

De Gaulle, Europe, and Counterfactual Baselines in Historical Explanation

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INCOMPLETE AND VERY ROUGH DRAFT
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APOLOGIES FOR A HALF-WRITTEN PAPER

Prepared for presentation at conference of the European Union Studies Association, Miami, FL,
May 4-7, 2017.

As we pass the 60th anniversary of the Treaties of Rome, the European Union (EU) is beset by crises: the prospect of “Brexit,” tensions over refugees, ongoing fears about sovereign debt, and rising Euroscepticism. Should we be surprised? Perhaps not. The EU is one of the most audacious projects in history, and has realized its ambitions to such a degree that challenges could be expected. It set out to end war in a fractured continent, and so succeeded that Europeans no longer feel an existential need for it. It aspired to build a free-flowing Single Market, and went so far that it hamstrung its options to manage migration and provoked sensitive distributional issues. It reached for a single currency, and achieved a partial federalization of macro-economic policy that was predictably vulnerable to external shocks and internal divisions. It expanded territorially beyond its original champions, adding ranks of more diverse and less committed members, and so created the potential for Brexit (and Hungarexit?). In a world still defined powerfully by the nation-state, the surprise is how far the EU has come—not the fact that it confronts obstacles and problems.

That many European observers express the opposite—how could this be happening?—presumably reflects the EU’s long run of success. With the exception of a lull in the 1970s, many today look back on EU history and see a steady roll toward “ever closer union” and wider membership from the 1960s through the early 2000s. To better calibrate our expectations for Europe, it is instructive to return to the beginning of that period, and the uncertainties that surrounded the emergence of what was then the European Economic Community (EEC). In 1957 when the Treaties of Rome were ratified in France—the ostensible leader of the EEC project—economic crisis made implementation look unlikely. Only thirty French deputies showed up personally to cast their votes. And that was before France descended into near civil war over the prospect of Algerian independence in 1958, forcing the political elite to summon wartime Resistance leader Charles de Gaulle out of retirement to hold the country together. A sharp nationalist critic of the supranational “community” project, de Gaulle had every excuse to delay the EEC deal or even scrap it entirely. Explaining why he did not—instead leaving a consolidated EEC when he left power a decade later, with the potential to pursue “ever closer union”—is a critical question for how we understand the subsequent arc of EU history. It may also hint at what we should expect amid today’s renewed uncertainty.

Happily, the past two decades have seen an extensive running debate about de Gaulle and the origins of the EU, involving both historians and methodologically-explicit social scientists.

While complex debates in historical explanation rarely produce uncontested lessons in any simple sense, we might at least hope that this scholarship would offer clear claims about how the EU project made it through this challenging period, along with a variety of clearly-stated approaches to methods and evidence that allow us to decide for ourselves which claims to favor. Was something like the EEC quasi-inevitable, being propelled by major structural forces at the time? If so, in our own period we would presumably understand the EU's troubles as reflecting changes in those propelling forces, or perhaps some process in which the EU became divorced from them. Or was the EEC's consolidation in the 1960s a highly contingent outcome, depending perhaps on a surprising intersection between de Gaulle's distinctive goals and an inherited EEC treaty that he would never have created himself? If so, we might see opposition to today's EU as less fundamentally new or surprising—relating to challenges that existed all along—but also perhaps more surmountable. An audacious project that survived major challenges due to an intersection of leaders and institutional inheritances might do so again.

Unhappily, such clear claims are not to be found. Some scholars certainly announce a preference for a structural quasi-inevitable view of de Gaulle and Europe, and others prefer more contingent accounts. But even in a debate among prominent experts that many characterize as exemplary for its combination of clashing hypotheses, clear methods and rich evidence, it turns out to be very difficult to find *any clear explanatory claims at all* about why the EU project made it through the 1960s. A variety of historians defend relatively contingent accounts centering on de Gaulle's distinctive ideology and other complex historical intersections, but they do not ever really tell us what conditions caused what difference in what. This is surely related to historians' common reluctance to use causal-explanatory language—but unfortunately the same is true of social-scientific contributions to the debate. In particular, Andrew Moravcsik, the single most widely-cited political science theorist of European integration, spills more ink on de Gaulle and Europe than on any other topic in his career. He asserts in very strong terms that he brings new causal-explanatory rigor to the topic. And yet he too fails to advance any clear arguments about what conditions caused what difference in what during this period. I do not mean that he fails to make causal-explanatory claims compelling; I mean that he does not even *state* comprehensible causal-explanatory claims to begin with.

