Kenneth Newton: ***Surprising News: How the Media Affect—and Do Not Affect—Politics***
Boulder, CO, 2019: Lynne Rienne Publishers, 271 pp.

How do the media affect political attitudes and behaviour? The question is as old as empirical social science. But it has received very different answers at different points in time—for example, concerning the magnitude of the effects and the type of dependent variables. Kenneth Newton's answers in this book are nuanced and knowledgeable and build on an admirable synthesis of complex multidisciplinary research. At the end of the day, though, he is sceptical about media effects in a book that 'sheds the harsh light of fact-based social science research on the influence of different kinds of media…' (p. 2). For better explanations of attitudes and behaviour, the books argues, we should turn to 'the standard model'. This is basically a list of 'usual suspects', like class, party identification, education, religion, income, occupation, age, and gender. As the author puts it: 'This book shows that the standard model, rather than the media, is generally the most powerful when it comes to explaining political attitudes and behaviour.' (p. 2) Relatedly, most chapters emphasise the many contingencies in media effects, such that 'part of the difficulty in pinning down media effects … is the interaction between the media and their audiences' (p. 123).

This well-written book has much to recommend it. Its signature strength is its impressive range of relevant topics, several of which are rarely considered next to each other. Thus, the first few chapters deal with interesting but quite expected topics like 'belief preservation'; that is, why defending our predispositions and resisting uncongenial messages 'induces a shot of dopamine-induced pleasure so that we feel good about ourselves even if we are wrong'. Subsequent chapters, however, discuss less expected but equally relevant areas, such as how people's personal life experiences provide much more political input than we think and can regulate which media messages matter. A subsequent chapter explains how media effects may be outcompeted and circumscribed by 'political talk' among family, friends, colleagues, etc. Similarly, the book gives due attention to the role of media trust. Here, Newton concludes that '[b]ecause there is plentiful evidence that trust in the media is comparatively low and declining, the media's persuasive powers may also be low and declining' (p. 122). The last one-third or so of the book shifts gear once again and addresses the nature and impact of the macro, 'supply-side' aspects of media systems. Thus, one chapter compares public service and commercial broadcasting, whereas two others consider the validity of the media's 'pluralist' ideal in the current digital era. This means mapping how pluralist the structure of the media landscape is as well as how pluralist individual citizens' 'news diets' are.

This enjoyable book also has some weaker points. For example, one can debate the relevance of a comparison with a 'standard model' (a term this reader had not heard before). To begin with, this exercise really amounts to comparing the impact of one (type) of variable with the impact of many. How *surprising* is it really that many factors beat one? But more than this, could one not argue that these variables explain different types of variation? The standard model may explain overall stable patterns. Media effects may be weak overall but very concentrated among the increasing number of volatile voters, thus accounting for short-term deviations from stable 'standard' patterns. And even small deviations from such patterns may have large macro-political consequences for, say, who wins elections and referendums. These remarks are not very original and should not be controversial. Still, they

could have been made more often as the book compares the significant—but contingent and allegedly weak—media impact with multiple stable 'standard' model predictors.

In some places, the book could have been clearer about how and why generalisations seem to differ from past research. The title of the book suggests strong argumentation against scholars or pundits who are somehow convinced that media effects are massive and widespread. But we are really never told for whom the conclusions are supposed to be surprising. This is mostly okay but creates problems in places, in particular for the discussion about the concept of 'agenda-setting'. Many readers will know that in the 1970s scholars largely abandoned 'persuasion'-type effects for the benefit of agenda-setting. The idea is that while the media may not directly dictate what people 'think', they may influence what people 'think about', i.e. which problem areas are salient. This can in turn have subtle but important consequences for voting, party competition, and even public policy. Many previous textbooks and overviews have concluded that agenda-setting is generally a well-documented (though surely contingent) media effect. Newton acknowledges the existence of these effects, but usually in passing, while reserving a greater number of words for the contingencies in such processes. Similarly, his handful of key examples from the US and the UK show weak agenda-setting effects. I would have liked greater precision in the conclusions drawn here. Does Newton actually argue that past work has exaggerated agenda-setting effects? If so, why does he come to a different conclusion? Is it because he is taking into account different, perhaps newer, studies or cases? Or is he evaluating the same evidence differently, perhaps using a different standard for what counts as a politically interesting media effect? Or perhaps the seeming disagreement is just a matter of emphasis, such that everybody agrees that the agenda-setting impact of the media exists but is contingent, and the book's contribution is to highlight contingencies and, using the 'standard model' to give effects reasonable proportions.

