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Threshold Effects in Cigarette Addiction: An Application of the Threshold Model in Dynamic Panels

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Abstract

We adopt the threshold model of myopic cigarette addiction to US state-level panel data. The threshold model is used to identify the structural effects of cigarette demand determinants across the income stratification. Furthermore, we apply a bootstrap approach to correct for the small-sample bias that arises in the dynamic panel threshold model with fixed effects. Our empirical results indicate that there exists the heterogeneity of smoking dynamics across consumers.

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1. Introduction

A considerable body of work exists on smoking addiction related to the price elasticity of demand for cigarettes, as the measure has important implications for optimal taxation and tobacco control programs.[1] Nevertheless, empirical research has not reached a consensus on the true value of the price elasticity (Chaloupka and Warner 2000). One possible explanation is the nonlinear structure of cigarette demand. From a theoretical point of view, Becker *et al.* (1991) and Grossman *et al.* (1998) argued that people differ in their attitudes toward the future, which in turn affects their cigarette consumption. Those who discount the future more heavily are likely to become addicted to cigarettes. Thus, the price responses of cigarette consumption could vary by socioeconomic status, as measured by income, for example.

Dependence of the demand for cigarettes on income has been verified empirically. For example, Fujii (1980) found that cigarette consumption is related positively to income, indicating that cigarettes are a normal good. On the other hand, contrasting evidence suggests that a rise in income leads to a decrease in smoking consumption and hence cigarettes are an inferior good (e.g., Wasserman *et al.* 1991, Townsend *et al.* 1994, and Lanoie and Leclair 1998). Moreover, Young (1983) indicated that consumers respond to price rises and decreases differently. Based on British survey data, Townsend (1987) discovered that the consumption patterns of cigarettes are heterogeneous across social classes. The price elasticity is relatively higher for low-income recipients. The amount of cigarettes smoked is related negatively to individual income. In addition, the impacts of taxation and advertising vary with income levels. In countrywide studies, the price elasticity is found to be higher among less developed economies than in industrialized nations (e.g., Warner 1990, Chapman and Richardson 1990, and Chaloupka *et al.* 2000). Recently, researchers have suggested that a nonlinear relationship exists between income and cigarette consumption. Huang and Yang (2006) were among the first to emphasize threshold effects with respect to cigarette demand.

The evidence highlights the importance of nonlinearities and threshold effects in the dynamics of cigarette demand. In order to investigate further the dynamics of addictive consumption across income groups, we apply the threshold model developed by Hansen (1999) in dynamic panels to US state-level data for cigarette consumption. To date, dynamic versions of Hansen's panel threshold model have been employed in several applications; for example, Huang and Yang (2006) for cigarette demand, Graff and Karmann (2006) and Masten *et al.* (2008) for the finance–growth nexus, Ho (2006) on growth convergence, and Shen and Chen (2008) on the causality between banking and currency fragilities.[2] Our study differs from

[1] See Goel and Nelson (2006) for a review.

[2] Another possible extension is to specify a panel smooth threshold regression (PSTR), which, instead of discrete shifts, allows the coefficients to change gradually from one regime to another.

existing studies in several aspects. First, the de-meaned treatment for fixed effects is used commonly but may not be suitable for a dynamic panel threshold model; this point is examined analytically in the Appendix. Second, unbalanced panel data are accounted for explicitly in our estimation without deletion of any information. Finally, we apply a bootstrap approach to correct for the small-sample bias that arises in the dynamic panel threshold model with fixed effects.

This paper is organized as follows. Section 2 discusses estimation issues and potential inconsistency in the context of a dynamic panel threshold framework with fixed effects. Section 3 outlines the empirical specification. Specifically, we adopt a myopic model of addiction to account for the persistence of smoking behavior. We then report the results from our threshold estimation after adjusting for bias. Two income thresholds define three distinct regimes in which the dynamics of addictive behavior are addressed. Some econometric issues relevant to the estimation of a dynamic panel threshold model with fixed effects are advanced in the Appendix.