How can this be? The answer has to do with inattention to counterfactuals. Causal-explanatory claims are about things that make a difference in the world, and in a historical case

differences have one factual endpoint and one counterfactual endpoint. Even political scientists from very different methodological orientations agree that clear causal claims thus depend on counterfactuals. Referring to statistician Paul Holland's labeling of this challenge as the "Fundamental Problem of Causal Inference," King, Keohane and Verba's methodological opus repeatedly tells us that we must "define the counterfactual conditions making up each causal effect very precisely" (1994: 89). Some of the most prominent qualitative critics of KKV's guidance, Gary Goertz and James Mahoney, similarly portray explicit counterfactuals as central to their enterprise: "the qualitative response to the Fundamental Problem of Causal Inference is to use general knowledge and within-case analysis to analyze counterfactually what would have happened if X had assumed a different value in a particular case" (Goertz and Mahoney 2012: 117). Yet there is literally no sustained and comprehensible attention to counterfactuals in the de Gaulle debates—even from Moravcsik, a student of Keohane who himself has stressed "the critical importance of explicit or counterfactual comparisons among theories and cases" (1995: 616). Without counterfactuals, both Moravcsik and his interlocutors are trapped on the single thread of factual history. They tussle over what appears to have motivated de Gaulle and others to move along that thread at various points. But the debate cannot speak to the differences anything made. It is silent on why history was on that thread rather than others.

Not only is this problem especially vexing for social scientists like Moravcsik who proclaim the goal of tight claims about necessary or sufficient causes, it is also especially challenging for the kinds of major structural or institutional theories that social scientists tend to favor. Though it may seem at first glance that contingent, leader-focused history is most dependent on playing out explicit counterfactuals—what if de Gaulle had a heart attack?—the reverse is actually true. The main claims of big structural and institutional theories are more directly counterfactual. After all, no one disputes that major events in EU history *did* pass through de Gaulle and many other proximate conditions. The more general and parsimonious a theoretical explanation, the more factual conditions it evacuates—and so the more its immediate claim is a *counterfactual* assertion that *the same outcome would have resulted even if many things had been different*. For example, to claim that economic conditions drove France to build the EEC is first and foremost to imagine *counterfactual* historical processes shorn of de Gaulle and many other factual elements. By contrast, more contextual and contingent interpretations of political events—like

those featuring individual leaders—hew closer to factual history in a simple sense. Their causal-explanatory claims follow more closely on *the way things actually happened*.

This is not to say that big theories need counterfactuals and contingent, leader-focused accounts do not. Any causal-explanatory claim about difference-making needs a counterfactual baseline. It is to say, though, that the counterfactuals upon which most social-science theories most directly depend are often more ambiguous than the “plausible world” counterfactuals implied by more contextual and proximate accounts. It is relatively easy to imagine de Gaulle dying and who might have replaced him. We can look at the factual careers of alternative leaders and actual coalitional dynamics to consider what others might have done in de Gaulle’s place. It is considerably harder to say what difference was made by economic conditions. Even speculating directly about that difference seems to require “miracle counterfactuals” that we find challenging to imagine or trace: what would have happened if the structure of the French countryside had been different? Current best-practice advice on counterfactuals counsels against such miraculous speculation, but that leaves us in a bind. Is it literally impossible to formulate a clear claim about a difference that economic conditions made in a historical case?

My view is that there is no reason for despair among social scientists interested in historical explanation, nor among EU observers interested in lessons from its past. With bolder reliance on more explicit counterfactuals we can produce useful causal-explanatory claims about historical cases. This drafty (and incomplete) conference paper first discusses literature on causal explanation and counterfactuals to set up these methodological concerns. Then it examines recent debates about de Gaulle and the early EEC to highlight the ambiguous causal claims in their historical explanations. It concludes with advice on more extensive use of explicit counterfactual baselines in historical explanation, and a bit of speculation about what such practices might tell us about EU history.

The mystery of the missing counterfactuals

To begin, consider how social scientists see the nature of causal-explanatory claims and the role of counterfactuals within them. My first key observation is that the dominant traditions in social-science explanation tell us that explicit counterfactuals are critical to causal-explanatory claims. I will focus on the two dominant positions in the literature, both of which depart from Holland’s

labeled the “Fundamental Problem of Causal Inference”: a causal-explanatory claim is about the difference something makes, and one of the two endpoints of this difference is always counterfactual (Holland 1986, 947). One major position is rooted in an orthodox, “regularity” view of causal explanation that tends to be favored by large-N or “quantitative” researchers. Again, the KKV book that is its most prominent statement holds that solving the Fundamental Problem requires us to define counterfactual conditions “very precisely” (1994: 89). The other major position is rooted in a more recently-elaborated “mechanisms” view of causal explanation that tends to be favored by small-N or “qualitative” researchers. Its most prominent statement, the 2012 volume *A Tale of Two Cultures* from Gary Goertz and James Mahoney, centers its solution to the Fundamental Problem on analyzing “counterfactually what would have happened” given different conditions (Goertz and Mahoney 2012: 117).

