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Evolution or Revolution?

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Stephen Biddle has written a book which is a major contribution to the analysis of military power and, in doing so, has done a great service both to the world of policy analysis, which seeks to generate and use military power in the most reasonable ways, and to political science, which uses power as a central concept in its theories, but often only with minimal efforts to understand its sources and character. Biddle is an expert on the subject, is deeply immersed in the technical analyses of military power, but makes every effort to make his arguments accessible and relevant to the broader audience of educated readers.

There are two clear themes to this book. Both can be stated briefly, though the first is the more straightforward of the two.

First, Biddle writes that that there is solid evidence that existing quantitative measures and models of military power have, empirically, little or no explanatory or predictive value when actual wars and battles are examined. He lays this argument out in broad terms in his chapter, 'A Literature Built on Weak Foundations', and returns to it in the chapters that review the German offensives in 1918, the thwarted British breakout campaign of July 1944 and in his discussion of the First Gulf War. The size of a given national economy, the number of soldiers in the army, the amount of money spent on them and the level of technology of the weapons, when matched against comparable figures for another country which is engaged in war, predict the outcomes of the war with a success rate that is little better, and sometimes worse, than a flip of a coin. More detailed analysis of the impact of analogous figures – such as the number of soldiers deployed in a theatre of military operations, or on a given front, or the relative age of their weapons – shows that the casualties taken by attackers compared to casualties taken by defenders, or the rate at which

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territory can be occupied are similarly unrelated to the quantitative data. These quantitative indicators, when employed in mathematical models of combat, are not good predictors of actual battlefield outcomes because they take no account of what Biddle calls force employment, or how the soldiers and military organisations behave in combat. These indicators take no account of the ability of soldiers to make decisions that are appropriate to their local circumstances and the behaviour of the enemy, and which are coordinated with the intent of their commander and the actions of other friendly units on the battlefield.

The second theme is distinct from the first argument, but related to it. If data and models that ignore force employment are flawed, how should we then incorporate an understanding of force employment? In order to do this, Biddle writes, we must recognise the origins and character of what he calls 'the modern system'. Biddle states that, beginning in the late nineteenth century, increases in the accuracy and density of firepower on the battlefield forced the Great Powers of Europe to shift to a new system of warfare, at the operational and tactical levels, on both the offence and the defence, if they were not to have their soldiers annihilated at the tactical level, or their military organisations thwarted or disrupted at the operational level.

By the end of World War I this shift had taken place and, Biddle argues, the character of conventional land warfare has not changed fundamentally or been displaced by another force employment concept since then. So, for example, the introduction on a large scale of tanks and combat aviation in the 1930s did not bring about a significant change in force employment concepts, but simply provided new ways of implementing the modern system on the battlefield. The modern system involves, at the tactical level, the use of cover and concealment and suppressing fires. At the level of operations, dispersed defences in depth and well-timed counter-attacks are employed on the defence, and, on the offensive side, the focus is on the use of local concentrations of force to create breakthroughs that can then be exploited to achieve rapid movement on the battlefield and the disruption of the defenders' forces. If both the attacker and defender employ the modern system, shifts of the battle front-lines will be moderate, one way or the other, and the result of numerical superiority to some extent, and technological superiority to a much lesser extent. If, however, one side employs the modern system offensively or defensively on the battlefield against an opponent who does not, the results will favour the side that employs the modern system in a lopsided fashion, in a way that would not be predicted or explained by quantitative measures. So radical success by forces that do not enjoy technological or numerical advantages can take place if one side employs the modern system and

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the other does not. Further, the greater the technological advantage the employer of the modern system has, the more lopsided success it will have. Put slightly differently, technological advantages multiply, or are multiplied by, asymmetries in the employment of the modern system. So while changing technology has not displaced the modern system, technological advantages do matter, if they occur together with asymmetries in force employment. While existing quantitative measures cannot explain or predict war and battle outcomes by themselves, they can help explain them if attention is also paid to the presence or absence of force employment concepts.

