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Electoral Systems and Real Prices: Panel Evidence for the OECD Countries, 1970–2000

In the article that originally motivated this book, Rogowski and Kayser (2002) performed only a plausibility check of the hypothesized link between electoral systems and real prices, based on cross-sectional analysis of OECD countries in 1990. The cross-sectional evidence was strongly supportive, suggesting that real prices were, controlling for all other influences commonly adduced[1] and employing a broad array of robustness checks, about 10 percent lower in the average OECD country with single-member district (SMD) electoral systems than in those that used some form of proportional representation.

As with all new empirical claims – no one had previously even suggested a relationship between electoral arrangements and real prices – healthy skepticism was warranted. Indeed, recent research on related areas of public policy

[1] These include GDP per capita, trade openness, exchange-rate stickiness, and market size. Again, see the expanded explanation in a later footnote.

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has contrasted with – but not contradicted – these price results, associating proportional electoral arrangements with such outcomes as (a) lower income inequality (Austin-Smith 2000; Birchfield and Crepaz 1998), (b) higher public spending (Persson and Tabellini 2003; Milesi-Feretti et al. 2002) or, in combination with central banking institutions, (c) greater price stability (Keefer and Stasavage 2003).[2] As we noted in the previous chapter, and will treat more extensively later

[2] Students of politics have analyzed and debated the effects – political, social, and economic – of electoral systems for almost a century and a half, beginning at the latest with a short passage in Walter Bagehot's classic work on *The English Constitution* (1867). Writing in 1941, Ferdinand A. Hermens blamed PR for the collapse of the German Weimar Republic, claiming that proportional electoral systems led inevitably to political polarization, policy paralysis, and the rise of anti-regime parties. Less controversially, Maurice Duverger propounded in the 1950s what he called "nearly . . . a true sociological law," namely "an almost complete correlation" between SMD and two-party systems – or, in the weaker form now known as "Duverger's law," that greater proportionality is associated with a higher number of parties. Douglas Rae (1971) categorized electoral systems and their effects, especially the effective number of parties and overrepresentation of large parties, laying down a path that all future work would follow.

More recent work has included, to name only a few of the most significant contributions, works by (in chronological order) Katz (1981, 1997), Powell (1982, 2000), Lijphart (1999), Roubini and Sachs (1989), Cox (1997), Birchfield and Crepaz (1998), Austen-Smith (2000), Persson and Tabellini (2003), and Cusack, Iversen, and Soskice (2007). From these and other sources, we now know with reasonable certainty that proportional methods of election are associated not only with more parties but with higher voter turnout; less strategic voting; less political violence; greater cabinet instability and shorter-lived governments; policy outcomes closer to the preferences of the median voter and, controlling for the position of the median voter, farther to the left; higher

(Chapter 7), these outcomes – some of them desirable on non-utilitarian (e.g., Rawlsian) normative grounds – are almost always purchased at the cost of an overall reduction in social welfare; but it remains a question of (social) taste whether more equal slices are preferred to a larger pie, or whether greater growth is worth greater volatility. In this chapter, we restrict ourselves to a closer study of the price relationship among OECD countries.

The original empirical analysis of Rogowski and Kayser (2002), though intriguing, left unaddressed questions of mechanism and dynamics in the relationship between electoral systems and real prices. Indeed, purely cross-sectional evidence cannot be conclusive on such substantively important issues. Despite the indirect corroboration of findings such as Scartascini (2002) that countries with majoritarian electoral systems have lower barriers to business entry, or Pagano and Volpin (2005), that proportional representation (PR) privileges entrepreneurs and employees over unorganized groups, a critical reader of the earlier paper might find the direct evidence wanting.[3] The relationship observed

governmental expenditures and budgetary deficits; more welfare and less "pork-barrel" spending; and greater equality of incomes.

[3] Relatedly, Hall, Iversen, Soskice, Estevez-Abe, and others (see, for a representative set of papers, Hall and Soskice 2001) have argued cogently that PR is the linchpin of an "organized market economy" characterized by anticompetitive mechanisms, and that these structures are so intermeshed with educational, labor-market, and political institutions as to be

between electoral systems and prices in Rogowski and Kayser (2002) could prove anomalous, spurious, or unfounded for too many reasons.

Most importantly, the observed effect might prevail only in 1990, the single year observed. The year 1990, for example, witnessed the beginning of a recession in a considerably larger proportion of majoritarian countries (including Australia, Canada, the United Kingdom, and the United States) than of proportional countries.[4] Depressed domestic demand could have diminished both components of real prices – nominal price baskets (PPP) and the exchange rate – yet, obviously, only a control for the latter could be included. Another anomalous event of 1990 was the first Gulf War. Might large military deployments have had distinct economic effects in those countries – all majoritarian: United States, United Kingdom, and France – that made the largest military commitments? Additionally, might the spike in oil prices from the anticipation and prosecution of the first Gulf War have raised prices less in OECD countries with domestic oil sources, which may have been disproportionately majoritarian (U.S., UK, Canada) rather than proportional (although Norway and

almost impervious to change. Lewis (2004) establishes the importance of competition and retail-sector efficiency for overall growth of productivity and income.

[4] See National Bureau for Economic Research (www.nber.org) and Economic Cycle Research Institute (www.businesscycle.com/pdfs/0012-businessChron.PDF).

the Netherlands would have been exceptions)? It is even possible that the Plaza Accord of 1985, which succeeded by 1990 in driving down the value of the dollar, may have affected prices differently in majoritarian than in proportional countries. Any one of these or many other possible anomalies would suffice to draw the reliability of conclusions founded on a 1990 cross-section into question.

Second, cross-sectional data cannot rule out a more enduring spurious relationship between electoral systems and real prices. Cross-nationally, countries with majoritarian and proportional systems exhibit systematic differences in many characteristics. Majoritarian electoral systems, for example, might simply be an instrument for British colonial heritage – an influence that, together with associated liberal market ideals, might explain both electoral arrangements and price levels. Panel data, such as those introduced here, permit fixed-effect models that absorb country-specific influences not articulated in the earlier specifications, and thereby assuage concerns about omitted variables. The implicit "natural experiments" of countries that switched electoral systems, but little else, during the panel period should similarly allay skepticism about such omitted variables. Moreover, use of fixed-effects models isolates the price effect, within countries, of a change in electoral system. Intertemporally, the use of panel data also enables us to explore the dynamic link between the price structure and its determinants (although we will deal with the intertemporal dimension more explicitly in Chapter 5). For instance, an increase in imports might lower prices through greater market competition, or a drop in unemployment might raise prices by stimulating consumption. None of these insights can be attained with cross-sectional data alone, nor can such data control for slow-changing or immutable national features. Panel data with country fixed-effects can exploit the natural experiments of electoral system change within countries while controlling for all cross-national effects.

