



Correspondence

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Correspondence

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American and European Economists

Bruno Frey and Reiner Eichenberger (Fall 1993, pp. 185–93) have written a very interesting account of the differences between (North-) American and (west-) European economics and economists.

The reason for these differences, they believe, lies with the market incentives for academics. The American academic market is large and competitive; European academic markets are segmented and thin. In a large and mobile market, it is not a good idea to invest in “local” knowledge of markets and institutions. Thus, American economists are oriented towards publishing, while their European counterparts are geared towards local and national affairs. Research in North America has a dynamic internal to the profession, which leaves it subject to intellectual fads mostly disconnected from society’s economic problems, while European research tends to be oriented towards local economic problems and institutions. Finally, American professors concentrate on graduate teaching while European ones concentrate on undergraduate teaching. The outcome is that American economists are more productive in scientific terms, but in Europe “economic knowledge is transformed more effectively into policy.”

Frey and Eichenberger are thus concerned that European economic integration, pointing to a larger, more competitive market, will move Europe’s economists towards the American model. The output of European economists will increase but at the cost of relevance. Their conclusion is pessimistic: “the

future of economics as a relevant social science seems rather gloomy.” In contrast, I would like to argue that the integration of the market for economists may even help economics as a relevant social science.

Frey and Eichenberger seem to believe that in small segmented markets, society’s demands for research and teaching valid for corporate and public policy decision making will find its way into academics more easily, since professors can reap a payoff by investing in specific knowledge about local and regional affairs. However, small markets may not provide the quality check of a large market, and mediocrity may flourish. Indeed, the competitive pressure of a large market and the incentives it gives to specialize are crucial in raising research standards. Even if the competitive pressure is felt more intensely in the most theoretical research, the general raising of quality will trickle down to more policy-oriented research. This seems to be confirmed by the high quality level of applied policy research in the United States.

Will this process be at the cost of “relevance” in research? Will it alter the personality of European economic research? European personality in research is already asserted in the choice of topics and approaches, by no means coincident with the other side of the Atlantic: for example, European researchers focus more heavily on unions and unemployment in macroeconomics, on monetary and customs unions and the economics of integration, on regulation in industrial organization, on banking in finance, and so on. The integration of the European market will probably reinforce the European personality, since it will increase the quality standards of the European journals and will develop and consolidate in the long-run graduate programs at levels of excellence similar to the United States.

The process of internationalization in the European economists’ market has delivered already an increase in high-quality, policy-relevant research. To explain this phenomenon together with the pressure of a larger integrated market, we have to add the formation of across countries networks of researchers interested in policy-oriented research, such as the London-based Center for Economic Policy Research. The danger of an increasing specialization and separation between theory in an international circuit, and applied work in a local circuit, can be overcome with the help of such networks of researchers. Such networks improve communication among the different segments of the profession and provide a check to research quality.

Further, the ties of Europeans to a local cultural identity will imply a concern for society’s economic problems and will provide incentives to invest in local knowledge. It is not farfetched to envision an academic career for a European economist with two tiers: an international one and a local one. Contrary to what Frey and Eichenberger think, regionalization need not erect “language barriers for scientific intercourse.” The language for the international tier will be English (as it is already); for the local tier, the local languages.

Let me illustrate the idea that regionalization need not hinder internationalization with the characteristics of the Barcelona research institute where I

work: Institut d'Anàlisi Econòmica. There are ten permanent researchers here (among them, one Hungarian, one German and one Italian), and the long-term visitors last year came from the United States, Belgium, Portugal, Korea and Japan. The usual work language is English, and the Institute does research ranging from abstract theory to applied European, Spanish and Catalan issues (indeed, in English, Spanish and Catalan). In short, Europe's tendency toward regionalization, coupled with the development of European-wide research networks, not only need not hinder the positive effects of European integration and market enlargement but may yield the appropriate incentives to associate with local problems and check the potential tendency to separate research from relevancy.

Research in economics has long been American dominated. In the past, it was not exaggerated to talk about a "brain drain" in the field, from Europe to America. Several factors have altered this picture, but the most important one is the prospect—however distant—of an integrated European academic market. The partial liberalization of the academic market in several European countries, the development of new active research centers in different parts of Europe, and the support of the EC Commission as well as some national governments have managed to change the tide and start a self-sustaining process of academic quality growth. Researchers from U.S. universities are coming back to Europe to continue in the leading edge of research—and not to go into politics!

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In their paper, "American and European Economics and Economists," Frey and Eichenberger (Fall 1993, pp. 185–93) state that differences between European and American economists can be explained by the different market conditions they face. We agree with Frey and Eichenberger on this point. However, we have difficulties in following the authors' defense of the institutional constraints and incentives in Europe.