My second key observation, which we will see played out in the de Gaulle debates, is that neither camp actually seems to follow this “fundamental” advice when they turn to empirical work. Instead they claim to offer historical explanations while paying no real attention to counterfactual baselines. The consequence is that they fail to state comprehensible causal-explanatory claims *by their own criteria*. This section discusses how scholars in both main positions find themselves in this predicament. Unfortunately I cannot resolve the mystery in a very satisfying way. It seems that they have simply overlooked this fundamental problem.

To see how this works, I provide a quick primer on views of causal explanation and their relationships to counterfactuals. In David Brady and Jason Seawright’s excellent summary of these debates (2004), there are four main views. We will see that for the key questions considered here they collapse into two that reflect the KKV and Goertz/Mahoney perspectives.

The oldest and most orthodox view of causal explanation is a neo-Humean *regularity* view. It sees A causing B given cross-case correlations of “constant conjunction” in which A exists independently from B, precedes B in time, and A is always followed by B (Hume 1975[1748]; Hempel 1942; Beauchamp & Rosenberg 1981; Lieberson 1992). That is, we see the difference A made by contrasting factual cases of its presence to factual cases of its absence. Holland developed this position in statistics, creating what is known as the Neyman-Rubin-Holland view. For Holland, the “fundamental problem” does not call for explicit counterfactuals, but for attention to factual cross-case regularities. The two solutions he considers are experimental—varying causes under experimental conditions to get close to reproducing (factually) our

counterfactual baseline—or statistical, in which we estimate average differences across otherwise-comparable observable cases that vary on the cause in question (Holland 1986: 946).

The *counterfactual* view of philosopher David Lewis (1973) and the regularity view are generally seen as “close relatives” (Brady and Seawright 2004:47). Lewis began from problems with the regularity view—some conditions meet its requirements but are clearly not causal, like movement in a barometer that does not cause a storm—and solved some of them by holding that A causes B given the truth of “If A then B,” and “If not A then not B,” in otherwise-similar worlds (and the later addition of “If not B then A may still occur,” to establish causal direction; Collins, Hall & Paul 2004). Despite its label, Lewis’s view is usually seen as a logical way to clean up neo-Humean regularity views, not as a call for making explicit counterfactual claims about how a given case would have turned out differently. Lewis and related scholars have not paid much attention to methodological advice about how to put their definition into practice for empirical research (Collins, Hall & Paul 2004: 20-21), but they seem to favor the notion that we just use *abstract* counterfactuals to help us evaluate causal claims against factual cross-case regularities. That is, rather than asserting an explicit counterfactual baseline that something would have turned out differently, we do counterfactual thought experiments to help sharpen a Holland-style search for constant conjunctions across factual cases.

The *manipulation* view of scholars like Huw Price, Jim Woodward and Judea Pearl began from efforts to solve enduring problems unresolved by Lewis, like situations where one cause “preempts” another (what caused the death of the man in the desert with a hole in his canteen of poisoned water?) (Pearl 2000; Woodward 2003; Beebe, Hitchcock & Price 2014). The core of this position is that most such problems can be solved by experimental manipulation, which allow us to zero in on what causes what. With respect to empirical applications that could be relevant to social scientists, however, this view too appears to come back around to Holland’s position. Where true laboratory experimentation is possible, the manipulation view proposes that tight controls could define our counterfactual baseline in a single case; we claim to know exactly what happens without the treatment. But in the social sciences (outside of psychology), we have almost no true controlled experiments. At best we have “field experiments” that use randomization across many instances, not tight controls, to identify statistical average effects of an intervention. In this sort of manipulation-based method, the counterfactual baseline is once

again established just by averaging observations across (randomized) factual non-treatment cases. No explicit counterfactuals are necessary.

