Letters

We welcome letters from readers, particularly commentaries that reflect upon or take issue with material we have published. The writer's name, affiliation, address, and telephone number should be included. Because of space limitations, letters are subject to abridgment.

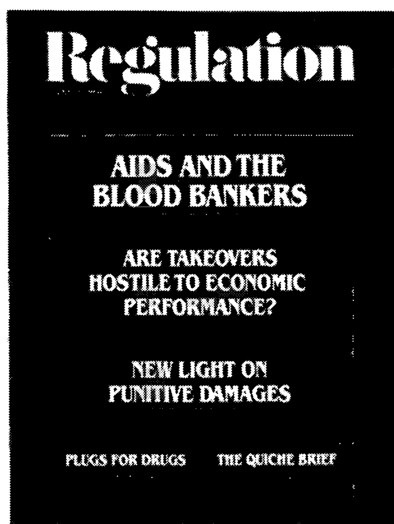
A Blood Banker Responds

TO THE EDITOR:

Professor Eckert's article *(Regulation,* September/October 1986) focuses on the quality and safety of the blood supply. Decisions to exclude donated units of blood for quality reasons will reduce the available supply, already somewhat marginal. Blood bankers have made and will continue to make decisions to exclude donors and to introduce new tests that will improve safety of the blood supply, but have done so only after thorough evaluation makes it clear that the increase in safety will justify the loss of blood that will ensue.

Professor Eckert seems to ignore the donor in the blood supply issue. Questioning of volunteer donors must not dampen their motivation to continue to donate, while still being effective in improving the safety of blood. Professor Eckert's data regarding donors who slipped through the screening net does not suggest that the net could have been constructed more tightly. Indeed, he eschews that suggestion in favor of two radical proposals—donor registries and paid donors—which were not supported by evidence available to blood bankers in the early stages of the AIDS situation, and certainly are not supported now that effective screening tests are available (and will likely improve). These two solutions are also discouraged by current federal policy.

Blood will soon be tested for alanine amino transferase and antibody to the hepatitis B core antigen which, as Professor Eckert notes, will reduce post-transfusion hepatitis. He also suggests that designated donations be encouraged. There now exists enough anecdotal and other evidence to indicate that designated donors are no safer than volunteer donors. Although there



are strong arguments against the practice, many individual transfusing communities have chosen to make them available.

Professor Eckert suggests that by removing limitations on lawsuits based on strict liability grounds, the blood supply will become safer due to implementation of increased testing, exclusive of donor registries and paid donors. This suggestion does not consider that blood banks can be sued for negligence, and thus cannot ignore reasonable measures to improve quality. More importantly, even if all of his suggested measures were in place there would still be a very limited amount of disease transmission resulting from transfusion. Under the doctrine of strict liability, blood banks would automatically lose the resulting lawsuits. Who will then insure blood banks? How can blood banks fulfill their community service obligations under such circumstances? Will the victorious plaintiff operate the blood bank after it has been bankrupted?

The American public deserves the safest blood possible. I believe that Professor Eckert should be congratulated for encouraging blood bankers to be the best that they can be, but blood transfusion is not, has never been, and probably never will be perfect. Professor Eckert's solutions suffer the same weakness.

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Challenging Assertions

TO THE EDITOR:

Congratulations on a fine first issue of *Regulation* under new editorial leadership. The magazine serves an important purpose in advancing understanding of regulatory issues. The September/October issue, however, contained two assertions that should be challenged.

The first, contained in the *Currents* section on "Nonbanks and Nonproblems," asserts: "A manufacturer *cum* bank cannot make money by lending money to itself at a below-market interest rate, since the benefit to the manufacturer is exactly offset by the foregone market rate to the bank." In at least one situation the statement is inaccurate, but acknowledging that situation should strengthen the author's case. Because the manufacturer may possess information on the risk of a loan to the manufacturing company that he may be unable to communicate to a non-affiliated bank (or persuade the bank of its veracity), it is highly plausible that in such a situation the manufacturer *cum* bank *could* make money lending to itself at an interest rate below that prevailing in the market for companies with apparently similar (to the banking community) risk characteristics.

Such a case, however, would

not represent a conflict of interest that is a public policy concern, as the article correctly notes. On the contrary, it represents a correction of a market imperfection. Moreover, if the manufacturer is wrong he will be the first to suffer the consequences of his bad judgment (as bank owner).

