A Great University Makes for a Great Department

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In the March 2004 *Perspectives on Politics* Gabriel Almond, Kristen Renwick Monroe, and Michael Neblo discuss what made the Chicago Political Science Department of the 1920s great.[1] I believe that my distinguished friend and erstwhile colleague, Gabriel Almond, set this important discussion off on a misleading note by talking about a Chicago school of politics, thus inducing an unconvincing discussion of what the leading figures of the department supposedly had in common. The character of the university and the innovative work of its leaders did indeed make the department great. But collective greatness does not in itself constitute a school.

Monroe states that there were two periods—from the 1920s to the 1940s and from the 1950s into the 1960s—in which the Chicago department played an important role in the development of American political science. She properly emphasizes the roles of the leading figures in the first Chicago department ("Chicago1") and makes a heart-warming reference to me as one of the young turks in the second Chicago department ("Chicago2"). But she does not sufficiently emphasize the role of the university itself.

It is beyond dispute that Chicago1 shaped the discipline in important ways. Harold Lasswell, Harold Gosnell, and Charles E. Merriam were great political scientists. They and their colleagues were a fountainhead of ideas and approaches, and they made the department an exciting place to be, as I know from the accounts of many friends who were there. They did not, however, constitute a school: there was no organic connection in their intellectual activities.

Lasswell, Gosnell, and Merriam could have done their most important work if they had never heard of pragmatism or if they had not been advocates of reform (as distinguished from moral analysis). Gosnell was a pioneer in the application of statistics to politics. Lasswell was the most seminal figure the profession ever produced. Merriam was a

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great organizer and promoter. But the effort to find common characteristics—to make of them a *school*—does not work.

John Dewey and pragmatism influenced Chicago1; positivism left its mark on Chicago2. In both cases the influence signifies nothing more than the propensity of a profession or field of inquiry to latch on to the latest scholarly fad. Dewey was neither a systematic nor a rigorous thinker. The research carried on in the Chicago1 department could, and likely would, have occurred in the absence of his influence. Positivism became influential in political science in the fifties, but it had long been subjected to damaging attack by philosophers such as Morris Cohen, on whom I wrote my dissertation in 1951. Despite Willard Van Orman Quine's demolition of Rudolf Carnap in that same decade—a demolition so decisive that leading positivists such as my friend and occasional colleague Carl Hempel regretfully abandoned the position—positivism became a mantra of much of the profession. In any case, the fact that groups of individuals are subjected to the same intellectual forces has little to do, at least not directly, with what—if anything—they attempt to project collectively.

Chicago1 flourished because it was led by innovative scholars, but even more importantly because it was immersed in the University of Chicago culture—a term that, unlike school, does not imply common intellectual orientation, though it does intimate the organic nature of the entity. I became familiar with that culture in 1955–56 at the Center for Advanced Studies in the Behavioral Sciences, where I wrote *System and Process*. The Center replicated, and even deepened, the Chicago culture because Ralph Tyler, who had been a dean of social sciences at Chicago, established a regimen that encouraged cooperative activities among scholars from diverse fields, from literature (Wallace Stegner) to mathematics (Howard Raiffa).

In the period Monroe cites, and I am sure during the Chicago1 period also, faculty members really did talk to, and learn from, one another across both departmental and divisional lines. We were not a collection of parts but an organic enterprise that extended beyond any particular political or philosophical perspective. When we participated in

university decisions, we cared even more about the impact of the decision on the university as a collegial entity than we did about its impact on our research programs and our departments.

I will not evaluate Chicago2, but I will illustrate through several episodes in my own experience how the culture of the university continued to influence its intellectual life. In my first year, 1956, I was coopted into the Saturday lunch roundtable, which included Leo Szilard, Nobel laureate Harold Urey, Nandor Balaczs, and Dick Meier, an organization specialist. We spent hours discussing everything from politics, physics, biology, and anthropology to philosophy. These were not nice discussions. We pounded the table. We shouted at each other. The core group came back week after week because we enjoyed and learned from these fights.

When we participated in university decisions, we cared even more about the impact of the decision on the university as a collegial entity than we did about its impact on our research programs and our departments. For instance, Chauncey Harris, the dean of the social sciences, who was in charge of the Harris Foundation funds, had an interest in international relations. In 1957 he decided that our offerings in that area lacked sufficient expertise on Asian affairs. He directed me to make a search for a young academic with skills in China or Japan whom the foundation should support for an appointment in political science or history that would reinforce the university's international relations program. Eventually I recommended Clifford Geertz, an anthropologist who worked on Indonesia, even though his work was peripheral to international relations and even though his approach to social science was antithetical to mine. Dean Harris supported this recommendation and anthropology made an offer.

During much of the sixties I played a key role in the department as chairman of appointments. I proposed appointments both for Suzanne and Lloyd Rudolph and for Norman Nie, an expert in the use of statistical methods. I thought the distinction the Rudolphs made—shared by many others—between the objective methods of physical science and the *verstehen* approach of Max Weber was misleading. Nevertheless, I recognized that their studies produced penetrating knowledge of Indian politics and felt they would contribute significantly to the overall expertise of the department.

The belief in a dichotomy between objective theory and the *verstehen* approach is replicated in some of the discussion of Almond's article. That some consider this dichotomy to be real tends to damage the collegiality that makes for a great university. Apart from its epistemological deficits, this belief oversimplifies the character of theory; it produces misunderstandings and methodological imperialisms that undermine research and teaching. Those who artificially distinguish between objective theory and the *verstehen* approach tend to understand in terms clouded by contention, regardless of which position one takes, approaches

that contribute to intellectual understanding. If we wish to foster true collegiality, then this false notion of theoretical disjunction must be rejected altogether. To see that this is not only possible but desirable requires an exploration of root causes.