2. Estimation

To investigate threshold-type behavior in cigarette demand, we extend Hansen's (1999) threshold model by allowing for lagged dependent variable regressors. That is, we consider a dynamic panel threshold model. The issue of small-sample bias is summarized in the Appendix. It turns out that bias correction is important to our empirical results in this paper. First of all, after accounting for the threshold effect of income, the data in each regime become unbalanced. Second, the number of observations in each regime determined by the threshold estimation may be as few as 2 or 3 when $T = 38$ (years), in the sense that the LSDV (least squares dummy variable) bias might not be ignorable in a dynamic panel threshold model. Finally, the exogenous variables included in our regression model can further complicate the bias approximations of the LSDV estimators.

2.1 The Dynamic Panel Data Model with Threshold Effects

The appeal of threshold regression models is that the data stratification can be determined by the value of an observed variable as opposed to alternative choices of a cutoff point in a purely *ad hoc* fashion. The least squares (LS) estimation and the corresponding asymptotic distribution theory are readily available in Hansen (1999). The Hansen method provides a simplified econometric technique for threshold regression with panel data; however, a major econometric limitation of his approach is that when lagged dependent variables are used, the LSDV might be severely biased. It is dangerous to apply Hansen's method to dynamic panel data directly without dealing with the LSDV bias.

See, for example, Fok *et al.* (2005), González *et al.* (2005), Colletaz and Hurlin (2006), and Fouquau *et al.* (2008).

In this section, we apply a bootstrap method to correct for bias in dynamic panel data, and then to determine possible regimes taking income as the threshold variable. We deal explicitly with potential bias in the threshold estimation. The model can be written as[3]

$$y_{it} = \alpha_i + (\beta_1 y_{i,t-1} + x_{it}\eta_1)\mathbf{1}(q_{it} \leq \gamma) + (\beta_2 y_{i,t-1} + x_{it}\eta_2)\mathbf{1}(q_{it} > \gamma) + e_{it}, \quad (1)$$

where, for $i = 1, \ldots, N$, $t = 1, \ldots, T$, y_{it} is the dependent variable, x_{it} is an m-vector of explanatory variables. q_{it} is the observed threshold variable, $\gamma \in \Gamma$ is the threshold parameter, Γ is the collection of potential threshold values, β_1 and β_2 are m-vectors of slope parameters that may differ depending on the value of q_{it}, and e_i is the disturbance term. $\mathbf{1}(q_{it} \leq \gamma)$ is an indicator variable that equals one if $q_{it} \leq \gamma$ and zero otherwise. Accordingly, $\mathbf{1}(q_{it} > \gamma)$ is defined in a similar fashion.

Estimation of (1) involves three steps. First, the LS method (with bias correction) is employed to calculate the sum of squared residuals (SSR) in (1) at a given $\gamma \in \Gamma$. Second, the estimator of the threshold value is γ, which generates the smallest SSR. Finally, with the sample split by the estimated threshold, the parameters of the equation can be estimated through the LS method (with bias correction) in each regime of the samples.

2.2 The Bootstrap Bias Correction for Dynamic Panels

In this section, we use a simple example to illustrate the main idea of bootstrap-based bias correction in the context of threshold models in dynamic panels and then extend this to the case of a demand equation for cigarettes.

For a given $\gamma \in \Gamma$, we first obtain fixed-effect estimates $\hat{\beta}_1(\gamma)$ and $\hat{\beta}_2(\gamma)$, and then we calculate $\hat{\alpha}_i(\gamma)$ and residuals $\hat{e}_{it}(\gamma)$ for all i's and t's. The bootstrap errors for individual i, $e_i^*(\gamma) = (e_{i,-49}^*(\gamma), e_{i,-48}^*(\gamma), \ldots, e_{iT}^*(\gamma))$ are drawn from $\hat{e}_i(\gamma) = (\hat{e}_{i1}(\gamma), \hat{e}_{i2}(\gamma), \ldots, \hat{e}_{iT}(\gamma))$ with replacement. Then, for $i = 1, \ldots, n$, $t = -49, \ldots, T$, the bootstrap sample is generated by

$$y_{it}^*(\gamma) = \hat{\alpha}_i(\gamma) + \hat{\beta}_1(\gamma) y_{i,t-1}^* \mathbf{1}(q_{it} \leq \gamma) + \hat{\beta}_2(\gamma) y_{i,t-1}^* \mathbf{1}(q_{it} > \gamma) + e_{it}^*(\gamma).$$

