



## Ranking Economics Departments

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## Correspondence

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### Ranking Economics Departments

As explored by Richard Dusansky and Clayton Vernon in “Rankings of U.S. Economics Departments” (Winter 1998, pp. 157-70), such rankings are dependent either on subjective reputation surveys, as in the recent National Research Council (NRC) study, or on publication performance. In turn, the latter type of ranking is heavily dependent on such matters as the choice of relevant journals, whether to weight journal pages and quality (and if so, how), and whether to attribute performance to a particular institution by faculty affiliation at time of publication or by affiliation as of one common date. Economists, of all people, should expect that choices made by authors, on the above decisions as well as on the decision to publish the resulting rankings, will tend to reflect the self-interest of the authors. In particular, one might expect that a particular ranking should tend to show the schools of the authors doing better than in other rankings.

Five recently published rankings of economics departments bear this out. The authors of the five studies are from the University of Texas (Conroy et al., 1995), University of Kentucky (Berger and F. Scott, 1990), Louisiana State University (L. Scott and Mitias, 1996), University of Wyoming (Tschirhart, 1989), and—when the research on the paper was done—Kansas State University (Tremblay et al., 1990). I include Con-

roy et al. (1995) rather than Dusansky and Vernon (1998)—the latter including an update of the former—since the time period of the earlier ranking better overlaps with the four other studies.

The first row of Table 1 shows how each of these five departments was ranked by the NRC. To be sure, the NRC rankings are subjective and rank only Ph.D.-granting departments, but they are at least not based on any one individual's choice of time period, journals, or method of assigning publications to schools. The other rows in Table 1 show the best ranking for each department in a given article, excluding some field specific rankings.

The first insight that jumps out of Table 1 is the wide range of rankings. Considering the six studies (including the NRC study), covering roughly comparable time periods, Texas is ranked from 18 to 73; Kentucky from 39 to above 65; LSU from 18 to at least 81.5 (they are not in the 187 departments listed in Berger and F. Scott); Wyoming from 12 to at least 77 (not ranked in the top 65 by Conroy et al.); and Kansas State from 92 to 132.

Moreover, the variation has a clear pattern: the institution of the author is always a large winner relative to the NRC ranking. Indeed, this pattern can be seen in fields other than economics: in Miller et al. (1996), the authors from the University of Iowa find that while their political science department placed 25th in the NRC ranking, it was as high as eighth in one of their own rankings.

One interpretation is that since reputations change slowly, faculty in recently improved departments may wish to publish a defensible study based on publications to document this improvement. However, if this is what is going on, it is difficult to explain why authors from different

Table 1

**Best Rankings Received by Each Department in Various Ranking Lists**

	<i>Texas</i>	<i>LSU</i>	<i>Kansas St.</i>	<i>Kentucky</i>	<i>Wyoming</i>
National Research Council	31	81.5	>107	64	77
Texas (Conroy et al.) (Based on publications, 1987–91, faculty as of Sept. 1992)	18	>65	>65	>65	>65
LSU (L. Scott and Mitias) (publications 1984–93, faculty as of Sept. 1988)	24	18	119	46	54
Kansas St. (Tremblay et al.) (publications 1980–86, faculty as of Sept. 1985)	73	49	92	56	63
Kentucky (Berger and F. Scott) (publications 1983–88, only 3 top journals, faculty at time of publication)	37	>187	132	39	67
Wyoming (Tschirhart) (publications 1975–84, faculty as of Fall 1984)	64	53	97	64	12

“improving” departments—over the same time frame—do not find the same group of newly-improved schools and instead consistently favor their own department.

To explore these findings further, a simple econometric exercise was performed, explaining a department’s publication ranking in each of the five studies by its NRC ranking and a dummy variable (OWN) equal to one for a ranking determined by authors from that department.<sup>1</sup> Given five schools and five ranking articles, the sample size is 25. A log specifica-

Because all five schools do not appear in all six rankings, I made the following assumptions to complete the data set. Kansas State was not ranked among the 107 departments in the NRC study—I placed them at 100. The four schools considered here other than Texas were not among the top 65 schools in Conroy et al.; however, in Dusansky and Vernon (1998), LSU’s best ranking was 61, Kentucky’s best ranking was 69, and Kansas State and Wyoming did not make the top 80, so for the Conroy et al. ranking I assigned LSU a rank of 66, Kentucky a rank of 70, Wyoming a rank of 85 and Kansas State a rank of 100. LSU was not among the top 187 schools in the Berger and F. Scott study; I assumed they would place 200 in that study. An alternative would be to drop the Conroy et al. and Berger and F. Scott rankings—the regression results after doing this were quite similar to those reported here.

tion was estimated using OLS (t-statistics are in parentheses):

$$\ln \text{Rank} = 1.39 + 0.68 \ln \text{NRCRank} - 0.93 \text{OWN} \\ (1.41) \quad (2.90) \quad (3.96)$$

$$R^2 = 0.52$$

The estimated coefficient of the dummy variable implies that own-rankings are about 40 percent of the level of rankings by authors from other schools, after controlling for the general subjective level of department quality as measured by the NRC study rankings. For example, if other authors rank a department 40th, the study by authors from that department will rank it about 16th. The coefficient of less than one on the NRCRank variable does suggest that the other motivation noted above—documenting real improvement not captured in reputation surveys—may also be playing a role.

