

where $I_{(i \in S_d)}$ is an indicator function that equals one if doctor i is a member of specialty d, S_d. The model incorporates heterogeneity by allowing different carryover (λ_d) and detailing effectiveness (β_d) in different specialties. The estimates for **Equation (13)** using various moment conditions are reported in **Table 7**. The general observable pattern with regard to different estimation methods is analogous to the homogenous model discussed in the previous subsection: presence of serial correlation, biased traditional estimators, and inefficiency of DGMM due to weak instruments. Thus, for model inference, we again turn our attention to the results from the SGMM

[9] We calculated the magnitude of percentage increase using exp(0.096)-1=0.1008 due to the log-transformed dependent variable.

estimator based on the SCC approach. Three observations are worth noting in **Table 7**: (1) a stronger long-term effect (greater inertia) for specialist physicians, (2) a greater short-term marginal effect of detailing for generalist physicians, and (3) a limited effect of free samples.

The parameter estimates of the lagged dependent variable (carryover effect) are larger for specialists, whose elasticity measures range from 0.591 to 0.706. In contrast, those of generalists range from 0.388 to 0.566; general surgeons exhibit the lowest inertia. These results imply that the *long-term* effect of sales efforts is more pronounced for specialists: because specialists tend to prescribe a narrower range of products, focused on specific symptoms and likely to have few if any substitutes, specialists commonly exhibit greater stickiness to a particular product (from a specific firm). In contrast, the *short-term* marginal effect of detailing is larger for generalists, indicating willingness to try and then to prescribe new drugs. Generalists typically prescribe a wide range of generic drugs, many of which have substitutes from competing firms.

Thus we observe a general trend: specialists exhibit high inertia and low sensitivity to detailing; generalist are less persistent in their prescribing behavior and more responsive to short-term detailing efforts. We also find that free samples dispensed during detailing calls have trivial effects on physicians' prescribing behavior.

5.2.3. Empirical Evidence: Failure of Tests for Serial Correlation

The analyses reported in the preceding subsections were conducted conditional on the Arellano-Bond AR(2) tests being rejected, i.e., on second-order serial correlation sufficiently strong to be detected across all methods. Hence, the researcher in those cases would have been cautious about applying the traditional dynamic panel data methods, and would have utilized restricted

moment conditions (lags *t-3* and below) as in our proposed method. This subsection presents a case in which the traditional methods fail to reject the model despite the presence of serial correlation, leading to biased estimates and incorrect inference.

For this analysis, we run the model in **Equation (12)** separately for each physician specialty. The results for diabetologists appear in **Table 8**. We find that the AR(2) test is rejected only when the SGMM estimator under the SCC approach is used. As is evident in **Table 2**, the AR(2) test statistic using moment conditions of the traditional approach exhibits weak power and fails to reject the null hypothesis of no second-order serial correlation. Also, the AR(2) test statistic for DGMM under the SCC approach suffers from both the weak-instruments problem and from the moderate effectiveness of explanatory variables, similar to the conditions reported in the rightmost columns of the table.

In this case, the AR(2) test statistic may falsely justifies the misspecified model using the unrestricted moment conditions provided by the traditional methods. Thus, by using estimates derived from the traditional methods, researchers can mistakenly infer that sales efforts have no significant effect (SGMM) or could even yield a negative outcome (DGMM).

6. Conclusion

Personal selling in the form of detailing to physicians is the prevailing go-to-market practice in the pharmaceutical industry. Nevertheless, findings on the impact of sales calls have varied widely, and controversially, from study to study. Inappropriate methods and imprecise data are the main causes of this variation. This paper develops and estimates a generalized model to accurately

derive the short- and long-term effects of detailing on physicians' prescribing behavior. To encompass the intertemporal nature of detailing and to control for physician heterogeneity, we utilize a dynamic panel data method as the basis of our empirical analysis.

We introduce a key methodological innovation to the marketing and economics literatures. We challenge the widely used serial correlation assumption (or the lack of such an assumption) about the error structure in traditional dynamic-panel-data settings and derive a more appropriate set of moment conditions that can properly address serial correlation. Such correlation is apt to be present in the empirical context of sum of marketing efforts characterized by geometric decay—for example, in the advertisement-as-investment model of Nerlove and Arrow (1962), which has been extensively utilized in the literature. Using the general structure of a dynamic panel data model, we review the validity of instruments with respect to assumptions about serial correlation and discuss corresponding plausible moment conditions for estimation.