The first 49 observations are discarded, and then we use the remaining observations to obtain fixed-effect estimates, $\beta_{1,b}^*(\gamma)$ and $\beta_{2,b}^*(\gamma)$. This procedure is repeated over $B = 400$ times, and the bias-corrected estimators of β_1 and β_2 for a given γ are defined as

$$\hat{\beta}_{1,B}(\gamma) = 2\hat{\beta}_1(\gamma) - \frac{1}{B}\sum_{b=1}^{B} \beta_{1,b}^*(\gamma),$$

$$\hat{\beta}_{2,B}(\gamma) = 2\hat{\beta}_2(\gamma) - \frac{1}{B}\sum_{b=1}^{B} \beta_{2,b}^*(\gamma).$$

[3]See Hansen (1999) for details on the technical issues and the statistical theory for threshold estimation.

The bias-corrected transition parameter is defined as

$$\hat{\gamma}_B = \arg\min_{\gamma\in\Gamma} \tilde{S}'_{NT}(\gamma), \tag{2}$$

where

$$\tilde{S}'_{NT}(\gamma) = \sum_{i=1}^{N}\sum_{t=1}^{T}\left[\tilde{y}_{it} - \hat{\beta}_{1,B}(\gamma)\tilde{y}_{i,t-1}1(q_{it}\le\gamma) - \hat{\beta}_{2,B}(\gamma)\tilde{y}_{i,t-1}1(q_{it}>\gamma)\right]^2.$$

Furthermore, the bias-corrected estimators are defined as $(\hat{\beta}_{1,B}, \hat{\beta}_{2,B}) = (\hat{\beta}_{1,B}(\hat{\gamma}_B), \hat{\beta}_{2,B}(\hat{\gamma}_B))$.

Then, consider a dynamic panel model with exogenous variables as in equation (1). For a given $\gamma \in \Gamma$, we obtain fixed-effect estimates $\hat{\beta}_1(\gamma)$, $\hat{\beta}_2(\gamma)$, $\hat{\eta}_1(\gamma)$, and $\hat{\eta}_2(\gamma)$, and then we calculate $\hat{\alpha}_i(\gamma)$ and residuals $\hat{e}_{it}(\gamma)$ for all i's and t's. The bootstrap exogenous variables and errors for individual i, $(x_i^*, e_i^*(\gamma))$, are drawn from $(x_{it}, \hat{e}_{it}(\gamma))$ with replacement in pairs (i.e., $(x_{is}^*, e_{is}^*) = (x_{i\tau}, \hat{e}_{i\tau})$, $s = 1, \ldots, T$, and τ is randomly drawn from $\{1, \cdots, T\}$ with replacement). Then, the bootstrap sample is generated, and the bias-corrected transition parameter can be estimated readily.

3. Empirical Results

In this paper, we estimate the demand function in the presence of a partial adjustment process; i.e., the myopic model of addiction; see Hamilton (1972) and McGuinness and Cowling (1975) for a theoretical introduction. For empirical applications, see Baltagi and Levin (1986) and Baltagi *et al.* (2000). The demand function for cigarette consumption of state i in period t across two regimes divided by an income threshold can be expressed as in equation (1), where $y_{it} = \ln C_{it}$, $x_{it} = (\ln DI_{it}, \ln AD_{it}, \ln P_{it}, LDT_{it}, STI_{it}, STE_{it})$.