A third problem arising from the systematic differences between countries of each electoral category is out-of-sample extrapolation. As Daniel Ho (2003) has noted, the price effect claims of the earlier paper often extended beyond what could be supported by the cross-sectional data. Because majoritarian countries differ so systematically from PR countries, inferences about the effect that majoritarian electoral arrangements have on prices extended beyond the available data range of the countries that used the other electoral system. Again, the panel data analyzed in this chapter at least partly remedy the problem: when countries change electoral systems, as do several in our panel sample, they provide overlapping variation to both electoral system categories. Juxtaposing the earlier Rogowski and Kayser findings with those of other authors noted earlier, more rigorous investigation promises considerable payoffs for our understanding of the role of electoral institutions in social spending, equality, and welfare.

Finally, and perhaps most saliently, Rogowski and Kayser were unable to preclude rival hypotheses. Characteristics of electoral systems other than seats-votes elasticities might affect regulatory incentives and barriers, and consequently real price levels. Consider two alternative mechanisms, untheorized and untested in Rogowski and Kayser (2002): differences in (a) campaign finance – state funding and limits on campaign spending may alter politicians' incentives and responsiveness to organized producers, and (b) clarity of governmental responsibility (cf. Powell 2000) – voters hold governments more responsible for changes in their real income and purchasing power when they are able to associate parties clearly with policies. Both of these rival explanations co-vary with the electoral system.

This chapter subjects the hypothesis of Rogowski and Kayser (2002) to more rigorous empirical scrutiny employing annual observations for twenty-three OECD countries over the period 1970–2000. This design permits country fixed-effects and enables the estimation of over-time effects of within-country changes in electoral systems: the shift from SMD to PR in France (1986) and New Zealand (1994); and from PR to SMD (or predominantly SMD) in France (1988), Italy (1993), and Japan (1994). Perhaps surprisingly, the results strongly support the original conjecture and indeed provide a better idea of how electoral-system change within a country affects consumer power and real prices. The panel analysis, after a variety of robustness checks, suggests that the long-term effect of a within-country change in electoral system is virtually identical (i.e., about a 10 percent change in prices) to that identified in the Rogowski-Kayser cross-sectional analysis as prevailing *between* countries with different electoral systems.

Why look first at panel data from the OECD countries, and not from the entire set of democracies around the world? Our reasons are chiefly two: (a) the underlying theory assumes that institutions and public policies have real effects, an assumption that may not hold in “weakly institutionalized” poorer democracies; and (b), the data for the OECD countries are simply better. To put the matter another way, we can say either: (a) the present OECD panel study serves as a considerably expanded plausibility check (if our hypothesis does not hold here, it would likely hold nowhere); or (b) our later investigation of worldwide data (Chapter 4) is a robustness check, on which in fact we had considerably lower expectations that the hypothesis would hold among poorer democracies.

Our underlying theoretical model has been presented in Chapter 2. We simply emphasize here its chief implication, which should hold cross-sectionally and intertemporally among well-institutionalized democracies, that is, controlling for other factors, countries with majoritarian electoral systems will have lower prices; and countries that change from PR to majoritarian electoral systems will decrease their average price levels.

It is instructive at this point to observe that, in the real world, governments manifestly do inhibit competition, and keep prices high, through an astonishingly inventive variety of measures; and that practicing politicians and policy advocates frequently note, or even more frequently simply take for granted, that PR regimes favor the enactment and survival of such measures.

In relatively open economies,[5] competition-inhibiting measures naturally concentrate on *nontraded* goods and services, and on the nontraded component of otherwise tradable goods (e.g., the retail price of apparel): construction, retailing of all sorts, baking, barbering, banking, printing, insurance, teaching, lawyering, hotel keeping, and medical and pharmaceutical services (to name only a few) may easily be restricted as to (*inter alia*) licensure, hours and size of operation, discounting, advertising, and compulsory guild, union, or associational membership.[6] Overall, such a system will (as intended) keep prices high, and indeed will impose further costs not fully captured by prices: the opportunity costs of

[5] We find consistently that openness itself – measured as the import share in GDP and controlling for population size – powerfully restrains prices.

[6] For convincing real-world examples, see Lewis (2004) and Walker (2004).

extra search and shopping, the dynamic costs of weak innovation, the incentives to prefer leisure over labor.

Against its obvious disadvantages must be set the (equally, or to some more) obvious advantages of such a system: higher wages, less inequality, greater security of employment, greater leisure, greater variety (particularly of services), more expert service, higher-quality goods, even perhaps a "decommodification" of work (Esping-Andersen 1990). We have addressed briefly, and will consider further in a concluding chapter, the classical welfare losses that follow directly from restricting competition, but we do not intend to neglect the advantages of such a system that we enumerate here. The normative assessment of which basket of outcomes is superior must, as noted earlier, ultimately be left to the preferences of the citizens affected.

In any event, it is precisely where voters are empowered that such market competition should be least fettered. Consider the incentives of both legislators and voters under the mechanism we posit. Legislators respond to both money and votes in optimizing their probability of retaining office. Barring systematic differences in campaign finance across electoral systems – a rival hypothesis that we test and reject – systems that magnify the effect of even small swings in the vote on the incumbent party's seat share should tilt the balance of legislators' (and their regulatory agents') attention in favor of consumers. Although organized producers – among them union members – also contribute votes, any institutional arrangement that increases the effect of a single factor will increase the influence of the group that provides only that factor. Voters, for their part, are perceived by legislators as responding, and likely do respond, to the improved purchasing power provided by a reduction in real prices by rewarding incumbents.[7] A majoritarian electoral system is thus likelier, at the margin, to produce pro-consumer, low-price policies, while a proportional system is likelier, also at the margin, to enact pro-producer, high-price policies.

Increasingly, politicians, policy experts, and advocates and opponents of economic reforms perceive this same link. In Germany's efforts to reform its exceptionally troubled system of local monopolies (e.g., in bakeries), cross-ownership of shares, highly restrictive retail hours, and labor-market rigidities, advocates of change have come increasingly to see the PR electoral system as a major obstacle (Quitzau 2002, 5). In Italy, as we demonstrate at greater length in a later chapter, advocates of economic reform saw a majoritarian electoral system as the *sine qua non* of a more competitive policy; and in New Zealand, the radical economic reforms of the 1980s were

[7] Indeed, the success of increasingly precise measures of voter welfare such as real disposable income – i.e., income adjusted for inflation and net of taxes – in predicting election outcomes (cf. Bartels and Zaller 2001) suggests that politicians may be correct in assigning great weight to the material welfare of their constituents.

made possible by an SMD system and – again, in the view of advocates of reform – have been halted or actually reversed by the switch to a PR system (Alvey 2000).

As a necessary prologue to the data analysis, we next consider issues of measurement and of model specification.