Each chapter typically starts by posing a big question and then proceeds to an in-depth discussion of 'example studies'. Squarely on the positive side, this strategy means the reader is pedagogically introduced to relevant theories as well as to empirical possibilities and limitations in real research. On the slightly negative side, the example studies are mostly from the US or the UK and occasionally feel slightly dated or at least very familiar (i.e. the Clinton–Lewinsky scandal, the Vietnam War, and the much-debated 1997 election that made Blair's New Labour). One sometimes wonders how representative the studies are in findings and methods. For example, the well-documented weak impact of the Clinton–Lewinsky scandal could have been complemented with one of the many studies that actually find more significant scandal effects on (some types of) dependent variables (see the meta study by von Sikorski [2018]).

These more critical remarks should not obscure the fact that Newton has written a very compelling account of one of the oldest, most complex, but still evolving accumulations of empirical social science. In the current era of article-based specialisation, social science needs high-quality books that structure the broader debate with well-considered 'stylised facts' for everyone else to chew on. Writing such books is clearly not for everyone. But those of us who enjoyed already *Beliefs in Government*—in which Kaase and Newton [1998] discussed the implications of a major international four-volume research project—knew that Newton is suited to the job. Consequently, he has produced a book that should work well for both master's students and advanced scholars who want a

trustworthy shortcut into a sprawling and multidisciplinary body of literature. The book deserves to be read and appreciated as the debate over the media impact on politics and democracy continues.

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References

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James C. Scott: ***Against the Grain. A Deep History of the Earliest States***
New Haven, CT, and London 2017: Yale University Press, 312 pp.

Although commonly acknowledged as an analytical problem that plagues the social sciences, the reach of *methodological statism* has never quite been fully pursued. Picking up the gauntlet, James C. Scott embarks on a daring interdisciplinary study, drawing on prehistory, archaeology, ancient history, and anthropology (p. x) so as to shed light on the earliest roots of state-building processes. Going as far back in history as 6500 BCE, Scott picks apart the rigid narrative of progress tying together crop-agriculture, sedentarism, and state formation (pp. 1–3). Throughout the book, the author skilfully employs the analytical toolkit of political sociology and political anthropology to the tricky field of prehistory, offering a set of hypotheses concerning a problematic bias—the *methodological statism* of archaeology qua state-sponsored research (p. 13).

Similar to some of the arguments outlined by Harari [2016: Chapters 2, 3], though more methodologically grounded, Scott's main argument is that '*stateness* must be understood as an institutional continuum, less an either/or proposition than a judgment of more or less' (p. 23). The key reconsideration is that sedentarism is not necessarily a corollary of crop-field agriculture (p. 10). As a crucial component of both crop-field agriculture and a sedentary lifestyle, domestication is also argued to have been somewhat arbitrarily placed in the *Homo Sapiens* timeline (p. 11). Further down the line Scott identifies a highly problematic 'essentializing element to the progress-settlement story that starts early in history as people codified how they were different from others' (p. 7).

The structure of the book follows these major reconsiderations proposed by the author. Chapter 1 delves into multiple issues of domestication. Scott's key point is that the connection between the domestication of plants, as a first stage towards mass crop-agriculture, and the creation of modern states is far more spurious than previously assumed, if not altogether missing. According to recent evidence cited by the author, there seems to be a gap of as much as 4000 years between the domestication of grains and livestock and the appearance of anything resembling an early agrarian state (p. 46). The source of the mistake, according to Scott, is a combination of a teleology of progress and an observation bias—'the narrative of civilizations arising from irrigation of arid lands because this fit with the contemporary landscape that those formulating the narrative were observing' (p. 55). Although he admits the speculative nature of his hypothesis, Scott proposes that wetland societies, which were far more likely the source of early urbanism and proto-states, have been ignored by existing narratives because they were not dependent on the type of central authority that would leave proof of its existence (p. 57). More broadly speaking, the main problem with the immediate link between farming and state-formation is that it involves far more intensive labour than these early states were capable of or-