More distinct from these three approaches is the *mechanism* view of causality. Its champions argue that causation does not lie in any pattern of constant conjunctions (factual or counterfactual, observed or manipulated) but in identification of mechanisms that lead from cause to effect, and process-based evidence of their operation (Scriven 1975; Cartwright 1983; Glennan 1996; Hedstrom & Swedberg 1998; Machamer, Darden & Craver 2000; Faletti & Lynch 2007). While the other three views ultimately rely on cross-case correlations for causal inferences, this one rests on “within-case inference” from tracing processes in a given case. Its practitioners do so by pointing to “causal process observations” (Collier, Brady and Seawright 2004) that link non-comparable bits of data (Gerring 2007, 173) to make a case that certain distinct mechanisms generated an outcome. Like a detective trying to trace one crime to one perpetrator “beyond a reasonable doubt,” we posit imaginable mechanisms and look for the full chain of steps they imply in the historical record. The steps or bits of data are non-comparable—a knife on the floor, a footprint in the bushes, a fight at the faculty party the night before—but they make sense as “hoop tests” or “smoking guns” for a distinctive mechanism (or set of mechanisms). It is within this logic that we come to reliance on explicit counterfactuals. If we claim to be able to draw causal inferences from one case outside of the tightly-controlled conditions of a laboratory, we must explicitly posit a counterfactual claim to specify the difference something made in that case. Sometimes these counterfactual baselines may be obvious—without the hole in her chest, the department chair would not be dead—but usually they will not. To argue, say, that the entrepreneurial leadership of Jacques Delors had some causal effect on the outcome of the Single European Act, we would need to say something about how things would have turned out without Delors (or without some specific move by Delors).

Consider next some ambiguities in where these underlying philosophical positions leave the main methodological gurus of political science. From the preceding summary it seems that Goertz and Mahoney’s commitment to explicit counterfactuals should be strong. They espouse a mechanisms view of causal explanation and define their solution to the “fundamental problem” around counterfactuals. Yet this commitment is hard to see in a deeper look into the counsel and empirical work of top scholars on qualitative process-tracing. Though Goertz and Mahoney’s chapter directly on counterfactuals seems unambiguous, the rest of their book (and the vast

majority of related literature) focuses on specifying causal claims as either *necessary* or *sufficient* for an outcome (or both). Nowhere in this literature can I find a discussion, or even mentions in passing, of the need to specify, “Necessary or sufficient for *what?*”—which is the question of difference-making for which, on their own account, we would need explicit counterfactuals. It seems to be taken for granted that we can imagine what would have happened in the absence of a certain necessary or sufficient condition. A subtle way in which this same ambiguity comes up in the related literature on process-tracing is in repeated assertions that even the best process-tracing does not speak well to “how much” questions; it may tell us if a condition was necessary or sufficient, but not how much of a difference it made (George and Bennett 2005: 25). This assertion sits extremely awkwardly with the notion in this literature that process-tracing is “good at exploring many...aspects of complex causality” (ibid, p. 10) and with acknowledgement of the “fundamental problem”: if a causal claim is defined as a statement of difference-making, then a claim that cannot speak to “how much” simply isn’t a causal claim at all.

If leading qualitative scholars’ position on explicit counterfactuals in historical explanation seems ambiguous, the position of leading quantitatively-oriented scholars seems truly mysterious. From the preceding summary we might conclude that KKV do not actually mean “spell out explicit counterfactuals” when they write, “define the counterfactual conditions making up each causal effect very precisely.” They just want us to employ counterfactual thought experiments to help identify real cases for comparisons along Holland’s lines, and perhaps causal or control variables within them. The problem with this conclusion, however, is that KKV ostensibly wrote their book to address “causal inference in qualitative research” for studies which tend to “have a small number of cases” (1994: ix, 4). Their target audience consists of scholars who presumably can neither set up experiments nor seek cross-case averages to construct counterfactual baselines. Moreover, at least one part of the KKV triumvirate—Keohane—has done largely small-N work, and has famous students who make strong causal-explanatory claims in historical explanation, including the EU-history specialist Andrew Moravcsik. Keohane applauds the results in Moravcsik’s main book, saying it “combines social science theory with extensive empirical research to produce the most compelling and significant analysis yet of the European Community.” Helen Milner, another methodologically-sophisticated, highly successful Keohane student who has done small-N work, calls Moravcsik’s

book “quite possibly the best historical analysis of European integration extant.”¹ Thus it does not seem that these scholars could mean that we can only define counterfactual baselines against large-N regularities. Without explicit counterfactuals, how could work like Moravcsik’s address the “fundamental problem” at all?²

