NBER WORKING PAPER SERIES

MEASURING THE ECONOMIC EFFECTS OF MILITARY BASE CLOSURES

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Working Paper 6941
http://www.nber.org/papers/w6941

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 1999

The authors thank the Rockefeller Center for Social Sciences Research at Dartmouth College for financial support, seminar participants at Dartmouth College and the Federal Reserve Board for helpful comments, and Kevin McHart and Michele Graham for research assistance. The views expressed here are those of the author and do not reflect those of the National Bureau of Economic Research or of the Federal Reserve Board.

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February 1999
JEL No. R23, J23, E24

ABSTRACT

Quite often, policy changes that are seen as welfare-improving at the national level encounter significant resistance in localities where the policies are implemented. Defense spending cuts and international trade agreements are classic examples. However, there is little systematic evidence on the magnitude of economic costs that fall on adversely affected communities. In this paper, we use a newly constructed dataset to analyze the county-level employment and personal income effects resulting from closures of military bases during 1971 - 1994. Our estimated multipliers are mostly less than one, and considerably smaller than those typically used in economic impact studies. We find that the employment costs are mostly limited to the direct job loss associated with military transfers out of the region, and per-capita income is little affected by closures on average.

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Measuring the Economic Effects of Military Base Closures

Military spending in the United States has been reduced dramatically in recent years. Federal outlays on national defense have fallen from 6.2% of GDP in 1986 to 3.2% in 1998, a decline of about 25% in real dollar terms.[1] One important component of this drawdown has been the closure of dozens of military bases, and the realignment (partial closure or combination) of many more. While most observers in government, the military and the involved communities have agreed that defense could and should be downsized in this post-cold war, budget-balancing environment, the decisions of which bases to close have been highly contentious. In fact, opposition to closures has been sufficiently intense for a bipartisan Base Realignment and Closure (BRAC) commission to be appointed, with special procedures for the base selection process, to minimize political interference.[2]

This resistance to base closures clearly stems primarily from perceived adverse economic consequences. The facts that bases often constitute a large fraction of local economic activity (representing up to one-third of a county's employment in some cases) and the taxes which pay salaries of base employees come almost entirely from outside the region, both underlie the widely held belief that bases are worth fighting for.

What may be overlooked by communities and their representatives is the opportunity cost of resources that bases occupy, principally land. Many bases are on coasts, in metropolitan areas, or have other attributes that make for high values in alternative use. Two examples among recent closures are the Presidio Army Base in San Francisco and Moffett Field Naval Air Station in Silicon Valley. In such cases, local economies may actually be better off after a closure, at least following a temporary adjustment period when some transition and cleanup costs are incurred.

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1. Estimates from *Economic Report of the President*, 1998, table B-79.

2. Twight (1989) discusses the creation of the BRAC Commission, and provides numerous examples of community and legislative opposition to closures.

Given uncertainty about the likely economic consequences of base closures, and the substantial resources that have been expended to prevent them, it would seem important to evaluate the economic costs of past closures. However, it appears that little such analysis exists: a search in *EconLit* found only 11 entries for either "base closures" or "military bases", with *none* of the referenced articles empirical. The evidence seems mainly limited to case studies, notably a recently published Rand report (Dardia, et. al. (1996)) which studied three closures in California in the early 1990s. Bradshaw et. al. (1995) reviews a number of such studies, and the Department of Defense's *Civilian Reuse of Former Military Bases* publications collect several newspaper articles summarizing closure communities' redevelopment experiences (mostly prior to 1978).[3] An important exception is a recent paper by Krizan (1998), which uses longitudinal micro data to study the impact of base closures in California.

In this paper, we attempt to fill that gap. We have compiled a database from Department of Defense reports that includes the total number of military and civilian jobs eliminated in all of the major base closures that have taken place since 1970. We match this data with the employment, per capita personal income, and population data in prior and succeeding years for base counties and their states, and use the combined data to assess the economic impacts of the closures.