We also review the Arellano-Bond specification test for serial correlation, routinely used in traditional dynamic panel data settings. We provide proof that, in the presence of serial correlation, the test statistic becomes weak and imprecise at detecting it. This shortcoming leads to a misuse of moment conditions that results in biased parameter estimates and incorrect inference. To validate our claim, we run simulation studies and verify the failure of test statistics under traditional methods. We provide a revised set of moment conditions appropriate for unbiased identification of model primitives in a dynamic panel data setting.

For our empirical analyses, we apply our proposed method to comprehensive data on detailing. We first show the existence of serial correlation in the data, and the corresponding failure of

traditional methods. Inadequate assumptions on serial correlation result in downward bias of parameter estimates. By analyzing differences in the effectiveness of detailing across medical practice areas, we find a substantial amount of heterogeneity in both persistence and short-term responsiveness to detailing efforts. Our results reveal that specialist physicians exhibit a greater long-term effect but only modest short-term responsiveness to detailing. In contrast, generalist physicians tend to be more responsive to sales calls in the short term, although the effect may not be long-lasting. Across all specialties, free samples are minimally effective at generating additional prescriptions.

In summary, this paper provides a simple and practical but rigorous framework to precisely analyze the effectiveness of personal selling efforts. We believe that our method and empirical insights can help firms allocate sales-force resources more efficiently and devise optimal routes and call-pattern designs. Although our empirical application is in the personal-selling domain, our model can be extended to other contexts, such as advertising. We believe that doing so will help both academics and practitioners to better understand economic phenomena of a dynamic nature.

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Table 1: Simulation Results without Serial Correlation (Case 1)

Mean Estimates											
True Value		λ	0.1000	0.2000	0.3000	0.4000	0.5000	0.6000	0.7000	0.8000	0.9000
		β	0.9000	0.8000	0.7000	0.6000	0.5000	0.4000	0.3000	0.2000	0.1000
Traditional	DGMM	λ	0.0989	0.1977	0.2968	0.3981	0.4940	0.5919	0.6883	0.7794	0.8402
		β	0.8982	0.7997	0.6989	0.5987	0.4984	0.3998	0.2975	0.1973	0.0944
	SGMM	λ	0.1007	0.2008	0.3012	0.4017	0.5019	0.6026	0.7032	0.8053	0.9078
		β	0.8989	0.8005	0.7003	0.5995	0.5006	0.4020	0.3001	0.2007	0.0999
SCC	DGMM	λ	0.0978	0.1959	0.2938	0.3966	0.4878	0.5844	0.6766	0.7635	0.8113
		β	0.9021	0.7910	0.6932	0.6013	0.5105	0.4032	0.2959	0.1908	0.0927
	SGMM	λ	0.1013	0.2002	0.3013	0.4044	0.5030	0.6031	0.7051	0.8055	0.9087
		β	0.9007	0.7878	0.7034	0.6027	0.5099	0.4001	0.2908	0.1954	0.1007
Testing for Serial Correlation (Rejection Frequency in %)											
Traditional	DGMM	AR(1)	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
		AR(2)	7.5	6.0	4.0	6.0	5.0	7.0	2.0	4.5	5.5
	SGMM	AR(1)	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
		AR(2)	7.0	5.5	4.0	5.0	5.0	6.0	2.5	4.5	3.5
SCC	DGMM	AR(1)	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
		AR(2)	5.0	5.0	5.5	5.5	6.0	6.5	3.5	4.0	5.5
	SGMM	AR(1)	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
		AR(2)	5.0	6.0	5.0	5.5	4.5	5.0	2.5	4.0	3.0

Table 2: Simulation Results with Serial Correlation (Case 2)