I. How to Measure and Compare Price Levels Cross-Nationally and Over Time

The standard method in the literature for comparing price levels cross-nationally is known as "purchasing power parity over exchange rate,"[8] or for short PPP/XR. It can readily be made transparent: Suppose that some standard good or service – let us say, for simplicity, a man's shirt of a particular brand and size – costs $50 in the United States but that the identical shirt is marketed for €100 in France. Suppose also that on exchange markets the dollar trades at parity with the Euro. Then a French consumer or merchant could convert the €100 into $100, go to (or order from) the United States, and get two shirts for the same money that would have bought him only one in France. In this sense, the price of the shirt is exactly twice in France what it is in the United States. In terms

[8] This is simply the inverse of the so-called "real exchange rate," i.e., XR/PPP.

of purchasing power, because PPP/XR = 2/1, French prices are twice those of the United States.

The standard efforts to compare prices cross-nationally, including particularly the International Comparisons Project (ICP) that produces the Penn World Tables, simply broaden this exercise to compare PPP/XR with respect to broad "baskets" of goods and services, of the kind that are familiar from calculations of consumer price indices. If the broadest possible "basket," representing all of the goods and services that a typical economy might consume, costs (let us say) €5,000 in Italy but $3,000 in the United States, while the Euro-dollar exchange rate is 1:1, then we can say that the overall price level in Italy is 5/3, or 1.6 times, what it is in the United States.[9]

In theory, any substantial cross-national price differences should be quickly arbitraged away. For this reason, theory suggests that real prices for identical goods should be the same everywhere: this is the well-known Law of One Price (LOP). In practice, as a considerable literature shows, the LOP obtains only in highly attenuated form (see, *inter alia,* Kravis and Lipsey 1988; Clague 1986; Bergstrand 1991). Several factors have long been understood, empirically if not theoretically, to make for persistent differences in price levels.

9 In practice, international price level comparisons adjust national baskets to account for local tastes, e.g., substituting beer in the German "basket" for wine in the French one. The International Comparisons Project has done this with considerable care and sophistication.

Foremost among these is **wealth**, usually measured as real GDP per capita. Richer countries, independent of other plausible factors, have higher real prices, a result that is robust across virtually every possible specification. Wealth, indeed, consistently emerges as the most important single determinant of national price levels, even when one controls for the two most commonly imputed causes (Bergstrand 1991), namely (a) differences in productivity between traded and nontraded sectors (Belassa 1964; Samuelson 1964) and (b) cross-national differences in capital/labor ratios (Kravis and Lipsey 1983; Bhagwati 1984).[10]

Second, there are obvious **natural, cultural, and policy barriers** to arbitrage. Our general prior here is that economies that are less open – whether because of physical isolation, idiosyncratic or xenophobic tastes, or their governments' isolationist tendencies – will be better able to maintain prices above world levels. Our overall measure is simply imports as a share of GDP,[11] and we anticipate that – again, all else equal – greater openness entails lower prices.

[10] Wealthier consumers may also be less price sensitive, allowing for pricing-to-market (Krugman 1987).

[11] We are well aware of the possible shortcomings of this summary measure, but (a) it is the one most readily available for our whole panel, (b) we have ascertained in cross-sectional analyses that it correlates at .9 or better with such more sophisticated measures of openness as deviations from a gravity model (see, e.g., Lee 1993), and (c) our insertion of a control for population size (see text) in any event gives us something more akin to a gravity model.

Third, we conjecture that **market size**, proxied here simply by the country's population, will be inversely related to price because of (a) the specialization a large domestic market permits[12] and (b) simple economies of scale. Moreover, because trade (or import) share of GDP is known to be inversely related to population size – small countries, all else equal, trade more – inserting this control for "natural" openness makes our imports/GDP variable a better test of the effects of "policy" openness.

Fourth, as changes in demand can affect both components of real prices (nominal prices and the exchange rate), we control for business cycles by including GDP growth. Our priors here are less obvious because, by also controlling for exchange rate fluctuations (see later), we are effectively simulating a "gold standard" of irrevocably fixed exchange rates. Just as economic expansion lowered price levels during the nineteenth-century gold standard, so should it here: imagine, for example, that a country doubles cheese production but that the money supply and exchange rate remain fixed; where a dollar used to buy two units of cheese, it is now equivalent to four. Thus economic growth, all else equal, lowers real prices.

Finally, and crucially in any time-series analysis, we must control for (a) exchange-rate fluctuations and (b), because of

[12] As Adam Smith (*Wealth of Nations*, I:3) first noted, "The Division of Labour is Limited by the Extent of the Market"; hence, in many specializations price will decrease as market size increases.

indexation issues, the U.S. rate of inflation. We discuss each separately.[13]

(a) **Sharp changes in a country's exchange rate:** That domestic prices remain "sticky" even under significant changes in a country's exchange rate is a commonplace of the literature, and indeed the whole reason that currency devaluations help to remedy imbalances on the current account; but this will have obvious and significant effects on the price level as defined by PPP/XR. Suppose the Argentine peso (to take a not-so-distant example) previously traded at parity with the U.S. dollar but suddenly devalues to a peso-dollar exchange rate of 4:1. Although we do not expect that all Argentine prices (in peso terms) immediately quadruple, they will certainly rise. [14] Suppose that Argentine prices only double in terms of PPP (i.e., the peso price of a given

[13] One might naturally suspect that some of the variance in real price levels results from differences in factor endowments, including physical capital, human capital (often proxied by education levels), and even arable land or other natural resources. In practice, however, capital/labor ratios and education measures are too highly correlated with wealth (measured by GDP per capita) to permit analysis of their separate effect on prices, if any; and per capita endowment of arable land is consistently insignificant. We therefore omit any measures of factor endowments in the present tests.

[14] Indeed, if PPP moved in perfect tandem with exchange rate, devaluations would have no point.

basket of goods doubles). Then the devaluation has effectively halved real Argentine prices: if previously PPP/XR equaled *p*, then the new price level would be $2p/4$ or exactly half what it previously was. We therefore employ year-to-year change in the given country's exchange rate – that is, the percentage increase or decrease from the previous year's nominal exchange rate against the U.S. dollar, $(XR_t - XR_{t-1})/XR_{t-1}$ as a control variable throughout our panel estimations.[15] Obviously, when XR rises but PPP is sticky, we expect real prices to decline; hence, the sign on this coefficient should be negative. In other words, we anticipate that a currency *depreciation* will be associated with *lower* real prices, while an *appreciation* will lead (at least in the short run) to higher real prices.

(b) If it is chiefly the United States that is undergoing an exchange-rate fluctuation, the problem is amplified because conventional measures of real prices (on which we also rely) are anchored to U.S. prices. Suppose the dollar is appreciating against all other currencies (as it did in the late 1980s): then XR for all

[15] For present purposes, we thus take nominal exchange-rate variation as exogenous. In fact, of course, it is very much an object of government policy; and we take it as a topic for future research to consider whether particular political institutions are biased toward particular exchange-rate policies.