State-specific and time-specific dummies are also incorporated into equation (1). C_{it} is consumption measured by per capita tax-paid cigarette sales in packs for state i. P_{it} is the average real retail price per pack in 1982–1984 dollars for state i. DI_{it} is real per capita disposable income for a specific state. AD_{it} is designed to capture state-specific advertising. The smuggling effects across states include: LDT_{it} for commercial smuggling, STI_{it} for import smuggling and STE_{it} for export smuggling.[4] The β's in equation (1) are expected to be positive, reflecting the phenomenon of intertemporal complementarity in addictive consumption, as a reduction in price raises consumption over time. The η parameters are negative, except for the undetermined income and adverting effects.

The data used in this study are obtained from Fenn *et al.* (2001). The panel covers $N = 50$ states including the District of Columbia over $T = 38$ years (1957–

[4]See Becker *et al.* (1994) for the construction of these indexes.

1994), except for several states missing data for a few years.[5] In total, there are 1874 observations available.

Prior to estimating the equation, we need to determine the correct number of regimes to describe the underlying dynamics of cigarette demand. The likelihood ratio (LR) test with the values from the bootstrap method is reported in Table 1. The LR values indicate that distinct regimes can be identified using income as a threshold. Having confirmed that a single threshold is significant at sensible levels, the LR test is then conducted for multiple break points. As indicated in Table 1, the test statistics are in support of three regimes with two income thresholds, over four regimes. While the calculated LR statistics for two thresholds (three regimes) in Table 1 far exceed the 5% critical value, three thresholds (four regimes) are not significant at any sensible level.

Table 2 reports the result of threshold estimation for cigarette demand.[6] Specifically, the table summarizes the estimates of the parameters of interest across distinct regimes with the associated t-statistics in parentheses. The first column shows the threshold values of income (denoted by $\widehat{\gamma}$) along with the corresponding percentiles of income.

As expected, the significant habit persistence effects across income classes are indicated by the positive lagged consumption coefficients. Moreover, all three smuggling measures are negative and significant, implying that the prevalence of bootlegging, because of the variation in tax rates across states, reduces cigarette demand within states. Three regimes associated with the threshold income levels are identified; namely, (1) below US$11288.89 (regime I); (2) between US$11288.89 and US$11569.88 (regime II); (3) greater than US$11569.88 (regime III).[7] The significant income thresholds suggest that smoking behavior varies by income level, in that the three distinct income classes have different responsiveness to the market forces. For regime I, income levels below the 80.5 percentile, the estimated price and income elasticities are -0.15 and 0.11, respectively, and both are significant at the 5% level. For regime II, these estimates are -0.31 and 0.54 and are significant. For regime III, the most wealthy group, the income effect is 0.04 and significant, while the price effect is marginal and not significant. Our estimates of the price

[5]Because of data availability, several states have a different starting year, although they all end in 1994: Missouri (1958), Maryland (1961), Alaska and California (1962), Hawaii and Virginia (1963), Colorado (1967), Oregon (1969) and North Carolina (1972). See Becker *et al.* (1994) for detailed description of the sources and construction of the variables.

[6]Although the unadjusted estimates are similar qualitatively to those in Table 2, the estimates of the given effect tend to be biased. For example, without accounting for the estimation bias, the price elasticity tends to be overestimated for all three income groups, whereas the income effects are likely to be underestimated. Thus, we attach stronger credence to the bootstrap estimates with bias correction as shown in Table 2. The results without bias correction are available upon request.

[7]The robustness of these threshold estimates is based on the bootstrap-based bias correction, along with a number of specification checks.

and income elasticities lie in a plausible range and are similar in comparison to estimates reported in the literature. The majority of the cigarette demand studies present a narrow range of price elasticities centered on -0.4 (Chaloupka and Warner 2000), and income elasticities from 0.12 to 0.82 (Baltagi and Levin 1986).

The negative coefficient on the advertising variable implies that an increase in advertising expenditure by the cigarette industry may reduce the consumption of cigarettes, which may reflect the net effect of both demand and supply interactions.[8] Our result suggests that the relative magnitude of the supply effect of advertising is higher. From the perspective of policy making, Wasserman *et al.* (1991) pointed out that "...it is important to recognize that developing effective and economically efficient policies to discourage smoking is inherently difficult." (p. 62) Our empirical result in this paper provides a feasible explanation—the heterogeneity of smoking dynamics across consumers. Thus, any antismoking proposals that are uniformly imposed on all consumers are deemed to be a failure.