The second assertion is contained in the article, "Are Takeovers Hostile to Economic Performance?" The authors contend: "If, as suggested by the critics' view, the bidder's role is no more substantial than that of a savvy used-car shopper, able to spot the underpriced gems on the lot, then it is hardly likely that takeovers increase efficiency." This assertion overlooks two efficiency producing benefits of such "savvy shoppers."

First, their actions improve the amount of information in the marketplace about the likely value of the assets in question. Second, they will increase the probability that the assets will be used in ways that maximize their productivity by placing them under the control of those managers and owners who value them most highly. In contrast to the authors' assertion, it is highly likely that this role alone would increase efficiency, even if the instant bidder is unable to directly increase profitability.

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Plugs for Plugs for Drugs

TO THE EDITOR:

I support the position taken by Alison Masson and Paul Rubin. I vividly remember the era where providing any kind of information on prescription drugs to consumers was considered illegal by the Food and Drug Administration. There is no doubt in my mind but that some years in the future both prescription drug advertising to consumers and patient package inserts will become commonplace.

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A Reader Challenges a Review

TO THE EDITOR:

I read with great interest the review of *The Economic Effects of Airline Deregulation*, by Steven Morrison and Clifford Winston of the Brookings Institution, appearing in your September/October 1986 issue. I disagree strongly with several of the book's findings. The views expressed here are my own and not necessarily those of my employer.

The book estimates the welfare gains from airline deregulation at $6 billion a year. The method is to estimate price and cost functions based on 1983 deregulated experience, and to backcast what prices and costs would have been in 1977 if deregulation had been in effect. All this is highly hypothetical, and the authors themselves show that, by using alternative assumptions, the range or the welfare gain is between $1.3 billion and $8.7 billion. Such a wide range does not inspire confidence in the precision of the results, although, to be sure, $1.3 billion is still a very impressive gain in efficiency.

Unfortunately, even the range of assumptions tested by the authors omits many crucial assumptions. Some would bias the results upward, others downward. The authors are careful to point out conservative assumptions that result in estimates of welfare gains that are probably too small, e.g., the U.S. economy was probably in better shape in 1977 than in 1983, and so the airline industry would have been in better shape under deregulation than 1983 conditions, backcast to 1977, indicate. Again, the present study neglects benefits due to the stimulation of additional passengers, and the need for bus and rail carriers to be more competitive.

The authors are not nearly as articulate in pointing out assumptions that lead to overestimating welfare gains. Chief of these is the measure they use of quality of service. In the study, quality of service is measured by only two variables, scheduled travel time and departure frequency. They assume that these two measures can equally be applied to 1977 and to 1983. In fact, they cannot.

First, scheduled travel time is much less important than actual travel time. The authors do not consider that late departures, late arrivals, and missed connections have increased significantly under deregulation. One need only visit the People Express terminal at Newark to realize the impact of late departures. As for missed connections, competing airlines no longer "hold" their departing flights if another carrier's arriving flight is late. However, these are probably minor effects, and in any case, scheduled travel time plays a negligible role in the authors' analysis.

Frequency is another matter. As the authors recognize, the important measure of travellers is schedule delay, i.e., the delay due to inconvenient scheduling *and* to the possibility of not finding a seat on the preferred flight. Such schedule delay is very imperfectly represented by the number of departures, the only variable used by the authors. There are at least three reasons for this.

First, under competitive conditions, departures tend to "bunch up," i.e., three competing airlines will tend to depart at the same preferred time (or within a few minutes of each other). By contrast, a monopolist will tend to spread out departures to more fully cover the range of desired departure times. Thus, the same frequency under regulation and deregulation will usually mean increased schedule delay.

Second, the possibility of not finding a seat on a preferred flight depends upon the load factor which the airline aims at. Since the number of passengers wanting to travel on a given flight is not known in advance, but fluctuates randomly, airlines aim to fill, on average, only some fraction of the seats, so as to accommodate unexpected surges in demand on specific flights. Under deregulation, load factors have increased, hence schedule delay has increased. In a footnote, the authors suggest that load factors in 1977 under deregulation would have been lower than what they were in 1983, due to lower fuel prices on 1977. Apart from this footnote, load factors nowhere enter their analysis of consumer benefits.

Third, even for a given load factor, the possibility of not finding a seat on a preferred flight depends on the size of the aircraft. This follows from the statistical law of large numbers. A 70 percent load factor on a 20-seat aircraft means that there are 6 seats available for unexpected passengers. A 70 percent load factor on a 120-seat aircraft means that there are 36 seats available for unexpected passengers. Under deregulation, aircraft size has dropped. Thus schedule de-

lay has increased. Nowhere in their paper do the authors analyze changes in aircraft size.