> other countries will rise (a dollar will buy more units of the local currency), and real prices (PPP/XR) outside the United States will fall. Conversely, if the dollar falls against other currencies – as it did under the chronic U.S. budgetary and current account deficits of the George Bush years – real prices outside the United States should rise. What we need is a "floating anchor" that takes into account internally driven changes[16] in the specific value of the U.S. currency, and we therefore insert the U.S. inflation rate (GDP deflator) as a control variable. When the United States is undergoing (or is anticipated to undergo) high inflation, the dollar will (according to standard currency-rate theory) depreciate against other currencies, leading every other country's XR – the units of its currency that a dollar will buy – to fall; when the dollar's domestic purchasing power is stable, it often appreciates against other currencies, leading the other countries' XR to rise. The expected sign on the coefficient of U.S. inflation should therefore be positive.

The measurement, data source, and summary statistics of all of the variables are presented in the Appendix.

[16] An obvious alternative measure, the U.S. deficit on current account, would often be driven by external factors, e.g., foreigners' willingness to invest in the United States.

II. The Panel Data and Empirical Testing

1. The Data

We analyze annual price data (PPP/XR) for twenty-three OECD countries[17] between 1970 and 2000. The price level for a given country in any year is indexed to U.S. prices so that (e.g.) a figure of 106 – as it happens, the mean over the whole period for this set of countries – signifies that overall prices are 1.06 times U.S. levels.

The dependent variable, purchasing power parity over exchange rate (PPP/XP), has a mean, as just mentioned, of around 106, a standard deviation of 24.6, and a range of 39.9 to 187.1. Preliminary analysis shows a large cross-country variation and – witness Figure 3.1 – generally higher real price levels in PR than in SMD systems.

2. Empirical Analysis

We first establish whether our dependent variable is stationary. As is well known, when the dependent variable is not

[17] The set consists of all twenty-four states that were members of the OECD in 1990, except Turkey, for which data are inadequate. The countries included are thus Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Luxembourg, the Netherlands, Norway, New Zealand, Portugal, Spain, Sweden, Switzerland, the United Kingdom, and the United States. Note that the periods under dictatorship in Greece (until 1974), Portugal (until 1975), and Spain (until 1977) are excluded.

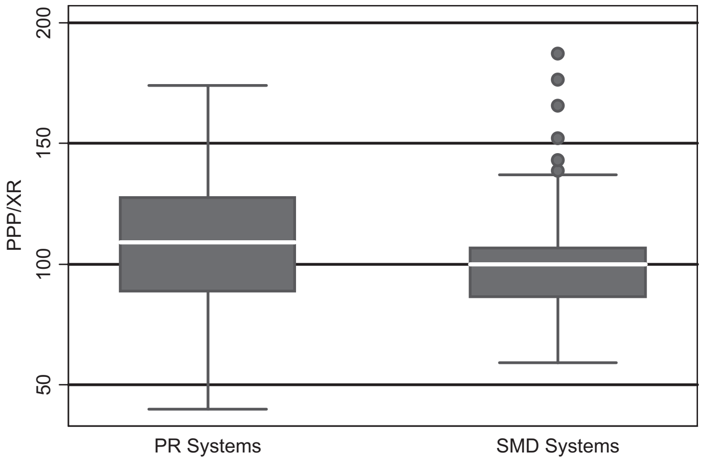

Figure 3.1. Purchasing power parity over exchange rate by electoral systems

stationary, the underlying data-generating process does not remain constant over time; hence the usual *t*-statistics will have nonstandard distributions and will generate misleading inferences. Our preliminary visual examination of the dependent variable, as plotted in Figure 3.2, finds no discernible trend (except in the case of Japan), and we conjecture that our dependent variable is stationary. To test in a more systematic way whether unit roots are present, we implement the Levin-Lin test in our cross-sectional time series data. The results, as shown in the upper panel of Table 3.1, indicate no evidence of nonstationarity. Because it is sometimes asserted that the Levin-Lin test (and other tests for unit roots in general) enjoys

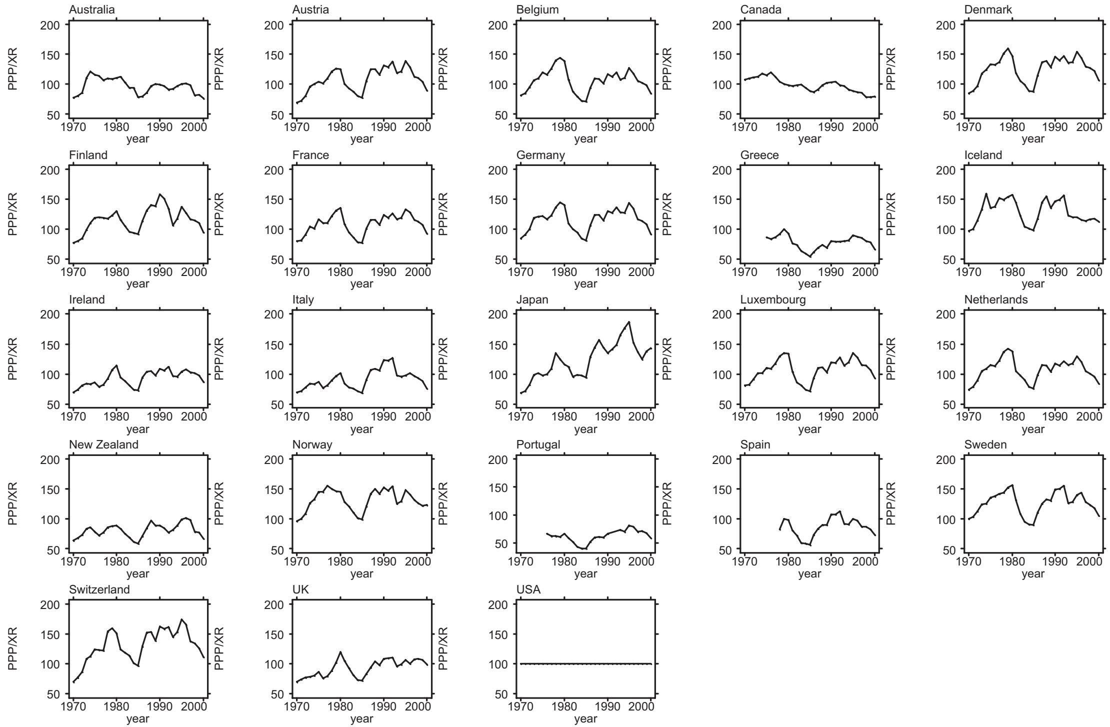

Figure 3.2. Purchasing power parity over exchange rate in 23 OECD countries: data-series evidence
No consistent, discernible trend, suggesting that the dependent variable, real price levels, is stationary.

