Elections, Uncertainty, and Irreversible Investment[*]

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Abstract

We argue that the policy uncertainty generated by elections encourages private actors to delay investments that entail high costs of reversal, creating pre-election declines in the associated sectors. Moreover, this incentive depends on the competitiveness of the race and the policy differences between the major parties/candidates. Using new survey and housing market data from the United States, we test these arguments. The survey analysis assesses whether respondents' perceptions of presidential candidates' policy differences increased the respondents' likelihood of delaying certain purchases and actions. The housing market analysis examines whether elections are associated with a pre-election decline in economic activity, and whether any such decline depends on electoral competitiveness. The results support the predictions and cannot be explained by existing theories.

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How do elections affect the economy? Most work on this question examines whether elections alter the economic policies of incumbent governments. In particular, theories of "opportunistic" political business cycles suggest that policymakers induce short-term economic expansions immediately before elections with the expectation that the gains will subside soon after the votes have been cast.[1] A separate line of work argues that left-wing governments pursue low unemployment at the expense of higher inflation while right-wing governments prioritize low inflation at the expense of higher unemployment. In the rational partisan theory, for instance, these partisan priorities and uncertainty about the electoral outcome cause a post-election increase (decrease) in unemployment if the left- (right-) wing party wins.[2]

We argue that a major and underappreciated impact of elections is that they generate policy uncertainty that encourages individuals and businesses to delay certain types of investments until the race concludes. These investments are ones that would be

[1] For reviews see James E. Alt and Alec K. Chrystal, *Political Economy* (Berkeley: Berkeley University Press, 1983); Allan Drazen, 'The Political Business Cycle after 25 Years', in Ben S. Bernanke and Kenneth Rogoff, eds, *NBER Macroeconomics Annual 2000*, 15 (Cambridge, Massachusetts: MIT Press, 2000), pp. 75-138; Robert J. Franzese, 'Electoral and Partisan Cycles in Economic Policies and Outcomes', *Annual Review of Political Science*, 5 (2002), 369-421; William R. Keech, *Economic Politics: The Costs of Democracy* (New York: Cambridge University Press, 1995).

[2] Alberto Alesina, John Londregan, and Howard Rosenthal, 'A Model of the Political Economy of the United States', *American Political Science Review*, 87 (1993), 12-33.

impossible or costly to reverse; scholars have termed them "irreversible investments."[3]

Examples include housing or automobiles for individuals and fixed capital for businesses.

In the sectors associated with such investments, the incentive for delay causes a pre-election decline. Moreover, because the incentive derives from the policy uncertainty associated with the election, the decline is larger the greater are the policy differences between the major parties/candidates and the more competitive is the race.

   We test the arguments about irreversible investment, elections, and uncertainty with survey and behavioral data that involve a canonical irreversible investment, housing. We collected the survey data in conjunction with the 2008 Cooperative Congressional Election Survey (hereinafter CCES), which involved pre- and post-election waves in association with the 2008 United States elections.[4] This survey analysis compares a respondent's pre-election perceptions of how much the presidential election outcome will affect his or her financial situation with post-election accounts of whether he or she delayed housing-related activities and purchases until learning the outcome. Notably, only some of these activities are irreversible investments, and therefore the theoretical arguments do not always predict a significant relationship. The survey analysis can

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[3] E.g., Ben S. Bernanke, 'Irreversibility, Uncertainty, and Cyclical Investment', *Quarterly Journal of Economics*, 98 (1983), 85-106.

[4] Stephen Ansolabehere, 'Guide to the 2008 Congressional Cooperative Election Study' (Harvard University Typescript, 2009), available at http://projects.iq.harvard.edu/cces/data?dvn_subpage=/faces/study/StudyPage.xhtml?globalId=hdl:1902.1/14003 (accessed December 14, 2011).

therefore repudiate the theory by finding an effect when one is not predicted as well as by failing to find an effect when one is predicted.

The second type of test involves data on US housing markets. More specifically, we make use of the staggering of US gubernatorial elections across time and states to analyze whether the variation in gubernatorial electoral cycles is associated with variation in state and local housing markets. Because the theoretical arguments apply most directly to the quantity of sales, we begin with an analysis of this factor using Zillow.com data from thirty-five states from 1999-2006. We proceed to discuss conditions under which the arguments apply to home prices and examine price data from over three hundred metropolitan statistical areas from 1979-2006.

The paper begins by reviewing research on irreversible investment. The second section develops the theoretical argument and compares it to other perspectives. The third section describes the survey analysis, and the fourth section the examination of housing markets. We conclude by discussing the implications of the analysis for understanding the impact of elections on the economy.

Research on Irreversible Investment

Various formal models analyze the incentive to delay irreversible investments when economic uncertainty is high.[5] The original models focused on business decisions, and show that delay can be optimal even for a risk neutral firm that incurs costs from postponing the investment. The firm's incentives depend on how soon the new

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[5] E.g., Bernanke, 'Irreversibility, Uncertainty, and Cyclical Investment'; Alex Cukierman, 'The Effects of Uncertainty on Investment under Risk Neutrality with Endogenous Information', *Journal of Political Economy*, 88 (1980), 462-475.

information will be revealed, the value of the information, the costs of delay, and the cost of undoing the investment (if undoing it is even possible). Subsequent scholarship applied the theories to consumer behavior and established that economic uncertainty can induce individuals to postpone costly-to-undo investments including homes and consumer durables.[6] Housing is a archetypal irreversible investment given the high transaction costs associated with undoing the purchase of the home; as Christopher Carroll and Wendy Dunn contend, housing is the "mother of all durable goods."[7] Furthermore, households commonly devote a significant portion of their savings to this asset.[8]

Some studies have conjectured that political events are an important source of economic uncertainty, but this possibility has more typically "been discussed casually"

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[6] E.g., Christopher D. Carroll and Wendy E. Dunn, 'Unemployment Expectations, Jumping (S,s) Triggers, and Household Balance Sheets', in Ben S. Bernanke and Julio J. Rotemberg, eds, *NBER Macroeconomics Annual*, 12 (Cambridge: MIT Press, 1997), pp. 165-230; John Hassler, 'Uncertainty and the Timing of Automobile Purchases', *Scandinavian Journal of Economics*, 103 (2001), 351-66; Christina D. Romer, 'The Great Crash and the Onset of the Great Depression', *Quarterly Journal of Economics*, 105 (1990), 597-624.

[7] Carroll and Dunn, 'Unemployment Expectations, Jumping (S,s) Triggers, and Household Balance Sheets', p. 179.

[8] Ben Ansell suggests that homes are such a large part of individuals' assets that one's home value influences preferences about social insurance policies. Ben W. Ansell, 'Bubbling Under: Political Preferences during Asset Bubbles' (University of Minnesota Typescript, 2008).

rather than scrutinized, to quote George Bittlingmayer.[9] Bittlingmayer himself offers

evidence within the context a major historical shift; in particular, he demonstrates that the

switch from Imperial to Weimar Germany induced stock market volatility and output

declines.[10] Likewise, Nicholas Bloom shows that major wars and acts of terrorism can

induce stock market volatility.[11] However, even Bittlingmayer and Bloom do not offer

evidence that more routine political events, let alone elections, affect irreversible

investment because of the associated political uncertainty.

Theoretical Framework

Because others have already developed formal theories of irreversible investment

and applied them to the housing industry, we do not take up space constructing a full

formalization. Instead, we begin by justifying the argument that elections are a key

source of uncertainty and then delineate how the uncertainty affects individuals' and

firms' incentives. We proceed to argue that this impact depends on the policy differences

between the major parties/candidates and electoral competitiveness.

Elections and Uncertainty

Elections are an important source of economic uncertainty due to the fact that the

outcome of a race affects subsequent government decisions. As Andrea Mattozzi

summarizes, "Political uncertainty…arises because different candidates running for

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[9] George Bittlingmayer, 'Output, Stock Volatility, and Political Uncertainty in a Natural

Experiment: Germany, 1880-1940', *Journal of Finance*, 53 (1998), 2243-57, at p. 2245.

[10] Ibid.

[11] Nicholas Bloom, 'The Impact of Uncertainty Shocks', *Econometrica*, 77 (2009), 623-685.

office, if elected, will implement different policies…"[12] These differences encompass fiscal as well as regulatory policies. Moreover, the policies may be targeted to a specific industry or instead designed to affect society more broadly.

Consider fiscal policy. A range of work suggests that conservative parties prefer lower spending on domestic programs and lower taxes than progressive parties do; this generalization has received support in a variety of countries including the US, where the difference holds at the national and state-levels.[13] In other words, for individuals as well as firms, the outcome of an election may affect their income and/or tax rates. For an individual, such an effect might occur if his or her taxes were likely to change under a new government; if his or her job were associated with government employment, government contracting, or an industry closely associated with public programs; or if his income depended on government benefits that the left- or right-wing party would likely alter. As these examples suggest, the uncertainty will not be identical across society but instead vary according to the likely effects of the candidates'/parties' policies.

Indeed, some types of fiscal policies are even targeted to particular investments or industries. For instance, the Obama administration has proposed reducing the mortgage-interest tax deduction for high-income households. This policy, if enacted, would

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[12] Andrea Mattozzi, 'Can We Insure Against Political Uncertainty? Evidence from the U.S. Stock Market', *Public Choice*, 137 (2008), 43-55, at p. 43.