The authors of the articles discussed here are all serious economists. But we all realize that doing calculations of this sort is not something one will embark upon without the expectation of gain, whether it is publication of an article in a refereed journal, or the validation of the quality of the author’s institution. I enjoy reading these ranking articles as much as anyone. But readers and referees of these papers should be on guard for potential biases and look to authors to control for them.

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The recent paper by Dusansky and Vernon (Winter 1998, pp. 157-170) in this journal on rankings of economics departments is seriously misleading and cries out for a response. Any ranking scheme that places the University of Chicago 17th, below the University of Texas and University of Pittsburgh, needs its entrails examined. Since our university and one of us individually has done rather well in the Dusansky-Vernon rankings, we feel free to offer some criticisms.

Dusansky and Vernon use "adjusted" pages in a limited number of journals during the limited time period from 1990-94. But the quality of scientific output is incredibly skewed. Pages just don't do it. Any scheme that does not recognize the current contribution of Heckman to Chicago or Barro to Harvard is just not right. Important scientific contributions, and hence also the associated reputations, have a longer life than a few years. Moreover, such contributions are made not only in the "core" journals, but also in specialized ones, in books, and in preprints.

A rating scheme that assigns little value to Becker or Lucas is doubly faulty: once because it does not recognize current contributions made

outside its narrow angle of vision (one of the most cited papers of the previous decade was Lucas's *Journal of Monetary Economics* article in 1988); and second, because it assumes that all of the older literature is "gone" while, in fact, older papers by Lucas, Hansen and others are still being cited in large numbers. A department's reputation is made by the contribution of its active members and that contribution is still there as long as it is still being cited, even if they have not published much recently. Graduate students may still find that they have something to learn from such authors.

In fact, the most relevant reputation measure is probably derivable from observing the choices of top economics students as to where they go for their graduate education. These decisions are made on the basis of all the information available to them, even when it may be out of date; for example, Griliches chose Chicago in 1954, in part because the Cowles Commission was there, not knowing that it was actually departing for Yale. More recent rankings and information, especially on forthcoming faculty moves, might help prospective graduate students, but rankings in the style of Dusansky and Vernon will only mislead them.

To illustrate our points we present some alternative output measures based on citation counts for selected individuals from the *Social Science Citation Index* constructed by the Institute of Scientific Information (ISI). Total citations to all publications represent a reasonable measure of scientific output; for example, they were also used by the National Academy of Sciences in its earlier rankings. Of course, citation counts have known difficulties. The counts were truncated somewhat in 1998, leading to a slight bias toward earlier papers. They are not easy to use. But they do capture the underlying skewness of intellectual output.

We have computed the figures only for selected individuals since it was not our intention to produce an alternative ranking, but only to illustrate the problems with the Dusansky-Vernon approach. We started looking at the Harvard economics department and compared the names listed by Dusansky and Vernon as being among the top 50 contributors to other members of the department. This made it clear that one should add to their original list of Fudenberg, Maskin, Campbell, Weitzman, and Griliches, also the names of (at least) Barro, Shleifer, Alesina, and Mankiw. We also added the names of Becker, Lucas, Heckman, and Hansen to the sole representative of Chicago, Townsend, on the Dusansky-Vernon list. In Table 1 we compare the "output"

*Table 1*  
**Comparison of Alternative Output Measures for Selected Individuals**

	<i>D-V</i>		<i>G-E</i>				
	<i>Total Adjusted (1990–94) Pages</i>	<i>Total Citations (1990–98) to 1990–94 Publications</i>		<i>1990–98 Citations (Fractionalized) to All “Linked” 1990–98 Articles</i>		<i>Pre-1990 Publication Cited in 1988–98</i>	
		<i>Rank</i>	<i>Citations</i>	<i>Rank</i>	<i>Citations<sup>a</sup></i>	<i>Rank</i>	<i>Citations</i>
Barro	*	1428	1	674	1	5939	3
Shleifer	*	1105	2	353	3	1044	12
Alesina	*	707	3	171	8	682	14
Mankiw	*	654	4	206	5	1475	8
Becker	*	653	5	154	10	9142	1
Campbell	27	607	6	192	7	1502	7
Fudenberg	7	605	7	152	11	1301	10
Andrews	1	556	8	406	2	281	17
Heckman	*	413	9	166	9	5230	4
Caballero	2	378	10	196	6	80	19
Lucas	*	355	11	280	4	6522	2
Griliches	34	295	12	152	12	3059	5
Roth	3	259	13	86	14	1065	11
Hansen	*	240	14	62	18	2633	6
Maskin	21	209	15	87	13	721	13
Weitzman	31	171	16	70	17	1405	9
McAfee	4	161	17	75	15	540	16
Townsend	46	91	18	55	19	582	15
Ausubel	5	89	19	71	16	105	18

<sup>a</sup> Rounded.