Table 3.1. Estimation results of panel unit roots tests

PPP/XR: Levels				
Levin and Lin tests	**Coefficient**	***t*-value**	***t*****	***p***
Constant	−0.2718	−10.602	−4.2652	<0.0001***
Constant, trend	−0.3677	−12.348	−3.2171	0.0006***
Im, Pesaran, & Shin tests	***t*-tar**	**CV1%**	**Ψ**	**P**
De-meaned, no trend	−2.355	−1.980	−4.168	<0.001***
De-meaned, trend	−2.699	−2.590	−2.815	0.002***

Notes:
Levin and Lin tests augmented by 1 lag. H_0: nonstationarity
coefficient: Coefficient on lagged levels.
t-value: t-value of coefficient.
t*: transformed t-value. $t^* \sim N(0, 1)$
p: p-value of t*; ***: $p < 0.01$.
Im, Pesaran, & Shin tests augmented by 1 lag. H_0: nonstationarity
t-bar: mean of country-specific Dickey-Fuller tests.
CV1%: 1 % critical value of the Im, Pesaran, and Shin tests
Ψ: transformed t-bar statistics. $\Psi \sim N(0, 1)$.
p: p-value of Ψ; ***: $p < 0.01$.

only limited explanatory power, we double-check for nonstationarity by using the Im-Pesaran-Shin test. The results from the lower panel of Table 3.1 suggest again that our dependent variable is stationary. Therefore, we proceed assuming stationarity of the dependent variable.

Model 1, incorporating all of the control variables discussed earlier into the model specification, tests the price-reducing effect of SMD systems. During our model-building process, we first are concerned with the possibility that the spherical errors assumption might not hold in our data set. Belgium and Luxembourg, to take a minor but clear example, were in a full currency union throughout this period and hence are not independent observations on the exchange-rate variable. We regard this as an empirical question, and the empirical results from the Breusch-Pagan LM test of independence and the modified Wald test for group-wise heteroskedasticity indicate strongly that the errors are not spherical. Accordingly, we reestimate the model by using panel-corrected standard errors, as originally proposed by Beck and Katz (1995), to guard against potential problems of panel heteroskedasticity across countries and contemporaneous correlation of error. Note that we also include the lagged dependent variable to account – albeit imperfectly, as we shall see – for first-order autocorrelation.

The result, confirming the visual impression of Figure 3.1, suggests strong negative price-level effects of SMD systems. According to this model, a short-run shift from PR systems to SMD systems leads to a reduction of price levels by 1.2 units (recall that the baseline is 100 in the United States). In the average OECD country, with a price level of 105.88, this amounts to a 1.1 percent drop in prices. In the long run, the "market basket" of goods and services under SMD systems is cheaper than under PR in the average OECD country by

11 percent,[18] and half of the long-term change will be achieved in 6.6 years.[19] We note also, here and in the subsequent models, that the estimated coefficients on our control variables are consistently of the expected sign, albeit not in every case statistically significant.

As Kittel and Winner (2005) note, cross-sectional time-series analysis may be unreliable owing to its sensitivity to the inclusion of country and year dummies. This pooled specification, of course, runs the risk of omitted-variable bias, most notably from such unobserved country-specific characteristics as political culture, geography, or institutional inheritance (e.g., a common-law system). To guard against this possibility, Model 2 of Table 3.2 reestimates the model with country fixed-effects: the country dummy variables now pick up any country-specific intercept that is not accounted for elsewhere. Model 3 in turn explores annual fixed-effects. By including year dummies, we would be able to eliminate any bias caused by unaccounted trends and external shocks to which all these OECD countries might have been jointly exposed. Importantly, as we can see from Model 2 and Model 3, our substantive finding – that SMD reduces price – is insensitive to the inclusion of both country and year effect. Yet, upon

18 $((1.198/(1-.9000))/105.6 = .1134$.
19 $-\log 2/\log(.9) = 6.58$ years.

Table 3.2. Model results: Dependent variable is purchasing power parity over exchange rate, PPP/XR

	Model 1 PPP/XR Pool	Model 2 PPP/XR FE(Country)	Model 3 PPP/XR FE(Year)	Model 4 PPP/XR FE(Decade)
Lagged Dependent	.900***	.842***	.886***	.863***
Variable	[.022]	[.030]	[.021]	[.022]
Single-member district	−1.198***	−2.317***	−1.684***	−1.727***
	[.461]	[.830]	[.589]	[.493]
Real per capita GDP	.0001	.0003***	.0003***	0.0003***
	[.0001]	[.0001]	[.0000]	[0.0001]
Imports as a share of GDP	−0.128***	−0.048	−.103***	−.126***
	[0.020]	[.049]	[.015]	[.017]
Population, log	−2.021***	−36.179***	−1.429***	−1.837***
	[0.343]	[7.333]	[.247]	[.313]
GDP growth rate	.051	−.193	−.002	−.090
	[.135]	[.124]	[.115]	[.129]
Change in exchange rate	−0.735***	−.759***	−.518***	−.672***
	[0.040]	[.038]	[.049]	[.041]
U.S. Inflation rate	0.300*	.406***	dropped	−0.026
	[0.165]	[.151]		[.189]
Constant	47.088***			
	[6.729]			
Joint significance of fixed effects		.000***	.000***	.000***
N	666	666	666	666

Note: Panel-corrected standard errors in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All tests are two-tailed. The individual country coefficients (including the constant) in fixed-effects models are omitted in the interest of space.

further consideration, two theoretical reasons impel us not to put much stock in annual fixed-effects.

First, our previous control variable, the U.S. inflation rate, provides a more coherent theoretical linkage between the common external shocks and the dependent variable than does a set of annual dummies. Secondly, the notion of annual fixed-effects is deceiving in the sense that the number of years in the model is theoretically unbounded as time goes to infinity. Econometrically, Neyman and Scott (1948) show that one's estimates can be inconsistent if the model includes variables that increase in tandem with the number of observations (aka, "the incidental parameter" problem). Therefore, instead of using year dummy variables, we include *decade* dummy variables to capture the time dimension in Model 4. From Model 4, we can clearly see that SMD electoral systems continue to show significant and strong negative price effects. We observe also that the coefficient of *USAINF* loses its significance once we include the year or decade fixed-effects – exactly as one might expect, because the variable USAINF is unit-invariant and highly correlated with the time dummies.[20]

[20] Parenthetically, we note also that the coefficients on the decadal dummies (using the 1970s as the baseline) are negative, suggesting the possibility that real prices relative to the United States were consistently dropping over this period. We also integrate country fixed-effects and decade fixed-effects in a unifying model, and our empirical results are again sustained.

3. *Robustness Checks*

To buttress our empirical analysis further, we now undertake a series of robustness checks. Particularly, we pay special attention to several substantive and methodological issues that are related to the construction of the dependent variable, the independent variables, and the model specification.

