

Quantitative Empirical Methods and the Context-Conditionality of Classic and Modern Comparative Politics

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Modern scholarly reviewers characterize the prewar and immediate-postwar study of comparative politics as legalistic, favoring categorical enumeration over positive-theoretical analysis of constitutional details, and parochial and, indeed, non-comparative, exhibiting Western (often specifically U.S.) bias in the topics studied and in normative conclusions and rather lacking in theoretical or empirical comparison.

From the mid-1950s, Gabriel Almond (1956) and contemporaries, following a Parsonian approach to social science, led a political-sociology revolution in comparative politics. Sparked by the catastrophic rise of fascism and dictatorship that plunged the globe into war, and by democracy's failure to advance and secure its initial postwar successes, the central question for these scholars was what fostered stable, democratic political development. Inspired by contemporary scientific sociology, they sought answers in the polity's social structure: e.g., its homogeneity and stratification (Almond 1956), its socio-economic development, or the cross-cutting or reinforcing nature of its political cleavages (Lipset 1960). Perhaps most notable about this revolution was the movement it signaled from configurative description toward a positive science of comparative politics that asks research questions (e.g., what contributes to democratic development and stability?) and proposes positive theories (hypotheses) in answer rather than unadulterated parochialism or bias. Empirical evaluation of these positive theories, however, remained depressingly impressionistic and, perhaps, too often as parochial and biased as earlier configurative descriptions had been.

The political-culture and political-behavior revolutions of the 1960-70s completed the movement in comparative politics from configurative description to positive social science. Almond and Verba's (1963) classic *Civic Culture* perhaps initiated and still best-exemplifies both revolutions in following the posing of a positive question – what fosters stable, well-functioning democracy – with logically-argued, positive-theoretical, *hypothetical* answers –

crudely: a citizenry with beneficial cognitive, affective, and evaluative orientation toward the political system – *and empirical evaluation* based on precise, objective measurement of key variables in the argument. *The Civic Culture*, however, suffered a critical limitation that its dependent variable, stable, well-functioning democracy, remained impressionistically measured and for only five nations. Later work in the cultural-behavioral tradition, e.g., Inglehart's (1990) *Culture Shift*, rectified these limitations, cementing the case, begun by pioneers like Karl Deutsch (1971), for the utility of large-N statistical analysis to the positive study of comparative politics.

By the 1980s, social structure, political culture, and public opinion and behavior had become the main sources of likely independent variables in the modern, positive, political science of comparative politics, and statistical analysis of comparative-historical data had become one important tool in empirical evaluation of those positive arguments. However, this tool also enabled scholars to discern that, in fact, social structure determines political outcomes – social homo- and heterogeneity predicts (in)stability (Powell 1982), societal fractionalization and polarization determines that of the party system (Sartori 1976), *etc.* – less fully and surely than previously thought. Spurred by the incompleteness or weakness of such structural explanations, and perhaps by the sometimes-unsatisfying directness and causal-proximity with which culture (beliefs and attitudes), opinion, and behavior link to the outcomes being explained, Sartori, Powell, Smith (1972), Berger (1981), Lehmbruch and Schmitter (1982), and Lijphart (1984) and others returned institutions – political, social, and economic – to center the analysis. Building from earlier institutional work that theoretically and empirically linked, e.g., electoral law to party-system outcomes (Rae 1964) and party and governmental systems to coalition politics (Riker 1962; Dodd 1976), these authors argued socio-economic structure *works through* political, social, and economic institutions to shape the incentives of political actors: voters, workers and employers, policymaking and party elites. Comparative-historical statistical analysis again helped establish these claims empirically, showing that, *in addition to or controlling for* socio-economic-cultural conditions, presidential, majoritarian-parliamentary, and representative-parliamentary institutions affect participation and social and governmental stability (Powell 1982), institutional

structures of labor help determine political-economic performance (Cameron 1984), majoritarian or consensual institutions shape democracies' performance (Lijphart 1984), *etc.*

