# REJOINDER TO ELIZABETH BURY

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It is always good to have additional empirical evidence for one's work. In her regressions on the relationship between U.S. military procurement spending and state-level changes in growth rates over the period 1977 to 1986, Elizabeth Bury adds to the body of work confirming a statistically significant and positive correlation between the two. What puzzles us is the interpretation that Bury places on these results, especially her suggestion that they refute our contention about the substantial contribution of American military preparedness to the economic remapping of the United States. In fact, Bury's formulation comes nowhere near capturing the extent of the phenomena we encompass with our term "the gunbelt."

Our work on the gunbelt was prompted by our own multivariate regression analysis, in the early 1980s, of the determinants of state and metropolitan-level high-tech plant and employment growth (Markusen, Hall, and Glasmeier 1986). There, we showed that defense spending was indeed a significant contributor to high-tech growth in the period from 1972 to 1977. Although it shared that distinction with a number of other features, several others which had received much attention in the high-tech literature university research and development spending, for instance – turned out to be negatively correlated with high-tech performance. At the time, the incipient high-tech literature was full of glowing accounts of places like Route 128, Orange County, and Silicon Valley, which celebrated the role of universities like MIT and Stanford but were completely silent on the role of defense industrial development in the building of such complexes. (An exception was Saxenian's [1985] brilliant recounting, in her Master's thesis at Berkeley, of the genesis of Silicon Valley, which she attributed in large part to early Cold War funding of semiconductors and computing.)

We decided to devote our next major research effort to understanding why and how Cold War defense activity had contributed to the remarkable shift in the center of American manufacturing and economic activity, away from the industrial heartland toward what we came several years later to call the "gunbelt." A shift of this sort had not occurred in any other advanced industrial country, save the Soviet Union. In European countries, with divided Germany perhaps an exception, pre-World War II dominant urban centers like London, Paris, and Milan continued to spawn new leading-edge manufacturing activity, albeit often on their peripheries. In the U.S., in contrast, the Cold War catapulted Los Angeles ahead of Chicago as the nation's number one industrial city, even past New York in total employment by 1990. Regional economist Charles Tiebout estimated that more than 40 percent of the Los Angeles economy in the 1960s was dependent upon defense outlays (Tiebout 1966).

*The Rise of the Gunbelt* is, as its title bluntly states, an account of the origins and behavior underlying this phenomenon. Its major theoretical foci are a series of hypotheses about locational behavior in defense-oriented industries and a set of plausible models of evolutionary military industrial complex-building. In our proposal to the National Science Foundation for funding, we pleaded to be allowed to use historical and interviewing techniques, arguing

that simple number crunching, a la *High Tech America*, would not allow us to comprehend the underlying behavioral phenomena. We included a long anecdote drawing on a conversation Ann Markusen had with a Grumman official about why, if southern California was such an inevitable mecca for aircraft industry hopefuls, as Cunningham (1951) contended in his famous study, Grumman had successfully been making airplanes on Long Island for most of the century. He told Markusen several interesting things, all of which involved discretionary behavior, personal preferences, interservice rivalries, etc. In the research proposal, we pointed out that if we just analyzed data, we'd throw Long Island out as an outlier; yet here was some very important behavioral evidence that contradicted the prevailing point of view. NSF bought our argument and funded the qualitative work.

At the book's outset, we do claim that the gunbelt is a major, even the major, phenomenon in the contemporary economic map of America. We do not prove this quantitatively, for reasons that will be clear below, but neither does Bury prove that it is not. Frankly, the emphatic nature of our statements at the time of writing had much to do with the fact that, despite publication of our regression results in *High Tech America* and other academic papers on the subject (Markusen 1986a, b, and c), scholars like Allan Scott continued to write adulatory empirical papers on endogenous growth in places like Orange County and Silicon Valley without once mentioning the region's heavy defense dependency. (In 1984, Santa Clara Valley and Orange County ranked third and fourth among American counties in DOD prime contract receipts, at $4.6 and $3.7 billion respectively [Markusen and Yudken 1992: 179].) Similarly, book after book, paper after paper, was written on interregional migration within the U.S., never once mentioning the role that the military services, through recruitment and mustering out, and military industrial companies, through government-funded relocation of scientists and engineers, had on differential regional growth patterns.

What we show qualitatively in our book, through our case studies, is the role of the U.S. commitment to Cold War foreign and military policy had in reshaping the economic fortunes and differential specialization of U.S. regions. Our analysis covers a long period of time, reaching back into the 1930s, even before, and with great emphasis, as Bury notes, on the 1950s. The story we tell is one of clusters of economic agents – military officials, industrial entrepreneurs, civic boosters, and members of Congress – working contentiously or together to site and build new industrial capability in the aircraft, electronics, communications, and computing industries. This was a long, evolutionary process, during which new pools of labor were built up in certain favored regions, new educational capacity pushed along, and new firms in new industries anchored in remarkably new and underdeveloped towns. In turn, some of these firms and cities successfully pioneered and nurtured new commercial technologies and products initially funded by the military, boosting them into the ranks of high-tech meccas. The inertia of this structure, once in place, helped the new military industrial cities to coast through periods of defense cutbacks, although not without a great deal of discomfort.