First, one might reasonably suspect that different electoral systems are associated with respectively different levels of taxation that, in turn, yield the price differences that we have attributed to differences in competition policy and regulation. Indeed, a comparison of consumption tax levels (value added and sales tax) shows consumption taxes to be higher in proportional systems, as Beramendi and Rueda (2007) have convincingly shown (cf. Chapter 7, later). Nevertheless, Model 5 in Table 3.3 uses prices *net of tax*[21] as a new dependent variable instead of PPP/XR. The SMD coefficient remains negative, significant, and nearly of the same magnitude as with the unmodified dependent variable. Consumption taxes may be higher in PR systems (1) because value-added tax (VAT) is preferred over sales tax in most PR countries and (2) because VAT levels are usually higher than sales taxes; but as Lindert (2003)

[21] Calculated using mean VAT or sales tax rate across all categories of goods and services. Source: OECD Revenue Statistics, www.sourceoecd.org. Note that we use the mean VAT and sales tax rates across all goods and services as opposed to standard rates, which are subject to many exemptions or reductions for many sectors.

Table 3.3. Robustness checks

	Model 5: Net of Tax FE(Decade)	Model 6: PPP/XR Pool	Model 7 PPP/XR GMM	Model 8 PPP/XR Pool
Lagged Dependent	.836***	.900***	.801	.903***
Variable	[.023]	[.022]	[.040]	[.017]
Single-member district	−1.822***		−4.407**	−.976*
	[.424]		[2.103]	[.510]
SMD plurality system		−1.188*		
		[.623]		
SMD majority system		−1.173**		
		[.461]		
Reinforced PR system		.207		
		[.525]		
Campaign regulation				1.069***
				[.251]
Clarity of responsibility				−.161
				[.291]
Real per capita GDP	0.0004***	.0001	.0008***	.0001
	[0.0001]	[.0001]	[.0002]	[.0001]
Imports as a share of GDP	−.100***	−.127***	.146	−.080***
	[.017]	[.020]	[.094]	[.017]
Population, log	−1.185***	−2.029***	−21.858	−.904***
	[.314]	[.346]	[27.485]	[.156]
GDP growth rate	−.226**	.049	−.319***	−.034
	[.110]	[.137]	[.086]	[.092]
Change in exchange rate	−.654***	−.736***	−.721***	−.956***
	[.038]	[.137]	[.107]	[.027]
U.S. Inflation rate	−.006	.302*	.237*	.235*
	[.177]	[.168]	[.136]	[.123]

	Model 5: Net of Tax FE(Decade)	Model 6: PPP/XR Pool	Model 7 PPP/XR GMM	Model 8 PPP/XR Pool
Constant		47.134***	−.724***	25.847***
		[6.747]	[.179]	[3.393]
Joint significance of fixed effects	.000***			
N	656	666	642	666

Note: Panel-corrected standard errors in bracket except in Model 7 where robust standard errors are reported.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All tests are two-tailed. The decade coefficients (including the constant) in Model 5 are omitted in the interest of space.

suggests, the causality is most likely the reverse: PR countries prefer a generous welfare state that demands higher taxes. PR countries therefore choose the most efficient consumption tax system (VAT) to minimize the distortions of their relatively high tax levels. In short, consumption taxes and prices net of tax show the same, theoretically consistent, result: both are substantively and statistically higher in PR systems.

Second, one might object that our key explanatory variable, SMD, is too crude to capture the notion of seats-votes elasticity, or "responsiveness" (see King 1990; Katz 1997, esp. chap. 9). Alternatively, others might question whether our empirical model is inadequate to entertain the heterogeneity across countries. To address these issues, we first refine our measurement by expanding our electoral system variable into four categories presumably in descending order of seats-votes elasticity:[22] (1) SMD plurality (U.S., UK, Canada, and New Zealand before 1994, plus "mixed" but predominantly SMD plurality systems (Italy and Japan after 1993); (2) SMD majority (Australia, France except 1986–1988); (3) "reinforced" PR[23] (Japan before 1993, Greece, Spain) plus Single Transferable Vote (STV; Ireland); and (4) pure or nearly pure PR (Austria, Belgium, Denmark, Finland, Germany, Iceland, Italy before 1993, Luxembourg, Netherlands, New Zealand since 1994, Norway, Portugal, Sweden, Switzerland). Our expectation, of course, is that these four categories will also affect prices in descending order (SMD-plurality will lower them most, pure PR least). To test this hypothesis, we create dummy variables for each category, leaving pure PR as the baseline for comparison. Then, we replace the SMD dummy with these three finer-grained variables and refit the model. As shown in Model 6, the effects do follow a descending order of magnitude. However, neither the difference between SMD plurality and SMD majority systems, nor the difference between "reinforced" and "pure" PR, is statistically significant. Therefore,

[22] We have also considered such more traditional measures of system proportionality as district magnitude, effective number of parties, and effective threshold; but these, it seems to us, do not accurately reflect seats-votes elasticity. For a specific examination of the effect of district magnitude, see Rogowski and Kayser (2002).

[23] The "reinforced" systems award the largest party a considerably higher share of seats than of votes.

we stick to our original parsimonious categorization and continue to use the SMD variable to capture the effect of electoral systems.

To further account for the unobserved heterogeneity across countries, we next consider a dynamic panel model in which we introduce unobservable unit heterogeneity into the error term.[24] Under such a circumstance, the lagged dependent variable model (as used from Model 1 to Model 6) will yield biased and inconsistent estimation due to the correlation between the lagged dependent variable and the composite error term. The bias remains of order 1/T even if we attempt to remove the unobserved heterogeneity by using the fixed effect estimator (Baltagi 2001). Hence, we turn to the Arellano–Bond generalized method of moments estimator (GMM) (Arellano and Bond 1991).[25] The results, presented in Model 7, continue to support a strong price-reducing effect of SMD systems even after we explicitly model the unobservable heterogeneity across countries.

24 Formally, our dynamic panel data structure takes the form of $y_{i,t} = \sum_{i=1}^{j} py_{i,t-j} + X_{i,t}B + \varepsilon_{i,t}$, where $\varepsilon_{i,t} = \alpha_i + u_{i,t}$.

25 Briefly summarized, the idea of this GMM estimator is to use first-differencing transformation to remove the unobservable unit effect. The resultant correlation between $\Delta y_{i,t-1}$ and $\Delta u_{i,t-1}$ from the transformation procedure is then instrumented out by using the dependent variable and all independent variables from two lags and before. For a detailed discussion of dynamic panel data in political science, see Wawro (2002).