As Frey and Eichenberger notice, in Europe research has hardly any effect on the salary and position of a tenured economist. In contrast, in American universities, quantity and quality of published articles are taken as the main indicator for an economist's quality. Hence, Frey and Eichenberger argue, American economists specialize in theory, but neglect local institutions, while European economists "are theoretically broad and institutionally specialized" (p. 188). The authors therefore "believe that in Europe, economic knowledge is transformed more effectively into policy because European professors of economics are more engaged in the study of existing local institutions" (p. 192).

But some questions then arise. Why do more American than European economists advise the less-developed countries? More surprising, why are most economists advising eastern and central European countries Americans? One should expect that all these European economists who have such an excellent and broad theoretical background and this huge amount of political experience play a much more important role. To us, Frey and Eichenberger's conclusions seem like wishful thinking.

Moreover, the evidence given for the political influence of European economists on politics seems somewhat questionable to us. In Germany, for example, an economist has not been Minister of Economics since 1977. Andreas Papandreou, the Prime Minister of Greece, was professor of economics mainly at *American* (U.S. and Canadian) universities; that means he was acting in the *American* market before he became a politician in Greece. Josef Schumpeter was Austrian Minister of Finance for only half a year in 1919, and a rather unsuccessful one; of course, he worked from 1932 to 1950 at Harvard, an American university. Eugen von Böhm-Bawerk is not a good example of a recent highly successful politician, since he died in 1914.

In addition, we do not agree that the European scientific publication market is so thin that the chance that high-quality scientific output will be recognized is lower than in the American market as Frey and Eichenberger state (p. 186). In fact, the economic publication market is a world market. Some European journals concentrate on highly theoretical issues, like the *Review Economic Studies*. Some American journals focus on institutional features, like the *Journal of Law and Economics*, the *Journal of Legal Studies*, the *Journal of Law, Economics, and Organization* or the *RAND Journal of Economics*. And of course, every European economist can submit papers to American journals.

Therefore, we do not agree with Frey and Eichenberger that "the performance of European economists has to be defined and measured differently" (p. 186). On the same line, one could argue that the performance of firms in socialist regimes had to be evaluated according to different standards. We conclude instead that economists acting on the world market simply perform better than those in the protected European market.

As Hicks already once noted, the main advantage of monopoly is a quiet life. Thus, if the economic integration of Europe does create an integrated European market that fosters competition between economists, we conclude on a rather optimistic note: the future of economics as a relevant social science seems rather shiny.

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A Response from Reiner Eichenberger and Bruno S. Frey

We are grateful for the two letters. In our view they effectively illustrate how much the economics profession has been captured by its own self-defined standards of performance, and thereby mixing up *inputs* (like reports, advising, activities of scientific institutes) with *output* (the effect of economists on policy outcomes).

Neither letter grapples with the issue of the so-defined output—the relevance of economics for real-life issues. This is the crucial issue in our contribution. We completely concur with Professor Vives and the group from Saarbrücken that an internationalization and therewith intensification of competition among economists raises the quantity and the “quality” of research output—provided quality is defined by economists’ own standards and measured by publications and citations. However, to capture the *actual* effect of economists’ activities on the real world, it does not suffice to mention reports, or to look at economists’ advising in former Communist countries (as the Saarbrücken group does), nor to refer to the “formation across countries of networks of researchers interested in policy-oriented research” (as Vives does). Most reports and advising by economists are politically scarcely relevant. In any case, one would have to compare the results of these international activities with today’s country-based reports and institutes. Following our analysis, we expect that the economists’ international activities find more attention and recognition within the economics profession, while the national activities are likely to have more impact on actual policy.

Our paper made an *empirical* proposition, and we are well aware that it is not a popular one. We invite those who disagree with our position to provide evidence of how the resulting type of abstract economic theorizing has changed the real world. For example, one point to start with would be the Maastricht Treaty: in our view economists have contributed precious little, if anything at all, to this important set of basic rules of the European Union.

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The Economic Case Against Higher Alcohol Taxes

The recent “Policy Watch” article by Grossman, Sindelar, Mullahy and Anderson (Fall 1993, pp. 211–222) calls for increased “sin” taxes. While I will not argue the case with respect to tobacco, there are strong arguments against such action in the case of alcohol. It seems likely that tax revenues from alcohol already exceed external costs, and that increased taxes will only burden the pocketbooks and health of moderate drinkers.