[13] E.g., James E. Alt and Robert C. Lowry, 'Divided Government, Fiscal Institutions, and Budget Deficits: Evidence from the States', *American Political Science Review*, 88 (1994), 811-28; David W. Brady and Craig Volden, *Revolving Gridlock*, 2nd ed. (Boulder, CO: Westview Press, 2005); Thomas R. Cusack, 'Partisan Politics and Public Finance: Changes in Public Spending in the Industrialized Democracies, 1955-1989', *Public Choice*, 91 (1997), 375-395.

effectively increase the cost of home mortgages to these households.[14] At the US state-level, some governments have limited the ability of localities to raise property taxes and provided property tax relief to certain classes of homeowners.[15]

Parties differ over not only fiscal policies but also regulatory policies that favor different mixes of industries. For example, environmental regulations that support alternative energy sources may increase the profitability of these businesses as well as the desirability of cars or homes that make use of such energy sources.[16] With respect to housing, industry- or firm-specific regulations can also influence the value of homes that are located near a firm affected by the regulations. Moreover, research suggests that homeowners are aware of these effects. For instance, Kenneth F. Scheve and Matthew J.

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[14] Walter Alarkon, 'Ax May Fall on Tax Break for Mortgages', *The Hill*, 8 June 2010.

[15] See, e.g., Elizabeth Albanese, 'Texas' Perry Signs School Finance Bill that Cuts Maximum Property Tax Rate', *Bond Buyer*, 6 June 2006, p. 4; 'Governor Rendell: Record 588,638 Pennsylvanians Received Property Tax/Rent Rebates', *PR Newswire*, 16 February 2010.

[16] Numerous studies suggest that industries favored by a particular party perform better in the stock market as that party's probability of winning increases. For evidence at the presidential level, see Michael C. Herron, James Lavin, Donald Cram, and Jay Silver, 'Measurement of Political Effects in the United States Economy: A Study of the 1992 Presidential Election', *Economics & Politics*, 11 (1999), 51-81; Mattozzi, 'Can We Insure against Political Uncertainty? Evidence from the U.S. Stock Market'. For evidence at the state-level, see Michael R. Butler and Edward M. McNertney, 'Election Returns as a Signal of Changing Regulatory Climate', *Energy Economics*, 13 (1991), 48-54.

Slaughter find that individuals care about the effects of trade policies on local firms due to the ensuing effects on local home prices.[17]

Finally, for some individuals, elections create economic uncertainty by affecting the behavior of the individuals' employers. Research on the timing of contract negotiations suggests that firms tend to delay these negotiations until after elections, and this research relates the delay to the policy uncertainty created by elections.[18] Accordingly, workers who are aware that their contract will be reviewed after an election may associate it with economic uncertainty even if the workers are not following the race closely. The policy uncertainty associated with elections thus engenders economic uncertainty for individuals and firms through a variety of mechanisms.

Incentives for Irreversible Investment

Because elections induce uncertainty, individuals and firms can have the incentive to delay costly-to-undo investments until after electoral outcomes are realized. Consider a given individual or firm X that is interested in making an irreversible investment i, and assume the outcome of the election affects the type or level of investment that is optimal. The investment is irreversible; for simplicity, assume it cannot be undone. (Alternatively, one could assume it can only be undone at high costs and incorporate how these costs affect X's incentives.) If X delays the investment until after the election, X chooses the optimal investment i^*. Delaying the investment postpones the utility and/or profits that X

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[17] Kenneth F. Scheve and Matthew J. Slaughter, 'What Determines Individual Trade-Policy Preferences?', *Journal of International Economics*, 54 (2001), 267-92.

[18] Michelle R. Garfinkel and Amihai Glazer, 'Does Electoral Uncertainty Cause Economic Fluctuations?' *American Economic Review, Papers and Proceedings of the 106th Annual Meeting of the American Economic Association*, 84 (1994), 169-73.

receives from the investment, however, and therefore carries a cost c. If X commits to an investment before the election, then X avoids the cost c but risks choosing an investment that is not first-best given the information that is subsequently revealed. The question for X is thus whether the expected utility from i^* versus that from the investment that X would make before the election outweighs the cost of delay c.[19]

In the previous sub-section, we provided a good deal of justification for the claim that elections create policy uncertainty. Given this effect, it seems reasonable to expect that the benefits of waiting will commonly outweigh the costs associated with doing so. If this expectation is correct, then on average elections will be associated with a pre-election decline in irreversible investment. That is, if we examine all elections taken together as a group, we should expect to observe a pre-election decline. We will refer to this prediction as the Pre-election Decline Prediction.

Policy Differences and Irreversible Investment

The discussion thus far has emphasized that the incentive for delaying costly-to-undo investments derives from the policy differences between the major parties/candidates. Yet of course, elections are not identical in terms of the policy differences between the parties. In some cases, the parties' positions might be relatively similar while in other cases, the parties might propose drastically different approaches to economic issues. Likewise, even within a given election, the policy differences that exist

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[19] This decision-theoretic framework follows that in Cukierman, 'The Effects of Uncertainty on Investment under Risk Neutrality with Endogenous Information'. Formal analysis by Avinash Dixit and Robert Pindyck incorporates competition among firms. See Avinash K. Dixit and Robert S. Pindyck, *Investment under Uncertainty* (Princeton: Princeton University Press, 1994).

between the parties will not affect all individuals similarly. For instance, one party could propose to freeze the pay of federal workers, and such a policy would clearly affect public employees more than private ones. Similarly, the Obama administration proposal to reduce the mortgage deduction for higher-income individuals should affect these individuals more than others.

This variation across elections and individuals suggests that the effect of elections on irreversible investment will depend on the policy differences between the major competitors. In particular, as the polarization between the major parties/candidates increase, the incentive to delay irreversible investment until after the election will also increase. We refer to this prediction as the Policy Differences Prediction. It suggests that the larger are the effects that an electoral outcome will have on an individual's financial situation, the more likely she should be to delay costly-to-undo investments. Likewise, the hypothesis implies that as the policy differences between parties/candidates increase, sectors associated with irreversible investment should experience a larger downturn.

Electoral Competitiveness and Irreversible Investment

Elections not only vary in terms of the policy differences between the major parties/candidates but also with respect to the competitiveness of the race. In some elections, a party/candidate holds a convincing lead throughout the campaign, while in other cases the race is highly competitive. As competitiveness increases, individuals and firms will be less able to predict the types of policies that the government will support after the election and consequently, the benefits from delaying costly-to-reverse investments increases. We therefore expect the decline in irreversible investment to be larger the more competitive is the race. We call this prediction the Electoral Competitiveness Prediction.

Notably, if the outcome is highly predictable, then the election may engender so little policy uncertainty that the benefits from postponing irreversible investments may be trivial. In that case, individuals and firms are unlikely to want to bear the costs associated with delay. The Electoral Competitiveness Prediction does not specify a threshold beyond which an election is insufficiently competitive to generate a pre-election decline in irreversible investment, but allows for this possibility. The empirical analysis will assess whether such a threshold exists.

COMPARISON WITH OTHER THEORETICAL PERSPECTIVES

The above-developed theoretical predictions and framework, which we will refer to as the Electoral Investment theory, contrast with other perspectives of how elections influence the economy. It is worth highlighting the differences with three major alternatives: opportunistic political business cycle theories, partisan business cycle theories, and recent work on how mass partisanship influences consumer behavior. Consider the opportunistic perspective, where incumbents engineer a pre-election expansion through fiscal and/or monetary policies.[20] Some scholarship suggests that the cycle will not occur if an incumbent is highly popular, and therefore quite likely to win reelection,[21] or highly unpopular.[22] Other work indicates that the cycle is larger in

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[20] E.g., William Nordhaus, 'The Political Business Cycle', *Review of Economic Studies*, 42 (1975), 169-90; Torsten Persson and Guido Tabellini, *Macroeconomic Policy, Credibility, and Politics* (Chur, Switzerland: Harwood Academic Publishers, 1990); Kenneth Rogoff, 'Equilibrium Political Business Cycles', *American Economic Review*, 80 (1990), 21-36.

[21] Kenneth A. Schultz, 'The Politics of the Political Business Cycle', *British Journal of Political Science*, 25 (1995), 79-99.

countries with higher levels of polarization.[23] Thus in contrast to the predictions of the Electoral Investment theory, the opportunistic perspective suggests that the pre-election period is associated with an expansion that increases with electoral competitiveness and elite polarization.

Partisan theories assume that growth and employment is lower under right-wing governments. In the traditional partisan cycle of Douglas Hibbs, this lower growth does not depend on the electoral cycle.[24] In the rational partisan theory of Alberto Alesina, John Londregan, and Howard Rosenthal, growth varies across the term but there is no pre-election contraction. As they note, in the latter half of a term, "the economy returns to its 'normal' rate of growth, implying that there is not a 'partisan' influence on the rate of growth in the second half of each term."[25] This rate is normal regardless of the level of electoral competitiveness or polarization between the parties.[26]

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[22] Simon Price, 'Comment on 'The Politics of the Political Business Cycle'', *British Journal of Political Science*, 28 (1998), 201-10.

[23] James E. Alt and David Dreyer Lassen, 'Transparency, Political Polarization, and Political Budget Cycles in OECD Countries,' *American Journal of Political Science* 50 (2006), 530-550.

[24] Douglas A. Hibbs Jr., 'Political Parties and Macroeconomic Policy', *American Political Science Review*, 71 (1977), 1467-87.

[25] Alesina, Londregan, and Rosenthal, 'A Model of the Political Economy of the United States', at pp. 13-14.