Mean Estimates											
True Value		λ	0.1000	0.2000	0.3000	0.4000	0.5000	0.6000	0.7000	0.8000	0.9000
		β	0.9000	0.8000	0.7000	0.6000	0.5000	0.4000	0.3000	0.2000	0.1000
Traditional	DGMM	λ	0.0481	0.0847	0.1092	0.1177	0.1094	0.0861	0.0440	0.0063	-0.0158
		β	0.8876	0.7792	0.6652	0.5487	0.4253	0.3030	0.1825	0.0877	0.0224
	SGMM	λ	0.0442	0.0749	0.0959	0.1047	0.1034	0.0938	0.0793	0.0743	0.1349
		β	0.8898	0.7828	0.6711	0.5567	0.4360	0.3165	0.2000	0.1080	0.0403
SCC	DGMM	λ	0.0960	0.1955	0.2926	0.3855	0.4816	0.5612	0.6294	0.5778	0.0301
		β	0.9003	0.7998	0.6962	0.6143	0.5058	0.4132	0.2988	0.1813	0.0636
	SGMM	λ	0.0994	0.2001	0.2988	0.3952	0.4977	0.5915	0.7075	0.8503	0.9637
		β	0.9040	0.8045	0.7008	0.6161	0.5019	0.4175	0.2943	0.1978	0.1067
Testing for Serial Correlation (Rejection Frequency in %)											
Traditional	DGMM	AR(1)	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
		AR(2)	32.5	67.0	85.5	87.5	76.5	58.0	25.5	6.5	8.0
	SGMM	AR(1)	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
		AR(2)	28.5	58.0	70.0	80.5	71.0	66.0	61.0	58.0	72.0
SCC	DGMM	AR(1)	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	82.5
		AR(2)	68.0	99.5	100.0	100.0	100.0	100.0	100.0	99.0	7.5
	SGMM	AR(1)	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
		AR(2)	71.5	99.5	100.0	100.0	100.0	100.0	100.0	100.0	100.0

Table 3: Descriptive Statistics: Physician Prescriptions

	Overall	Cardio-logist	Diabeto-logist	Endocrino-logist	Consulting Physician	General Practitioner	General Surgeon
Mean	19.96	17.64	40.42	32.63	19.87	17.10	17.77
Standard Deviation	32.53	15.08	106.97	40.61	26.80	17.71	14.74
Maximum	1590.00	220.00	1590.00	377.00	1300.00	760.00	108.00
Minimum	1.00	1.00	1.00	1.00	1.00	1.00	1.00
N	9595	628	422	206	4988	3069	282

Table 4: Descriptive Statistics: Detailing Calls

	Overall	Cardio-logist	Diabeto-logist	Endocrino-logist	Consulting Physician	General Practitioner	General Surgeon
Mean	2.55	2.70	3.23	4.69	2.68	2.10	2.18
Standard Deviation	1.56	1.67	2.05	2.94	1.55	1.14	1.02
Maximum	24.00	16.00	23.00	24.00	23.00	17.00	7.00
Minimum	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Table 5: Descriptive Statistics: Free Samples

	Overall	Cardio-logist	Diabeto-logist	Endocrino-logist	Consulting Physician	General Practitioner	General Surgeon
Mean	3.39	2.50	3.90	4.28	3.69	2.97	3.17
Standard Deviation	17.99	10.48	14.07	32.62	18.27	18.04	15.46
Maximum	1100.00	225.00	340.00	1100.00	1100.00	1050.00	540.00
Minimum	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Table 6: Estimation Results: Homogenous Model

	Traditional		**SCC**	
	DGMM	SGMM	DGMM	SGMM
Lagged log(prescription)	**0.215** (0.019)	**0.254** (0.014)	**0.492** (0.038)	**0.545** (0.035)
Detailing Calls	**-0.016** (0.004)	-0.003 (0.004)	0.084 (0.047)	**0.096** (0.015)
Free Samples (×10)	**0.005** (0.001)	**0.005** (0.001)	0.020 (0.027)	-0.001 (0.014)
Specification Tests				
Arellano-Bond AR(1)	Reject	Reject	Reject	Reject
Arellano-Bond AR(2)	Reject	Reject	Reject	Reject
Number of Instruments	22	35	18	28
Number of Observations	38380	47975	38380	47975

Dependent variable: logarithm of prescriptions per month. Standard errors are reported in parentheses. Significance (at the 0.05 level) appears in boldface.

