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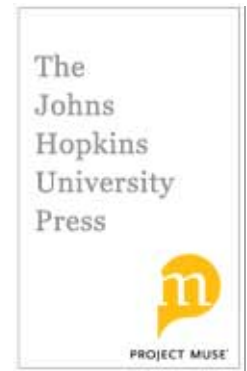
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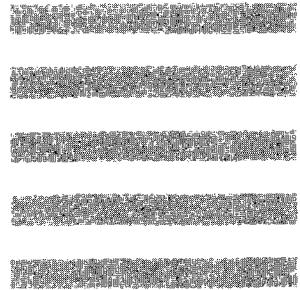
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The Impact of College on Students: The Nature of the Evidence

Ernest T. Pascarella

About five years ago, Pat Terenzini and I were having one of our weekly afternoon phone conversations, and it went something like this:

- P: Got anything big planned for the weekend?
T: No, not much. I'll probably rotate my tires and paint the trim on my porch. How about you?
P: I guess Diana wants me to spray the clover on the front lawn and route-out the kitchen sink. Maybe I'll seal the blacktop in the driveway.
T: Sounds thrilling! Another big-fun weekend in Oak Park, eh?
P: Yeah, right! Well, give my best to Caroline and the kids.
T: Yep, same to your family. Talk to you soon.
P: Oh, by the way, you want to update Feldman and Newcomb?

Ernest T. Pascarella is a professor of educational psychology at the University of Illinois. This paper was the 1990 presidential address of the Association for the Study of Higher Education's annual meeting held in Portland, Oregon, 3 November 1990.

T: Sure, why not?

P: Yeah, how hard can it be?

Well, that's not exactly what happened; but like many of my data analyses, it's almost, pretty close to approximately what happened.

The toughest decision having been made (i.e., deciding to do it in the first place), Pat and I set off on a five-year scholarly journey which has (from our perspective at least) varied somewhere between an exhilarating and perhaps failed assault on Mount Everest and a forced march through five miles of Silly Putty. The completion (or perhaps survival is a better word) of that journey has been realized in a fourteen-hundred-page manuscript which synthesizes the results of about twenty-six-hundred studies about the impact of college on students, conducted primarily in the last two decades.

While it would be blatantly crass to use the ASHE Presidential Address to push our book, I do feel compelled to point out that it will have two potentially important functions that can enhance personal well-being.

1. Jossey-Bass points out that the printed version of the book will be thick enough to stop a .357 magnum slug at thirty paces. I think it was "stop." Maybe it was "slow down."
2. For those of you living in the North, the book can also be tossed into the trunk of your car for added traction on ice and snow.

These two safety features are yours for the price of the book alone. The deal on a set of steak knives for multiple orders fell through.

Well, what about the impact of college on students? I'm not going to review our major findings and conclusions. Instead, I want to talk about the nature of the evidence Pat and I reviewed. I'd like to say some things we didn't say in the book but which I think are important and say some things we *did* say in the book which Pat and I feel deserve some special emphasis. This approach is a combination of substantive and methodological issues.

First let me comment briefly on what the research on the impact of college may have to say to us about higher education research and scholarship generally. It has been argued quite cogently by some of the most respected names in the field that as a scholarly/research enterprise, higher education is a field of study not a social science discipline. I'm not about to argue the point. By and large, it's probably true, and I usually lose arguments of this type. Anything even approaching an epistemological debate with me is not unlike fighting a duel of wits with an

unarmed opponent, me being him.

Nevertheless, let me suggest that higher education scholarship shows substantial variability in the perspectives of higher education scholars themselves. Perhaps as much as any area of postsecondary inquiry, those conducting research on the impact of college have labored rather comfortably and productively within the framework of traditional social science disciplines. Indeed, as a body of evidence, college impact research itself displays many of the trappings of a discipline. These include: (1) a body of enabling theoretical and conceptual frameworks to guide inquiry, which not only suggests how students grow and mature during and after college, but also how to study college effects (e.g., Astin 1984; Chickering 1969; Holland 1985; Kitchener and King 1981; Pace 1984, 1990; Rest 1986; Tinto 1975, 1987; Weidman 1989); (2) a substantial body of evidence—literally thousands of studies. The impact of college on students forms perhaps, the single largest base of empirical investigations in higher education; and (3) some generally agreed-upon concepts of methodological rigor and what constitutes valid evidence of college impact versus the impact of extraneous influences.