[26] Larry M. Bartels finds a pre-election decline in personal income growth in the US when Democratic presidents are in office. The analysis of housing markets controls for personal income growth and the survey analysis accounts for personal income. Larry M. Bartels, *Unequal*

Perhaps closest to the predictions of the Electoral Investment theory is recent work by Alan Gerber and Greg Huber about the impact of mass partisanship on consumer behavior.[27] Gerber and Huber find that in the months following an US presidential election, a county's sales tax receipts positively correlate with the county's support for the electoral winner. This effect differs from the Electoral Investment perspective in important respects, however. First, Gerber and Huber do not predict a pre-election decline. Second, they do not distinguish between irreversible investment and other types of purchases. Third, in Gerber and Huber the aggregate change between the pre- and post-election periods decreases with electoral competitiveness. By comparison, the Electoral Competitiveness Prediction suggests that the change should increase as the race becomes tighter.

In sum, existing theoretical perspectives do not predict any sort of pre-election decline in irreversible investment, let alone relate such a decline to electoral competitiveness or polarization between the major parties. The following analyses subject these new predictions to survey and housing market data.

Survey Analysis

We conducted the survey as part of the 2008 CCES, which encompassed a set of nationally representative modules purchased by individual universities/groups. Each

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Democracy: The Political Economy of the New Gilded Age (New York: Russell Sage Foundation, 2008).

[27] Alan S. Gerber and Gregory A. Huber, 'Partisanship and Economic Behavior: Do Partisan Differences in Economic Forecasts Predict Real Economic Behavior?' *American Political Science Review* 103 (2009), 407-426.

module included questions written by that group. In addition, all modules contained a core content of profile items about the respondent's background and questions designed to be of interest to a range of social scientists. The 2008 CCES consisted of a pre- and post-election wave, with 1000 respondents per module in the initial wave.

In the first wave we asked about the extent to which the electoral outcome would affect various aspects of the respondent's financial situation including his or her job security, taxes, and income. The question was worded, "Now I'd like you to consider some different factors that may affect your personal financial situation. For each of these factors, how much difference do you think it will make who wins the upcoming presidential election…A big difference, some difference, only a little difference, or no difference at all?" This four-point scale is commonly employed in questions about financial matters.[28] The specific factors included "job security," "the amount of taxes you pay," and "personal income." In the post-election wave we asked whether the respondent had delayed certain purchases and actions until after the election. The precise question was, "Please tell me whether you put off any of the following things until you knew the results of the November elections…" The possible answers were "yes" or "no."

We solicited this information for three related items, only two of which are canonical irreversible investments. The investment-related items included "buying or

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[28] See, for instance, the Gallup poll "Looking ahead, how much of a difference would this tax cut (you expected per year) make for you and your family--a big difference, some difference, only a little difference, or no difference at all?" Gallup/CNN/USA Today Poll, February 2001. Retrieved from the iPOLL Databank, The Roper Center for Public Opinion Research, University of Connecticut.

selling a home" and "undertaking a home renovation." The quasi-placebo item was "moving." The purpose of the quasi-placebo item was to ensure that any effects uncovered for home transactions or renovations were not due to certain respondents having a tendency to delay all actions until after the election. We chose moving because the activity is correlated with housing investments yet, given the option of renting, still distinct. Research suggests that moving itself is heavily influenced by family events such as marriage, divorce, and childbearing.[29]

If the Policy Differences Prediction is correct, we would expect individuals' likelihood of delaying the irreversible investments to correlate with the perceived policy differences between the candidates. At the same time, we would not expect these policy differences to have a significant effect on the likelihood of moving. To assess whether these relationships hold, we conduct a separate probit regression for each combination of the pre- and post-election items. The dependent variables, *Delay Moving*, *Delay Renovation* and *Delay Home Purchase/Sale*, each equal one if the respondent stated she had delayed the activity until learning the election results, and zero otherwise. The key independent variables are based on the four-point scale regarding how much difference the election would make. Specifically, *Job Security Difference* equals zero if the respondent stated that it made no difference who won the election for her job security, one if "only a little difference," two if "some difference," and three if "a big difference." *Tax Difference* and *Income Difference* are coded similarly. The combination of the pre- and post-election items creates nine separate regressions.

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[29] Alden Speare Jr. and Frances Kobrin Goldscheider, 'Effects of Marital Status Change on Residential Mobility', *Journal of Marriage and the Family*, 49 (1987), 455-464.

These regressions include demographic and political controls to account for the possibility that certain types of individuals were more likely to delay actions until after the election independent of how much the outcome affected them. The control variables, which are from the CCES core content, account for age, gender, marital status, race, political party affiliation, income, homeownership, and number of children under eighteen. The coding is based on the CCES categories and described in Appendix Table A. We also control for respondents' confidence about future income using the CCES common content question "How would you guess your total family income to be six months from now?" The available responses were higher, the same, or lower, which we code as an ordinal three-point scale. Including this variable helps ensure that any effects of the policy difference variables are not a function of respondents' general anxiety about future income; without this control, the results still support the Policy Differences Prediction. Finally, as discussed below, we have also split the sample by certain control variables in an effort to ascertain whether the findings are driven by particular types of respondents.

In each regression we analyze all available survey observations. Almost two hundred of the pre-election CCES respondents did not participate in the post-election wave and even in the pre-election wave, respondents often failed to provide basic profile information. Given these constraints, we received a high proportion of responses. For each combination of pre- and post- items, the response rate was a minimum of 97 percent among those that provided the basic profile information. The number of potential respondents was 697 for the *Tax Difference* question, which was asked of everyone, and 385 for the *Job Security Difference* and *Income Difference* questions, which were asked

of all those that reported working full- or part-time according to the profile item on employment. Appendix Table A describes this profile item.

As is standard, the CCES survey weights are used in the analysis of the data. The purpose of survey weights is to minimize any sample-related bias. For instance, if the proportion of retirees that participate is lower than expected, then the retirees that participated receive a higher weight. The CCES target population is the national US adult population, and the CCES employed the 2006 American Community Survey in designing the weights. Further details on the weighting are given in the 2008 CCES guide.[30]

Figure 1 depicts the distribution of the weighted responses for each of the policy difference questions.[31]

[Figure 1 about here]

Notably, each item produced a different distribution of responses. Only seven percent of respondents believed that the election would make no difference with respect to their taxes, eleven percent that it would make little difference, thirty-two percent some difference, and fifty percent that it would make a big difference. By contrast, twenty-seven percent of respondents believed the election would have no effect on their job security, close to the same percentage that believed the election would have some (31%) or a big effect (28%). For personal income, nineteen percent believed the election would

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[30] Ansolabehere, 'Guide to the 2008 Congressional Cooperative Election Study'.

[31] The weighting does not significantly alter the distribution of respondents in each category. The largest change is a four percentage point decrease in the percent of individuals who perceive the election to make some difference in their income; in all other categories, the weighting causes less than a three percentage point shift.

have no effect, seventeen percent a little effect, thirty-six percent some effect, and twenty-eight percent a big effect.

The post-election responses about the likelihood of delay are more stable. Fourteen percent of individuals reported that they had delayed purchasing or selling a home, sixteen percent had delayed home renovations, and thirteen percent a move. Despite this stability, however, the correlation between the items is far from perfect. For instance, the correlation between postponing renovations and moving is $\rho=0.5$ and between a home sale/purchase and renovations $\rho=0.6$. Of the ninety-four respondents who professed that they had delayed buying or selling a home until after the election, twenty-six percent of them did not delay moving.

The critical question is whether such variation relates to the variation in how much respondents perceived the election would affect their personal finances. Table 1 presents the key results for each probit regression. For space reasons, parameter estimates for the control variables are provided in Appendix Table B.

[Table 1 about here]

The analysis strongly supports the Policy Differences Prediction. As the rows of results regarding home transactions and renovations show, the greater is the effect of the election on a respondent's financial circumstances, the more likely she is to postpone these irreversible investments until after the election. Moreover, in each case but one this effect is significant at $p<0.05$, two-tailed, and even the exception is significant at $p<0.1$, two-tailed. By contrast, the effect of the quasi-placebo item, moving, is never significant by any conventional standard.

The magnitudes of the estimates further suggest that the incentive to delay costly-to-undo investments relates to candidates' policy differences. Table 2 presents the marginal effects for every combination of pre- and post-item. These effects are calculated at each level of policy difference on the four-point scale—i.e., no difference (0), only a little difference (1), some difference (2), and a big difference (3)—holding all control variables at their means.

[Table 2 about here]

For each of the irreversible investments, the marginal effect is larger for higher values of the policy difference scale.

Consider the first row of results, which concern home transactions and how much the electoral outcome affects job security. A shift from perceiving that the election makes no difference to a little difference increases the likelihood of delay by three percent, while a shift from perceiving some difference to a big difference increases the likelihood by five and one-half percent. For renovations, the analogous changes increase the probability of delay by three and one-half percent and six and one-half percent, respectively. These marginal effects are even more substantial when one considers larger changes. For instance, a shift from perceiving a big effect on taxes to perceiving no effect reduces the likelihood of delaying a home sale/purchase by fourteen percent (0.047 x 3). By comparison, the marginal effects on the quasi-placebo item of moving are substantially smaller in magnitude (as well as statistically insignificant).

The results for the control variables, reported in Appendix Table B, suggest that perceived policy differences were not the only factors that influenced respondents' decisions. Blacks were more likely than others to delay home purchases/sales, women

less likely than men to postpone moves, and homeowners more likely than others to delay renovations. Also, there is some evidence that women were less likely than men to postpone home purchases and higher income individuals less likely to delay moves.