Table 7: Estimation Results: Heterogeneous Model

	Traditional		**SCC**	
	DGMM	SGMM	DGMM	SGMM
Lagged log(prescription)				
Cardiologist	0.115 (0.078)	**0.238** (0.031)	0.109 (0.213)	**0.699** (0.061)
Diabetologist	0.069 (0.075)	**0.394** (0.031)	0.079 (0.399)	**0.591** (0.059)
Endocrinologist	0.018 (0.143)	**0.328** (0.030)	0.207 (0.299)	**0.706** (0.079)
Consulting Physician	**0.202** (0.029)	**0.265** (0.016)	**1.176** (0.173)	**0.566** (0.040)
General Practitioner	**0.251** (0.041)	**0.215** (0.018)	**-0.286** (0.132)	**0.530** (0.042)
General Surgeon	0.208 (0.125)	**0.232** (0.037)	-0.119 (0.276)	**0.388** (0.109)
Detailing Calls				
Cardiologist	0.002 (0.015)	0.015 (0.013)	-0.011 (0.018)	-0.015 (0.042)
Diabetologist	**-0.036** (0.015)	0.006 (0.013)	-0.029 (0.023)	**0.100** (0.036)
Endocrinologist	0.006 (0.013)	**0.032** (0.011)	0.013 (0.017)	-0.002 (0.047)
Consulting Physician	**-0.022** (0.005)	-0.009 (0.005)	**0.028** (0.012)	**0.092** (0.022)
General Practitioner	-0.005 (0.008)	0.000 (0.007)	**-0.040** (0.012)	**0.109** (0.034)
General Surgeon	0.013 (0.031)	-0.004 (0.025)	-0.028 (0.042)	**0.251** (0.092)
Free Samples (×10)				
Cardiologist	0.002 (0.011)	0.010 (0.010)	0.002 (0.012)	0.057 (0.038)
Diabetologist	**0.012** (0.007)	0.007 (0.005)	0.011 (0.007)	0.023 (0.048)
Endocrinologist	0.000 (0.002)	0.004 (0.003)	0.001 (0.002)	0.004 (0.012)
Consulting Physician	**0.005** (0.001)	**0.005** (0.001)	**0.009** (0.002)	-0.016 (0.013)
General Practitioner	**0.004** (0.002)	**0.004** (0.002)	**0.002** (0.001)	0.032 (0.022)
General Surgeon	**0.015** (0.003)	0.010 (0.006)	0.009 (0.005)	0.171 (0.099)
Specification Tests				
Arellano-Bond AR(1)	Reject	Reject	Reject	Reject
Arellano-Bond AR(2)	Reject	Reject	Reject	Reject
Number of Instruments	112	185	88	143
Number of Observations	38380	47975	38380	47975

Dependent variable: logarithm of prescriptions per month. Standard errors are reported in parentheses. Significance (at the 0.05 level) appears in boldface.

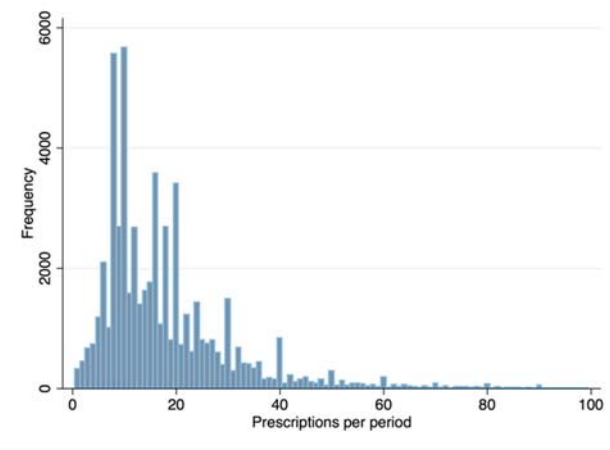
Table 8: Estimation Results: Diabetologists

	Traditional		**SCC**	
	DGMM	SGMM	DGMM	SGMM
Lagged log(prescription)	0.118 (0.075)	**0.459** (0.063)	0.471 (0.263)	**0.639** (0.113)
Detailing Calls	**-0.034** (0.016)	0.027 (0.015)	0.105 (0.057)	**0.097** (0.037)
Free Sample (×10)	**0.014** (0.006)	0.009 (0.008)	0.098 (0.053)	0.080 (0.054)
Specification Tests				
Arellano-Bond AR(1)	Reject	Reject	Reject	Reject
Arellano-Bond AR(2)	Not Reject	Not Reject	Not Reject	Reject
Number of Instruments	22	35	18	28
Number of Observations	1688	2110	1688	2110

Dependent variable: logarithm of prescriptions per month. Standard errors are reported in parentheses. Significance (at the 0.05 level) appears in boldfce.

Figure 1: Distribution of Prescriptions / Calls

a) Number of Prescriptions



b) Number of Calls