Our final database contains 57 closures, ranging in absolute size from 150 to over 16,000 jobs. They represent from .05% to over 30% of county employment, with the median closure equalling 2.24%. We measure counties' responses to these shocks compared to two counterfactual scenarios: one which assumes that the county would have had employment or

3. Many studies have been performed by consultants on the projected impacts of a given closure. These are not very useful for several reasons: they are often commissioned to lobby against proposed closures, they use forecasts rather than actual outcomes, and they usually employ static input-output multipliers, the limitations of which are discussed in more detail below.

per-capita personal income growth at the state's rate, and another which assumes that the difference between county and state growth rates which prevailed in the pre-closure years would have persisted. These assumptions control for any region-specific shocks that would be expected to impact counties and their states roughly equally.

The job-loss "multipliers" that we estimate -- how many fewer jobs a closure county had than it would have if it grew at the state's rate, per job lost in the closure -- are less than one in most of our calculations. This implies that nonbase employment in closure counties, on average, actually grows *faster* than employment in the state. Rather than job loss spilling over from the base to other sectors, as is typically assumed in "impact analyses", job creation is induced -- as one might expect if the base's resources can be reasonably deployed in alternative uses. This finding is in accord with much of the recent evidence on the dynamism of labor markets for larger regions and industries collected by S. Davis, Haltiwanger and Schuh (1996).

Our results for personal income are even more striking. Here the multiplier coefficients are not significantly different from zero, indicating that closures have no statistically reliable impact on per-capita income. In fact, many of the point estimates, and the median county's income loss relative to the state growth rate, are negative, indicating that counties average slightly *faster* per-capita personal income growth than the state in some years after a closure. Further analysis, incorporating the employment and population data, suggests two potential explanations. First, the military personnel, who leave the area, generally have incomes below the county average. Second, the civilian job losers who remain in the county may be older, more experienced workers who are capable of earning higher wages than the civilian job losers who leave the county.

Despite this characterization of the historical record, two factors should temper an overly sanguine view of the economic consequences of future closures. First, the DoD's Office of

Economic Adjustment (OEA) dispensed technical and financial assistance to the affected communities, and it may be the case that without such assistance the impacts would have been much greater. The anecdotal evidence from the articles collected in *Civilian Reuse ...* suggests that a number of common-sense aspects of conversion efforts, such as local initiative and clear delineation of property rights, tax obligations, etc., aid economic transition.

The second is that the closure counties were not chosen randomly. It may be the case that successful oppositions are mounted against closures which would have poor outcomes, and thus the sample that we analyze is representative of more adaptable counties. We feel that this possibility is quite limited for several reasons,[4] but it cannot be ruled out. We *can* be confident, however, that if the behavior of the OEA and the communities continue roughly as they have in the past, then local economies are unlikely to suffer much damage on average, and they may even benefit from base closures.[5]

The remainder of the paper is organized as follows. Sections 2 and 3 describe the base closures dataset, and our employment and personal income economic impact measures. Section 4 contains regression analysis and discusses the results, while section 5 provides a summary and conclusions.

2. Data

We begin by compiling the official civilian and military job loss counts from all of the major base closures and realignments since 1970 which were completed by 1994. Since our

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4. These include the disproportionately large number of closures in California and Texas, two politically powerful states, and the existence of several closures in *a priori* vulnerable economies (e.g. rural counties with more than 20% of their jobs on a base).

5. It is unlikely that politicians will stop fighting closures in their districts, for at least two reasons. First, their goals are broader than maximizing nonbase employment or per-capita income. In particular, it is likely that size matters to them, and we do find that closure counties typically lose some population. Second, they dislike uncertainty -- while the average closure county doesn't suffer much economic harm, *some* do.

data for assessing closure impacts runs from 1969 to 1996, bases that closed before 1971 or after 1994 have insufficient response information (we use employment and income levels two years before the closure as a baseline in some cases). The raw data come from the Department of Defense publications "Civilian Reuse of Former Military Bases 1961-93" and "1988, 1991, 1993, and 1995 BRAC Actions Base Reutilization Status." This criteria yields data on 62 closures. After dropping five observations because there was a second closure in the same county within five years, our final database contains 57 base closures over the 24-year period.

Figure 1 plots the raw data, with military and civilian job loss broken out. It shows that most of the closures involve more than 1000 jobs, and the majority of the job loss (69%) is military. The closures are somewhat concentrated in space -- many states had none over the sample period, while California had five[6] and Texas eight -- and quite concentrated in time, with most of the closures taking place in either the Vietnam drawdown (22 closures from 1971 through 1975) or the present post-coldwar drawdown (25 closures in 1991-1994). In fact, there were only five closures between 1977 and 1991. About half of the closures occurred in rural counties, defined as having less than 75,000 county population in 1970 and not being contiguous to a metropolitan area. These are mostly Air Force bases.