The same mystery arises in explicit work on counterfactuals from large-N-minded scholars, most notably James Fearon’s landmark article (1991) on counterfactuals in political science (which is cited approvingly by both KKV and Moravcsik). His whole discussion seems to be about the legitimacy of employing a “counterfactual case strategy” that spells out explicit counterfactuals to address the “fundamental problem.” But consider where he arrives in a crucial passage on what actually makes a counterfactual baseline credible:

In the counterfactual case strategy... frequencies of associations cannot be meaningfully assessed. They are arguably irrelevant in any case, since the researcher is attempting to perform the perfect experiment, in which everything but the test factor is equal. Instead, support for a causal hypothesis in the counterfactual strategy comes from *arguments* about what would have happened. These arguments are made credible (1) by invoking general principles, theories, laws or regularities distinct from the hypothesis being tested; and (2) by drawing on knowledge of historical facts relevant to a counterfactual scenario.

Point (1), together with a footnote citing Lewis, seems to suggest that Fearon—like Holland—ultimately sees counterfactuals as thought-experiments that still rest empirically on appeals to cross-case regularities. They are not really concrete assertions about something that would have happened in a historical case. But point (2) seems to take more of a process-tracing, mechanisms-style direction that actually calls for explicit empirical counterfactuals. And for an example following this passage, Fearon features Stephen Van Evera’s argument about what would have happened had the “cult of the offensive” that provoked a rush into World War I been replaced by beliefs that defense would prevail (Van Evera 1984). The only general point in Van Evera’s counterfactual invokes “general rationality” to trace out the rational consequences of beliefs in defensive advantage in the pre-war context. Rationality is not an empirical regularity,

¹Both comments on the jacket of Moravcsik 1998.

²Include somewhere in this paragraph mention of King and Zheng 2006; 2007, also cited in G/M.

of course, but a theoretical assumption. The rest of the argument historically reconstructs chains of events that would have followed given different beliefs. From this example, then, it appears that Fearon (and Van Evera) accept that we can support counterfactuals without much (or any) reference to cross-case empirical patterns. As Fearon’s language of relying on “arguments” rather than “frequency of associations” suggests, we can judge counterfactuals by how persuasively they combine plausible process-tracing mechanisms and factual evidence about things that were present or possible in the very same case. He reminds us that the main point of doing so is to “define the range of variation that the analyst accounts for...” (1991: 172).

In sum, both qualitatively-minded and quantitatively-minded scholars seem to point right at the notion that explicit counterfactual baselines are logically critical to any historical explanation in a single case. Their own logic and language suggest that any account that does not specify a counterfactual baseline for a given causal factor—sketching what difference it made in what—would simply not be speaking comprehensibly to causality at all. The next section shows that prominent accounts of de Gaulle and Europe fall into this trap.

The missing counterfactuals in accounts of de Gaulle and Europe

In addition to being highly consequential for our understanding of the history of the EU, and perhaps its current challenges as well, the topic of de Gaulle and Europe is marvelously suited as an example for this article’s methodological ambitions. In a 105-page series of articles in 2000, Andrew Moravcsik launched a social-science broadside at the vast historical literature on de Gaulle and Europe. Part of his agenda concerned methodological explicitness, announcing his analysis as a model for “the proper use of qualitative methods in explaining contemporary history” (Moravcsik 2000a, 2000b, 2000c). Even historians who criticized his work praised his “wonderfully clear” causal claims (Trachtenberg 2000: 101). Rarely have social scientists and historians engaged in such debates to explain particular choices and policy outcomes in empirical detail with explicit reference to theory and methods. Moravcsik is also arguably the most prominent living scholar of the EU (with over 28,000 cites on Google Scholar), and a regular presence in prestigious debates about methods in political science. I begin by summarizing the roughly-dominant view of de Gaulle among historians—which also lets me provide a quick

summary of the basic story for less familiar readers—and then turn to the debate Moravcsik launched and the (non)place of counterfactuals within it.

The traditional view of de Gaulle and Europe

In over 2,000 works on Charles de Gaulle, the fascinating Frenchman is typically portrayed as holding a very distinctive ideological vision of France's position in the world. He believed that political legitimacy flowed solely from the independent nation-state. Leaders of great nations, then, must focus above all on national power and prestige (*grandeur*). In postwar Europe, the most basic foreign-policy position of a conservative like de Gaulle was a background assumption of siding with the West in the Cold War. Within that global frame, though, his ideology pointed to three main goals in the European arena: careful assertion of some independence from the hegemonic United States (and their Anglo-Saxon “Trojan horse,” Britain), formation of a west European bloc behind French leadership, and protection of French sovereignty against utopian projects for supranational European institutions.