While it may be accurate in general to call higher education a field of study, substantial numbers of higher-education scholars concerned with college impact on students may perceive their work as developing within the parameters of a discipline rather than a field of study. We need to keep this potential diversity of perspectives in mind when we try to describe and characterize the nature of postsecondary research and scholarship.

My second observation is that assessments of student change or development may indicate little about the net impact of college. The net impact of college is the term Pat and I use for impacts that can be attributed to college attendance itself rather than other influences. Unfortunately, freshman-to-senior change probably substantially overestimates the net effect of college in many areas where outcomes are developmentally based, such as critical thinking, post-formal reasoning, intellectual development, psychosocial development, and verbal skills. A person usually advances in these and similar dimensions just by growing older—a phenomenon which, unfortunately, usually accompanies college attendance. Disaggregating the maturation effects from those of college attendance is almost impossible in the literally hundreds of studies focusing on change which we read in our synthesis.

Moreover, change during college may not always be a necessary, if not sufficient, condition for the presence of college impact. College may be having an impact even if no change occurs.

This seems illogical: how can college be having an effect if students don't change? I would remind you, however, that perhaps nothing in life is as it seems—except professional wrestling. Consider Lee Wolfle's (1983, 1987) excellent analysis of college effects on quantitative skills. College students in general made little or no gains over high school levels of quantitative achievement; they stayed about the same as when they were high school seniors. Should we conclude that college had no effect? Probably not. Individuals whose education stopped with high school actually lost a substantial part of the quantitative skills they had when they finished high school. Thus, the effect of college was not so much to change people as to anchor their development and prevent significant retrogression.

One might wonder if similar effects hold in the area of political/social values, for example. Some evidence has suggested that recent gains in liberalism, for example, are smaller than in previous college generations. Does this mean that college effects on liberalism are smaller? The answer is that we really don't know unless we see what changes occur in people who don't attend college. In fact, the relative net impact of college on liberalism may be just as strong now as it was in the 1960s or 1970s, even though the changes occurring during college are smaller.

Similarly, just looking at change during college can be quite misleading as a way of comparing the relative impact of college on different subgroups (e.g., gender or race). For example, if men demonstrate greater changes in intellectual development or moral reasoning during college than women, it may reflect different maturational patterns rather than a differential effect of college. To determine whether college attendance has a differential impact across gender, we need to compare the changes occurring in men and women during college with the changes of their counterparts who do not attend college.

The bottom line is that, without a comparison group of individuals who do not attend college, it is quite easy, perhaps likely, to be misled by change or its absence. Studies of change are legion; studies which attempt to estimate the net effect of college are few and far between. If they are done rigorously, these studies are worth their weight in gold. Estimating the net effects of college, not simply describing student change, is clearly a worthy place to put our intellectual resources in the future.

My third observation is that important indirect impacts of college are probably going unnoticed. What do I mean by an indirect effect? Consider the case where a pool shooter sinks the

eight-ball without ever striking it with the cue ball. Instead the cue ball hits the six-ball, which in turn strikes the eight-ball and sinks it. The cue ball has an *indirect* effect on the eight-ball.

The typical analytical and conceptual approaches characterizing the literature on college impacts are concerned with direct, unmediated effects (cue-ball hitting the eight-ball). Indirect effects tend to be ignored (or at least unestimated). Pat and I were quite impressed, however, by how many college influences are, or potentially could be, indirect. This may particularly be the case in the long-term or enduring effects of college.

Consider, as an example, the long-term effects of college on cognitive development. (See Figure 1.) Cognitive development is directly influenced by the intellectual stimulation of your work and also by college attendance. The research has had a hard time showing the net long-term, direct impact of college attendance on measures of cognitive development. However, we do know that young people who graduate from college, compared to those whose education ends with high school, tend to enter jobs with higher levels of ideational content and social interaction. So high-school graduates not only miss the direct intellectual stimulation of college but also tend to hold less intellectually stimulating jobs. In contrast, college graduates are more likely to find themselves in occupational contexts that encourage continuing intellectual and cognitive development over their life span. Thus, through its occupational positioning effects in our society, college may exert an important indirect influence on continuing cognitive and intellectual growth.