The empirical exercise Bury performs doesn't come close to capturing the complexity of this process or the many tentacles of the Pentagon's reach. Bury quotes us stating "in large part, . . . (the) differential growth rates and income effects (in states over the past forty years) were the result of military spending differentials and the construction of high tech industrial complexes." Besides

ignoring the "in large part," she then goes on to formulate a hypothesis that includes only military spending differentials, and only those in recent years, ignoring completely the second half of the statement, which captures the investment in a built environment and labor pools that are so central to our concept of the gunbelt.

The gunbelt as a phenomenon encompasses much more than contemporary Pentagon procurement spending. First of all, procurement, reflected in both Bury's and our use of prime contract data, accounts for only 40 to 60 percent of the Pentagon budget, depending on the year, the rest going into operations and maintenance and personnel. In 1987, for instance, the latter two categories accounted for 59 percent of all spending, compared with just 41 percent for procurement. Patterns of spending in the latter categories reinforce the gunbelt-oriented bias – in 1983, for instance, only the South Atlantic, West South Central, Mountain, and Pacific regions had personnel receipts per capita in excess of the national average, while the Mid-Atlantic and East North Central states were more than 50 percent below the national norm (Stein 1985: Table 3). The fact that military bases and facilities are predominantly in the gunbelt both attracted private defense activity there and is a contributor in and of itself to postwar shifts in population and economic growth. Bury's independent variable does not pick up this rather major contribution.

Second, in addition to the omissions of NASA and DOE defense-related activity Bury mentions as problems with her data, defense prime contract measures do not reflect the considerable companion effect of arms sales, a major American export component in the postwar period. Last year alone, U.S. firms exorted $41 billion in arms overseas, exclusive of black market transactions, and throughout the 1980s the level fluctuated between $10 and $20 billion. Those orders are registered particularly strongly in existing defense procurement regions, especially in aerospace and electronics complexes, and are thus a military-related source of growth. Bury's dependent variable does not capture these, and leaves them to be interpreted, by default, as "nondefense" growth factors.

Third, and most problematic, the contemporary performance of a place like Silicon Valley or Orange County or Route 128 is much indebted to the rounds of prior accumulation – of technology, of technical expertise or personnel, of equipment and facilities underwritten by the defense effort. Labor pools were constructed through government-financed moves of scientists and engineers, for instance, over decades. More than 50 percent of the graduates of top-rated engineering schools in the midwest have gone south or west after graduation for decades, an internal brain drain heavily financed by taxpayers. Similarly, the fact that the services recruit or draft ubiquitously and pay for military-related migration to disproportionately gunbelt-sited military facilities means that both blue-collar labor and military retirement communities' location is skewed. In the single, remarkably ignored study inquiring into the effect of military-related moves, Long (1976) found that military-related migration was larger in size than any other source of internal migration in the U.S. for the period 1969 to 1976. We have recently completed two empirical exercises into military-industrial-related migration and have found substantial evidence for heavy net flows in the direction of the gunbelt (Markusen and Campbell 1989; Ellis, Barff, and Markusen 1992). No analysis, even of one decade,

could capture the lagged effect of such military investments operating through the subsequent activities of these various occupational and population groups.

In recent pathbreaking work, Hooks and Bloomquist (1992) have developed empirical measures of military industrial infrastructure that show that public investment in plant and equipment during World War II, the bulk of it subsequently sold to private firms, made a sizeable and significant contribution to growth in manufacturing in the 1947-1972 period. They argue that the impact of much of this expenditure occurs over decades, and that lagged time series regressions over long periods are the only way to capture the cumulative effects (Hooks and Bloomquist 1992).

It's not just a question of labor flows and capital location, however. The discovery, innovation, and provision of markets during crucial infant industry stages for everything from computers to semiconductors to communications satellites and jet aircraft was linked to the Cold War. The phenomenon of the gunbelt is designed, in our conception, to capture this enduring contribution to regional growth and development. Contemporary levels of defense spending capture only the current government demand for output from these complexes, not the investment effects of military-initiated assembly of land, labor, and capital there over the years.

We see no contradiction between our contention of a major, even dominant, role of military-commissioned activity in the postwar repatterning of American economic activity and Bury's findings. In our view, a good deal of the variation in state growth rates that remains unexplained in her exercise can be attributed to both the omissions and the longer-term growth effects of military-related location and regional investment patterns. Our argument here is analogous to that of Lichtenberg's, where he posits that defense shares of national investment have been underestimated in the U.S. and then shows that a good deal of what was considered "private" investment in the economy was actually "military" in nature, undertaken in anticipation of future defense contracts (Lichtenberg 1987).