Biddle's first argument, that is, his critique of past quantitative analysis based on measures of national power that mostly ignores operational practice, is certainly warranted. It has particular power precisely because it is made by Biddle, who is deeply grounded in the quantitative measures and methods he critiques. Whether broad measures of national preponderance are easily distinguishable from the presence of the modern system will be addressed below.

It is his second argument, however, that will draw the most attention. It already has. Some background not provided in Biddle's book is needed to understand the reaction to his second argument. Beginning in the late 1980s, the director of the Office of Net Assessment in the Office of the Secretary of Defence, Andrew Marshall, began to circulate memos that called attention to writings emerging from the General Staff of the Soviet Army. These writings discussed recent developments in the American military, in particular, a new doctrine for the defence of the NATO area against Warsaw Pact attack that was called AirLand Battle. The new doctrine would be made possible by new airborne sensors that could detect targets on the ground, and new weapons, many unmanned, that could strike precisely at armoured vehicles. The Soviet General Staff writers noted that, if successful, this new system of systems would have the same military impact as tactical nuclear weapons, though they would use non-nuclear warheads. This was a revolution, they argued, what they called a Military-Technical Revolution. Military-Technical Revolutions had occurred earlier, when railroads and rifled weapons radically increased the amount of firepower that could be brought to bear on a battlefield, and when the internal combustion engine, radios and aviation made possible the form of warfare that was called blitzkrieg. This revolution made World War II a more fluid and rapidly moving war than World War I. Marshall argued that the Soviet General Staff, for a variety of reasons, might be picking up on a major shift in the character of warfare that we had not noticed ourselves. One criticism that Marshall made was that the term Military-Technical Revolution, invented by the Soviets, was misleading because it made the phenomenon appear to be a revolution

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in technology only, while the historical examples provided by the Soviets emphasised the fact that a shift in military organisation and concepts of operations was necessary to take advantage of the opportunities created by combinations of new technology. Technological advances by themselves did not produce lopsided military outcomes. It was only new technology plus a new force employment concept, to use Biddle's term, that produced a Revolution in Military Affairs, or RMA, the term that Marshall preferred, following the example of the historian Geoffrey Parker, who wrote in 1988 about the 'military revolution' of European armies that was associated not with new technologies, but with the revival of Roman battlefield tactics and new fortifications in the late sixteenth century.

The debate that has heat today is whether there might be an RMA underway or forthcoming in the near future. As Biddle briefly notes, 'the [1991 Gulf] war changed the whole course of American military thought – the revolution in military affairs thesis that now dominates the defence debate is a product of the radically surprising nature of the Desert Storm loss rate. Though the RMA thesis had been presented before the war, few were persuaded'. The blurb by Michael O'Hanlon for Biddle's book asserts that Biddle 'transcends the popular theory that a revolution in military affairs is now underway'. The rapid success of the US in Afghanistan, with few American ground forces but greatly increased use of precision-guided munitions, as well as the earlier successful NATO air campaigns in Serbia and Bosnia added political fuel to the idea that an RMA was underway, and that a 'transformation' of the American military was either necessary or was already happening. Anything that could claim to be linked to an RMA or 'transformation' was suddenly good. Even very traditional weapons, such as the Crusader artillery system, as well as the F-22 fighter, were touted as transformational. The term RMA became a promotional slogan, associated primarily with selling new pieces of technology.

And yet it began as a serious effort to understand the nature of non-incremental changes in the amount of military power that could be generated by a given quantity of material resources, changes that could affect battlefield outcomes, and that were the result of changes in force employment together with new technology. If the argument is right, that there can be RMAs, as opposed to simply incremental change, the consequences could be large. Past RMAs arguably made nation-states' military viable against much larger empires, made possible European military empires in Asia, and nearly made possible a Nazi empire in Europe. So setting aside hyperbole, it is important to understand what an RMA is, and what it looks like when you see it.

