# A Rejoinder: Remembrance of Things Past

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I am immensely grateful to Allen Scott for his exceptionally kind words. They mean an enormous amount to me. With his imprimatur, I feel, although now deep into middle age, that I have finally made it.

Allen Scott has been in my life as a geographer since the beginning. He dogged me as an undergraduate. I was required to read his (Scott 1971) book on combinatorics as a first-year student. Then in my final year, I applied to graduate school to study what I thought at the time was an original topic: Piero Sraffa and economic geography. But Allen had got there first. I was not aware that he had until an awkward interview for an ESRC post graduate fellowship with Derek Diamond and Simon Duncan at the London School of Economics (LSE) in spring 1978. "Didn't I know," they badgered me as if this were everyday, common-garden knowledge, "that my topic had already been done by Allen Scott two years before" (Scott 1976)? It did not stop me from doing it again, albeit predictably not at LSE, but at the University of Minnesota, where fortunately the news had not yet reached.

Six months later, and I looked up the exact date, Friday, November 10, 1978, I saw Allen in person for the first time. It was in the breakfast room of the Ambassador Hotel in Chicago. He was eating a bowl of strawberries and looked striking in a puce crushed velvet jacket. We were there to attend the annual meeting of the North American Regional Science Association. As on this occasion of the Roepke lecture, Allen was a discussant. Commenting on Stephen Gale and Michael Atkinson's (1979) paper on fuzzy set theory, he was brilliant although slightly unconventional. He had what looked like a roll of toilet paper on which he furiously wrote notes with a blue Biro as Gale talked. When it was his turn, Allen got up, slowly unwinding the roll as he spoke, providing a dazzling commentary and critique, cutting through arguments with clean-stroke forensic precision. I still remember clearly what he said 32 years later. The late Les Curry, at the time Allen's colleague at the University of Toronto, was impressed, too. "You



88(1):33–36.  www.economicgeography.org

made more of that paper than I thought possible," I heard him say as the two men left the room together at the end of the session.

I hope that same judgment is not expressed about Allen's comments on my article. I fully recognize their incisiveness, relevance, and penetration. I continue to admire Allen's clearheadedness, his logical steel trap of a mind, the chiseled quality of his precise prose, and his ability to grasp and sharply delineate the systematic and the essential from the contingent and the superficial. But to echo Allen, what can an author do who has been criticized other than to put up a defense?

I find Allen's first criticism the most difficult. It comes in two parts: first, in a proximate form, and second in an ultimate form. The proximate form is why should I discuss war when Christaller devised central place theory before any hostilities had broken out? Indeed, Christaller's doctoral dissertation was completed before Hitler even became chancellor in 1933. The ultimate form, and suggested by Allen's reference to "epistemological terms," is why should any scientific knowledge claims backstopped by epistemology be subject to social influence at all, whether it is war or something else? The latter argument was the clearest when Allen drew a distinction between the social purposes to which central place theory is put ("evil" under wartime Nazism, "benign" under postwar Fordism) and scientific theory ("abstract analytical models," to use his term). On the one hand, there is the logic of theoretical knowledge (central place theory), and, on the other, there is the context in which knowledge is applied. For Allen, these two realms do not interpenetrate, but remain separate (this is the upshot also of his remarks toward the end of his commentary).



The distinction that Allen made between scientific logic and its context of application is well known and often made to uphold rationalism. The central thrust of (antirationalist) science studies over the past 40 years, and on which I have relied, is to reject this distinction, undercutting Allen's criticism. The rebuttal is that even the most rarefied, the most arcane, theory or model is born of the social. Theories or models do not begin life as a gobbet of purified logic, "abstract," "analytical," but are from the get-go, social: the social goes all the way down, as Richard Rorty (1989) argued. If this argument is accepted, it then becomes an empirical matter to determine which social influences are most important in the formation of any individual piece of knowledge like central place theory. In this regard, I agree with Allen that I need to do more work to make the case that Christaller's formulation of central place theory directly originated with war. I would make two points in my defense, however. First, the natural scientific sensibility and instrumentalism that Christaller deployed in his famous doctoral thesis reflected a wider German cultural enchantment with science that partly originated with war and the military (World War I was "the chemists' war"). Second, central place theory changed over time, never fixed in aspic. Once it was invented and taken up by the Nazis, it underwent further revision in response to war. As Richard Preston (2009) showed, Christaller introduced a new planning principle in response to his duties in Himmler's office. The larger point is that theory and social context are dynamically related: reality makes one move, theory another, in a never-ending set of shifts and transformation. This is the character of the mangle on which I drew.