A few words are appropriate regarding the distribution of observations across regimes. Table 3 summarizes the number of states corresponding to the specific regime in chronological order. Prior to 1971, most of the states were in the first regime as the smoking behavior was fairly similar across states. Starting from 1971, a few states reached regimes II and III, which might be attributable to a series of advertising restrictions enacted by the government, such as the Fairness Doctrine Act and the US Broadcast Advertising Ban (Sloan *et al.* 2002, and Iwasaki *et al.* 2006). Such marketing regulations and health warnings on cigarettes might have changed public attitudes toward smoking; in particular, among the upper-income class. A more than doubling of the number of states in regime III occurred in 1984. The legislation of the Comprehensive Smoking Education Act requiring rotating warnings on cigarette packages may be the main cause. Another sharp break appeared in 1986, which coincided with the release of a US Surgeon General's report and the subsequent enactment of the Comprehensive Smokeless Tobacco Health Education Act (Tauras *et al.* 2007). The evidence above seems to suggest that government interventions play a crucial role in cigarette consumption.

[8] As pointed out by Iwasaki *et al.* (2006), advertising creates two parallel effects on consumption. An increase in persuasive advertising may induce higher demand for smoking and hence higher consumption. Nevertheless, advertising can discourage price competition by increasing oligopoly power, which in turn lowers cigarettes consumed (see Baltagi and Levin (1986) and the references cited there for support of this view).

Appendix

Small-Sample Bias and Within-Group Transformation for Dynamic Panel Threshold Models

Because of the nature of habit persistence, previous studies on addictive substances are based largely on dynamic panel models. A typical feature of such panel data is a small number of time dimensions ($T = 38$ in our study) and a moderate number of individual (state) dimensions ($N = 51$). The estimation bias resulting from the short (small T) and wide (large N) panels is likely to be significant and could have biased the empirical results. For example, Judson and Owen (1999) argued that with the short time horizon of the panel, the LSDV that is used in the dynamic panel models can produce biased estimates, which can be as large as 20% above the true estimate of the coefficient even when[9] $T = 30$. Moreover, because the likelihood of having an unbalanced panel may increase as the time dimension becomes large, the empirical practice of excluding missing observations to maintain balanced panels may not be desirable (Bruno 2005). Judson and Owen (1999) pointed out that the method of implementing corrected LSDV for an unbalanced panel can produce superior results. In a recent work, Bruno (2005) extended the LSDV bias approximations in Bun and Kiviet (2003) to unbalanced panels.

The traditional method used to estimate the transition parameters in nondynamic panel data models is based on the assumption that the estimators of the slope parameters are consistent for a given value of $\gamma \in \Gamma$. Unfortunately, it is well known that in dynamic panel data models with fixed effects, the possibility of severe downward bias arises for the within-group estimators of the slope coefficients (in each regime) when T is small. Furthermore, because the threshold effects will make the data in each regime unbalanced and discontinuous, it is likely that the bias is significant when the average length of time for any regime is short even though T is large. As a result, it is critical to adjust for the bias in estimation. Although the bias corrections in Bun and Kiviet (2003) can be applied readily to the case of unbalanced panels, as discussed below, they cannot be applied directly to the dynamic panel threshold model with fixed effects.