The serious writers concerned with RMAs, such as Eliot Cohen, Andrew Krepinevich and Michael Vickers, separate what an RMA is

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from what its consequences are. An RMA is a combination of new military organisational goals and structures with new operational practices on the battlefield that are sometimes but not always driven by new technologies. The shift from loosely organised Dutch soldiers to highly disciplined and drilled units is the archetypical RMA, generally called the Early-Modern European RMA, and was not primarily driven by technology. The creation of integrated combined arms units that employed motorised infantry, tanks, self-propelled artillery and aviation, not to destroy, but to disrupt the rear area of an adversary, is a more modern example, and was visible conceptually in Plan 1919 created by the British but never employed by them, and visible in practice in the blitzkrieg forces commanded by Heinz Guderian in 1940. It was a revolution because it was not an incremental change in organisational structure and practice, but a new military order, in which previously dominant elements were replaced or subordinated to new organisational constructs. RMAs have historically produced an increase in military power that was observable by an order of magnitude increase in the scope of military operations within a given time frame and with a given number of forces, or an order of magnitude decrease in the number of forces needed to defend objectives for a specified amount of time. Forces employed, territorial scope and time are critical observable measures of the consequences of RMAs.

How does Biddle come down on the subject of RMAs? Towards the beginning of his book, he seems to adopt an approach to the study of the modern system that is quite compatible with this understanding of RMAs. The modern system, he writes, requires new combined arms organisations, and new freedom for junior officers to exercise battlefield initiative. The effects are order of magnitude in scale. He notes that the introduction of modern system artillery practices by the Germans in 1918 created both a new objective – suppression rather than destruction – as well as new methods that produced a reduction in the number of artillery shells fired that was one-tenth of that previously required (p.37). The charts in Chapter Four show the modelling assumptions about the amount of territory gained by an attacker using the modern system versus attackers that do not, that are a factor of ten greater, or more (Figures 4.1, 4.2, 4.3). In his discussion of the German offensives in 1918, he notes that rather than earlier gains of a mile or so, at most, the Germans advanced 20 to 40 miles. So the introduction of the modern system looks very much like an RMA.

What, then, is the argument about? Biddle says that the introduction of the modern system was a major change, but it was the last such change. What is the primary claim to the contrary? Most proponents of the RMA thesis argue that the next change is the introduction of blitzkrieg, using tanks, radios and airplanes. Instead of advancing 20 to

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40 miles, as they did in 1918, the Germans advanced over 200 miles in France in 1940, and twice that distance in the Soviet Union in 1941. Was this the result of an RMA? Biddle says no, that the modern system was introduced by the end of World War I by the Germans without the use of any significant use of tanks. That is absolutely true. But what are the dominant metrics for determining the existence of an RMA? Organisational shifts and order of magnitude increases in scope of operations. The fact that the Germans did not use tanks in 1918 says nothing about whether the operations in 1940 and 1941 constituted a subsequent RMA. One reads Biddle's book and eagerly looks for his discussion of Heinz Guderian, and the Battle of France, but it is not there. There is a very brief mention of the fact that by 1940, German tanks were more mobile than infantry was in 1918, but that is all. He dismisses the idea that blitzkrieg radically increased operational mobility on the European battlefield without a discussion of blitzkrieg. The readers are left to resolve for themselves why an order of magnitude increase occurred in the scope of operations in May 1940 and July 1941 compared to 1918. The only explanation Biddle might give us is that the Germans employed the modern system in 1940 and 1941 but the French and Soviets did not. This asymmetry could produce, Biddle writes, lopsided victories for the modern system practitioners. But Biddle makes a point of saying that the European Great Powers all ended World War I with a modern system, for the defence at least (pp.33–4, 52). So did France have a modern system defence in 1940 or not? Biddle discusses the defensive systems in place in Europe in 1940–41 on page 122 but is silent on the specific issue of French defences. How much can any nation deviate from the norms of modern system practices and still be said to have a modern system? In a tantalising passage on page 73, Biddle says that in his models, anything more than 'modest' departures from the modern system defences lead to rapid breakthroughs. Was this the case for France in 1940? The Soviet Union in 1941? How could we tell today if a given country had a 'real' modern system or not? This is left unclear, and is central to his argument that once the modern system was in place nothing changed. In the absence of a discussion of these cases, the possibility remains that an RMA did occur at least once after 1918, even though elements of the modern system, such as the use of cover and concealment, did remain, and still matter.