## Methodology

A few comments on Bury's empirical results and interpretations are also in order. Because she doesn't do a multivariate analysis, she doesn't show us that any other variables could do better than defense spending at explaining state growth differentials, a fact she acknowledges. It is strange that she considers her results to belie a "strong" relationship, referring to an r-squared of .22, not bad for a cross-sectional analysis. (Few cross-sectional analyses of U.S. regional growth differentials, even kitchen sink models, have ever attained r-squares in excess of .50 or .60.) However, "strength" in an econometric relationship is usually assessed by looking at the *size* of the coefficient, not the r-squared, and gauging just how much increase in growth one might expect from a given increase in military spending. Bury doesn't do this for us, and we wish she would. It might help communities now facing severe cuts in military prime contracts to plan, at least in the short run.

Bury admits that adjusting procurement data for subcontracting will give a more accurate estimate of regional procurement flows, but understandably does not do that here and uses prime contracts in the independent variable. (However frustrating, most agree that available subcontracting data are of

poor quality.) Thus, the measurement errors in the independent variable imply that the least-squares estimates are biased and inconsistent. In part because of the inconsistency and bias of the results – which again cannot be estimated accurately with the data sets available – we elected to present the data through simple location quotients and standardized indices rather than compound the errors through more statistical testing. The author points out that probable measurement errors with procurement data would bias the results downward. Thus the true regression model would strengthen both the significance and the fit of the model.

In specifying her variables, she standardizes them and opts against using real defense spending for her independent variable. While it's reasonable to conclude that the CPI, the GNP deflator, and the government purchases deflator might have some problems for defense spending, why didn't the author select the national defense implicit price deflator? It is precisely the deflator she is describing and allows her to look at real dollar changes.

The most original and interesting of Bury's findings are those that follow from her experiments in lagging the formulation. She begins her model with the assumption that time makes no difference in the relationship between procurement and regional income by specifying prime contracts as an average over the years from 1977 to 1986. She relaxes this assumption by adding time lags to the model. She uses three years, and finds that the correlation is stronger the longer the lag. With time becoming increasingly important, we are curious if Bury considered respecifying her independent variable to account for this very important factor, rather than using an average over the years. This bears out our experience and helps to explain why the pain is currently only beginning to be felt in defense-dependent subnational economies more than two years after defense outlays began to fall significantly in 1989. These findings underscore the long, evolutionary nature of the phenomenon at hand. They suggest that if we could build a complex model, with military investments in plant, equipment, and human capital included, we might indeed find that contemporary regional growth rate differentials were as much if not more a product of prior rounds of military industrial expenditure than of current outlays.

Even with these comments in mind, Bury's model as it stands shows defense spending "explaining" over one-fifth of regional (state) growth, actually more than its employment share. Bury overreacts here by focusing on the statistics, rather than the variables they represent. "The data clearly show that military spending is NOT the overriding determinant of growth: other factors account for a far greater proportion of the variation. . . ." Yes, statistically, but what are these factors? We are interested in regional growth and the role of planning and policymaking in that growth. Bury's model estimates that defense explains more than 20 percent of regional income growth. She does not go on to demonstrate that any other factor by itself accounts for that much growth. It's important for us as planners to understand that Bury's 22 percent of regional growth resulted from policies and decisions in the public sector. With the end of the Cold War, the U.S. will spend $270 billion (down from a high of $300 billion) this coming fiscal year on defense. In part, this is not because defense spending is an insignificant component of regional growth, but because it *is significant*.

We also disagree with Bury's speculation that the multiplier effects of defense spending may be lower than those for other economic activities. There is a

substantial literature simulating defense versus non-defense expenditure patterns (including tax reductions) that suggests that military spending may indeed be less expansionary (see for instance Bezdek 1975; Anderson, Bischak, and Oden 1991). However, there are other studies, by the Congressional Budget Office, for instance, that claim that the effects are a wash. Our own view is that all existing macroeconomic models do not take into account the extent to which defense spending is manufacturing-intensive and "buy-America" in nature, which amplifies its impacts on the economy rather more than equivalent outlays in social spending or private consumption. Indeed, in the 1980s, the military buildup was probably more stimulative than most economists have understood, obscuring the structural deterioration in commercial manufacturing and setting the stage for the current severe recession (Markusen 1992).

For our part, we think Bury's work is a good start at documenting quantitatively the ways in which American military policy has and continues to affect regional growth patterns. It would be a formidable task to thoroughly chart the contribution of the Cold War buildup to contemporary economic geography, just as it would be to untangle the total net contribution of the interstate freeway system (also a defense activity in its origins!). We will be content if the bodies of location theory and regional growth theory, with empirical counterparts, simply acknowledge the powerful role that military activity has played, ending a long and unwarranted silence in the post-Viet Nam literature.

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