We separately investigated whether the results were driven by certain subgroups of the population, including party members and homeowners. Partisans may be more emotionally invested in the election, and therefore more likely to delay actions and/or believe it will have large policy implications. Party members are defined as those who identify as Democrats or Republicans in the coding for the control variable regarding party affiliation. If we add to the regressions an interaction term between party membership and the perceived policy difference plus a main effect for party membership, the coefficient on the interaction term is never significant ($p>0.3$, two-tailed). If instead we run separate regressions for party members and unaffiliated respondents, party membership appears to matter in the expected direction for the renovations-job regressions, but in the opposite direction for the renovations-tax analysis.[32] Even more basic t-tests suggest that the responses do not differ significantly according to party membership. Overall, the evidence suggests that support for the Policy Differences Prediction does not derive solely from partisans.

One could envision that effects would vary according to homeownership. Homeowners could be more affected if they are concerned about the possibility of

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[32] For the renovations-tax policy analysis, the coefficient and standard error were 0.124(0.083) for party members and 0.236(0.140) for independents. For the renovations-job security analysis, the coefficient and standard error were 0.272(0.096) for party members and 0.116(0.139) for independents.

holding two mortgages, or alternatively, less affected if they are buying and selling simultaneously. In nested tests, there is no evidence of a difference between homeowners and others; if one adds an interaction term between homeownership and the policy difference variable the hypothesized effects are never significantly larger for homeowners, with or without interactions between homeownership and the controls. However, if instead one splits the sample and runs separate (non-nested) regressions, the hypothesized effect of perceived policy differences on irreversible investment is always significant with the homeowner sample ($p<0.05$, two-tailed) and never significant at conventional levels with the non-homeowner sample. These results change somewhat if we consider non-homeowners with incomes of at least $40,000. The hypothesized effects then hold at conventional significance levels ($p<0.05$, two-tailed) in analyses of tax policy differences. The supplemental tests thus suggest that homeowners may be more affected than others, although the null results from the nested tests indicate that one cannot make strong conclusions about this issue from these data.

In sum, the survey analysis provides strong support for the Policy Differences Prediction. The larger the impact the individual perceived the election to have on her personal financial situation, the more likely she was to delay the irreversible investments of homes or renovations until after the election. No such effect occurred for the quasi-placebo item. While the results are highly supportive of the theory, one could argue that the campaign season of 2008 was special in many ways: the housing market was in turmoil, this turmoil lead to a collapse of financial markets, and the presidential election involved the unexpected rise of Barack Obama. None of these events, of course, implies that one would necessarily find the propensity to delay irreversible investments to be

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correlated with respondents' perceptions of policy differences between the candidates.

Moreover, the analysis of the quasi-placebo item suggests that the events did not cause

respondents who perceived larger policy differences to delay all decisions. Still, we

recognize that any given election may have qualities that make it uncharacteristic. For

this reason, we proceed to examine data that involve housing markets across scores of

elections.

Housing Market Analysis

The examination of housing market data is advantageous for several reasons.

Perhaps most obviously, it offers a straightforward examination of market activity; if

people are indeed delaying purchases until after an election, then the sales records should

reveal this pattern. Furthermore, substantial regional variation exists in US housing

markets,[33] and a variety of policies enacted by US state governments affect individuals'

incentives with respect to housing purchases. Accordingly, we can assemble a dataset

with cross-sectional as well as over-time variation.

Like the national government, US state governments may influence an individual's

taxes and job security. For example, state-level income taxes are as high as eleven

percent and state governments directly employ almost four million individuals.

Moreover, these governments enact many laws that explicitly deal with real estate. As

mentioned previously, states can limit the ability of localities to raise property taxes and

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[33] E.g., Alan K. Reichert, 'The Impact of Interest Rates, Income, and Employment upon Regional

Housing Prices', *Journal of Real Estate Finance and Economics*, 3 (1990), 373-391.

provide property tax relief to groups of homeowners.[34] State governments also influence

housing markets through policies that attract businesses to particular locations. For

instance, when a state creates or expands tax-free zones for businesses, then the tax-free

areas become more desirable.[35] Likewise, state governments offer tax credits to firms to

encourage them to move to the state, and such relocations affect local housing markets.[36]

More generally, as Michael Ebeid and Jonathan Rodden note, US state governments have

"wide-ranging control over fiscal and regulatory policy."[37]

Thus if the Electoral Investment theory is correct, we should observe that

gubernatorial elections affect state and local housing markets. An advantage of focusing

on gubernatorial elections is that we can exploit the sub-national variation in these

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[34] E.g., Albanese, 'Texas' Perry Signs School Finance Bill that Cuts Maximum Property Tax

Rate'; 'Governor Rendell: Record 588,638 Pennsylvanians Received Property Tax/Rent

Rebates'.

[35] Mike Hughlett, 'Job Zones Unveiled: State Designates 325 Communities Eligible for Business

Subsidy', *Saint Paul Pioneer Press*, 19 December 2003, p. C1.

[36] David Brunori, 'Principles of Tax and Targeted Tax Incentives', *State & Local Government

Review*, 29 (1997), 50-61.

[37] Michael Ebeid and Jonathan Rodden, "Economic Geography and Economic Voting: Evidence

from the US States", *British Journal of Political Science*, 36 (2006), 527-547, at p. 530.

Additionally, states can enact education policies that influence home values. See, e.g., Lori L.

Taylor, 'Revealed-Preference Measures of School Quality', in Leanna Stiefel, Amy Ellen

Schwartz, Ross Rubenstein, and Jeffrey Zabel, eds, *Measuring School Performance and

Efficiency: Implications for Practice and Research, 2005 Yearbook of the American Education

Finance Association* (Larchmont, NY: Eye on Education Publishing, 2005), pp. 163-85.

electoral cycles.[38] Gubernatorial elections are staggered such that in every year multiple states have an election. The analysis can therefore control for national influences with year indicators, and assess whether the states with gubernatorial elections in a given year have weaker housing markets than other states. In addition, of course, the tests will account for local variation in the economy, demographics, and other factors.

Two measures of housing market performance are examined. First, Zillow.com offers data on home sales, as a percentage of all homes, for thirty-five states from 1999-2006. These data allow us to analyze seventy-three gubernatorial elections.[39] Second, as a supplementary analysis, we make use of the Conventional Mortgage Home Price Index (CMHPI), which is published by the Federal Home Loan Mortgage Corporation (Freddie Mac) and encompasses over three hundred metropolitan statistical areas (MSAs).[40] For some MSAs, the index extends back as far as 1975. Due to the more limited time span of

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[38] By analyzing gubernatorial elections, we do not mean to suggest that local elections are necessarily irrelevant. However, Ferreira Fernando and Joseph Gyourko find that partisan differences in mayors do not significantly affect policies. Moreover, in the US many local elections are nonpartisan. Finally, the housing data do not correspond to the political boundaries for local elections. See Ferreira Fernando and Joseph Gyourko, 'Do Political Parties Matter? Evidence from U.S. Cities', *Quarterly Journal of Economics*, 124 (2009), 399-422.

[39] These states include AL, AZ, AR, CA, CO, CT, DE, FL, GA, HI, IL, IA, KY, MA, MD, MA, MI, MN, NE, NV, NH, NJ, NY, NC, OH, OK, OR, PA, RI, SC, VT, VA, WA, WV, and WI. The data for Vermont span only four years.

[40] A metropolitan statistical area is a population center as defined by the US Office of Management and Budget. MSAs typically encompass multiple counties and/or towns.

some of the control variables, the CMHPI is examined from 1979-2006 for 338 MSAs. These data encompass 339 separate gubernatorial elections.

The measure of sales clearly offers a more direct test of the theoretical predictions. However, research on housing markets suggests that new home construction is strongly persistent and correspondingly, that supply responds more slowly than demand does to external dynamics.[41] If one takes these stylized facts as given, then demand should respond more than supply to the political uncertainty engendered by an upcoming election. We therefore analyze price data in addition to the sales data, but focus on the analysis of sales given that it does not depend on additional stylized facts and assumptions.

For the data on sales, we test the Pre-election Decline and Electoral Competitiveness Predictions with the following general specifications, respectively, for each state i and year t,

[1]   %Homes Sold$_{it}$ = f(Gubernatorial Election Year$_{it}$, Sub-National Controls$_{it}$, Year Indicators$_t$, η_i)

[2]   %Homes Sold$_{it}$ = f(Gubernatorial Election Year$_{it}$ × Competitive$_{it}$, Gubernatorial Election Year$_{it}$ × Not Competitive$_{it,}$, Competitive$_{it}$, Sub-National Controls$_{it}$, Year Indicators$_t$, η_i),

where η_i represent state fixed effects. The general specifications for home prices are analogous for each MSA m and year t except that some controls are available at the

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[41] Edward Glaeser and Joseph Gyourko, 'Housing Dynamics', NBER Working Paper 12787 (2006).

MSA-level while others only at the state-level. The following descriptions clarify the level at which each control is available.