Table 3.4. Jackknife analysis

	Maximum coefficient	Country omitted at max. coeff.	All countries (Model 3)	Minimum coefficient	Country omitted at min. coeff.
Lagged DV	0.871***	Portugal	0.863***	0.848***	Iceland
	[.024]		[0.022]	[.019]	
Single-member district	−2.284***	UK	−1.727***	−0.879*	New Zealand
	[.519]		[0.493]	[.547]	
Real per capita GDP	.0004***	Luxembourg	0.0003***	.0002***	Iceland
	[.0001]		[0.0001]	[.0000]	
Imports as a share of GDP	−0.143***	New Zealand	−0.1266***	−0.080***	Iceland
	[.0198]		[0.0173]	[.010]	
Population, log	−2.213***	New Zealand	−1.8375***	−0.844***	Iceland
	[.378]		[0.3136]	[.130]	
GDP growth rate	−.265***	Iceland	−0.0904	−.050	Japan
	[.083]		[0.1290]	[.131]	
Change in exchange rate	−0.830***	Iceland	−0.6723***	−0.655***	Japan
	[.029]		[0.0415]	[.041]	
U.S. Inflation rate	0.108	Iceland	−0.0264	−0.061	Italy
	[.140]		[0.1898]	[.193]	

Note: The maximum and the minimum are defined in terms of absolute value. Panel-corrected standard errors in brackets. * if $p < 0.1$, ** if $p < 0.05$, *** if $p < 0.01$. All tests are two-tailed.

Next, to ensure that our empirical results are not driven by any particular country, we follow Kittel and Winner (2005) and perform a Jackknife analysis (Elton and Tibshirani 1993; Gentle 2002) on our Model 4. Specifically, we reestimate the model repeatedly, excluding one country in each run. The resulting minimum and maximum values of the estimates of key co-variates are presented in Table 3.2. We can clearly see that the coefficient of SMD remains negative and significant throughout, even under this demanding procedure.

Finally, we test our electoral systems hypothesis against other competing theories in the political economy literature. Indeed, even should we succeed in controlling for all possible confounders and, additionally, should we nevertheless continue to estimate a robust effect of electoral systems on real prices, we still will not have distinguished between several mechanisms by which electoral systems could yield lower price levels. Multiple governmental and electoral features covary with electoral system, any one of which could alone or in combination affect prices.

First, systematic differences between electoral systems in the provision of state funding for and spending caps on election campaigns could alter politicians' responsiveness to organized producers. As Denzau and Munger (1986) demonstrate in their model of how politicians optimize their allocation of effort between organized interest groups (read: producers) that provide electoral resources and unorganized interests (read: consumers) that provide only votes, resources attract legislators' favor. Extending this model to nomination procedures under different electoral systems, Bawn and Thies (2003) find that legislators favor organized interests more under (closed list) PR than SMD. This, however, is a marginal effect: we, of course, are also interested in levels.

Setting aside different marginal effects of producer resources such as campaign donations on legislative favor, differences in the amount of campaign donations legally permitted may also matter. Greater governmental regulation and financing of campaigns could theoretically reduce producer resources, weaken producers vis-à-vis consumers, and offset the previous marginal effect. Hence, we utilize the Political Finance Database provided by the International IDEA.[26] We focus especially on two important indexes of governmental regulation and financing of campaigns – whether political parties receive direct public funding and whether there exists a ceiling on party election expenditure. In search of parsimony, we build a composite variable, *RESTRICT*, by taking the average of these two indexes. We remain agnostic about whether the greater marginal effect or smaller sum of producer contributions in PR systems has the greater effect on legislative attentions. A large net effect in either direction, however, cannot be ignored.

Powell and Whitten (1993) offer a second alternative mechanism, certainly strongly associated with electoral system. To explain the instability of the economic vote estimated in cross-national election studies (cf. Paldam 1991), they argue that institutional arrangements clarify or obscure

[26] See www.idea.int/parties/finance/db/. Note that Greece and Luxembourg are not covered in this database and hence are dropped from this part of our analysis.

the responsibility of governing parties for policy outcomes. Voters hold high-clarity governments accountable for policy outcomes but cannot assign blame or credit as easily when multiple parties have hands on the wheel. We note that many of the components that increase clarity of responsibility, including those introduced by subsequent research (*inter alia*, Whitten and Palmer (1999) and Nadeau, Niemi, and Yoshinaka (2002)), are associated with majoritarian electoral arrangements: majority governments, single-party governments, absence of proportional committee systems, a low number of parties in government, ideological cohesion of governing parties. Single-party governments, which commonly emerge in SMD systems, understand that voters will punish or reward them – not a coalition member – for changes in the purchasing power of their income. Accountability, in turn, may tilt legislators' favors from producers to consumers. Entertaining this rival mechanism that might be driving a spurious relationship between electoral systems and price levels, we construct a dummy variable that takes the value of one in countries with high levels of clarity of responsibility.[27]

Model 8 provides a level playing field for these three competing hypotheses. As we can see, the positive and significant coefficient on campaign regulation, contrary to

[27] Luxembourg, Portugal, Spain, and Iceland are not covered in Powell and Whitten's study.

expectations, suggests that countries with stricter regulation of campaign finance are also associated with *higher* price levels. It is possible that campaign finance regulations and prices might both be determined by an omitted variable but, whatever this might be, it is not the electoral system. As of the year 2000, thirteen of fourteen PR and six of seven SMD states offered direct state funding of political campaigns. Campaign spending caps vary more across electoral systems – three of fourteen PR and four of seven SMD states imposed them in 2000 – but the stability of the SMD coefficient in Model 7 suggests that campaign finance regulations have little systematic association with electoral system. While the regulation of campaign spending has a significant and positive effect that contradicts the expectations of the campaign finance rival hypothesis, the coefficient on clarity of responsibility simply reveals no significant effect, and seems not to impose any consequence on prices. Despite controls for rival mechanisms, a majoritarian electoral system remains a crucial institutional force that suppresses price levels.

During the course of our empirical inquiry, we remained cautious about issues of autocorrelation. As astute readers will have noted, estimates of the coefficient of the lagged dependent hover around 0.85 across all models. This alerts us that the lagged dependent variable alone may fail to handle adequately the autocorrelation that underlies the data-generating process. To address this concern, we follow the standard practice and estimate a first difference model. The results in Model 9 (Table 3.5) show that the change of electoral system (i.e., a shift from PR to SMD) does reduce the price after a one-year lag. In this specification, all variables enter the regression in the form of first differences. The first-differenced SMD variable (ΔSMD) is also entered with a one-year lag (ΔSMD_{t-1}), because we suspect that a shift from PR to SMD systems might take time to register its effect on the price. We find that the unlagged first-differenced SMD variable has the correct sign but is only marginally significant. However, the lagged first-differenced SMD variable is significant at less than the .05 level; the value of the coefficient is −3.52, indicating that a shift from PR systems to SMD systems is associated with a reduction of the change in price level by 3.3 percent.