We choose to measure closures and their effects at the county level of disaggregation. While this obviously is imperfect -- counties differ in size, and bases are located in various parts of counties -- data limitations make such a compromise unavoidable. Base closures are local economic shocks, and counties are probably the best local economy measure available.[7]

6. California actually had several more closures that didn't meet the criteria of the dataset: there were multiple closures within five years in Sacramento and San Bernadino counties, and two California bases from the 1991 BRAC round did not close until after 1994.

7. SMSA data cannot be used for the many rural closures, and SMSA-type designations have changed several times over the sample period.

The magnitudes of the closures are too small to matter much for states; excluding Rhode Island (which had two very large closures in 1974 and 1975, and has only six counties) the largest closure represented three-quarters of a percent of its state's jobs, with the mean and median closures equaling .2% and .07% of state employment, respectively.

We measure the economic size of a closure relative to the number of jobs in the county, either one or two years before the closure. Using two years prior as the base year allows us to incorporate some response of the economies before the official closure date, either because the closure takes place over time (years in the database are for closure completions) or because the closures are announced earlier or otherwise anticipated. The scaling to county employment normalizes shock sizes across counties of different absolute size.

Figure 2 shows the distribution of closure sizes as a percentage of county employment two years prior. It shows a similar distribution to Figure 1, with a wide spread of shock magnitudes. The 10 or 15 smallest closures are probably negligible to their economies, at less than .5% of county employment. About 25 shocks might qualify as moderate to large, accounting for between 1 and 10% of the county's jobs, while the largest 15 closures range from 10% up to nearly 33% of county employment. The shocks in rural areas are considerably larger in relation to their economies, with total direct job losses equalling 12.7% of county employment at the mean and 10% at the median.

The employment, income, and population data used in the paper come from the Regional Economic Information System CD-ROM assembled by the Bureau of Economic Analysis. Employment is establishment-based, and is measured as the average annual number of jobs, full-time plus part-time. The underlying source for the wage and salary employment data that we use in most of the analysis is the ES-202 survey. The REIS estimates the self-employment component of total employment from tax returns. Military and civilian employment on bases is included in the employment measures.

The state and county estimates of personal income are designed to be conceptually and statistically consistent with the national estimates of personal income: the personal income of an area is defined as the income received by, or on behalf of, all residents of the area. Thus employment and personal income do not necessarily cover the same individuals. Personal income is calculated as the sum of wage and salary distributions, other labor income, proprietors' and rental income, personal dividend and interest income, and net transfer payments to residents.[8] Population data are mid-year Census Bureau estimates as of March, 1998, and per-capita personal income is personal income divided by population.

3. Measuring the Economic Impacts of Closures

A. Methodology

Traditionally, economic impacts of base closures have been projected, rather than estimated, using input-output (I-O) methods to extrapolate indirect job loss from a given reduction in military and civilian jobs on base.[9] There are many reasons why I-O projections may not be borne out; one we think is particularly important is that they ignore the capacity of regional economies to endogenously adjust to shocks. Physical assets at the base will have some potential for reuse, and new capital may be attracted to the area by the availability of resources -- including land, buildings, and experienced workers -- following a base closure. How much and how quickly endogenous job creation can offset the job destruction associated with a base closure is an empirical question we hope to answer.

The main issue in measuring the actual economic impacts is to estimate what would

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8. The personal income data are compiled from a wide variety of sources. About 90% of these estimates are based on censuses of agriculture, population and housing, and on administrative-records data, chiefly from unemployment and social insurance programs and tax and payroll records, that come from the Departments of Labor and Defense, the Social Security Administration, IRS, and Bureau of Veterans Affairs.