The modern study of comparative politics, therefore, emphasizes the societal structure of interests, political culture and public opinion, and socio-politico-economic institutions, in explaining the cross-national variation observed over time in political outcomes. In this regard, the field has come full circle. The central tenet of modern comparative politics is, as that of classical pre- and post-war comparative politics was, that *context*—structural, cultural, and institutional—matters. This, of course, is what underlay the parochialism and relative non-comparativeness of pre-scientific comparative politics. It is also foundational to cultural and behavioral approaches, in which the meaning and effect of various objective circumstances and factors depends critically on cultural and socio-psychological context. Likewise in institutional approaches, as Smith (1972) perhaps emphasized first, the effects of societal structures of interest manifest through, are shaped by, and therefore depend upon the institutional structure of the society, economy, and polity. By the 1980s, scholars had also firmly established that statistical analysis of comparative-historical data could help evaluate and fruitfully inform positive theories relating social structure, culture, and institutions to political outcomes. However, the early empirical work on such theories, as hinted above, did not reflect the context-conditionality of the arguments.

As just noted, culture matters, if it does, by modifying the relationships between objective conditions, like poverty and underdevelopment, and outcomes, like democratic stability. Individuals' interpretation of poverty and appropriate responses thereto—so a cultural argument may contend—hinge on cultural symbols and understandings. Likewise, institutions matter, if they do, by altering the relationships between objective interests and the institutionally shaped actions perceived as possible and most-effective by individuals or groups with those interests. For example, the degree to which some polity's cleavage structure will induce leaders to form political parties based on the societal groups drawn by that structure and voters to support such parties depends on the electoral rules and party-systemic strategic-structure that determine the relationships between votes and representation and between representation and governmental power. Complex, context-

conditional hypotheses of this sort are now the hallmark of positive comparative politics; the effect of X (e.g., institutions) on Y (outcomes) *depends on* Z (culture, structure, etc.): formally, $dY/dX=f(Z)$. Empirical work, such as described above, which establishes that institutions matter *in addition to* culture and structure (and *vice versa*) by controlling for the latter in regressions of outcomes on the former, do not reflect this conditionality. They show only that the effect of X (institutions) on Y (outcomes), given or controlling for Z (culture, structure, etc.) is not zero, not that the effect of X on Y *depends on* Z : formally, $dY/dX | Z \neq 0$ not (necessarily) $dY/dX=f(Z)$.

Critics of statistical analysis in comparative politics often cite this concern (*inter alia*) that regression coefficients impose a *constant* effect for each independent variable, albeit controlling for others, not effects that differ depending on context. That is, they object that broad statistical comparison neglects the context conditionality of comparative politics. This criticism, however, applies only to the simplest linear-additive regression. Multiple statistical devices exist to incorporate the context-conditionality of comparative phenomena (and other complexities) into practical models for empirical analyses. The statistical device most frequently used to evaluate theoretical claims of this sort – that the effect(s) on some dependent variable(s), Y , of some independent variable(s), X , depend upon or are moderated by a third (set of) independent variable(s), Z – is the linear-interactive, or multiplicative, term. In the simplest case, one includes X times Z among the regressors. Such interaction terms are hardly new to political science. Indeed, their use is now almost common, yet, especially given current and growing attention to the roles of institutions in comparative politics, they should perhaps become more common still.

TABLE 1: TYPES OF ARTICLES IN MAJOR POLITICAL SCIENCE JOURNALS, 1996-2001

Journal (1996-2001)	Total Articles	Statistical Analysis		Interaction-term Usage		
		Count	% of Tot	Count	% of Tot	% of Stat
<i>American Political Science Review</i>	279	274	77%	69	19%	25%
<i>American Journal of Political Science</i>	355	155	55%	47	17%	30%
<i>Comparative Politics</i>	130	12	9%	1	1%	8%
<i>Comparative Political Studies</i>	189	92	49%	23	12%	25%
<i>International Organization</i>	170	43	25%	9	5%	21%
<i>International Studies Quarterly</i>	173	70	40%	10	6%	14%
<i>Journal of Politics</i>	284	226	80%	55	19%	24%
<i>Legislative Studies Quarterly</i>	157	104	66%	19	12%	18%
<i>World Politics</i>	116	28	24%	6	5%	25%
TOTALS	2446	1323	54%	311	13%	24%