As a result, the use of departure frequency seriously overstates the quality of service under deregulation, relative to regulation. This is all the more serious in that, in their calculations, "the largest contribution to the welfare changes comes from changes in departure frequency." Instead of schedule delay decreasing, as the authors suggest, it has probably increased.

A final section of the book discusses further potential welfare gains through "optimal" prices and frequencies. Curiously, welfare could be improved by increasing frequencies and increasing discount fares in most markets, while decreasing coach fares in many markets as well. This is consistent with their previous findings that welfare gains to date have been largely due to increased frequencies. This is somewhat ironic. Indeed, before deregulation took place, opponents of regulation charged that it: (1) led to prices that were too high; (2) led to quality of service that was too high; and, (3) led to price averaging, i.e., suppressed pricing flexibility. Deregulation was to lead to the lower price-lower quality of service combination that travellers were deemed to prefer in many cases, and to a larger choice of price-quality options. Now more competition is being urged so as to lead to a higher price-higher quality of service combination, and to reduce the discrepancy between coach and discount fares. Perhaps reregulation would be easier.

That last sentence is, of course, facetious. Air deregulation is here to stay, and, in a mature market such as the U.S., there have been major welfare gains from the deregulation of air carriers, as well as major transfers from shareholders and labor to consumers. This study, however, is of very little use in better understanding these changes.

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The Authors Respond

TO THE EDITOR:

George Hariton's letter contains a mixture of anecdotal thinking and regulatory-bequeathed perspectives on the effects of deregulation on airline markets. As such, it fails to provide insight into how future research on airline deregulation can improve the the existing literature.

There is an important distinction to be drawn between the precision of an estimate and the sensitivity of an estimate. Our estimate of the welfare gain from deregulation to travelers of $6 billion, based on the most accurate assumptions regarding the proportion of discount-couch business travel and the deflation of fares, is statistically precise (i.e., based on estimated parameters with relatively low standard errors) but is sensitive to the aforementioned assumptions. Obviously, if we assume all business travel occurs at discount fares instead of at coach fares, then the estimate of the gain to travelers from deregulation will be significantly increased. This is not a flaw in our analysis, especially because we can identify the most accurate assumption regarding the proportion of discount-couch business travel. In addition, we point out that our estimates do not include the benefits from the generation of additional trips and the benefits from lower rail and bus fares in response to deregulation.

The main thrust of Hariton's criticisms concern service quality. He claims that under deregulation there is a significant divergence between scheduled travel time and actual travel time. He provides no empirical evidence that a significant divergence exists in many markets and, more importantly, that such a divergence is attributable to deregulation.

We use schedule delay as a measure of service convenience. It is composed of frequent delay, a function of departure frequencies, and stochastic delay, a function of departure frequencies and load factor. Thus our use of departure frequencies as a measure of service convenience controls for a substantial portion of schedule delay. We show in footnote 37 (page 35) that our omission of the effect of changes in load factor on stochastic delay does not affect our estimates. Thus, our use of departure frequencies to measure the welfare effects of changes in the schedule delay is conceptually and empirically sound. Hariton's three hypothetical points to challenge this are incorrect. First, bunching is more likely to occur under a regulated oligopolistic industrial structure than under deregulation. It is incorrect to equate regulation with pure monopoly when in fact regulation created an oligopolistic structure characterized by excessive service competition. Second, as mentioned above, we do consider the effects of load factor. Finally, we do control for changes in aircraft size when we calculate the effects of load factor on stochastic delay. Thus, the assertion that schedule delay has increased—a regulatory-bequeathed view—does not even hold on a hypothetical basis.

Hariton notes that it was expected that deregulation would lead to a lower price-lower quality of service combination. He finds it "ironic" that we found deregulation led to a lower price-higher quality of service combination. However he provides no substantive critique of this result but rather misleads by claiming that we argue optimality requires a higher price-higher quality of service combination. We in fact argue optimality generally requires further improvements in the gains that have already been achieved: further reductions in fares and increases in departure frequencies.

Hariton's final point summarizes the regulatory-bequeathed view of the effects of deregulation as if it were based on hard evidence: major welfare gains and transfers from shareholders and labor to consumers. We have empirically demonstrated the former conclusion is correct—although challenging the conventional view as to why this has happened—and have found little evidence to support the latter conclusion.

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