External costs of alcohol abuse are paradoxical because there are two distinct classes of consumers: abusers (9 percent) and moderate drinkers (91 percent). The main externalities of alcohol consumption—like drunk-

driving fatalities and increased medical insurance premiums—are associated almost exclusively with abusers. However, it is impossible to levy taxes solely on abusers. Moreover, crime models such as Becker's (1968) rely on the severity and certainty of punishment to restrict the supply of offenses. Taxation does little to increase the severity and the vast majority of those punished are not offenders.

To make the issue even tougher, a significant and growing medical literature shows that moderate alcohol consumption has beneficial aspects (Maclure, 1993; Moore and Pearson, 1988, WHO, 1994). Over 20 years of research have provided increasing evidence that moderate alcohol consumption is associated with decreased coronary artery disease and increased longevity. Although it is well known that alcohol abusers have health problems, what is less well known is that moderate drinkers have above-average health; that is, above the average of both abusers and nondrinkers alike.

A half million Americans die annually from coronary artery disease. The medical costs exceed \$60 billion. Medical benefits associated with moderate drinking are sufficiently accepted that drinkers are informed in some prominent health-related publications that moderate consumption may carry health benefits. Among these publications are the *Berkeley Wellness Letter* (May 1994), the *Harvard Heart Letter* (March 1994), the *New England Journal of Medicine* (Manson et al., May 1992, later adapted for publication in the Johns Hopkins University newsletter *Health After 50*), and the National Institutes of Health (1992). An increase in taxation, which reduces consumption among moderate drinkers, will likely cause an increase in coronary artery disease in that group. Lewin-VHI (1994) estimated per capita medical care costs for drinkers (including both abusers and moderate drinkers) to be \$3295 versus \$4430 for nondrinkers, reflecting *inter alia* that moderate drinkers have lower rates of coronary artery disease.

In a regression model of demand by level of drinking, Blumberg (1992) estimated a median price elasticity -1.37 for all drinkers, but for the 9 percent who are abusers, the price elasticity was not significantly different from zero. Hence, an increased alcohol tax will have little impact on abuse, but will cause substantial losses for moderate drinkers.

In calculating the external costs of alcohol abuse, some studies have used NIAAA estimates. These estimates are conceptually and empirically flawed and, as a result, overstate the external costs by as much as 600 percent (Heien and Pittman, 1989). For example, over 60 percent of these estimates are composed of lower earnings due to alcohol abuse, which are clearly internal costs.

Recent studies by Manning et al. (1991) and by Heien and Pittman (1993) have produced estimates that are consistent with the definition used in economics. The estimated external cost by Manning was approximately \$20 billion and \$9.5 billion for Heien and Pittman. The main difference between these findings is in how they calculate the value of human life. Without investigating the different methodologies of the two studies here, it should be noted that

both figures are for 1985, and these costs have fallen by about half since then because drunk-driving fatalities have fallen considerably since 1985. In fact, the main two indicators of abuse, cirrhosis death rate and DUI fatalities, have both fallen dramatically. The cirrhosis death rate has declined 3.2 percent annually since 1973. Auto fatalities involving drunk driving have been falling at 3.3 percent annually since 1982. This latter trend clearly coincides with the increased emphasis on enforcement of laws about driving under the influence.

Alcohol beverages are taxed by federal, state, and local governments, with total revenue currently at \$17 billion. Although federal tax rates on alcohol remained constant in nominal terms from 1951 until 1985, and then again until 1991, there have been substantial increases in state tax rates. As a result, real levels of tax rates on alcohol have not declined. Also, as federal taxes are increased (and quantity demanded falls), state excise tax revenues fall. This has been a source of frustration to state governments.

Rather than raising alcohol taxes, which may already be too high and are surely ill-distributed between abusers and moderate drinkers, it makes more sense to focus on policies aimed directly at alcohol abusers: for example, public health education and strict enforcement of drunk-driving laws.

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Response from Michael Grossman, Jody L. Sindelar, John Mullahy, and Richard Anderson

Dale Heien offers three criticisms of our conclusion that the optimal tax on alcohol is significantly greater than the current average federal and state tax. First, abusers are responsible for the main external costs, but they alone cannot be taxed. Second, over the past 20 years, evidence has accumulated suggesting that moderate alcohol consumption is associated with decreased coronary heart disease and increased longevity. Third, Blumberg (1992) finds that consumption by alcohol abusers is not sensitive to price.