Both Carnap and Wittgenstein, although in different ways, failed to comprehend the contrasting roles deduction and judgment play with respect to objectivity. Carnap's positivism denied the objectivity of moral issues by restricting objectivity to sense data. Wittgenstein, conversely, by failing to comprehend the role of judgment in assessing evidence, denied that competing paradigms could be adjudicated objectively. However, judgment does not test competing premises directly. It makes use of grounds from an evolving realm of knowledge for judging whether claims consistent with a particular paradigm are better supported by evidence than claims resulting from a competing paradigm.

Even though his philosophy excluded the prior dichotomy, the great pragmatist, Charles Peirce, nonetheless further confused the issue in some respects by replacing truth with the concept of meaning. He likely did this because he thought the concept of truth could not be disentangled from its classical Greek roots. But this formulation led to a perceived, if not a real, problem: how can one meaningfully talk about the use of a concept unless one can distinguish between true and false accounts of its use?

I think Peirce knew so precisely what he was doing that he could neither anticipate nor understand the mistakes others made in interpreting his writings. Quine faced a similar problem with his monumental "The Two Dogmas of Empiricism."[2] His colleague Hilary Putnam accused him of denying the concept of logical truth. What he really denied were the implications Putnam attached to the concept.

Quine demonstrated in that deservedly famous paper and elsewhere that there is no foundational ground from which proofs can be derived. In a world in which interpretation, as Peirce taught us, affects all claims, no attribution of use is independent of context. I therefore prefer to distinguish between the Aristotelian and a pragmatic concept of truth. Whereas a definition, if true, is necessarily true in Aristotle's philosophy, the truth of a definition does not implicate its necessity within a consistent pragmatist position. There is no one-to-one relationship between concepts, signs, and referents (or between signs uniquely used and referents, which Saul Kripke and Hilary Putnam call "rigid designation"). Thus, all truth claims vary recursively within a transforming realm of knowledge.

The objective world is one in which the objects of knowledge are recursively transformed by a mind that characterizes them transactionally: that is, the nature of something is revealed by its transactions. Subjectivity is the knowing of that world, not the personal elements that produce characterization within it. These personal elements are themselves

objective when they are treated as objects of knowledge within a recursively transformed realm of knowledge.[3]

Greek accounts of theory and logic were superseded recursively in early modernity because of their perceived limitations. The model of theory in early modernity was that of physics, which the Greek syllogistic model was incapable of comprehending. In physics, the qualities of entities are related to one another in formulas that are supposedly universal and the measures of which are common. Early modern logic was in fact closer to contemporary biological theories. Recent biological theories are cybernetic and involve feedback. Even more recent ones involve the concept of self-organizing systems, in which the rules that govern the behavior of entities may produce a system that is not derivable from any set of formulas.

The model of physics has never successfully been applied to international or political systems because common measures do not exist and assumptions cannot be limited to the relations of qualities. The attempt to mimic universalism in other form, for example, realism in international theory and utilitarianism in moral theory, is truistic at best and misleading in many cases. On the other hand, the essential rules of the "balance of power" system provide a first approximation of considerations that an official who wants to protect the security of a national actor in that type of system should keep in mind, subject to boundary conditions. Considerations of regime change or of adaptation to moral or other considerations require more extensive analysis.

Rich accounts of the workings of particular systems at a particular time and place run the risk of underestimating the role of system. Unless carefully done, they divert attention from comparative analyses that permit judgments of effectiveness in solving problems and of moral evaluation from other than a local position. On the other hand, in addition to providing rich knowledge of how systems work in practice, they may serve to show why suggestions for change from outside the system, unless attuned to a wide range of comparative knowledge, may be not only impracticable but harmful.

If one wants a rich account of the inner workings of a system in a particular period, the method of the Rudolphs or Clifford Geertz is the way to go. If one wants to understand how actor decisions affect system stability and actor security within different types of international systems, then a case can be made for my comparative approach to theory. The two approaches properly differ because the level of analysis differs. Still other types of problems may require still other types of inquiries.

A powerful contemporary department would engage in work and teaching that employed a variety of techniques, that understood where they were useful, and that provided a framework for moral analysis. It would recognize that science is objective, that truth is meaningful even if claims concerning it are uncertain and subject to context, and that a vigorous clash of opinions is a ground for innovation. That department and the university in which it is embedded would discourage the proliferation of isolated baronies.

The department that chooses the former course and that has the good fortune to be part of a great educational institution might recreate the glory of Chicago1. We tried in Chicago2 to build a department that would make a creative contribution to our understanding of the political world of our time, of how it compared with and differed from those of other times and places, and of what processes within it might make it a better place.

Notes

1 Almond 2004; Monroe 2004; Neblo 2004.

2 Quine 1951. A simplification of Quine's paper is provided as an appendix to Kaplan 1976.

3 Recursivity occurs when an experiential flow is subjected to analysis and then resubjected to analysis taking additional material, possibly including characteristics and understandings of the analyst, into account. The recursive nature of truth claims, including moral claims, is argued in Kaplan 2002. (To request a copy of this article, please e-mail the author.) Recursive analysis is used in Kaplan 1989, 59–61, to show that the so-called class of all classes paradoxes are not paradoxes because they have the form of what Aristotle called an analogy by proportion. Because experiential flows are not yet part of the objective world (free will), they are free of causal inference until recursively included in objective accounting. The book includes a glossary that defines many terms consistent with my own philosophical position rather than with those common in social science.

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