As **Table 1** (from Kam and Franzese 2002) shows, 54% of articles since 1996 in leading political science journals use some statistical methods; 24% of those employ interactions. In the exclusively comparative journals, the figures are 49%/25% for *Comparative Political Studies* and 9%/8% in *Comparative Politics*. All of the other journals, except perhaps the two quarterlies, have significant comparative publications, and statistical analyses comprise from 25-80% of those articles, with interactive analyses representing a relatively fixed 20-25%. Thus, about half of top-journal political-science articles employ some statistical methods, and about one-quarter of those and over one-eighth of all articles use interaction terms (moreover, these shares include formal-mathematical and philosophical political theory in the denominator). Comparative politics, at least judging by *CPS*, *IO*, and *WP*, operates somewhere between the discipline's mean and half that on these dimensions. The trends in comparative politics and the broader discipline are likely mildly upward in both regards, although these last six years of data suggest a plateau being reached.

This widespread and perhaps expanding usage of interactions notwithstanding, still more empirical work should contain interactions than currently do, given the substance of many comparative-politics arguments. Consider, for example, the gist of most institutional arguments. In one influential statement of the approach, Hall states (1986, p. 19):

...institutional analysis of politics... emphasizes institutional relationships, both formal and conventional, that bind the components of the state together and structure its relations with society... [I]nstitutions...refers to the formal rules, compliance procedures, and standard operating practices that structure the relationship between individuals in various units of the polity and economy... Institutional factors play two fundamental roles... [They] affect the degree of power that any one set of actors has over policy outcomes [...and they...] influence an actor's definition of his own interests, by establishing his... responsibilities and relationship to other actors... With an institutionalist model we can see policy as more than the sum of countervailing pressure from social groups. That pressure is mediated by an organizational [i.e., institutional] dynamic... [emphases added].

Thus, in this approach to institutional analysis, and, as I suggested above, inherently in all approaches, institutions are intervening variables that *funnel*, *mediate*, or otherwise *shape* the political processes that translate the societal structure of interests into effective political pressures, and/or those pressures into public-policymaking responses, and/or those policies into outcomes. (Extending the list of synonyms might prove a useful means of

identifying interactive arguments. When one says X *alters, modifies, magnifies, augments, increases, moderates, dampens, diminishes, reduces, etc.* some effect (of Z) on Y , one has offered an interactive argument.) For example, one prominent line of research connects the societal structure of interests to effective political pressure through institutional features of the electoral system: plurality-majority versus proportional representation, *etc.* (e.g., Cox 1997; Lijphart 1994). Another emphasizes how governmental institutions, especially the number and polarization of key policymakers (veto actors) that comprise it, shape policymaking responses to such pressures (e.g., Tsebelis 2002). A third stresses how the institutional configuration of the economy, such as the coordination of wage-price bargaining, shapes the effect of certain policies, such as monetary policy (e.g., Franzese 2002b:ch.4). In every case, and at each step of the analysis from interest structure to outcomes (and back), the role of institutions is to mediate, shape, structure, or condition the impact of some other variable(s) on the dependent variable of interest. *I.e.*, institutional arguments are inherently interactive, yet, with relatively rare exceptions – see, e.g., Ordeshook and Shvetsova 1994, Franzese 2002b:ch. 3, and Franzese 2002b:ch.4, respectively, regarding the above cases – empirical work on institutional arguments have ignored this interactivity.

Another example further illustrates the ubiquity of the interactive implications of comparative-institutional theories. Scholars consider principal-agent (*i.e.*, delegation) situations interesting, problematic, and worthy of study because, if each had full control, agents would determine policy, y_1 , in response to some (set of) factor(s), X , according to some function, $y_1=f(X)$, whereas principals would respond to some perhaps different (set of) factor(s), Z , perhaps differently according to, $y_2=g(Z)$. (For example, the principals might be the current government, responding to political-economic conditions X , and the agents unresponsive central banks, giving $Z=\emptyset$, as in Franzese 1999.) Theorists then offer some arguments about how institutional and other environmental conditions determine the monitoring, enforcement, and other costs, C , principals must incur to induce agents to enact $g(Z)$ instead of $f(X)$. In such situations, realized policy, y , will usually be given by some $y=k(C)\cdot f(X)+[1-k(C)]\cdot g(Z)$ with $0\leq k(C)\leq 1$ and $k(C)$ weakly increasing. Thus, the effects on y of each $c \in C$ generally depends on X and Z , and those of each $x \in X$ and $z \in Z$ generally

depend on C . That is, the effect on y of everything that contributes to monitoring and enforcement costs generally depends on all factors to which the principals and agents would respond differently, and, *vice versa*, the effect on y of all such factors depends on everything that affects monitoring and enforcement costs. Empirical applications of principal-agent models usually seem to have missed this point.

