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Reuter, Ora John, *The Origins of Dominant Parties: Building Authoritarian Institutions in Post-Soviet Russia*. Cambridge: Cambridge University Press, 2017, xiii + 316pp., £64.99 h/b.

Why do dominant parties *not* emerge in some authoritarian settings? As Ora John Reuter notes at the outset of this magisterial work, ‘the puzzling thing about dominant parties is not their prevalence but rather their nonexistence in so many non-democracies’ (p. 1). Reuter’s solution to this puzzle is both novel and intuitive. Presenting the construction of dominant parties as the solution to a two-sided commitment problem between political leaders and elites, Reuter argues that ‘*dominant parties are most likely when elites hold enough independent political resources that leaders need to co-opt them, but not so many autonomous resources that they themselves are reluctant to commit to any dominant party project*’ (p. 3). Although many readers might be familiar with the general argument, which was presented in a 2009 co-authored article with Thomas Remington, this book fleshes out the core claim with rich country-specific detail from post-Soviet Russia, comparative data, and a well-crafted research design. In contrast to much existing work, which often centres on the functional attractions of dominant parties for authoritarian leaders, this book spells out the key *conditions* under which dominant parties are likely to form – and not to form.

The book consists of nine chapters. Following the introduction, the second chapter presents a general theory of dominant party formation. The third chapter then provides a theory-informed narrative of the failure to create pro-presidential parties under Boris Yeltsin in 1990s Russia. The fourth chapter moves onto Putin and the creation of United Russia, looking at the balance of power in Russian between the central leadership and regional elites, 2000-2010. Chapter five then discusses the operation of United Russia as the only successful ‘party of power’ to have emerged in post-Soviet Russia. The sixth and seventh chapters extend the logic of the overall argument regarding the general leader-elite balance of power to finer-grained studies of governors and regional legislators, respectively. The eighth chapter then zooms out from the specificities of Russia to look at dominant party emergence in non-democracies across the world, 1945-2006. The combination of various types of evidence and levels of analysis is clear, clever, and compelling.

United Russia is presented in the book as a much more interesting, complex phenomenon than usually encountered. I have, in my own research, been guilty of assuming United Russia’s impotence in the policy-making process – of moving too swiftly from the ‘absence of public conflict between legislators and the executive [...] to conclude that the former do not influence the content of legislation’ (pp. 165-166). And yet, we *do* sometimes see conflict between the legislature and the executive, including between senior United Russia State Duma deputies and members of the Government. In addition, although Reuter provides fascinating details – often drawn from interviews with political actors in Russia – suggestive of United Russia’s influence on the policy-making process, we still need to know more about, say, how the delegation of writing legislation from the executive to United Russia actors (mentioned on page 187) actually works. If

policy formulation is delegated, then in which areas does United Russia have more scope for policy entrepreneurship? And how does the Kremlin prevent ‘drift’ – that is, the likely tendency of agents to craft proposals closer to their ideal points than those of their principals? Finally, what happens in the (very likely) situation of *competing* principals? Is United Russia’s ability to function as a stable, disciplined pro-executive force compromised by intra-executive policy (and other) differences? It would be unrealistic to require such details in this book, but these are questions that will hopefully be addressed in future research.

Our comparative knowledge of dominant parties is disproportionately influenced by the study of the Mexican Institutional Revolutionary Party (PRI) and the Communist Party of the Soviet Union (CPSU). And yet, as Reuter notes on page 32, there are good reasons to suggest that these cases are not representative of the broader population of interest. By providing a detailed case study of another, high-profile dominant party, and by testing theoretical claims on cross-national data relating to such parties, this book is a significant step forward for the field.

It is tricky to find fault with the book. Indeed, it speaks to the quality of the book that an apparent typo is responsible for the clearest factual error: on page 35, Reuter writes of the “colored’ revolutions [that] unseated long-serving incumbents in Ukraine and Georgia in 2003 and 2004, respectively’ – but the Georgian ‘Rose Revolution’ took place in 2003 and the Ukrainian ‘Orange Revolution’ took place in 2004. My only stylistic quibble relates to Reuter’s use of phrases such as ‘[t]o use the jargon of social science’ (p. 49) and ‘[t]o use the jargon of game theory’ (p. 50). This book is clearly a work of social science, not a crossover publication that might benefit from jargon flagging. Moreover, there are other, much more technically sophisticated sections of the book that are devoid of such jargon warnings – see, for example, the text in chapter 8. These are very small points, however.

This book will be of interest to a large number of audiences: from scholars of authoritarianism, to those interested in Russian regional politics; from comparative scholars of political parties, to those interested in legislative politics. Combining the country specialist’s depth of knowledge with the comparativist’s broad view, this is political science at its best – meticulous, engaged with an interesting question, methodologically sophisticated, and cautious regarding causal claims. We need more work like this.