This issue derives from Biddle's explanation of the modern system. While Biddle spends an entire chapter usefully explaining exactly what the modern system entails *in battle* and why it leads to success, he does not outline a mechanism to actually define when a military has achieved the modern system, such as organisational and/or technological footprints.[1] Since Biddle's theory can only identify the existence of

the modern system after a battle occurs, it risks tautology. The indicators Biddle outlines as defining the modern system in practice, like dispersion, firepower and manoeuvre are sufficiently general that they could be seen in the tactics of almost all sides of almost all battles, at least to some extent. Failures could always represent a lack of mastery of the modern system while success represents mastery. So regardless of the actual outcome of a battle or even its conduct, within reason presuming both sides fight conventionally, Biddle can find positive evidence for his theory.

One very important concern involves the sources of force employment and their impact on military operations, a subject Biddle deals with only briefly at the end of Chapter Three. As an explanation for military effectiveness, force employment may simply proxy for the impact of other, more important variables that determine force employment in a given case. This means it is not the modern system that matters, but precursors related to its successful adoption that may also have their own independent impact on battlefield success. In political science terminology, Biddle may have an endogeneity problem. Biddle says adoption of the modern system is not only non-random, but based on factors related to strategic and organisational culture, regime type, civil-military relations, past experiences and economics (pp.48–51). These variables generally fit into one of two categories. Either they are variables like regime type or organisational culture that other scholars think are critical to producing military power and that Biddle admits influence force employment, or they are variables like material economic power, preponderance and technology that Biddle argues in Chapter Two matter less than force employment for predicting war outcomes. But if these variables may substantially influence force employment, there is a theoretically coherent reason to include these variables and their potential relationships with force employment in Biddle's explanation and especially in his empirical tests. But Biddle's theory cannot account for this possibility because it assumes that the variance of force employment strategies is constant over time (p.156).

Even beyond variables like regime type, why does Biddle *assume* a weak relationship between preponderance, technology and force employment (pp.156–7)? One could imagine entirely rational military leaders calibrating their strategy on the basis of the size of their military and its technological sophistication. Given that Biddle himself argues that increasing lethality, a technological development, drove the development of the modern system, this seems especially reasonable. Why would technological developments influence force deployment only in that one very specific case? And it is entirely plausible that the force-to-force ratio would influence the way commanders think about fighting a battle. Assuming that a relationship does not exist ignores

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entirely plausible relationships previously discussed by Biddle whose exclusion from the model biases the results.

But if these variables drive the probability of adoption of the modern system and behaviour on the battlefield in general, force employment becomes an intervening variable without much independent weight. While force employment might be a proximate cause for battlefield success, it would not represent the type of structural cause that both academics and policy-makers care about. More important for *predicting* success on the battlefield is understanding the scope conditions that make the adoption of the modern system more or less likely. Given the very general nature of Biddle's description of the modern system and his inability to lay out the organisational footprints that demonstrate its existence, this severely undermines his explanatory power. While the theory still has value for explaining the transmission mechanism by which factors like regime type impact military performance, its wide-ranging impact is curtailed.

Biddle could respond that technology and force size only matter within the constraints of the modern system, which means the variations commanders would imagine do not challenge his theory. Biddle could also point out that his statistical results show the national identity dummy variables matter more than preponderance or technology. But if Biddle's assumption about the independence of decisions about force employment is not realistic and that assumption provides the justification for how Biddle shapes his empirical model, it is not valid to use the results from the model to answer criticisms of the assumption.

Biddle also assumes that in the absence of specific variables measuring relative technological sophistication, time is an accurate proxy for increasing technological sophistication (p.156). But if Biddle's point is to use his empirical evidence to challenge the RMA hypothesis that military power can increase in a non-linear fashion, he cannot *assume* that technological sophistication increases in a linear and consistent fashion. This assumption is also curious because empirically testing the impact of time is relatively simple, but Biddle fails to include such a variable in most of his models.[2] While Table 8.3 includes some measurements for specific periods in history, there is no variable to capture the assumed variation in technological sophistication. A fix for the technological sophistication assumption is relatively straightforward. We inserted a 'date' variable into the model used to estimate Table 8.2. The time variable is both negative and highly significant, showing an overall dampening effect on loss-exchange ratios. Deriving the substantive significance by holding the other variables constant reveals severe time-dependence in the data and a substantive impact, especially when broken down by year, that outstrips the national identity dummies Biddle argues prove his theory

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is accurate.[3] Before World War II, increasingly modern dates make higher loss-exchange ratios more likely, though decreasingly so over time. The linear (due to the constraints of his statistical model) relationship between time and loss-exchange ratios is positive but decreasingly so prior to World War II.