A rough quantification of the magnitude of such principal-agent and other institutional-interactions omissions from empirical specifications is startling. Of **Table 1**'s 1012 articles with non-interactive statistical analyses, half or so offer some sort of institutional argument. Even if only half of all institutional arguments reflect the interactivity I argue is actually inherent to institutions, that alone would imply that almost as many articles, $\frac{1}{2} \cdot \frac{1}{2} \cdot 1012 = 253$, incorrectly employ non-interactive empirical techniques to evaluate interactive hypotheses as actually employ interactive terms (311). If, as I expect is closer the truth, institutional arguments are all interactive and many other arguments (e.g., contextual effects in cultural-behavioral theories), say half of those remaining, are also, that would imply that roughly two-and-a-half times as many articles made interactive arguments but empirically evaluated them non-interactively ($\{\frac{1}{2} + \frac{1}{4}\} \cdot 1012 = 759$) as actually employed interactions.

The theoretical and substantive interestingness of such complex, context-conditionality is readily apparent in comparative political economy. As I suggested in a recent review, for example, venerable electoral- and partisan-cycles may be due a theoretical and empirical revisit to explore the institutional, structural, and strategic conditionality of such cycles:

Policymakers in democracies have strong partisan and electoral incentives regarding the amount, nature, and timing of economic-policy activity. Given these incentives, many observers expected government control of effective economic policies to induce clear economic-outcome cycles that track the electoral calendar in timing and incumbent partisanship in character. Empirics, however, typically revealed stronger evidence of partisan than of electoral shifts in real economic performance and stronger and more persistent electoral and partisan shifts in certain fiscal, monetary, and other policies than in real outcomes. Later political-economic general-equilibrium approaches incorporated rational expectations into citizens' and policymakers' economic and political behavior to explain much of this empirical pattern, yet critical anomalies and insufficiencies remain. Moreover, until recently, both rational- and adaptive-expectations electoral-and-partisan-cycle work underemphasized crucial variation in the contexts – international and domestic, political and economic, institutional, structural, and strategic – in which elected partisan incumbents make policy. This contextual variation conditions policymaker incentives and

abilities to manipulate economic policy for electoral and partisan gain, as well as the effectiveness of such manipulation, differently across democracies, elections, and policies. Although relatively new, research into such context-conditional electoral and partisan cycles seems to offer much promise for resolving anomalies and an ideal substantive venue for theoretical and empirical advancement in the study of political economy and comparative democratic politics more generally (Franzese 2002a: 369).

For example, in small, open economies, domestic policymakers may retain less autonomy over some policies, or some policies may be less economically effective, so that electoral and partisan cycles in those policies and outcomes are less pronounced than in larger, less-exposed economies. Some polities, moreover, concentrate policymaking control in fewer, more-disciplined partisan actors, which may induce sharper cycles in, *e.g.*, Westminsterian than in other democracies. Furthermore, some policies may have more effect and so be more useful and so more used for electoral or partisan purposes, and this too varies with institutional, structural, or strategic context. For instance, the political benefits of demographic versus geographic targeting of spending may vary by electoral system, *e.g.*, single-member plurality favoring the latter and proportional representation the former. These and other contextual variations condition policymakers' incentives and abilities to manipulate policies and outcomes for electoral and partisan gain, and modify the political and economic efficacy of such manipulation, in manifold ways across democracies, elections, and policies, all of which suggests exciting opportunities for interactive models that inform comparative politics. Another obvious locus of interactive effects lies in recent work on *Varieties of Capitalism* (Hall and Soskice 2001) or that on globalization, the comparative political-economy approach to which stresses that the domestic response to international-economic integration varies, depending critically on domestic political and institutional context (*e.g.*, Boix 1998, Garrett 1998, Swank 2002). Similar examples from outside political economy are not hard to imagine. The propensity for (apparent) directional voting *versus* proximity voting in individual electoral behavior, for example, depends on electoral and party systems and the types of government they tend to produce (see, *e.g.*, Kedar 2002).