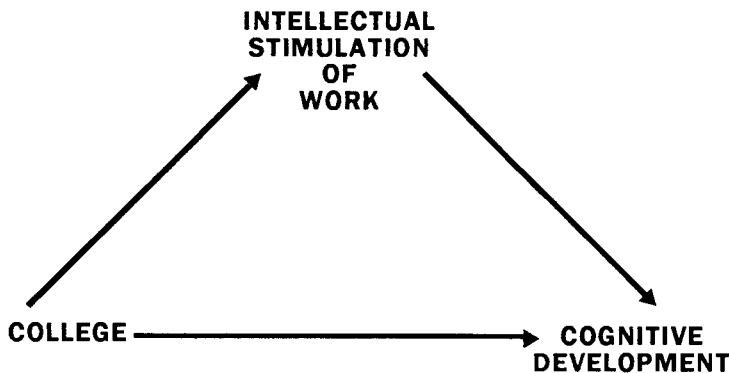


Figure 1. Direct and indirect effects of college on cognitive development

My bet is that we have probably seriously underestimated (and perhaps even misrepresented) the impact of college on students by failing to consider such indirect influences. These influences constitute another important area for future study. You'll also notice that Figure 1 looks like a causal or path model; there is a reason for that. The major contribution of causal modeling (or path analysis or structural equation modeling) is not so much, I believe, in letting you draw nifty diagrams nor in the edifice of statistical esoterica that has grown up around causal modeling. Rather, I believe its great strength is that it forces researchers to consider and to specify *indirect* as well as *direct* effects. Thus, the emphasis is on understanding college impacts as the result of a dynamic web of influences, not simply on predicting what will happen.

My fourth observation is that individual differences among students have not played a major role in guiding research on college impacts. The body of evidence focuses far more on general effects (the average impact of college or college experiences for everyone) than on conditional effects (college experiences' differential impact on different kinds of students). It is almost as if we thought that individual differences don't really count and that everyone benefitted the same way or the same amount from college or from the vast range of different college experiences. The psychology of individual differences should hint differently.

To what extent do differences in gender, race, age, academic preparation, life aspirations, personal learning style, and different patterns of attendance determine the magnitude of college impact or the impact of different college experiences? These are not simply socio-political or ideological questions; they are educational questions and can be addressed empirically. The radically changing demographics of American postsecondary student (about whose college experience we know little or nothing) strongly suggest that we had better get interested in these questions quickly if our research is to be relevant and useful in guiding the development of specific programs and policies.

A fifth conclusion that Pat and I came to is that traditional and publicly accepted indicators of college "quality" (e.g., student body selectivity, prestige measures, educational resources, large libraries, and scholarly faculty) tell us little about the quality and impact of the undergraduate education a student receives. Once student background is taken into account, such college quality indications appear to have small and perhaps, in a practical sense, unimportant net effects on learning, cognitive development, moral reasoning, and various dimensions of personal maturation.

This suggests that real quality in undergraduate education resides more in what we do programmatically than in just what resources we have. If we are to understand undergraduate educational quality, we may simply have to look beyond the obvious and easy-to-measure aspects of institutional wealth and advantage to policies, teaching, curriculum, and programmatic integrity. What we have traditionally called “quality” measures are more appropriately measures of institutional advantage. They look good but may tell us little of substance in terms of educational impact.

A related conclusion in this area is that the net effects of traditional college “quality” measures on an individual’s socioeconomic attainments are very small, perhaps considerably smaller than the common folklore about the so-called “best colleges” would have us believe. Consider income. Our best estimate from a synthesis of over twenty-one studies conducted on different samples is that such factors as college selectivity or prestige may uniquely explain an average about 1 or 2 percent of the differences in individual income. In other words, 98 or 99 percent of income differences may be due to influences other than undergraduate college pedigree. So maybe the world doesn’t end if your kid doesn’t get into an Ivy League school? There’s still an outside chance he or she can go on to live a reasonably full and productive life. Incrementally, it is much more important to complete a bachelor’s degree than where you complete it.