Our re-estimation of Table 8.2 with a 'time' variable shows that the most powerful variable explaining the loss exchange ratio is the date the battle occurred, even more so than the participants. This places Biddle in a double bind. If the time variable substantively matters, it shows a degree of change in warfare his theory does not predict or at the very least that the national identity dummies are not the primary force driving the results. Alternatively, the time variable may not matter – but the reason is a specific measurement problem with the dataset as a whole that undermines its utility for predicting the future of war. Most scholars would concede that World War I and World War II were fairly unique in terms of duration and intensity – but battles from these wars encompass over 73 per cent of the observations in the CBD90 dataset used by Biddle. This means that if the time variable does not matter, it is due to peculiarities in the dataset that limit the applicability of Biddle's theory as a whole to almost any conflicts *except* World War I and World War II.

Beyond the impact of Biddle's assumptions on the results, issues remain with his actual statistical analysis. Biddle has no variable that actually measures his quantity of interest, skill on the battlefield/force employment. He just adds dummy variables showing the participants in a given battle. The results do indeed show that these particular battle dyads, or national identity dummies, such as the US against Germany in World War II or Germany against France in World War II, are more important than the overall impact of the number of troops brought to a particular battle. But this does not mean Biddle's theory of force employment is correct. Biddle presumes that any time a particular battle dyad is statistically significant, it proves that force employment and skill really matter. However, a multiplicity of factors is represented by those dyads. Regime type, culture, defence spending, learning from previous battles, allied assistance, weather, etc., are represented by a given national identity dummy, not just 'force employment'. Biddle acknowledges this point (p.163), but counters that as long as some of the national identity variables are significant, it demonstrates his theory is true. However, there is no theoretically informed reason to think that this is the case. That a national identity dummy variable is significant only demonstrates something specific about a particular dyadic interaction, such as between the US and Germany in World War II. This includes quite a bit more than 'force employment' and gets back to the questions of what causes the adoption of certain force employment

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strategies and the non-independence of individual battle observations, especially for the world wars encompassing two of his three case studies and 73 per cent of his CBD90 observations. Biddle's own discussion of the factors influencing the adoption of the modern system highlights the wide-ranging possibilities for variables like regime type to both influence force employment and exercise an independent impact on military operations. Either way, this reduces the explanatory power of Biddle's theory

With the exception of numerical preponderance on the battlefield, Biddle's statistical tests not only fail to include a variable that actually measures force employment, but they do not actually test his theory against the 'failed' indicators he outlines in Chapter Two. Especially given the endogeneity issued raised above between elements of national power, for example, and the likelihood that the modern system is adopted, controlling for relative economic capacity, etc., would have constituted an important validity test for Biddle. Absent such a check, this is another reason to question whether his national identity dummies actually represent force employment or one of a number of other variables that may both directly influence battlefield outcomes and influence the adoption of particular force employment strategies.[4]

What does this mean for the analysis of specific cases? Was there an RMA in the 1991 Gulf War? There was clearly a rapid collapse of the Iraqi forces in 1991, but was this the result of an RMA or the absence of an Iraqi modern system? As Biddle notes, there were many factors that contributed to the Iraqi collapse. The factor that Biddle emphasises is the lack of tactical skills on the Iraqi side that precluded the execution of a modern system defence. The interaction of technological inferiority and poor tactical skills produced the Iraqi collapse with casualty rates and order of magnitude less than the Israelis experienced in the 1967 war. This is plausible, but so is the claim that American technological superiority was large enough to produce order of magnitude changes in battlefield results. On the other hand, the 1991 war, as Andrew Marshall has noted, was perhaps the last mass industrial war. A massive 44-day air campaign in preparation for the battle, a ground force of hundreds of thousands of troops, and the absence of a significant shift in organisational goals, structures and concepts of operations certainly suggest that an RMA had not occurred. American Air Force officers even denied that there was anything that could be called precision-guided munitions, as opposed to precision-delivered munitions that human pilots could drop on targets. Small segments of the war may have given glimpses of a future RMA, but that is all. Biddle may be justified in debunking any overselling of the 1991 war as an RMA, but the originators of the term might well agree with his basic conclusion.