In the conventional approach to bias correction for dynamic panel data models, y_{it} can be decomposed into two parts: a function of $e_{it}, e_{i,t-1}, \ldots$ and a function of α_i. Utilizing the within-group operator (i.e., $y_{it} - (1/T)\sum_{t=1}^{T} y_{it}$) can eliminate the second part to simplify the analytic form of the bias at the rate of $1/T$. With the threshold effects, however, the second part depends not only on α_i, β_1 and β_2 but also on how the regime switches from one to another. The within-group operator cannot fully remove this part in the panel model with threshold effects, even though the bias of the fixed effect estimator of the slope coefficient in each regime still diminishes at the rate of $1/T$. In addition, because different individuals

[9] Although Nickell (1981) suggested that the bias in the LSDV estimate can be eliminated as T approaches infinity; however, such long-span data in smoking addiction studies are rare.

generally have different switching times, the analytic form of the first-order bias approximation also varies from case to case. To avoid this difficulty in bias correction, we apply the renovated method based on an application of the bootstrap estimation.[10]

We next illustrate briefly that in the presence of threshold effects, the conventional approach to bias correction for the within-group estimator becomes intractable. In particular, the within-group estimator for the dynamic panel threshold model is not the same as that for a simple dynamic panel model and is a function of $E[\alpha^2]$.

Consider the following simple dynamic panel model with a structure change at time $1 < S < T$ for $i = 1, \ldots, N$ and $t = 1, \ldots, T$,

$$y_{it} = \alpha_i + e_{it} + \beta_1 y_{i,t-1} 1(t \leq S) + \beta_2 y_{i,t-1} 1(t > S),$$

where α_i indicates individual fixed effects, $e_{it} \sim N(0, \sigma^2)$ is cross-sectionally and serially independent, and β_j, $j = 1, 2$, denotes the AR(1) coefficient, $|\beta_j| < 0$.

Suppose that $k = S/T$ is a strictly positive real number between zero and one. Without loss of generality, here we assume that $y_0 = \alpha_i/(1 - \beta_1)$. Let $x_{i1,t} = y_{i,t-1} 1(t \leq S)$, $x_{i2,t} = y_{i,t-1} 1(t > S)$, and let $X_i = (X_{i1}, X_{i2})$, where $X_{ij} = (x_{ij,1}, \ldots, x_{ij,T})'$, $j = 1, 2$. This can be written as

$$\begin{aligned} X_{i1} &= (y_{i,0}, \ldots, y_{i,S-1}, 0, \ldots, 0), \\ X_{i2} &= (\quad 0\ , \ldots, \quad 0 \quad , y_{i,S}, \ldots, y_{i,T}). \end{aligned}$$

Therefore, for $t = 1, \ldots, S$,

$$x_{i1,t} = \frac{\alpha_i}{1 - \beta_1} + \sum_{j=0}^{t-2} \beta_1^j e_{i,t-1-j} = \frac{\alpha_i}{1 - \beta_1} + \ddot{y}_{i,t-1},$$

where $\ddot{y}_{i,t-1} = \sum_{j=0}^{t-2} \beta_1^j e_{i,t-1-j}$. Similarly, for $t = S + 1, \ldots, T$,

$$x_{i2,t} = \alpha_i (\frac{1 - \beta_2^{t-S-1}}{1 - \beta_2}) + \sum_{j=0}^{t-S-2} \beta_2^j e_{i,t-1-j} + \beta_2^{t-S-1} \cdot y_{iS} = \alpha_i (\frac{1 - \beta_2^{t-S-1}}{1 - \beta_2}) + \check{y}_{i,t-1} + \beta_2^{t-S-1} \cdot y_{iS},$$

where $\check{y}_{i,t-1} = \sum_{j=0}^{t-S-2} \beta_2^j e_{i,t-1-j}$.

[10] Alternatively, we can adopt the GMM estimation for our empirical estimation; however, it should be noted that the bias of the GMM estimator for simple dynamic panel data usually diminishes at the rate of $1/N$ (see, for example, Alvarez and Arellano 2003). Thus, it can be expected that the GMM estimator for a dynamic panel threshold model becomes feasible only if the number of individuals (N) is large enough.