The “traditional” view among historians that this ideology largely explains de Gaulle's European choices (for surveys, Ludlow 2010; Moravcsik 2012). In 1958 he was summoned from retirement to become prime minister to prevent a French civil war over the prospect of Algerian independence. His legitimacy as a military man and head of the postwar Resistance made him the consensus choice to hold the country together. In European policy, his first challenge was whether or not to implement the European Economic Community (EEC) treaty that had been ratified in 1957—and which he had previously opposed. Fairly quickly de Gaulle decided to accept the EEC, at least for the time being. He did so to avoid alienating West Germany (the crucial ally to form a European bloc), because it provided a platform from which the British had excluded themselves (due, ironically, to their own opposition to supranationality); and because it gave him a way to pressure French business to liberalize. Then he moved to recraft the EEC into a French-led intergovernmental forum. His “Fouchet Plan” proposal tried to subordinate the EEC institutions to regular intergovernmental meetings and to rally his EEC partners to challenge US leadership in NATO. He killed off a British proposal to build a larger Free Trade Area (FTA) around the EEC, and championed an acceleration of internal EEC tariff reductions to further distance the UK economy.

Around 1962, however, it became clear that other EEC members refused to question supranationality and NATO and welcomed Britain's move to apply for EEC membership. His vision foiled, de Gaulle scuttled the Fouchet Plan and vetoed the UK application in 1963. Thereafter his attitude to the EEC became negative. He pushed aggressively to extract economic benefits in the Common Agricultural Policy (CAP) negotiations that had begun in 1961. Repeated threats to walk away from the EEC entirely helped secure very pro-French CAP deals in 1964. Then he launched the "empty chair" crisis of 1965, withdrawing all French participation in EEC until his partners agreed to the "Luxembourg Compromise," which suspended the EEC's treaty's provisions for majority voting and increasing supranational influence. This took the form of a unilateral French declaration, not a legal agreement, but the EEC partners effectively recognized that they would accept French vetoes in order to get back to EEC business.

Overall, then, de Gaulle presided over implementation of the EEC treaty, including an acceleration of its tariff reductions; creation of a supranationally-administered CAP that disproportionately benefited France; repeated failures to alter the legal structure of the EEC institutions; and a successful but informal move to halt the final steps in supranationalization envisaged in the treaty. When de Gaulle left power in 1969, his European legacy was a solidly consolidated EEC, without British participation, and with a legal and institutional framework whose supranational elements could be reenergized whenever future national leaders chose to do so. The traditional view among historians is that his distinctive ideological ambitions led him to first consolidate the EEC in hopes of turning it to his geopolitical purposes, and then that his lack of support from other EEC countries frustrated those purposes and also limited him to stalling (but not undoing) its institutional architecture.

Moravcsik's economic revisionism

Moravcsik entered this terrain as a proponent of a "liberal intergovernmentalist" theory of European integration. In his view the EU can be explained with the same theories that explain most international organizations, and especially the versions of those theories that center on states cooperating over economic interests. He allows that the EU is far and away the most ambitious international organization ever conceived, but ascribes this unusual outcome to the presence of an unusually interdependent set of states whose configuration of economic concerns

were conducive to especially elaborate cooperation. The EU displays “normal politics” in a globalizing context (Moravcsik 1998: 4).

As the foregoing summary of traditional views of de Gaulle suggests, Moravcsik’s account was more revisionist for this step in EU history than for any other. Thus he gave it special attention, expanding his original book chapter in a lengthy set of articles. He does not reject that de Gaulle held distinctive geopolitical views, but argues that these were trumped by pressures and incentives flowing from the French economy: “the pursuit of mundane agricultural and industrial interests, combined with domestic economic reforms, constitutes a *predominant influence on and sufficient explanation of French policy toward the EEC under de Gaulle*” (2000a: 6, original emphasis). The core claim is that de Gaulle confronted rising agricultural surpluses and unruly farmers, and needed above all to secure external markets for farm goods. “Commercial and above all agricultural concerns” overcame both de Gaulle’s aversion to supranationality and geopolitical concerns in French choices during this period (2000a: 23).