This result suggests more complex dynamics than a first-difference model can provide. Moreover, because our key independent variable SMD varies only in France, Japan, Italy, and New Zealand over the period we consider, the first-difference approach would provide too little variation to test our hypothesis adequately. Therefore, we re-cast our model into a single-equation error correction form (ECM) to estimate the long-run relationship and the short-run dynamics simultaneously.[28] Notice that while the ECM and the

[28] Concretely, the single equation ECM regresses the first difference of the dependent variable on (1) its lagged level and any lagged changes

Table 3.5. Robustness checks: First differenced and error correction models

	Model 9: First differenced	Model 10: ECM
Lagged dependent variable		−.046***
		[.014]
△ Single-member district	−2.072	−.282
	[1.676]	[1.257]
△ Single-member district, lag	−3.522**	
	[1.760]	
Single-member district, lag		−.970**
		[.392]
△ Real per capita GDP	.004***	.004***
	[.000]	[.000]
Real per capita GDP, lag		−.000
		[.000]
△ Imports as a share of GDP	.162	.168
	[.221]	[.120]
Imports as a share of GDP, lag		−.057***
		[.011]
△ Population, log	16.942	67.446*
	[53.615]	[35.129]
Population, log, lag		−.775***
		[.174]
△ GDP growth rate	−.228*	−.528***
	[.126]	[.082]
GDP growth rate, lag		−.416***
		[.096]
△ Change in exchange rate	−.219***	−.331***
	[.044]	[.035]
Change in exchange rate, lag		−.281***
		[.034]

	Model 9: First differenced	Model 10: ECM
Δ U.S. Inflation rate	.889***	−.187
	[.283]	[.164]
U.S. Inflation rate, lag		−.786***
		[.138]
Constant	−3.190	
	[.787]	
Joint significance of fixed effects		.000***
N	643	643

Note: Panel-corrected standard errors in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All tests are two-tailed. The decade coefficients (including the constant) in Model 10 are omitted in the interest of space.

auto-distributed lag models (where the lagged dependent variable model can be seen as a special case) are mathematically equivalent,[29] an important advantage of the ECM is that it avoids a spurious relationship because its dependent variable is first-differenced.

suggested by the data as necessary to pick up the auto-correlation components, (2) lagged values of potential cointegrating independent variables, and (3) whatever changes in the independent variables are suggested by the theory. The coefficients on first-differenced independent variables represent "transitory effects" of changes in independent variables on changes in the dependent variable; coefficients on lagged independent variables represent "permanent relationships" between levels of the independent and dependent variables (Franzese 2002). Because we do not have any theoretical prior belief about whether the effects of the explanatory variables are temporary or persistent, we include all explanatory variables in both contemporaneous differences and lagged terms.

29 A recent study by DeBoef and Keele (2006) provides strong evidence using a Monte Carlo simulation.

The results are reported in Model 10. Note that the coefficient on PPP/XR_{t-1} reflects the dynamics of the relationship between co-variates and price levels, and its estimated coefficient, $-.046$, implies a considerably slower adjustment process than did our simple lagged dependent variable (LDV) model. Specifically, more than 95 percent $(1 + (-.046))$ of a shock to price level in the current year will last into the next. In addition, the result indicates that for half of a price shock's long-run impact to emerge will require almost fourteen years, rather than the four to seven years suggested by the LDV model.[30] More importantly, note that the coefficient on SMD_{t-1} is negative and significant. The estimate of the long-run effect of a permanent change from PR to SMD systems is thus $(-.970)/-(-.046) = -21.08$, which again supports, and indeed strengthens, our hypothesis: in the long run, real price levels are lower under SMD than under PR systems by more than 15 percent $(21.08/105.6 = 19.96)$. We suspect that this estimate of time to reequilibration, and perhaps also of long-term effect, is closer to reality than that of our LDV model. In addition, the corroborating evidence from the ECM model should further alleviate any lingering concern regarding the potential threat of the unit roots.

In sum, all the analyses presented so far, including checks for robustness against most (but, obviously, not all)

[30] $-\log 2/\log(.954) = 14.71$.

conceivable sources of estimation error, underscore our basic result: real prices are indeed lower under SMD systems. Controlling for wealth, trade barriers, population size, GDP growth rate, exchange rate fluctuations, and the inflation rate in the United States, SMD electoral systems are associated with at least a 10 percent drop in real prices, and likely an even larger one, in the average OECD country.

III. Discussion and Conclusion

The model laid out in Chapter 2 strongly suggests that governmental policy will be biased toward consumers under almost all majoritarian electoral systems, toward producers – or, more generally, toward organized interests – under systems of PR; and one clear manifestation of this bias will be higher real price levels under PR, lower prices under majoritarian systems. The empirical analysis undergirding this claim, however, has been limited to a cross-section of the OECD countries for a single year presented in the original Rogowski and Kayser (2002) paper, which could do little more than establish the plausibility of the hypothesis. Evidence for the OECD countries over a period of thirty years presented here now bears out the theoretical expectation, both cross-sectionally and over time, with considerable robustness and under a much greater variety of statistical tests: SMD electoral systems are associated with lower prices; and hence, we conclude, also with greater consumer power. Moreover, we are able to establish, as the earlier study did not: (a) the likely effects of a change of electoral system in a single county and (b) the short- versus long-term impact and the length of time required to reach the new equilibrium. We attach particular importance to the present finding that the long-term effects of electoral systems are at least as strong as the cross-sectional ones that the earlier study established. Finally, the present study, by exploiting the fortuitous fact that several OECD countries changed electoral systems in the 1980s and 1990s, substantially remedies problems of systematic differences between PR and SMD systems.

Obviously, more questions remain, not least about endogenous institutions and the role of electoral competitiveness. Consider endogenous institutions: While this chapter treats the electoral system as exogenous, one might reasonably suspect that policy outcomes induced by alternative electoral systems in turn shape voters' preferences about the choice of electoral systems. Put differently, while this chapter shows that PR (majoritarian) systems systematically lead to higher (lower) prices and higher socioeconomic equality (inequality), it might well be the case that voters in societies characterized by greater equality (inequality) are motivated to support PR (majoritarian) systems. In this sense, electoral systems could be self-sustaining, and the self-reinforcing cycle between the choice of electoral system and social equality may provide a previously unnoticed account for institutional stability. We address precisely this possibility in Chapter 6.

No less important a topic for future research is the role of electoral competitiveness. Readers of Chapter 2 will recall that the seats-votes elasticities are predicated on an equal division of the vote. Once a single party in a majoritarian system becomes sufficiently dominant, the price predictions of the model actually reverse. That is, majoritarian countries with a traditionally dominant party can expect higher prices than they would have had under PR. Investigating the precise role of electoral competitiveness in mediating or moderating the relationship between electoral systems and real price levels promises considerable gains.

These and other questions call for investigation. However, we take it by now as at least highly likely that, among the economically advanced democracies, more majoritarian systems produce policies markedly friendlier to consumers, and less favorable to producers, than do systems of proportional representation.