%Homes Sold. Zillow.com calculates the percentage of single-family homes that sold within a given state and year out of all single-family homes in that state, whether they were explicitly on the market or not. *%Homes Sold* equals this value, which captures turnover in the single-family housing market. Importantly, the measure is based on both new construction and "existing" homes. Obtaining an accurate estimate of total home sales is important for inter-state comparisons because new homes are a more significant portion of some markets than others. Also beneficial is the fact that the Zillow.com data are based on actual sales, not pending contracts that may ultimately fall through. By comparison, the US government collects data on sales of new homes versus existing homes separately, and the series are not compatible. The series on new homes is not even available at the state- or local levels. Moreover, it is based on pending contracts while the existing home sales series reflects actual purchases.[42]

%Change in Real Home Prices. The Conventional Mortgage Home Price Index (CMHPI) is constructed from over thirty million "repeat transactions." A particular home's value must be observed at least twice in order for that home to contribute to the price index.

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[42] The government collects data on new home sales as part of the Survey of Construction, while the National Association of Realtors provides the data on existing home sales.

This feature and the breadth of the data make the CMHPI attractive to scholars.[43] The

index encompasses not only high profile metropolitan statistical areas such as Boston-

Quincy, Massachusetts but also lower profile areas such as Altoona, Pennsylvania.[44] The

index is based on conventional, conforming mortgages for single unit residency houses

purchased or securitized by the Federal Home Loan Mortgage Corporation (Freddie Mac)

or the Federal National Mortgage Association (Fannie Mae). All data are converted to

2008 dollars for purposes of analyzing real prices; the results are robust to analyzing

nominal prices. Due to the substantial variation in price across localities, we follow the

standard practice of analyzing percentage change rather than the absolute level, although

the substantive results are robust to using the level.[45]

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[43] E.g., William Stephens, Ying Li, Vassilis Lekkas, Jesse Abraham, Charles Calhoun, and

Thomas Kimner, 'Conventional Mortgage Home Price Index', *Journal of Housing Research*, 6

(1995), 389-418.

[44] We have also analyzed the almost identical index published by the Office of Housing

Enterprise Oversight (OFHEO), and the results are similar. We use the CMHPI because it has

larger coverage for some MSAs.

[45] A working paper by Justin Wolfers argues that home prices reflect voters' future economic

expectations. We considered potential implications of this interpretation of home prices,

particularly the possibility that the findings could be driven by bad economic conditions that

cause voters to have weak economic expectations. Accordingly, we examined whether the

findings held when personal income growth was higher than the median level. The results were

robust to examining this subsample. For instance, the coefficient and standard error on

Gubernatorial Election Year are -0.354(0.181) for the fixed effects estimation of Equation [1].

Justin Wolfers, 'Are Voters Rational? Evidence from Gubernatorial Elections' (Wharton

Gubernatorial Election Year. This key independent variable equals one if the state has a gubernatorial election that year and zero otherwise.

Electoral Competitiveness. We measure electoral competitiveness as a function of the winning gubernatorial candidate's share of the two-party vote.[46] For the data on home sales, the median value of this share is fifty-five percent and for prices it is fifty-six percent. To minimize collinearity among interaction terms, we categorize races as either competitive or uncompetitive. Specifically, *Competitive* equals one if the winner receives less than fifty-five percent of the two-party vote and zero otherwise. Likewise, *Not Competitive* equals one if the winner receives at least fifty-five percent of the vote. We have experimented with alternative cutpoints, and the results are robust to alternatives.[47]

Real Income Growth. Various studies suggest that home prices increase in response to growth in personal income.[48] To account for this influence, we use the US Bureau of

Business School Typescript, 2007), available at

http://bpp.wharton.upenn.edu/jwolfers/Papers/Voterrationality%28latest%29.pdf (accessed December 5, 2011).

[46] Generally the two-party vote concerns Democratic and Republican candidates. However, the percentage is based on the two candidates with the largest vote shares even in the unusual case where one of them is a third-party candidate.

[47] For instance, if we increase the cutpoint to six or seven percentage points, all of the major results hold.

[48] E.g., Karl E. Case and Robert J. Shiller, 'Is There a Bubble in the Housing Market?', *Brookings Papers on Economic Activity* 2003 (2003), 299-342; James M. Poterba, 'House Price Dynamics:

Economic Analysis personal income data. These data are available at the state- and MSA-levels, and each test uses the level consistent with that of the dependent variable (states for home sales and MSAs for home prices). In each case, the variable equals the annual percentage change in real income.[49]

Change in Unemployment. Some research suggests that increases in unemployment may harm housing markets.[50] Using data from the US Bureau of Labor Statistics, we control for this possibility. Because unemployment data are available by state for the entire time period while by MSA only since 1990, all analyses employ the annual percentage change in state-level unemployment.

Demographic Demand. The tests control for the impact of demographic change with Census Bureau data on population. For most MSAs, population data are not available on an annual basis and therefore all analyses employ state-level data. Mankiw and Weil recommend weights for each age group to account for the fact that demand for housing is not constant across the life cycle, and we use these weights.[51]

The Role of Tax Policy and Demography', *Brookings Papers on Economic Activity*, 1991 (1991), 143-203.

[49] Using the absolute level of real income rather than the percentage change does not alter the main findings but does reduce the effect of income on housing market performance.

[50] E.g., Case and Shiller, 'Is There a Bubble in the Housing Market?'.

[51] Gregory N. Mankiw and David N. Weil, 'The Baby Boom, the Baby Bust, and the Housing Market', *Regional Science and Urban Economics*, 19 (1989), 235-58.

Lagged Foreclosure Rate. The National Delinquency Survey published by the Mortgage Bankers Association records the percentage of loans in foreclosure for each state and year beginning in 1979. In many states, banks must give the previous owners of the foreclosed home a redemption period of up to a year before selling the property. We therefore control for the foreclosure rate of year *t-1* when analyzing housing market performance in year *t*. As the media recognizes, at times foreclosures constitute a substantial proportion of sales. For instance, a 2009 *Wall Street Journal* piece reported that "existing-home sales jumped 5.1%...driven by foreclosure sales that are sending prices plunging."[52] Thus we should expect foreclosures to increase home sales. However, as the quote attests, foreclosures reduce prices. We accordingly expect a negative relationship between foreclosures and prices.

Year Indicators. A set of year indicators accounts for national-level factors such as mortgage/interest rates and the national economy. These indicators ensure that any effects associated with gubernatorial elections are not an artifact of certain states happening to hold elections in particular years.

A consistent finding in the real estate literature is short-term persistence in housing market performance.[53] The most straightforward way to handle short-term persistence is to include lagged dependent variables. While the inclusion of lags will

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[52] Kelly E. Grace, 'US Foreclosures Jumped in March', *Wall Street Journal Online*, 15 April 2009.

[53] Some research argues that this persistence results from social contagion. See Robert Shiller, *Irrational Exuberance*, 2nd edn (Princeton: Princeton University Press, 1991).

cause the coefficients to estimate short-term effects, this is consistent with the fact that the hypothesized effects are limited to the pre-election period. A more problematic issue is the potential for so-called Nickell-bias, which refers to the bias and inconsistency generated by estimating panel data with fixed effects and a set of lagged dependent variables.[54] While the inconsistency disappears as the number of time periods increases, the home sales data do not encompass a particularly large number of time periods.

A standard practice in such situations is to estimate a range of models, including not only the basic fixed effects and ordinary least squares (OLS) estimators but also difference- and system-generalized methods of moments (GMM). The latter two estimators are designed for the analysis of short, wide panel data sets when both fixed effects and lagged dependent variables are desired. Difference-GMM, which is also known as the Arellano-Bond estimator, eliminates the fixed effects by first-differencing the original equation. Then to account for any endogeneity of the first-differenced lagged dependent variables, they are instrumented with their own past levels, lagged two or more periods (as well as the first-differenced exogenous variables).[55] The system-GMM estimator, which was developed by Arellano and Olympia Bover and by Richard Blundell and Bond, builds on difference-GMM by additionally estimating the original equation in levels, with the additional equation instrumented by lagged first-differences

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[54] Stephen Nickell, 'Biases in Dynamic Models with Fixed Effects', *Econometrica*, 49 (1981), 1417-26.

[55] Manuel Arellano and Stephen Bond, 'Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations', *Review of Economic Studies* 58 (1991), 277-97.

of the endogenous variables.[56] Because the system-GMM estimator is associated with more instruments, the potential for overidentification is higher than with difference-GMM.[57] As a part of the analysis, we will assess the validity of the overidentifying restrictions. One concern with difference-GMM is that it is relatively inefficient, and assuming that specification tests support the use of system-GMM, it is superior to difference-GMM on the grounds of efficiency and finite sample bias.

Across the various specifications, analysis of the home sales data suggests that the first and second lags of the dependent variable have significant effects while additional lags do not; we accordingly estimate models that include two lags. We begin by presenting the results from OLS with standard errors clustered by state (and excluding the fixed effects), fixed effects with robust standard errors, and difference-GMM with robust standard errors. The difference-GMM results are from one-step estimation and the instruments for the endogenous variables include the second through fifth lags of *%Homes Sold*. Also, specification testing suggests that the economic controls for income and unemployment are predetermined. Consistent with difference-GMM procedures for predetermined variables, they are instrumented with their first lags.[58] If income and

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[56] Manuel Arellano and Olympia Bover, 'Another Look at the Instrumental Variables Estimation of Error Components Models', *Journal of Econometrics*, 68 (1995), 29-51; Richard Blundell and Stephen Bond, 'Initial Conditions and Moment Restrictions in Dynamic Panel Data Models', *Journal of Econometrics*, 87 (1998), 115-143.

[57] David Roodman, 'A Note on the Theme of Too Many Instruments', *Oxford Bulletin of Economics and Statistics,* 71 (2009), 135-158.