Do I expect this evidence to change many minds about the “best colleges”? Not really. The status rankings of colleges in our country have taken on the trappings of rational myth—compelling social beliefs reinforced by the annual *U.S. News and World Report* on “America’s Best Colleges”—that are not going to be altered by mere evidence to the contrary, even if it is backed up by a good deal of very valid and rigorous evidence. Still in the back of my mind lurks an observation by Joe Stalin, who you must admit knew a thing or two about structuring social environments to achieve desired behaviors: “Facts are obstinate things.” Sooner or later we may need to confront the factual evidence in spite of what we believe or know in our hearts to be true.

My sixth observation is that we are still unclear about the important long-term impacts of curriculum and the teaching-learning process. In other words, the areas we should perhaps know most about are still not mapped very well. To be sure, we know quite a lot about effective teaching (i.e., teaching that enhances subject-matter achievement). Moreover, many of the skills

of effective teaching practice (such as teacher clarity and course organization) can themselves be taught and learned.

Still, some of the most important questions are just now starting to be asked. These include, but are not limited to the following:

1. How do different instructional approaches and course-taking patterns influence not only learned content but also higher order thinking skills, values, personal development, non-classroom experiences, and long-term job/career skills? In short, how does student academic experience influence the broader and longer-term outcomes of college?
2. Are different instructional approaches and course-taking patterns differentially effective for different kinds of students? Or in other words, what are the conditional effects of the college academic experience?

Another important discovery emerging from our study is the importance of macro studies that focus on the big picture. A substantial amount of what we know about college impact has come from creative secondary analysis of large, nationally representative data sets like CIRP, NLS 1972, HSB, NBER-Thorndike data, Census Track Data. Despite the obvious limitations of variable content and construct validity, analysis of these data sets *has* and, I believe, *will* continue to make important contributions to our understanding of college impact. In fact, in the absence of such macro-level analyses, I'd estimate that we would probably know about half or less of what we currently know about the net effects of college attendance, between-college effects, and college impacts on socioeconomic attainments, quality of life, occupational behavior and success, values, and attitudes.

At the same time, I recognize clear problems with the often overly broad and distal measures used in much secondary analysis. Variables such as residence, college major, and college characteristics tend to mask important details about the important social and intellectual subenvironments or climates in which students live and carry on their collegiate lives.

Clearly we need more focused looks at proximal subenvironments (e.g., classrooms, peers, faculty, work) if we are to fully understand the interplay of social/psychological and intellectual forces that shape college impact. Yet, we can't just assume that focused small-sample or single-institution studies will automatically give us the depth and complexity of variable measurement that is often lacking in secondary analysis of large data bases. The literature we reviewed is simply too cluttered with small-sample studies which not only lack generalizability and fail to replicate

across settings, but which also fall short of providing the content and construct validity of measures we frequently attribute to large data sets.

The issue, however, is not one of making a choice between big- and little-picture research. We need both. They can inform and complement each another, one providing generalizability and broad scope, the other providing depth, intensity, and nuance. Most useful will be findings that are consistent across big-picture/little-picture perspectives.

And this brings me to my next point: the importance of the whole picture. Sometimes the whole really is greater than the sum of its parts. A little over twenty-two years ago I was sitting in a cool, dark bar in Okinawa listening to a grizzled old Marine Corps master gunnery sergeant tell stories about the Old Corps. This guy was the quintessential Marine Corps Top Kick, voice like number four sandpaper, nose broken in four places, a face so scarred I didn't know how he shaved, and a stack of combat ribbons that included six purple hearts. Vietnam was his third war, and the Marine Corps was his home.

He told the story of an amphibious landing he made in Korea in 1950. Two hours late and on the wrong beach, he hadn't gone a hundred yards when his platoon was attacked by American planes. He lost contact with his platoon commander, his radio didn't work, his squad was almost out of ammunition, and he had lost his map and compass wading ashore so he wasn't sure where he was or how he was supposed to get where he was supposed to be. His unit was also supplied with three contraceptives per man—although what, exactly, they were supposed to do with them while wading ashore under fire was not entirely clear. Obviously, I thought this must be one of those landings the Marine Corps hadn't carved in stone on the Iwo Jima Memorial. As it turned out, however, my friend was right in the middle of General Douglas MacArthur's Inchon Landing. The Inchon Landing is considered perhaps *the* masterpiece of amphibious warfare: it changed the history of the Korean Peninsula, is studied at almost every military academy in the world, and is considered roughly the equivalent of Hannibal at Cannae or Lee at Chancellorsville.