Allen's second criticism is that war and the military played at best a marginal role in geography's quantitative revolution. Much more important, he suggested, was postwar urban and regional reconstruction and planning. My countercontention is that geographers practiced quantitative methods not because of the emergence of urban and regional projects that called for their use, but because the discipline had changed as a result of the forces of war, allowing it to produce techniques and theories applicable to such projects. In particular, to participate in postwar planning and urban analysis, geographers needed to carve out an intellectual space within the discipline to allow for the possibility of devising such techniques and theories. That was the significance of Ullman's work at the Office of Strategic Services (OSS), along with Ackerman's and Harris's. It was an embryonic marking out of that necessary space that would later be expanded and populated by the quantitative revolutionaries. I realize that many of the other geographers who were employed at OSS, such as Preston James, Richard Hartshorne, and Joe Spencer, did not change their spots, remaining committed regionalists. But their careers in American geography, certainly the careers of the first two, were established before World War II on that regionalist basis, whereas Ullman's, Harris's, and Ackerman's were not. Because of this vested interest, the former resisted the force of the scientific impress, what Carl Schorske (1997) called the "new rigorism," that increasingly defined American social sciences and some humanities from the end World War II into the cold war (Barnes 2008). By the late 1950s, geography could resist no longer either. As geography joined mainstream social science, it was finally able to participate in those various projects involving postwar urban planning and reconstruction. It was not these projects that caused geographers to be quantitative, but the much longer process of intellectual change toward science, fostered initially by pioneers at OSS and later by that wider movement toward a "new rigorism" that unfolded during the cold war (Barnes 2008).





Allen's third criticism begins with some wariness about my vocabulary that then shifts into an even greater uneasiness about the use of notions of performance. I am not keen on excessive jargon myself, but I recognize that one person's jargon can be another person's everyday prose. I once showed the Preface to my *Logics of Dislocation*, about which I (once) had warm and fuzzy feelings, to a friend who is a real writer (Barnes 1996). My friend scanned the first paragraph, sighed, closed the book, and said, "Too much jargon." As academics, we cannot help ourselves. Allen, have you reread your *Combinatorial Programming, Planning and Spatial Analysis* (1971) recently? Like Msr. Jourdain, we speak jargon without knowing it. But there is a purpose. We use specialized words to get things done. Like pieces of cutlery, jargon allows us to accomplish particular tasks. Of course, we still need to be scrupulously clear in defining the terms we use, but once we are, then I would much rather as a reader see the use of the shortcut "interpellation" than some elegantly phrased but necessarily long-winded ordinary prose circumlocution.

As for performance or performativity, I am attracted to the term precisely because it points to real material consequences of action and, in doing so, raises basic moral questions. Action is not theoretical, but real. Christaller *performed* his central place theory in Warthegau, annexed Poland, bringing into being a different world. Central place theory was no longer a theoretical script contained within a Ph.D. thesis or in a *Habilitation*. It was enacted and became true. Residents were moved out, bulldozers came in, new settlers were resettled. Calling it a performance does not imply superficiality or make-believe. In this case, the performance was deeply tragic, brutally real. Christaller saw in front of his eyes the moral consequences of his actions. Doing so did not make it easier for him to do the right thing. It was very difficult for him to do so. But that is not an excuse, only the beginning of an explanation. Without doubt, Christaller did the wrong thing.

Maybe this is more of a conversation than Allen wanted with me. I hope not, I believe not. In spite of our differences in the past, or maybe because of those differences, we need to talk. This is not so we can forget the past, but so we can bring it into relation to the present. This is the key argument of my article also: we must not forget; we must strive for remembrance of things past.

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