[58] If the income and unemployment variables are considered exogenous, then the Difference-in-Hansen test for the exogenous instruments suggests that one can reject at p=0.03 the null that they

unemployment are considered exogenous, the substantive results are identical but specification tests suggest that the model is overidentified; Appendix Table C shows these details.

Table 3 presents the main findings.

[Table 3 about here]

Columns [1], [2], and [3] concern the Pre-election Decline Prediction and Columns [4], [5], and [6] the Electoral Competitiveness Prediction. The estimates in the first three columns suggest that the average effect of a gubernatorial election is to depress home sales significantly in the pre-election period. In all of the specifications, the coefficient on *Gubernatorial Election Year* is negative and significant at p<0.05, two-tailed.

The magnitude of the effect is remarkably similar across the different estimators. In each case, the percentage of homes sold (out of all homes in the area, regardless of whether they are explicitly on the market) drops between two and three-tenths of a percentage point. This impact is comparable to that from a standard deviation decrease in per capita income growth, which is 2.471 percentage points; such a change also produces a decline of two to three-tenths of a percentage point in the percentage of homes sold (e.g., in Column [1], $0.106 \times 2.471 = 0.262$). Thus the size of the electoral effect is similar to that of other, well-established influences on housing markets.

Columns [4] through [6] indicate that this impact is driven by competitive elections. The coefficients on *Competitive × Gubernatorial Election Year* suggest that the percentage of homes sold, out of all homes in the state, drops between a third and a half

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are exogenous. Once income and unemployment are considered predetermined, the p-value for this test increases to p=0.76.

of a percentage point when the race is competitive. By comparison, the estimates on *Not Competitive × Gubernatorial Election Year* suggest a decline of one-tenth of a percentage point when one candidate leads by more than five percentage points of the vote. In addition, the estimates for the competitive elections are significant at $p<0.05$, two-tailed, while those for the uncompetitive elections are not significant.

In each of the difference-GMM analyses in Table 3, the Hansen J-statistic indicates that the model is not overidentified. David Roodman argues that p-values below or even as great as 0.25 "should be viewed with concern" while ones equal to 1.00 are a "classic sign" that instrument proliferation has vitiated the capacity of the test to detect an overidentified model.[59] Neither Hansen J-statistic in Table 3 suggests cause for concern. Also, the tests for serial correlation support the models. First-order serial correlation is expected in difference-GMM because the first-differencing creates first-order serial correlation even when the errors in the original equation are independently and identically distributed. Second-order serial correlation should not be present, however, and as Table 3 shows, there is no evidence of it. We have also analyzed models that restrict the instruments for the endogenous (lagged dependent) variables to the second through fourth lags and, separately, the third through fifth lags. In each case, specification testing supports the model and the theoretical predictions are substantiated at conventional significance levels.[60]

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[59] Roodman, 'A Note on the Theme of Too Many Instruments', p. 142 and p. 151.

[60] The coefficients are similar in magnitude to those from those difference-GMM in Table 3, and are significant at $p<0.5$, two-tailed, except in one case where the significance level is $p=0.07$, two-tailed.

Appendix Table C indicates that the substantive findings are supported by system-GMM although specification tests reject these models. In Columns [3] and [4], which show the results from system-GMM applied to the same instrument structure adopted for difference-GMM, the Hansen J-Statistics and Difference-in-Hansen tests suggest an overidentified model. Columns [5] and [6] report results from assuming the economic controls are exogenous. While overidentification is not a problem in these models, the exogeneity of the economic controls is rejected at p=0.025 for the specification of Column [5] and p=0.096 for that of Column [6]. We additionally analyzed a battery of system-GMM models that reduced the instrument count yet considered the economic controls predetermined; the results of these alternative models were similar to those in Columns [3] and [4] in that the substantive results held but the Hansen tests suggested the models were overidentified.[61]

Across all of the models in Appendix Table C and Table 3, the results for the control variables present few surprises. Home sales increase with real income. Also, they are positively associated with foreclosures; after a property forecloses, banks tend to sell it (at substantially reduced prices). The estimates for unemployment and demographics are not statistically significant. The literature on housing markets has generally focused on home prices rather than sales, and it may be that these factors affect the former but not the latter. Moreover, some previous research suggests that demographics do not have a significant influence.[62]

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[61] We tried reducing the lag structure so that it included only the second or the third lag, and even collapsed this lag, but the Hansen tests still suggested that the models were overidentifed.

[62] E.g., Poterba, 'House Price Dynamics: The Role of Tax Policy and Demography'.

In sum, the analysis of home sales provides substantial support for the Pre-election Decline and Electoral Competitiveness Predictions. Even accounting for a variety of economic and other determinants of housing markets, housing market activity is associated with a pre-election decline. Moreover, this overall effect appears to be driven by the competitive elections.

Table 4 shows that these results extend to home prices. As with the analysis of home sales, we begin by focusing on the results from the OLS, fixed effects, and difference-GMM models.

[Table 4 about here]

Columns [1], [2], and [3] estimate the average impact of a gubernatorial election, and in all models the coefficient on *Gubernatorial Election Year* is significant ($p<0.05$, two-tailed). In the year before an election, average home prices decline. Moreover, the size of the impact is relatively consistent across the disparate specifications. In each, prices fall by approximately a third of a percentage point in that year.

As with the home sales data, this effect appears to be driven largely by the competitive elections. Columns [4], [5], and [6] suggest that when the candidates are within five percentage points of each other, home prices drop nine-tenths of a percentage point ($p<0.05$, two-tailed). By comparison, when the election is uncompetitive, there is not a substantial drop in home prices. The coefficient on *Gubernatorial Election Year × Not Competitive* is even positive in some of the specifications, although in these cases it is small and not at all statistically significant.

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The results for the other coefficients in Table 4 largely agree with expectations. Across all of the models, higher home prices are associated with increases in real income and decreases in unemployment. Also, in the OLS and fixed effects specifications, the effects of foreclosures and demographic demand are in the expected direction and statistically significant. In the GMM models, these two control variables are not in the expected direction but the effects are not at all significant.

The price data involve a substantially longer time series than the home sales data, and thus for the difference-GMM models a more restrictive instrument set is required to avoid overidentification. The instruments for the endogenous variables include the third through fifth lags, and these instruments are collapsed as described by Thorsten Beck and Ross Levine and by Roodman.[63] Specification testing suggests that the economic controls are not predetermined in these models and therefore the variables are considered exogenous; however, if instead they are considered predetermined and the instruments include the first lag collapsed, the key coefficients remain significant at conventional levels, the magnitudes of these coefficients are similar, and the Hansen J-statistics suggest that the models are not overidentified. The results are also robust to alternative sets of instruments for the lagged dependent variables, including the collapsed third and

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[63] Thorsten Beck and Ross Levine, 'Stock Markets, Banks, and Growth: Panel Evidence', *Journal of Banking and Finance*, 28 (3), 423–42; Roodman, 'A Note on the Theme of Too Many Instruments'. If we reduce the number of lags used as instruments but do not collapse them, the models are still overidentified.

fourth lags or the collapsed second through fourth lags.[64] Finally, we assessed the appropriateness of system-GMM with the home price data. Under any specification attempted, including ones that severely restricted the number of instruments, the substantive results held yet the Hansen tests suggested the models were overidentified.[65]

In sum, the findings on home prices corroborate those on home sales. With each data set, the Pre-Election Decline and Electoral Competitiveness Predictions receive support. On average the pre-election period is associated with a decline in home sales and prices. Furthermore, when the effects are analyzed separately for competitive versus uncompetitive elections, it becomes clear that the average effect is driven by the competitive races. The housing market analysis thus provides substantial support for the Electoral Investment theory.

Conclusion

This paper has argued that sectors of irreversible investments experience a pre-election decline due to the policy uncertainty generated by elections. Moreover, because policy uncertainty is associated with the competitiveness of the race and the policy differences between candidates/parties, the pre-election decline depends on these factors. The paper has conducted a variety of tests of the predictions, including analyses of individual-level survey data and aggregate housing market data. The results have provided substantial support for the predictions.

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[64] The results on the key coefficients are also robust to increasing the number of lags used as instruments, but the Hansen tests recommend against these models.

[65] For instance, if the only instrument is the collapsed second or third lag, then the Hansen J-statistic and Difference-in-Hansen test suggest the model is overidentified.

Additionally, the findings offer a possible reason that studies of opportunistic political business cycles have generally uncovered little evidence that economic growth within OECD countries follows such cycles.[66] In particular, this paper suggests that sectors of irreversible investment experience a decline at precisely the time that opportunistic theories predict governments are simulating short-term expansions. Accordingly, future research might investigate whether theories of opportunistic cycles receive greater support if irreversible investment is removed from output.