With so many opportunities, some currently being taken but many as-yet ignored, to explore interactions – indeed, with the logically inherent interactive nature of comparative politics theory – the good news is that quantitative empirical modeling of such context-

conditionality can be quite simple (Kam and Franzese 2002 discuss more thoroughly). First, one must understand empirical models that embody interactive hypotheses. For example, one typical theoretical argument might be that X generally reduces Y and does so more in the presence of or the larger is Z . Note that this is actually two hypotheses: (a) that dY/dX is negative (X reduces Y) and (b) that $d^2Y/dXdZ$ is negative (and increasingly so with Z). In a model containing regressors X , Z , and $X \cdot Z$, such as $Y = \dots a \cdot X + b \cdot Z + c \cdot X \cdot Z \dots$, the interpretation of the results regarding (b) is straightforward. $d^2Y/dXdZ = c$, so the coefficient c simply and directly tells us how the effect of X changes *per* unit increase in Z and, *vice versa*, how the effect of Z changes per unit increase in X . (These converses are logically identical; this is not some unique function of regression modeling.) Thus, the standard t-test on coefficient c corresponds to hypothesis (b). The effect on Y of X , dY/dX , however, is not simply a , nor is the effect on Y of Z , dY/dZ , equal to b ; nor, even, are these “main” effects of X or Z . The effect on Y of X , dY/dX , equals $a + c \cdot Z$ depends, as the hypothesis said, on the value of Z (and *vice versa*: $dY/dZ = b + c \cdot X$). The effects of X and Z each depend on the other variable’s value (again, a logical fact not a regression artifact), and the coefficient a or b are just the effects of an increase in X or Z when the other variable equals zero (which need not be “main” in any way, and could even be out-of-sample or logically impossible). In interactive models, the *effect* of variables involved in interactions do not correspond directly to just one *coefficient*; the effects depend on the values of other variables, exactly as argued. Nor do the standard errors (or t-tests) of these *effects* correspond directly to those of any one coefficient; just as the effects of X and Z vary depending on the value of the other, so too do the standard errors of those effects (see Kam and Franzese 2002 for details). The best approach for researchers presenting interactive results is to graph or tabulate the *effect* of each variable involved in an interaction as a function of the others, along with the standard errors or confidence intervals of those effects (Kam and Franzese 2002 offer spreadsheet formulae to do so). Even with a relatively firm understanding of interactive models, some scholars express considerable reservations over them.

Some note, correctly, that the empirical task to distinguish not just a single, constant effect for X , but one that varies (albeit only linearly) depending on Z imposes much heavier

burden on the data. This is also the substantive meaning of concerns expressed regarding the high multicollinearity (i.e., correlation) among regressors X or Z and $X \cdot Z$ in interactive models. Moreover, one must discard the notion that “centering” the interacting variables (subtracting their means), as several methodological texts advise, eases this empirical task. *Centering alters nothing important mathematically and nothing at all substantively* (Kam and Franzese 2002 offer proof).¹ The multicollinearity concern is quite valid, then. The empirical task that interactive analyses set is *very* demanding, and these demands will heighten dramatically as the number of interactions increase as the complex, context-conditionality of comparative politics suggests they should. However, this concern, too, is unavoidable *logical necessity*. Comparative researchers have four options, each with characteristic perils:

Ignore the context-conditionality of their arguments by omitting interactive terms; judging by **Table 1**, most analysts do this, but this does violence to the inherently (and interestingly) interactive nature of comparative politics and plagues those effects actually estimated with omitted-variable bias and statistical inefficiency.

Reduce context-conditionality by allowing only one or few of the hypothesized interactions in their model; this enables more exclusive focus on those included interactions and reduces the omitted-variable biases and inefficiencies relative to excluding interactions altogether, but it does not eliminate these problems and it ignores the complexity of the context-conditionality in comparative politics.

Constrain the context-conditionality to follow a specific functional form suggested by theory (see, e.g., above regarding principal-agent models; Franzese 1999; Franzese 2002c); this reduces the empirical demand on the data in revealing more of the theorized complex, context-conditionality in comparative phenomena, thereby reducing still further the mis-specification and inefficiency problems of the above two approaches, but many comparative theories may not be sufficiently precise to determine the form of interactions, the gained strength arises from leaning more heavily on theory, and the multicollinearity concerns re-emerge as the allowed-complexity increases, albeit at a lesser pace.