9. See, for example, H.C. Davis (1990).

have happened had a base remained open. We begin with the simplest possible baseline: assuming that the county would have grown, in employment and per-capita personal income, at its state's rate. That is, we assume that on average closure counties have the same trend growth rate as their states, and the non-closure shocks they received were equal, or that the trends and shock differentials on average cancel out. We choose the state growth rate as a baseline because there are likely to be common factors such as fiscal, regulatory, and industry-mix effects that influence county and state growth in a similar manner. We also use a differences-in-differences approach in which we assume that any pre-closure difference in county and state growth rates persists in the post-closure period.[10]

Given a counterfactual scenario, we compute job loss attributed to the closure by taking the difference between number of jobs that a county would have had, under the counterfactual assumption, and the number that it actually had, at any horizon after the closure. The empirical job loss multipliers are then ratios of these differences to the number of jobs lost directly in the closure. Per-capita income loss multipliers are computed in an analogous manner.

Let $E_{c,\tau+j}$ denote actual employment in closure county c, and $E_{s,\tau+j}$ denote its state's employment, j years after the closure, which occurs in year τ. Let $E^{*}_{c,\tau+j}$ denote the counterfactual level of employment (e.g. that which would have prevailed in that year if the closure county's employment had grown at the state rate). For the case where we use the year prior to the closure as the reference year,

(1) $E^{*}_{c,\tau+j} = E_{c,\tau-1} \, (E_{s,\tau+j}/E_{s,\tau-1}) \; \forall \, j > 0,$

and for the case where we use two years prior as the reference year (to allow for effects of the closure on τ-1),

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10. Because the differences-in-differences construction results in the loss of a substantial number of observations, and the results we obtain are very similar to those with the state growth baseline, we focus on the latter. The differences-in-differences results are discussed in section 4C.

(1′) $E^{**}_{c,\tau+j} = E_{c,\tau-2}\,(E_{s,\tau+j}/E_{s,\tau-2})\ \forall\, j > -1.$

Job loss j years after the date of closure from these two measures, $JL^*(j)$ and $JL^{**}(j)$, is given by the deviation between actual and counterfactual employment relative to the stock of jobs existing before the closure:

(2) $JL^*(j) = (E^*_{c,\tau+j} - E_{c,\tau+j})/E_{c,\tau-1}$

and

(2′) $JL^{**}(j) = (E^{**}_{c,\tau+j} - E_{c,\tau+j})/E_{c,\tau-2}$.

These job loss measures will include both the total direct job loss (civilian and military) associated with the closure, and any associated multiplier effects as well. The analogous impact variables are also constructed for per-capita personal income lost, denoted by IL^* and IL^{**}, again measured relative to the two counterfactual scenarios.

B. Results

Table 1 provides some summary statistics on the magnitude and distribution of employment and per-capita personal income responses. Panel A uses the year before the closure as the baseline, and Panel B two years earlier, allowing for a protracted closure or anticipation effects in the year prior. The left pair of columns contains results for jobs lost, and the right side for per-capita income lost. In each case, we report the impacts relative to the counterfactual baseline, at the two-year horizon and cumulated through the two-year horizon. We focus on the two-year horizon to allow some time for adjustment to the shock, while keeping the 13 observations from 1994 closures.

In the case with the year before the closure as the baseline, the employment shortfalls are substantial for both the mean and the median county. Two years after the closure, counties have on average more than 5% fewer jobs than if their employment had grown at the same rate as the state, and the median closure county has about 3.5% fewer jobs. The cumulative effects through year two are of course larger, representing the shortfall of "job-years" relative to the stock of jobs in the county at the date of the closure. They indicate that, on average, closure counties have a cumulative shortfall of jobs equal to nearly 14% of the base year employment. For the median county, this shortfall is slightly less than 9%.

While the employment loss appears to be substantial, typical per-capita income effects appear negligible. Cumulative effects for the mean and median counties at the two-year horizon actually suggest a 1% increase (positive table entries denote a loss, negative a gain) in per-capita income! Looking only at the two-year horizon, per-capita income for the average closure county is three quarters of a percentage point higher than if those counties had grown at the state rate. For the median county, it is one-half a percentage point higher. Thus, although closure counties appear to suffer a decline in the number of jobs, living standards for remaining residents are hardly affected. This implies that workers who lose their jobs due to the base closure either leave the county or find alternative income to replace lost wages.