With regard to Heien's first point, we indicated that the estimates of the optimal tax on alcohol that we cite take account of the welfare losses suffered by consumers who do not abuse alcohol when the tax rate rises (p. 219). With regard to his second point, increased information concerning the benefits of moderate alcohol consumption shifts the demand function for moderate consumption to the right and increases the welfare cost of the tax hike. Presumably, this higher welfare cost is reflected in estimates of the optimal tax that use alcohol consumption for the 1990s. The studies that we cite do not use such recent information, suggesting that they overestimate the optimal tax. But it is unlikely that this factor will eliminate the two-fold difference between the optimal and current taxes contained in these studies.

Moreover, Ashley et al. (1994) and Camargo et al. (1994) indicate that the benefits of moderate drinking may have been overstated in previous research. In a sample of over 22,000 male physicians, Camargo et al. report that the lower risk of dying from heart disease was offset by an increase in cancer in those who had more than a drink a day. Both studies conclude that most of the reduction in the risk of premature death from all causes accrues to persons who drink as little as one drink every other day.

A revised version of Blumberg's analysis of the demand for alcohol consumption is contained in Manning, Blumberg, and Moulton (forthcoming). Using data from the 1983 National Health Interview Survey, they fit a quantile model of the demand for alcohol for eleven different quantiles of drinkers. This model allows the price effect or elasticity to vary with consumption. They report negative and significant price elasticities for all quantiles except for the 95th percentile. This result means that an increase in price does not affect the amount of ethanol consumed by persons in the top 5 percent of the distribution of alcohol consumption. The estimates from the quantile model, however, do not indicate what happens to the incidence of heavy drinking when price changes, a point ignored by Heien. Put differently, an increase in price may have a substantial negative impact on the fraction of heavy drinkers even if it has little or no impact on consumption by heavy drinkers. Evidence in support of the former effect is contained in a number of studies that we cite.

Indeed, Manning, Blumberg, and Moulton (forthcoming) report a negative and significant price elasticity for one standard measure of the incidence of heavy drinking: the presence of any days in the past year with five or more

drinks of alcohol. The estimated price elasticity of the number of these days, given that there are any, is positive but insignificant. The combined elasticity, which takes account of the effects of price on the incidence of heavy drinking and the number of heavy drinking days, is negative but not significant. Using more recent data (the 1985 National Health Interview Survey) and a different estimation technique, Kenkel (1993) finds a significant combined elasticity of $-.92$ for persons 18 years of age and older and -2.24 for youth between the ages of 18 and 21. These estimates, which were cited in our paper, should be compared to the combined elasticity of $-.29$ in the study by Manning, Blumberg, and Moulton. Regardless of which estimates one selects, policies that reduce the number of persons who consume five or more drinks in a row at least once in year may prevent a substantial number of alcohol-involved motor vehicle crashes.

Other considerations and findings raise serious questions about Heien's conclusion that "an increased alcohol tax will have little impact on abuse." He ignores the evidence we cite concerning the negative effects of alcohol tax rates or alcoholic beverage prices on such consequences of alcohol abuse as deaths from motor vehicle accidents and liver cirrhosis, crime rates, and school dropout rates. Grossman, Chaloupka, and Sirtalan (1994) and Moore and Cook (1994) show that estimates of the price elasticity of alcohol consumption rise when the dependence of current consumption on past and future consumption is taken into account. These dynamic effects are not considered by Manning, Blumberg, and Moulton. While Heien uses the Manning-Blumberg-Moulton results to justify the current tax, Blumberg (1992) uses these same results to justify doubling the tax. Manning et al. (1991) reach the same conclusion based on other studies of the demand for alcohol.

Heien is certainly correct that policies aimed at alcohol abusers are alternatives to excise tax hikes. But such targeted policies are not costless. One example is deterrence of drunk driving by increasing the probability that a drunk driver is apprehended and convicted and by raising the penalty if convicted. Kenkel (1993) finds that a tax hike that raises the price of alcohol by 23 percent has the same impact on drunk driving as a policy under which half the U.S. population lives in states with stiff deterrence laws in place. But the cost of the tax policy is somewhat smaller than the cost of the deterrence policy, indicating that the former is more cost-effective. This conclusion might be modified if the welfare cost of a tax hike were obtained from a demand function that fully incorporated the benefits of moderate alcohol consumption. Research on this issue and on the price sensitivity of alcohol consumption with current data are required to obtain definitive estimates of the optimal tax. It is doubtful, however, that this research will support Heien's conclusion that "alcohol taxes . . . may already be too high."

His quote is based in part on the reduction in drunk-driving fatalities since 1985. Lives lost by nondrinkers in drinking-related accidents account for 60 percent of the Manning et al. estimates of the external costs of alcohol abuse.

To go from a tax rate that is one-half of what it should be to one that is too high, alcohol-related fatalities would have had to have declined by approximately 75 percent since the mid-1980s.

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