The moral of this little story is that sometimes the whole turns out to be greater than the sum of its constituent parts. What you see depends on your perspective. When you look at individual pieces of research on college impact, there is a tendency to identify and dwell on the many conceptual, methodological, and epistemological problems characterizing research in higher edu-

cation generally. Indeed, the many recent critiques of postsecondary education inquiry seem reasonably justifiable if one takes a micro view. There are flaws, often substantial, in any single study. When the body of evidence on college impact is considered from a cumulative, integrated perspective, however, the resultant image is often clearer and, at times, elegant.

In this sense, the act of conducting a large synthesis of evidence has reinforced in me the importance of a traditional cumulative view of knowledge. Now, I cheerfully admit to a certain tautology here. You probably don't take on a massive synthesis of studies unless you first believe in the cumulative nature of knowledge. Moreover, after spending four or five years trying to provide a meaningful synthesis for the existing evidence, it would be pretty difficult, perhaps even a perverse act of self-abuse, if you ended up concluding that the body of knowledge was not, in fact, cumulative. Social psychologists have an ironic expression for this: "I wouldn't have seen it if I hadn't believed it."

Still, today I am considerably less impressed with the importance of any single piece of research than with what it contributes to the body of knowledge. I am convinced more than ever that genuine breakthrough studies—those that fundamentally change beliefs, practices, or approaches to inquiry—are extremely rare. To expect them to appear with any regularity in our journals or books is naive.

And finally, I am quite concerned that, as a community of higher education scholars, we give insufficient attention to the unglamorous work of doing puzzle-solving research that adds cumulatively to the body of knowledge about college impacts. You'll forgive me, I hope, if the quantitative/logical positivist in me rears its ugly head. (Try to remember that logical positivism is at worst a disease, not a crime.) I sometimes worry, however, that if higher education scholarship becomes overly fixated with such higher order pursuits as changing paradigms, critiquing the current state of research, and moving the field in new epistemological directions (worthy and necessary though they may be), insufficient attention is going to be paid to conducting the important, meticulous research and scholarship that add in their own modest way, if not to a cumulative body of knowledge, then at least to a finite universe of understandings about the influence of college.

Having sounded this warning, I now want to bring up the all-important topic of methodology. Whether by chance or design, the quantitatively oriented faculty in the College of Education at Illinois tend to be grouped together even though they are in

different departments. I'm in a cluster of offices, which reading from east to west, includes Herb Walberg, Corinna Ethington, John Smart, myself, Amaury Nora, and Larry Braskamp. This is a pretty congenial group of quantitative social scientists. We don't indulge in some of the more blatantly nasty behaviors I've heard of like putting up no-pest strips to keep ethnographers at the other end of the building. Still, I must admit there is a certain dominant climate and culture. To paraphrase a famous song, seldom is heard a non-quantitative word.

Obviously Pat's and my own methodological training and perspectives on social reality and orientations to the study of social phenomena created an intellectual context that influenced how we organized, analyzed, and interpreted the body of evidence on college impacts. In all fairness, logical positivism has almost completely dominated the body of existing evidence and has served us well. The vast preponderance (perhaps 90-95 percent) of what we know about the impact of college on students (and we know considerable) comes from inquiry that is firmly rooted in the hypothetico-deductive, logical positivist, quantitative paradigm. Even while some are delivering funeral orations for its unlamented demise, my sense is that this general approach will continue to serve us usefully for a long time to come.