For Moravcsik, the agricultural problem so dominated these choices that he can present a single “case” of similarly-motivated decision-making that stretches from de Gaulle’s initial decision in 1958 to implement the EEC treaty to pursuit of a Common Agricultural Policy (CAP) from 1961 to 1965. As key evidence for the primacy of agricultural concerns, he stresses de Gaulle’s willingness to implement a supranational project he disliked in order to get the CAP; as Moravcsik sees it, these concessions show how important the CAP goal was to de Gaulle. Further evidence of its importance, he argues, is the series of threats during the CAP negotiations that France would walk away from the EEC entirely if its agricultural demands were not met. Moravcsik also traces de Gaulle’s other major moves on the EEC to French agricultural positions. A desire for guaranteed, protected agricultural exports drove de Gaulle’s opposition to British accession to the EEC, since the UK wanted to maintain openness to agriculturally-competitive members of the Commonwealth. The need to exclude the UK in turn drove de Gaulle’s decisions in 1960 to veto talks on a wider Free Trade Area (a British proposal), to accelerate the EEC’s schedule of internal tariff reductions (thereby widening the gap between treatment of intra- and extra-EEC goods, above all British ones), and to veto British applications to the EEC in 1963 and 1967.

Moravcsik concedes that de Gaulle’s Fouchet Plan for EEC-based foreign-policy consultations reflected a geopolitical agenda, but underscores that de Gaulle made no

concessions in order to prevent its eventual rejection by his EEC partners. It was a “sideshow” or a “smokescreen” in a CAP-focused policy (2000a: 35; 2000c: 119). Moravcsik also appears to admit some ideological or geopolitical concerns in the “empty chair” crisis of 1965, where the explicit reason for the French withdrawal was to object to proposals for EEC reform that included near-term passage to majority voting. But he argues that the crisis and de Gaulle’s calls for renegotiating the EEC Treaty erupted “perhaps not even primarily” for ideological reasons, but “because [de Gaulle] was concerned that French agricultural gains might be threatened by majority voting...” (2000: 37). Moreover, Moravcsik emphasizes, even if de Gaulle did seek to alter the EEC for ideological reasons, French farmers mobilized to protest the “empty chair” policy during the 1965 presidential election, and thus de Gaulle was soon forced back to the table to the Luxembourg Compromise. In sum, economic interests dictated the French strategy. Together with other countries’ interests, they caused the overall outcome of an institutionally-strong, geographically-limited EEC with a CAP.

Missing counterfactuals and resultant ambiguities

Let me now seek causal claims in this debate, both from Moravcsik and from several historians’ direct responses to his work. I am concerned with specifying what is being claimed in causal-explanatory terms, not the evidence for any particular claim. Moravcsik certainly raises our expectations that we will find clear claims. He announces that “[T]he discipline imposed by social scientific and historical methods—the statement of clear competing theories, the specification of explicit hypotheses, and the careful presentation and balancing of the evidence both for and against each explanation—is the metaphorical equivalent in diplomatic history, albeit admittedly an inexact one, to the DNA testing that identifies criminal suspects” (2000a: 68).

Fortunately for my argument, but depressingly for our discipline, it is easy to see that there are no clear counterfactuals in Moravcsik’s work or from his interlocutors. Consider first the six direct responses to Moravcsik’s articles by historians. One explicit counterfactual comes up. Arguing for the “traditional” ideological view of de Gaulle, Jeffrey Vanke writes, “What if French farmers had been politically disorganized and unable to come to an agreement on what they wanted from the French government? What if de Gaulle’s policies...had been developed in

the absence of pressure from commercial interests and their political representatives? The evidence suggests that de Gaulle would have pursued the same policies” (Vanke 2000: 98). These strike me as significant questions, but Vanke does not develop direct answers. Instead his brief foray into counterfactual terrain goes straight to the crude assertion that de Gaulle would have pursued *the same* policies despite variation in these economic pressures. That sits awkwardly with his more nuanced arguments on factual terrain, which recognize a substantial role for economic conditions. Indeed, he points out (correctly, in my view) that no one suggests that French agricultural exports were irrelevant to de Gaulle’s EEC choices—just that Moravcsik is revisionist in claiming that they explain French policies by themselves (Vanke 2000, 100). But if agricultural pressures played *some* causal role in the story, that seems to mean that de Gaulle’s policies would not have been *the same* given variation in the organization or transmission of such pressures. Does he mean *mostly* the same? The same in certain areas but a bit different in others? We would need more explicit counterfactuals from Vanke to know what difference he thinks made what difference in what.