Panel B summarizes the results that include the effects in the year before the closure. As the Table shows, it makes an important difference to the mean and median employment impacts. Using τ-2 as the baseline, the gap between actual and counterfactual employment as of year two is now over 7% for the average county and nearly 5% for the median county. The cumulative effects are also much larger, but this partly reflects the inclusion of an additional year of impacts in the sum. Changing the baseline also worsens the picture for per-capita income, but only slightly. Actual per-capita income exceeds counterfactual income at both the mean and median as of year two, but there is a cumulative shortfall of about half a percentage point when the four impact years are summed together.

Looking more closely at the differences arising from the choice of base year, we find that the effects are concentrated on a subset of bases where the year before the closure seems to have been quite important. The largest example is Quonset Point Naval Air Station in Washington County, Rhode Island, which officially closed in 1975. The county's employment grew four, eight, and eight percent faster than the state's in the years 1975-77, despite the fact that the closure destroyed over 10,000 jobs, amounting to 32% of 1973 employment. This appears to be a large outlier, until one incorporates the fact that employment fell 18% in 1974 (versus a 3% drop in state employment).

4. Estimating Employment and Income Multipliers for Base Closures

The descriptive statistics presented in Section 3 give an impression of the magnitude of employment and income loss associated with base closures. The basic story is that there is typically a substantial impact on employment associated with a base closure, but not much impact on per-capita income. We also found that for many counties, using two years before the closure as a reference point leads to considerably larger employment responses, suggesting gradual implementation of the closure or an anticipation effect. In this section we relate the impacts to their counterpart shocks, enabling the estimation of multipliers from our historical data. This will tell us whether the employment impacts are large only in an absolute sense, or whether they are also large in relation to the shocks. We also explore whether the multipliers are influenced by the type of shock (size and employment composition), base location (urban vs. rural), and base type (Air Force vs. others).

The multipliers that we estimate for base closures are a central focus of the paper, since they play an important role in the public policy process surrounding base closures. At present, the Department of Defense uses job loss multipliers that are based on input-output models of the regional economy in projecting the economic impacts of base closures. While these models are useful in describing the linkages between sectors at a point in time, they may fail to capture the ability of the economy to adjust to shocks. As a result, such multipliers may overstate the true economic impacts likely to result from a base closure. The "all else equal" assumption implicit in their predictions may be systematically violated by the natural response of market forces or by government policies.[11]

The multipliers constructed here represent an empirically-based alternative that takes account of both the self-correcting properties of markets and the effects of governmental assistance that has traditionally been provided to counties in the wake of a base closure. Since our multipliers summarize the experiences from base closures in recent U.S. history, they naturally provide a guide for predicting the likely outcomes of future closures.

A. Simple Multiplier Estimates

Table 2 reports simple employment and per-capita income multipliers, with the results for employment in the upper panel. The dependent variable is the shortfall of jobs at various horizons relative to the pre-closure county employment (the $JL(j)$ variable). While cumulative job and income losses may be better measures of the costs of shocks, we compare stocks at a point in time with shock sizes for consistency with most related literature. The independent variable is the size of the closure -- the total number of jobs eliminated by the closure, both civilian and military, as a percentage of pre-closure employment. The constant is constrained to be zero, since our framework assumes that, but for the base closure, counties would grow at the same rate as the state on average. The estimated multiplier is given by the regression coefficient.[12]

Turning first to the results with the τ-1 baseline, we see that the multiplier is significantly different from zero, but also significantly less than one in magnitude at all

11. The OEA estimates impacts on communities primarily as a function of the civilian jobs lost, and applies multipliers that are functions of the type of work done on the base. Multipliers range from 0.5 with shipbuilding to 2.0 for depos, and average in the range of 1.2-1.7 for a typical closure.

12. In these simple regressions with the intercept set to zero, the estimated multiplier is equal to the average response divided by the average shock. We use a regression framework because it allows us to condition on base characteristics, as in the next subsection.

horizons. The estimated multiplier peaks one and two years after the closure at 0.57, and then slowly declines to 0.53 four years after the closure. The (adjusted) R^2 value of the regression mostly declines steadily from the closure year onward, beginning at 0.52 and dropping to 0.22 four years after the closure. This suggests that the heterogeneity in county experiences increases as we move away from the closure date, which is not surprising, since the importance of other factors relative to the base closure will increase with time.