Note that $S = kT$. After employing the within-group transformation on X_{i1}, for $t = 1, \ldots S$,

$$
\begin{aligned}
x_{i1,t} - \frac{1}{T}\sum_{t=1}^{T} x_{i1,t} &= \left(x_{i1,t} - \frac{1}{S}\sum_{t=1}^{S} x_{it} \right) + \frac{1-k}{S}\sum_{t=1}^{S} x_{it} \\
&= \frac{\alpha_i(1-k)}{1-\beta_1} + \ddot{y}_{i,t-1} - \frac{k}{S}\sum_{t=1}^{S} \ddot{y}_{i,t-1}.
\end{aligned}
$$

Unlike a dynamic panel data model, we *CANNOT* remove α_i in X_{i1} completely by the within-group transformation. Analogously, for $t = S+1, \ldots, T$,

$$
\begin{aligned}
x_{i2,t} - \frac{1}{T}\sum_{t=1}^{T} x_{i2,t} = \alpha_i \left(\frac{k}{1-\beta_2} - \frac{k(1-\beta_2^{T-S})}{(1-\beta_2)^2} \right) + \check{y}_{i,t-1} - \frac{1-k}{T-S}\sum_{t=S+1}^{T} \check{y}_{i,t-1} \\
+ (\alpha_i + y_{iS}) \left(\frac{1}{(T-S)} \frac{1-\beta_2^{T-S}}{1-\beta_2} - \beta_2^{t-S-1} \right).
\end{aligned}
$$

It is obvious that the same problem remains for α_i in X_{i2}. Suppose that $\sum_{i=1}^{N}(X_i' X_i)/NT \rightarrow Q$ as $N \rightarrow \infty$. As a result, $E[\alpha_i^2]$ would appear in the bias function through Q.

More importantly, for the dynamic panel threshold model, the bias correction procedure is further complicated by the fact that in most cases, the cutoff points vary across individuals, and hence this is evidently going to become rather intractable.

Table 1: Test for threshold effects (fixed effects)

	LR-test	P-value	10% CV	5% CV	1% CV
Two regimes	60.68	0.67%	16.36	25.12	58.59
Three regimes	134.19	1.33%	65.76	76.62	135.75
Four regimes	91.46	56.67%	170.90	195.36	259.27

Note: The critical values (CV) are obtained by the bootstrap method.

Table 2: Bootstrap estimation results (fixed effects)

$\widehat{\gamma}$	$\ln C_{t-1}$	$\ln DI$	$\ln AD$	$\ln P$	LDT	STI	STE
	0.72	0.11	-0.06	-0.15	-0.05	-0.31	-0.18
	(50.60)	(9.37)	(-13.18)	(-8.82)	(-5.48)	(-3.57)	(-4.60)
11288.89 [80.5%]	0.35	0.54	-0.08	-0.31	-0.01	0.17	-0.42
	(11.47)	(15.70)	(-11.93)	(-6.43)	(-0.47)	(0.44)	(-2.07)
11569.88 [84%]	0.78	0.04	-0.06	-0.02	-0.08	-0.53	-0.15
	(36.21)	(1.81)	(-10.08)	(-0.54)	(-1.97)	(-7.13)	(-1.80)

Note: $\widehat{\gamma}$ denotes the estimated values of the income thresholds. The threshold values are in US dollars followed by income percentiles in brackets. The numbers in parentheses denote t-statistics.

Table 3: Distribution of regimes in chronology

Year\regime	I	II	III	Year\regime	I	II	III
1957	42	0	0	1976	45	2	4
1958	43	0	0	1977	44	2	5
1959	43	0	0	1978	41	4	6
1960	43	0	0	1979	41	2	8
1961	44	0	0	1980	43	2	6
1962	46	0	0	1981	45	0	6
1963	48	0	0	1982	45	1	5
1964	48	0	0	1983	41	5	5
1965	48	0	0	1984	38	1	12
1966	48	0	0	1985	34	3	14
1967	49	0	0	1986	31	0	20
1968	49	0	0	1987	29	2	20
1969	50	0	0	1988	27	4	20
1970	50	0	0	1989	23	5	23
1971	49	1	0	1990	23	3	25
1972	48	2	1	1991	21	5	25
1973	45	1	5	1992	20	4	27
1974	44	3	4	1993	17	3	31
1975	46	1	4	1994	15	2	34

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