The findings also provoke other suggestions for future research. Most naturally, studies might examine whether the patterns observed in this paper extend to business investment or consumer durables such as automobiles. Separately, future work should analyze whether the relationship among irreversible investment, elections, and uncertainty extends to countries other than the United States. The endogenous nature of

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[66] For a discussion of the lack of evidence for opportunistic cycles in economic growth in OECD countries, see Franzese, 'Electoral and Partisan Cycles in Economic Policies and Outcomes'. Although cf. for the United States as an exception: Stephen E. Haynes and Joe A. Stone, 'Political Parties and the Variable Duration of Business Cycles', *Southern Economic Journal*, 60 (1994), 869-85; George A. Krause, 'Electoral Incentives, Political Business Cycles and Macroeconomic Performance: Empirical Evidence from Post-War US Personal Income Growth', *British Journal of Political Science*, 35 (2005), 77-101. The opportunistic model also receives support in analyses of developing countries. See, e.g., Lawrence Sáez and Aseema Sinha, 'Political Cycles, Political Institutions, and Public Expenditure in India, 1980-2000', *British Journal of Political Science*, 40 (2009), 91-113; Daniel Treisman and Vladimir Gimpelson, 'Political Business Cycles and Russian Elections, or the Manipulations of 'Chuda' ', *British Journal of Political Science*, 31 (2001), 225-46.

elections in many nations would necessitate additional theorizing about the precise

relationship between the timing of elections and economic performance, but the

arguments are broadly relevant to democratic nations.

Figure 1. Survey Responses, Perceived Policy Differences
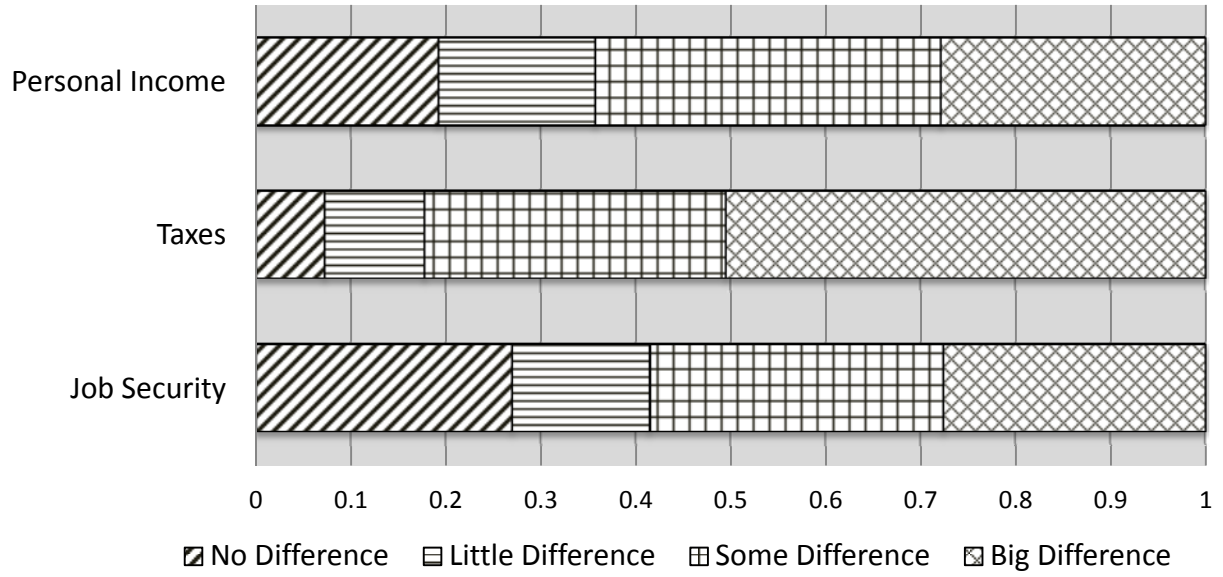
Table 1. Test of the Policy Differences Prediction, Survey Analysis

Post-election: … put off until you knew the results of the November elections?	Pre-election: …difference it makes who wins the upcoming presidential election for your…		
	Job Security	**Taxes**	**Income**
Investment			
Home Purchase/Sale	*0.195*	*0.191*	*0.170*
	(0.079)	*(0.079)*	*(0.084)*
	N=379	N=679	N=380
Renovation	*0.219*	*0.169*	0.142
	(0.076)	*(0.075)*	(0.082)
	N=379	N=681	N=380
Non-investment			
Moving	-0.029	0.065	0.025
	(0.087)	(0.077)	(0.104)
	N=379	N=675	N=380

Notes: Probit coefficients given above standard errors. Controls include age, gender, race, marital status, party affiliation, income, number of children under eighteen, homeownership, and future income confidence. Results for the control variables are detailed in Appendix Table B. Parameter estimates in bold are significant at $p<0.1$, two-tailed, and estimates in bold italics are significant at $p<0.05$, two-tailed.

Table 2. Marginal Effects, Survey Analysis of Policy Differences Prediction

	Starting Value			
Regression	0	1	2	3
Home purchase/sale-Job Security	*0.030*	*0.038*	*0.046*	*0.054*
	(0.008)	*(0.014)*	*(0.020)*	*(0.026)*
Home purchase/sale-Taxes	*0.024*	*0.031*	*0.039*	*0.047*
	(0.005)	*(0.009)*	*(0.015)*	*(0.022)*
Home purchase/sale-Income	*0.027*	*0.033*	*0.039*	0.045
	(0.009)	*(0.014)*	*(0.020)*	(0.026)
Renovations-Job Security	*0.034*	*0.044*	*0.055*	*0.065*
	(0.008)	*(0.014)*	*(0.020)*	*(0.026)*
Renovations-Taxes	*0.026*	*0.031*	*0.038*	*0.044*
	(0.006)	*(0.011)*	*(0.016)*	*(0.021)*
Renovations-Income	*0.026*	*0.030*	**0.034**	0.039
	(0.011)	*(0.016)*	**(0.021)**	(0.025)
Moving-Job Security	-0.006	-0.006	-0.005	-0.005
	(0.018)	(0.017)	(0.016)	(0.015)
Moving-Taxes	0.011	0.012	0.013	0.014
	(0.011)	(0.013)	(0.015)	(0.017)
Moving-Income	0.005	0.005	0.005	0.005
	(0.018)	(0.019)	(0.020)	(0.021)

Notes: Marginal effects given above standard errors. Estimates in bold are significant at p<0.1, two-tailed, and estimates in bold italics are significant at p<0.05, two-tailed.

Table 3. Analysis of Pre-election Decline and Electoral Competitiveness Predictions, Home Sales Data

	OLS [1]	Fixed Effects [2]	Difference GMM [3]	OLS [4]	Fixed Effects [5]	Difference GMM [6]
Gubernatorial Election Year	*-0.215* *(0.098)*	*-0.213* *(0.099)*	*-0.241* *(0.114)*	--	--	--
Gubernatorial Election Year × Competitive	--	--	--	*-0.456* *(0.140)*	*-0.351* *(0.127)*	*-0.368* *(0.141)*
Gubernatorial Election Year × Not Competitive	--	--	--	-0.006 (0.176)	-0.095 (0.154)	-0.143 (0.160)
Competitive	--	--	--	**0.185** **(0.105)**	0.116 (0.148)	0.264 (0.163)
Real Income Growth	*0.106* *(0.041)*	*0.114* *(0.044)*	**0.098** **(0.052)**	*0.107* *(0.040)*	*0.113* *(0.043)*	**0.942** **(0.050)**
Change in Unemployment	0.217 (0.620)	0.215 (0.667)	0.109 (0.793)	0.207 (0.642)	0.174 (0.689)	0.098 (0.852)
Demographic Demand	-0.007 (0.009)	0.114 (0.478)	0.328 (0.551)	-0.007 (0.010)	0.089 (0.468)	0.263 (0.585)
Foreclosure Rate$_{(t-1)}$	*0.254* *(0.068)*	*0.392* *(0.130)*	*0.402* *(0.161)*	*0.260* *(0.071)*	*0.383* *(0.132)*	*0.402* *(0.166)*
%Homes Sold$_{(t-1)}$	*1.166* *(0.114)*	*0.875* *(0.126)*	*0.880* *(0.253)*	*1.159* *(0.117)*	*0.874* *(0.128)*	*0.867* *(0.250)*
%Homes Sold$_{(t-2)}$	-0.206 (0.134)	*-0.400* *(0.166)*	*-0.410* *(0.182)*	-0.201 (0.137)	*-0.393* *(0.176)*	*-0.406* *(0.185)*
Constant	*-0.638* *(0.319)*	1.663 (1.738)	--	*-1.249* *(0.297)*	1.329 (1.794)	--
Year Indicators	Yes	Yes	Yes	Yes	Yes	Yes
N	206	206	171	206	206	171
AR(1)	--	--	0.014	--	--	0.018
AR(2)	--	--	0.369	--	--	0.301
Hansen J Test (p-value)	--	--	0.601	--	--	0.637
R^2	0.92	0.81	--	0.92	0.83	--

Notes: Coefficients given above standard errors. Dependent variable equals %Homes Sold. In difference-GMM, the instruments for the lagged dependent variables include their second through fifth lags and the instruments for the predetermined economic controls include their first lags. All analyses conducted with STATA, which differences out the constant term in difference-GMM estimation. Estimates in bold significant at $p<0.1$, two-tailed, and estimates in bold italics significant at $p<0.05$, two-tailed.