Having said that, however, I must admit to a very real feeling of personal discontent at how often and how severely we limit ourselves and what we can learn if we depend only on those approaches to understanding that have served us (however admirably) in the past. After spending nearly five years with the research evidence (and grinding out many thousands of multiple regression equations before that), I have come to believe that we have mapped only part of the higher-education landscape with quantitative, logical positivist approaches—perhaps not even the most interesting part. It is my considered opinion that judicious and creative qualitative, naturalistic, or ethnographic approaches may simply be better and more sensitive ways of capturing many of the subtle and fine-grained complexities of college impact than more traditional quantitative approaches. In particular, naturalistic and ethnographic inquiries may be more sensitive than quantitative inquiry, not only to the social and psychological realities created by students, but also to the important indirect and conditional effects (individual differences) central to an in-depth understanding of college impacts. Pat and I anticipate that in the next decade important, and probably landmark, contributions to our understanding of college impact will be produced by ethnographic and naturalistic studies. As an extra treat, such

studies may be more interesting and fun to read than studies based on more traditional social science methodologies. What would you rather read—one of John Thelin's carefully crafted essays or one of my structural equation tables?

Let me make three additional points, however. First, qualitative research should not be thought of as what you do if you didn't like research design and statistics courses in graduate school. Qualitative methods are *not* a less rigorous and intellectually demanding approach to social inquiry. Properly applied, they are every bit as meticulous, painstaking, and rigorous as the most sophisticated and complex quantitative analyses—in many ways, even more so. Moreover, they represent in their own right a topography of understanding that we have yet to map, and perhaps never can, with a functionalist view of social reality. Second, in terms of contributions to understanding college impact, existing qualitative studies have, as a group, been particularly rich in descriptive detail. This has been an important contribution; but to make the next generation of contributions, I urge scholars to move beyond descriptions of what it is like to be in college and turn the powerful analytic light of qualitative inquiry on identifying the multifaceted effects of college—in other words, to focus on explanation not just description.

As an aside, let me say that those who have championed alternative paradigms for conducting inquiry have been quite successful on at least one important level. Fewer and fewer serious scholars steeped in traditions of logical positivist social science would question the value of different paradigms for guiding the conduct of inquiry. What we need now is that inquiry guided by these alternative paradigms actually be conducted so that it can become a significant part of the mainstream literature. Finally, there is not only room but a need for multiple paradigms in guiding future inquiry on college impact. I've always thought that a single dominant research paradigm has been historically characteristic of the logical positivist world of the natural sciences. Where is it written that the study of social behavior must proceed that way? Multiple approaches to inquiry are a particularly healthy trend in studying social phenomena when they are seen as mutually supporting and enlightening each other. After all, the goal is understanding the impact of college, not seeing which paradigm can rub the other's nose in the dirt.

And my final point is a call for theoretical diversity, not theoretical consensus. Research on the impact of college on students has been most highly influenced by psychological theory. This pattern perhaps reflects the predominant approach to

graduate education in U.S. colleges and schools of education. Theories from other disciplines have only recently begun to guide inquiry. With some very notable exceptions, higher education researchers are only now beginning to understand how sociology, anthropology, economics, and related social science disciplines can contribute to an understanding of college impacts.

For example, research in many areas of noncognitive, psychosocial (personality) change tends to interpret changes in students psychologically—attributing change to a developmental or psychological restructuring within students. It is equally likely, however, that such changes may result from a socialization process involving normative attitudes, values, and peer-group behaviors. While discipline-based theoretical frameworks can focus and guide our inquiry, they can also restrict our analytical vision and circumscribe our full understanding of the complexities of college impact. College impact is a complex phenomenon, the contours and topography of which may be completely understood only through an integrated synthesis of multiple intellectual perspectives.

I see our work in attempting to synthesize the research of the last twenty years as a celebration of our field and of the excellent intellectual and empirical work produced by our colleagues and friends. Our work simply could not have proceeded without their willingness to share their work with us and to offer constructive and enlightened criticisms of our efforts. To the fifteen or so colleagues who waded through initial chapter drafts, we are deeply indebted. We owe an even larger debt to Len Baird and Oscar Lenning who read the entire manuscript. And finally, we owe an incomparable debt to Ken Feldman. Ken not only read and critiqued our entire manuscript, he also provided intellectual guidance and encouragement during those times when we were convinced that what we were trying to do couldn't be done. (As he said, "It couldn't be done the first time.") Moreover, his own classic work (Feldman and Newcomb 1969) set the standard for future syntheses of this magnitude. So if we've fallen short in our own efforts, it isn't because we didn't have a good role model.

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