When we use τ-2 as the base year, the estimated multipliers are much higher in all years. The multiplier in the closure year is 0.69 and then increases to a peak of 0.97 three years after the closure; most of these values are not significantly different from unity at the 5% level. The adjusted R^2's are also considerably larger in these regressions, at 0.63 in the first two years, declining to around 0.50 in the last three years, which supports the view that either the closures occur gradually, or anticipation effects are important.

Although the multipliers are significantly different from zero in both cases and larger when τ-2 is the base year, it is striking that the estimated impact does not extend beyond the direct job loss associated with the base closure. At every horizon, and using both baselines, the multiplier is always estimated to be less than one. This implies that but for the direct job loss associated with the closure, employment in closure counties actually grows faster than the state baseline on average in our sample. There is no evidence in support of indirect or induced job loss due to the closure, but rather evidence of indirect or induced job creation![13] This result is consistent with Krizan's (1998) finding that workers' employment prospects *improve* following a base closure -- which he attributes to a redirection of spending by retired military personnel away from commissaries and towards local retailers.

It is not surprising, given the descriptive statistics in the previous section, that the

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13. Since our multipliers relate jobs lost in the closure to jobs that the county would have had if it grew at the state's rate, a multiplier of one corresponds to nonbase employment growth at roughly the state's rate.

estimated per-capita personal income multipliers are very different from the employment multipliers. Panel B of Table 2 shows that they are close to and usually not significantly different from zero. They often have a negative sign, indicating that closures typically lead to per-capita income growth in excess of the state's rate, and moreso the larger the shock. However, the only case in which a negative coefficient is statistically significant is at the 4-year horizon, which uses only 36 observations.

There are a couple of possible explanations for why per-capita incomes might remain steady in the wake of the base closure. First, military personnel generally have incomes below the civilian population in the county. For the counties in the database, military personal income per military job is about two-thirds of total personal income (earnings by place of work) per job.[14] The closure removes these individuals from the county average. Second, the same may be true for the civilian base employees who choose to leave the region and thus help account for the decline in employment. It seems likely that the selection process that determines which individuals will choose to move away from the county may be biased toward younger people who have lower incomes, since they are less likely to be homeowners and thus have lower costs of moving.

B. Sources of Variation in Multipliers

The next issue we address is whether the magnitudes of these multipliers are systematically related to observable features of the base closure. Theoretically, a number of factors might affect a region's ability to recover from a given shock. For example, bases in rural locations might have a lack of alternative employment in other sectors and less potential for reuse of base assets, and thus have larger multipliers. A related issue is whether particularly large shocks, relative to the local economy, might have larger multipliers. If a

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14. Military income per military job is higher than total income over total jobs in only 11 of the 57 counties. Nine of these have per capita income below the national median.

shock destroys a large enough fraction of jobs in a county, it may cause financial distress in the region, driving down asset values and with them the capital that local lenders need to help facilitate a recovery. We also consider whether the dynamics of adjustment to civilian and military job loss differ. Civilian employees of the base likely spend a higher fraction of their income off-base than do military employees. However, civilians are also more likely to look for alternative employment within the county rather than moving. Finally, we will consider whether the branch of service of the base influences the multiplier. It is possible that Air Force bases, for example, bring more highly skilled workers and more income on a per person basis. If so, a given employment shock could have a bigger multiplier.

In order to economize on the volume of information presented, we focus on estimating multipliers at the two year horizon with the τ-2 baseline. As mentioned above, the two-year horizon reflects a compromise between time for the dynamics to play out and keeping the many 1994 closure observations. The results are similar at other horizons. We use the τ-2 base year because of the evidence above suggesting that τ-1 impacts are important.

Our strategy is to add variables to the simple model reported in Table 2 to get a sense of which factors can cause the multiplier to vary across bases and counties. The first column of Table 3A reports a regression of jobs lost ($JL^{**}(2)$) on shock size and shock size squared. Our intention is to identify whether there are important non-linearities in the relationship between these variables. The squared term is negative, but not significant.

The second column reports the results when a "rural" dummy interacted with the shock size is added to the basic model. (All dummies are entered by interacting them with the shocks, since we are looking for factors which affect the multipliers, rather than those which contribute some fixed percentage job loss, as a separate dummy term would). Surprisingly, the interaction term is negative, suggesting a smaller multiplier at rural bases, although the coefficient estimate is far from significant. The third column reports an analogous test on an

Air Force base dummy. The AFB dummy is statistically and economically significant. -- Force base closures appear to have a multiplier that is 0.6 larger than, or twice the size -- that on other bases.