Conduct qualitative empirical analysis to supplement or substitute for quantitative analysis; this may partially counteract the lack of information underlying the multicollinearity problem by enriching the detail and depth of the empirics, but it

¹ Likewise, the oft-raised concern that multiplicative terms cannot distinguish, for example, $X \cdot Z = 12$ with $X = 3$ and $Z = 4$ from $X \cdot Z = 12$ with $X = 2$ and $Z = 6$, is of little import because the model, that is the model of the effect of X and Z on Y , can and will distinguish those cases insofar as they actually do differ logically. Incidentally, under the heading misleading common admonitions, that one should include both X and Z if the model contains an $X \cdot Z$ term, is usually a highly advisable philosophy-of-science guideline (a mathematical application of Occam’s razor) but is neither a logical nor a statistical necessity.

typically enhances the quality of the information thusly at the cost of severely reducing the quantity (see, e.g., King et al. 1994 for advice on making such trades optimally) and the ability to discern complex interactions qualitatively, i.e. without precise numerical measurement and statistical control of independent variables is inherently more difficult.

As is perhaps clear, I advocate the third of these options. Ultimately, the problems raised by complex context-conditionality are logically inherent, so qualitative recourse cannot evade them and the other two options evade them only to the degree they suppress the (interesting) conditionality. To see the promise of this third² approach, return to the principal-agent (*i.e.*, delegation) case described above. Generally, in such situations, we argue that, if each had full control, agents would act according to some function, $y_1=f(X)$, while principals would act differently, $y_2=g(Z)$. We then argue that some institutional and other contextual conditions determine the monitoring, enforcement, and other costs, C , principals must incur to force agents to enact $g(Z)$ instead of $f(X)$. Realized policy, y , will then typically be given by some $y=k(C)\cdot f(X)+[1-k(C)]\cdot g(Z)$ with $0\leq k(C)\leq 1$ and $k(C)$ weakly increasing as noted. If the comparative theory can identify $k(C)$, that is, the function $k(\cdot)$ and contextual conditions, C , that determine the degree to which principle or agent has effective control, and the functions $f(\cdot)$ and $g(\cdot)$ and factors X and Z that state to what and how principal and agent would respond left (hypothetically) completely in charge, and if these functions and/or factors are not identical, then regression techniques (nonlinear regression, see Franzese 1999, 2002c) can gain leverage on *all* the complex conditional effects predicted in that comparative context. Moving beyond delegation to other shared policy-control situations, researchers might also fruitfully apply this approach to study the relative weight in policy control of, *e.g.*, executive and legislative branches in (semi-) presidential systems, or of different chambers in multicameral systems, or of prime-, cabinet-average-, cabinet-median-, and portfolio-ministers in parliamentary systems, or of committees or cabinets and legislature floors or backbenchers or oppositions, or, even, of the degree to which elected representatives act legislatively as if they represent the residents of their electoral district, those therein who support them, or their national-party's or some other constituency. Finally, even more generally, researchers can apply

similar non-linear approaches to any situation in which some factor or set of factors modify the impact of several others proportionately, thereby bringing many more of their highly interactive theoretical propositions under empirical scrutiny than perhaps previously thought possible. Indeed, institutions often operate in this way. For example, institutions that foster greater party discipline may induce legislators to behave less (geographically) distributively and more (class/ideological) redistributively, implying a proportionate modification in their response to a range of political economic conditions (see, e.g., Franzese and Nooruddin 2002 for an inroad). Similarly, institutions that facilitate voter participation tend to broaden the distribution of interests represented in the electorate and so that influence policy, again suggesting the effect of many political-economic conditions on government policies will be modified proportionately by such electoral institutions (see, e.g., Franzese 2002:ch.2).

The approach is not panacea of course. It does require that researchers know well how policy would be determined under the hypothetical extremes of key parameters and that the inputs to these policy-response functions vary empirically in sample, and it gains empirical leverage and produces truly revealing estimates of those parameters only to the degrees they do so. Still, many important substantive problems in comparative politics, and in positive political-science more generally, involve similar complex, context-conditional relationships, and this approach seems to offer a more theoretically, methodologically, and empirically promising way to address those issues than the alternatives.

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