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Is there an RMA in the making? This is where there is the most controversy. Though he does not use the term RMA, Biddle writes that a shift in the character of war as large as the one associated with the introduction of the modern system would occur when new technology made cover and concealment impossible. Given the technologies now plausible, and the possibility of operational and technical countermeasures, Biddle says that such a shift is implausible any time soon, though he is not much more precise than that. This is not satisfying on several grounds.

Changes that make cover and concealment completely ineffective are not necessary, by Biddle's own metrics, to create a new order. An order of magnitude change in the execution of military operations was the standard Biddle used to argue that the modern system was a revolutionary change. Was American air power an order of magnitude more effective against Taliban battlefield positions in Afghanistan in Operation 'Anaconda' in 2002, compared to British bombing of German positions in 1944? On page 56, Biddle notes that roughly half of the Taliban battlefield positions were destroyed by American air power before traditional ground operations made contact with the enemy. How does that stack up against the fraction of German positions neutralised by pre-battle bombing in 1944? This kind of appropriate comparison is what is needed, but not supplied. Biddle becomes slightly tendentious when he argues that the NATO 78-day air campaign against Serbs in 1999 was not an RMA because it involved 'tens of thousands of sorties [that] killed at most a few hundred ground force targets', though that campaign was directed at many other targets that were not ground force targets. Since Biddle is honourably committed to rigorous empirical analysis, it is no dishonour to him to hold him to the most rigorous of standards when he makes assertions of his own.

More fundamentally, the proponents of the RMA thesis have consistently said that it is not technology alone that produces an RMA. New organisational structures, goals and concepts are the key to RMAs. The American uniformed services, while embracing the terms RMA and transformation, have only recently begun to act in ways that suggest that these kinds of changes are thinkable. So it is too soon to conclude, as Biddle does, that no fundamental change in the character of ground warfare is foreseeable.

Notes

1 This is not an argument for a binary (0, 1) coding. There are a variety of ways one could measure the modern system based on types of military organisations, doctrine, training etc. If it is not possible to know, before a military campaign, whether a country actually is employing the

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modern system or a particular level of competency with regards to the modern system, the theory cannot make predictions about the outcomes of battles because all of its predictive power is post-hoc, meaning it risks tautology.

2 Though beyond our scope due to space constraints, this also matters due to the assumed independence of his observations. The independence of observations issue is a methodological issue with substantive consequences. It is untrue that each observation in the dataset, especially since so many observations involve the same country or countries, mostly in the same handful of conflicts, is independent. Battles in 1945 between the US and Germany were clearly influenced by previous battles between the two sides and by battles they had with other actors. The failure to correct for this biases the results. For a more in-depth explanation of the 'time' issue, see Nathaniel Beck, Jonathan N. Katz and Richard Tucker, 'Taking Time Seriously: Time-Series-Cross-Section Analysis with a Binary Dependent Variable', *American Journal of Political Science* 42 (Oct. 1998), pp.1260–88.

3 For a more complete explanation and/or replication information, please contact the authors. These results were generated using the postgr3 command in STATA 8.0 and Clarify. For more on Clarify, see Gary King, Michael, and Jason Wittenberg, 'Making the Most of Statistical Analyses: Improving Interpretation and Presentation', *American Journal of Political Science* 44/2 (2000) pp.347–61.

4 Also, while we lack the space to fully describe this issue, a replication of Biddle's results in Table 8.2 and Table 8.5 with robust standard errors, appropriate due to the variance in the dataset, makes the F test of relationships in the model insignificant. This means all of the relationships described by Table 8.2 and Table 8.5 could be spurious and deserve more investigation.

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