Table 4. Analysis of Pre-election Decline and Electoral Competitiveness Predictions, Home Price Data

	OLS [1]	Fixed Effects [2]	Difference GMM [3]	OLS [4]	Fixed Effects [5]	Difference GMM [6]
Gubernatorial Election Year	*-0.326* (0.123)	*-0.325* (0.120)	*-0.356* (0.149)	--	--	--
Gubernatorial Election Year × Competitive	--	--	--	*-0.890* (0.198)	*-0.845* (0.194)	*-0.854* (0.240)
Gubernatorial Election Year × Not Competitive	--	--	--	0.091 (0.131)	0.057 (0.127)	0.026 (0.157)
Competitive	--	--	--	*0.424* (0.103)	*0.512* (0.117)	0.198 (0.195)
Real Income Growth	*0.229* (0.052)	*0.226* (0.052)	*0.104* (0.028)	*0.230* (0.052)	*0.227* (0.053)	*0.105* (0.027)
Change in Unemployment	*-7.387* (0.619)	*-7.382* (0.627)	*-3.240* (0.740)	*-7.421* (0.616)	*-7.407* (0.625)	*-3.231* (0.735)
Demographic Demand	*0.062* (0.007)	*0.325* (0.065)	-0.266 (0.251)	*0.062* (0.007)	*0.346* (0.064)	-0.263 (0.253)
Foreclosure Rate$_{(t-1)}$	*-0.991* (0.096)	*-1.384* (0.131)	0.589 (0.410)	*-0.960* (0.093)	*-1.345* (0.130)	0.584 (0.408)
%Homes Sold$_{(t-1)}$	*0.625* (0.033)	*0.579* (0.034)	*0.692* (0.209)	0.625 (0.039)	*0.578* (0.034)	*0.690* (0.209)
%Homes Sold$_{(t-2)}$	-0.041 (0.032)	*-0.072* (0.032)	*0.239* (0.135)	-0.039 (0.032)	*-0.068* (0.031)	*0.240* (0.135)
Constant	*2.079* (0.599)	*-3.376* (0.565)	--	*1.972* (0.603)	*-3.789* (0.540)	--
Year Indicators	Yes	Yes	Yes	Yes	Yes	Yes
N	7265	7265	6923	7265	7265	6923
AR(1)	--	--	0.056	--	--	0.055
AR(2)	--	--	0.173	--	--	0.177
Hansen J Test (p-value)	--	--	0.898	--	--	0.899
R^2	0.64	0.59	--	0.64	0.58	--

Notes: Coefficients given above standard errors. Dependent variable equals %Change in Real Home Prices. In difference-GMM, the instruments for the lagged dependent variables include the third through fifth lags, collapsed. All analyses conducted with STATA, which differences out the constant term in difference-GMM estimation. Estimates in bold significant at p<0.1, two-tailed, and estimate in bold italics significant at p<0.05, two-tailed.

Appendix Table A. CCES Profile Variables in Survey Analysis

Profile Variables	Coding
Income	14-point scale based on CCES categories: less than $10,000; $10,000-$14,999; $15,000-$19,999; $20,000-$24,999; $25,000-$29,999; $30,000-$39,999; $40,000-$49,999; $50,000-$59,999; $60,000-$69,999; $70,000-$79,999; $80,000-$99,999; $100,000-$119,999; $120,000-$149,999; $150,000 or more
Age	Year of birth
Gender	1 if female, 0 if male
Marital Status	1 if married, 0 if not married
Race	1 if Black, 0 otherwise
Party Affiliation	0 if Republican, 0.5 if Independent, 1 if Democrat
Homeownership	1 if own apartment or house; 0 if rent or live with someone else (such as a family member) in non-institutional housing but do not pay rent
Children under 18	Number of children under 18
Employment	Employed: Full-time, Part-time Not employed: Temporarily laid off, Unemployed, Retired, Permanently disabled, Homemaker, Student

Appendix Table B. Control Variable Results for Survey Analysis

	Buying/Selling Home			Home Renovation			Moving		
	Job Security [1]	Taxes [2]	Income [3]	Job Security [4]	Taxes [5]	Income [6]	Job Security [7]	Taxes [8]	Income [9]
Year of Birth	0.004	0.003	0.008	0.010	0.005	0.013	0.002	0.006	0.001
	(0.079)	(0.005)	(0.008)	(0.010)	(0.007)	(0.010)	(0.007)	(0.006)	(0.008)
Female	-0.299	*-0.309*	-0.255	-0.203	**-0.265**	-0.176	*-0.481*	*-0.373*	*-0.492*
	(0.007)	*(0.152)*	(0.203)	(0.216)	**(0.152)**	(0.209)	*(0.200)*	*(0.157)*	*(0.207)*
Married	-0.128	0.148	-0.104	-0.071	0.119	-0.057	-0.076	0.225	-0.077
	(0.216)	(0.168)	(0.218)	(0.211)	(0.163)	(0.217)	(0.212)	(0.176)	(0.210)
Black	*0.755*	*0.676*	*0.764*	0.462	**0.481**	0.478	0.326	0.356	0.371
	(0.340)	*(0.258)*	*(0.338)*	(0.368)	**(0.269)**	(0.366)	(0.309)	(0.266)	(0.304)
Party Affiliation	-0.202	0.072	-0.163	-0.020	**0.280**	0.018	-0.032	0.190	-0.077
	(0.235)	(0.169)	(0.234)	(0.227)	**(0.158)**	(0.224)	(0.260)	(0.188)	(0.263)
Income	-0.185	0.012	-0.010	0.022	0.015	0.029	**-0.057**	-0.018	**-0.058**
	(0.030)	(0.023)	(0.032)	(0.031)	(0.023)	(0.032)	**(0.030)**	(0.025)	**(0.031)**
Homeownership	0.254	-0.046	0.261	*0.589*	**0.329**	*0.590*	0.194	-0.200	0.164
	(0.229)	(0.183)	(0.233)	*(0.257)*	**(0.194)**	*(0.256)*	(0.248)	(0.195)	(0.247)
Children under 18	-0.015	-0.095	-0.008	-0.002	-0.073	0.003	0.029	-0.060	0.031
	(0.058)	(0.082)	(0.056)	(0.060)	(0.080)	(0.059)	(0.048)	(0.073)	(0.047)
Future Income Confidence	0.085	0.158	0.114	0.229	**0.215**	0.269	-0.137	0.060	-0.152
	(0.175)	(0.123)	(0.177)	(0.168)	**(0.115)**	(0.172)	(0.164)	(0.121)	(0.164)
Constant	-10.72	-8.364	-16.83	-21.441	-13.398	-27.948	-4.527	-12.569	-2.446
	(14.601)	(10.646)	(15.289)	(19.502)	(13.113)	(19.573)	(15.329)	(11.988)	(14.910)
N	379	679	380	379	681	380	379	675	380

Notes: Probit coefficients given above standard errors. Estimates in bold are significant at p<0.1, two-tailed, and estimates in bold italics are significant at p<0.05, two-tailed. Table 1 presents results for the variables that test the Policy Differences Prediction.

Appendix Table C. Additional Analyses of Home Sales Data

	Difference GMM [1]	Difference GMM [2]	System GMM [3]	System GMM [4]	System GMM [5]	System GMM [6]
Gubernatorial Election Year	*-0.254* *(0.118)*	--	*-0.239* *(0.108)*	--	*-0.257* *(0.120)*	--
Gubernatorial Election Year × Competitive	--	*-0.377* *(0.142)*	--	*-0.491* *(0.141)*	--	*-0.473* *(0.150)*
Gubernatorial Election Year × Not Competitive	--	-0.168 (0.162)	--	0.001 (0.172)	--	-0.066 (0.179)
Competitive	--	**0.283** **(0.171)**	--	0.020 (0.098)	--	0.182 (0.112)
Real Income Growth	**0.083** **(0.046)**	**0.079** **(0.044)**	*0.126* *(0.046)*	*0.133* *(0.042)*	**0.086** **(0.043)**	**0.088** **(0.040)**
Change in Unemployment	0.265 (0.697)	0.266 (0.723)	0.122 (0.635)	-0.006 (0.671)	0.127 (0.612)	0.083 (0.629)
Demographic Demand	0.137 (0.677)	0.062 (0.732)	-0.004 (0.009)	-0.010 (0.010)	0.002 (0.011)	0.001 (0.011)
Foreclosure Rate$_{(t-1)}$	*0.448* *(0.193)*	*0.455* *(0.204)*	*0.271* *(0.066)*	*0.261* *(0.065)*	*0.239* *(0.070)*	*0.247* *(0.068)*
%Homes Sold$_{(t-1)}$	*0.969* *(0.296)*	*0.963* *(0.304)*	*1.160* *(0.122)*	*1.151* *(0.125)*	*1.243* *(0.129)*	*1.234* *(0.129)*
%Homes Sold$_{(t-2)}$	*-0.460* *(0.201)*	*-0.461* *(0.205)*	-0.218 (0.150)	-0.212 (0.155)	*-0.347* *(0.159)*	*-0.342* *(0.161)*
Constant	--	--	*-1.114* *(0.279)*	*-1.216* *(0.204)*	**0.670** **(0.356)**	*-0.773* *(0.368)*
Year Indicators	Yes	Yes	Yes	Yes	Yes	Yes
N	171	171	206	206	206	206
AR(1)	0.024	0.031	0.014	0.003	0.002	0.002
AR(2)	0.396	0.327	0.369	0.409	0.469	0.456
Hansen J Test (p-value)	0.165	0.167	0.982	1.000	0.524	0.615
Difference-in-Hansen (p-value)	--	---	1.000	1.000	0.835	0.884
Hansen tests suggest model overidentified?	Y	Y	Y	Y	N	N

Notes: Coefficients given above standard errors. Columns [1] and [2] alter the difference-GMM model of Table 3 by assuming unemployment and income are exogenous. Columns [3] and [4] apply system-GMM to the difference-GMM model of Table 3, and Columns [5] and [6] adjust this model by assuming unemployment and income are exogenous. Estimates in bold are significant at $p<0.1$, two-tailed, and estimates in bold italics are significant at $p<0.05$, two-tailed.