We might forgive historians for such ambiguities, since they do not typically aim to offer tight causal explanations. Moravcsik’s social-science training, logical language of sufficient conditions, and DNA-testing analogy set a higher bar. Yet his ventures onto counterfactual terrain are... the same. In his 105 pages there is one direct counterfactual claim that is the opposite of Vanke’s: due to economic pressures, “Any other French government of the period would have sought the same objectives” (2000b: 54).³ Again such a crude counterfactual sits awkwardly with assertions that his factual argument is “more nuanced and multicausal” and “more precise and modest” that critics suggest (2000c: 121, 129). Despite his language that economic conditions were “predominant and sufficient,” he clearly does not mean to suggest that *nothing* else could have made *any* difference in what the French pursued. He says so himself immediately after this counterfactual, seeking to specify “what I do *not* assert”: he does not mean

³There are three other semi-counterfactual sentences, but they come across more as artifices of writing than actual consideration of counterfactuals: “If agriculture had been clearly secondary, the General could simply have challenged supranational institutions outright, but the simultaneous pursuit of agricultural integration and British exclusion, while necessary, posed tactical problems” (2000a: 38); “...even if the British had been inclined to concede to the French trade demands in exchange for membership, the British government had no way to provide a credible commitment to permit centralized financing arrangements to be created...” and just thereafter, “...if French opposition alienated the five other governments and undermined the EEC in favor of a British FTA, the outcome would be little worse in economic terms than an EEC that included the British” (2000b: 9).

that de Gaulle himself “made no difference for French policy toward Europe” (Moravcsik 2000b, 54). He stresses that De Gaulle implemented domestic economic reforms that made it possible for France to implement the EEC, and radically revised French political institutions with the new presidential constitution of the Fifth Republic. For Moravcsik, this “French domestic policy shift was, in part, the result of structural economic change, but it also reflected constitutional reform, electoral support, and personal perseverance—each connected with Gaullist initiatives. Internationally, moreover, de Gaulle played a relatively weak hand brilliantly.” He concludes that “a less confident, less skillful leader might have been forced to compromise” (Moravcsik 2000b, 55).

These strike me as reasonable qualifications, but consider where they leave us with respect to causal claims. What made what difference in what? Moravcsik’s only unambiguous claim is that French farmers’ pressures motivated de Gaulle to seek export opportunities, and that any other government of the time would have done so as well. But as Vanke notes, no one argues against this extremely broad notion. The outcomes under debate are far more specific: endorsing the EEC framework, implementing it, sinking the British FTA idea, accelerating internal EEC tariff reductions, launching the Fouchet Plan, and so on. Moravcsik is apparently not saying that agricultural pressures alone would have led any French government of the period to do all these things; he attributes de Gaulle’s ability to implement the EEC to key domestic reforms (themselves founded, he says, on de Gaulle’s electoral support and constitutional changes) and highlights de Gaulle’s inclination to pursue distinctive international strategies (founded, he says, on de Gaulle’s brilliance and confidence). He also argues that several of these moves were only constrained, not inspired or compelled, by economic conditions, seeming to lend credence to Vanke’s counterfactual that de Gaulle would still have pursued some similar policies in the absence of commercial pressures. Without some more explicit counterfactuals, we have *no sense at all* of what difference Moravcsik thinks any non-economic factors could have made. He hints that a “less skilled” leader would have just had to “compromise.” Does that mean getting a less France-friendly agricultural deal? Accepting the British FTA or British EEC membership? At the same time he suggests that other leaders would not have accomplished French domestic reforms. That hints at a more radically different no-de-Gaulle scenario: a France that couldn’t implement the EEC at all. If France could not implement the EEC, it is hard to see how it could have

compelled the Germans to agree to anything like that CAP, or what leverage it would have had to unilaterally sabotage British proposals for broader but less binding trade liberalization.

Let me be clear: the problem is not just a failure to specify concessions to other arguments. It is a failure to specify his *own* argument. Moravcsik's inattention to counterfactuals means that he gives us no meaningfully concrete sense of the difference that economic conditions made. In factual history, whatever role economic conditions played in French choices and European outcomes, they passed through de Gaulle and his various machinations. Moravcsik's own core goal is to draw our attention to the causal consequences of underlying economic conditions shorn of these historical delivery mechanisms—separate from de Gaulle's leadership and what he portrays as other extraneous features around it. Such a deep structural argument occurs substantially on counterfactual terrain, asking us quite directly to imagine the parameters or incentives set by economic conditions not just without a looming megalomaniac French general but also without other rather major things, like an institutional context (particular domestic French institutions, an inherited EEC treaty). To even begin to *state* that argument in a way that makes any sense relative to history—let alone support it—we need a set of counterfactuals that tells us how French economic conditions connected to major variation in French options. As both KKV and Goertz and Mahoney seem to have told us very clearly (though without getting through to others or even to themselves), we cannot speak to causal-explanatory debates without them.

How to use counterfactuals to do better

[Apologies! Remainder of the paper not quite in readable form...]