\* Below 50.

of this group based on alternative citation counts, to that of the top five scorers on the DV list: Andrews, Cabalero, Roth, McAfee, and Ausubel. This last set of names is indeed distinguished, but it is our contention that it does not represent fairly, or at least fully, the current “top” of the profession.

Table 1 starts by reproducing the Dusansky-Vernon ranking for the selected individuals. Almost half of them, eight out of 19, do not appear in the top 50 of the Dusansky-Vernon list. Column 2 gives our attempt to produce a more relevant ranking, based on total citation counts, but limited to the same vintages (1990–94) of cited publications, cited during the 1990 through early 1998 period. It differs from DV in using citations rather than pages

weighted by the citation impact of the journal, and in including citations to all references dated 1990–94, including papers in other journals, articles in books and preprints. It is clear that total citations to the 1990–94 work of the top five individuals are significantly larger than those that were included in the Dusansky-Vernon listing. This column also illustrates the extreme skewness of such measures.

One of the difficulties with such counts and the ISI data in general is that it is difficult to control for the number of co-authors. It can only be done for the “linked” papers in the ISI (Internet version) database; that is, those included in its set of citing articles, and that excludes all citations to books preprints, and misspelled papers, which represent, actually, a

large fraction of the overall citations (about 40 percent of the total). But this problem is not a major one for our purposes. At the tail that we are looking at, the overall ranking is not very sensitive to it. Columns 4 and 5 illustrate this proposition. Here we present a ranking based on total cites to all 1990-98 papers which we were able to "link" in the ISI to their full citation and then "fractionalize" the counts in the case of multiple co-authors. The rankings are largely unchanged except for the downward movement of Alesina and Becker, since many of their citations come from articles in books not indexed in the ISI.

The final column illustrates the importance and survival of some of the pre-1990 literature. It lists the 1988-98 citations to the pre-1990 publications of the same individuals. Here one can see the error that is implicit in leaving out Becker, Lucas, Barro, Heckman and Hansen from such comparisons. While some individuals may be close to retirement, others are still in their prime and represent an important component of the reputational capital of the respective departments.

To summarize, we view the Dusansky-Vernon rankings as flawed because they focus on the current flow of additions to reputational capital rather than its stock, and because they measure the flow by the largely irrelevant metric of total pages in a limited set of journals and a narrow time frame. The result is very misleading. One should be able to do better than that.

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### Response from Richard Dusansky and Clayton Vernon

There are many ways to measure the productivity and standing of economics departments. Our metric is publications over a recent five-year period in a set of core journals. We thus emphasize current research productivity—the generation of new ideas, new advances, new thoughts—and thus attempt to capture the current flow of addition to scholarship. It is true, of course, that many fine scholars have made important contributions prior to the five-year period under review. This work may be cited today, influencing current thought, thus contributing significantly to the *reputational* capital of departments. However, if one seeks a picture of the current research productivity of economics departments,

reflecting current contributions to scholarship, we firmly believe that a focus on refereed papers in our most prominent journals to be the preferred yardstick. Such a yardstick places emphasis on the contemporaneous flow of research; it looks to the present, rather than to the past.

Zvi Griliches and Liran Einav prefer to look to the past. Their metric is citation counts and their emphasis is on reputational capital rather than current research productivity. While our approach follows upon a lengthy tradition of measuring pages in a selected set of journals, there is a place for the study of citations. However, in working with citations one must audit the data that underlie tables such as those presented by Griliches and Einav, because electronic citation indexes are replete with substantive errors due to the anachronistic method of manual input of information. Without such careful auditing, reports of results are meaningless.

Robert Feinberg alleges investigators' bias in choosing a ranking methodology. We cannot speak for the other studies to which he refers, but we can remind him of our precommitment to our methodology in writing to department chairs nationwide. The commitment to methodology preceded the collection and analysis of the data. Furthermore, the rise of the University of Texas in the ranking of economics departments is of little surprise to those who read the literature and are aware of the faculty buildup that has taken place in Austin over the past decade. The tenured hiring of Preston McAfee and David Sibley in industrial organization, Dale Stahl and Max Stinchcombe in micro theory, Steven Bronars and Daniel Hamermesh in labor economics, Scott Freeman and Bruce Smith in monetary economics and Don Fullerton in public finance, scholars who are well-known in their respective fields, has had a significant impact on the department's productivity. The ranking arithmetic simply confirmed what most of the profession had already known about the revitalization of the economics program at the University of Texas.

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### Expanding the AEA Membership

Professor John Siegfried, in "Who is a Member of the AEA?" (Spring 1998, pp. 211-22), suggests that if AEA membership is to increase substantially, greater efforts must be made to recruit