The fourth column reports the results obtained by splitting the direct job loss into -- civilian and military components. These are arguably the most interesting results in the --. Here, the civilian shock multiplier is near zero and insignificant, while the military mu-- is somewhat larger than unity with a very large t-statistic. A t-test for equality of the -- and military coefficients has a p-value of about .06.

These results have a clear explanation, which is also consistent with the simple regression results in Table 2. The military multiplier is close to one because military personnel are typically transferred to other bases, outside the county, in the wake of a --. There is a little evidence of spillover employment loss from military job reductions -- the coefficient is equal to 1.24, although it is not significantly different from one. By contrast, the civilian multiplier's near-zero value suggests that most civilian job losses -- replaced by offsetting civilian job creation within this two-year horizon. This unders-- again how surprisingly small the impacts of the base closures are, on average: they -- very little employment impact beyond the approximate 1:1 loss of military personnel.

The final column in Table 3A adds interaction terms between civilian and milit-- employment and the Air Force Base dummy variable. This column appears to conf-- -- importance of both the military employment and Air Force dummy variables. Both -- involving civilian employment are small and insignificant. The two military emplo-- terms are of about the same size, roughly splitting the coefficient of 1.24 between the-- although the interaction term is not significant at conventional levels (p-value of .1--.

Table 3B reports the analogous regressions using per-capita personal income los-- -- dependent variable. None of the regressions contribute much of anything to explain--

variation in income across the counties: no individual coefficients are significant in any of the specifications, and the adjusted R^2 values are mostly negative. When the shock is split into its civilian and military components, the sign on civilian shocks indicates an adverse effect on per-capita income growth, while that on military shock a favorable effect. While the difference is not statistically significant, it could be explained by the fact that civilian job losers are more likely to remain in the local labor market. When the shocks are large, wages are more likely to be depressed. By contrast, since military personnel are sure to leave the county, they may have little effect on per-capita income other than the positive effect of removing their income from the average.

To gain additional insight into the adjustment process, we examine the civilian labor market in isolation. Military personnel are unusual economic agents in two respects. First, they are routinely transferred out of the closure county. Second, they spend substantial fractions of their income on the base, rather than in the local economy. Focussing only on the civilian piece of the closure enables us to study an event that looks more like a plant closure in the private sector.

In Table 4, we re-estimate the simple regressions in Table 2 after eliminating military personnel from the county employment data. The shocks and impacts are then measured as the civilian job loss on the base relative to county civilian employment. This biases upward the estimated civilian job-loss multipliers, because the coincident military job loss is an omitted factor which is positively correlated with civilian job loss on the base, and positively correlated with civilian job loss outside the base from spillovers to the local economy. Alternatively, the multipliers estimated in Table 4 may be viewed as upper bounds of civilian job loss impacts.

The results in Table 4 confirm our interpretation of the job loss impacts being mostly confined to the military transfers: the estimated multipliers are considerably smaller and often insignificant. Using τ-1 as our base year, the multipliers peak at .35 in τ+1 and fall to .13 by τ+4, and are only significantly different from zero in the impact year and at τ+1 at the 10% level. The R^2 values are also much smaller then they are in the case where military employment is included.

When we use τ-2 as the base year, there is a substantial increase in multipliers, as was the case in Table 2. This is consistent with the interpretation of this base year as allowing for anticipation effects, since the difference in multipliers from the change in base years is due to civilian and not direct military job loss. The multipliers are now significantly different from zero out to the two-year horizon, the last one that uses the full dataset, but remain well below one at all horizons. It is worth emphasizing that our civilian job loss multiplier estimates are biased upwards by the omission of coincident military job losses.

Taken together, the employment and income results suggest that migration plays a role in the adjustment process. The behavior of population may help confirm the evidence on income and employment. Some basic statistics for county population loss are reported in Table 5. In general, population loss runs at about two-thirds that of employment. At the two-year horizon, the median population losses relative to the state baseline are 2.4% and 3.4% using the two base years, compared with 3.5% and 4.7% for employment. The median cumulative impacts through year two are, respectively, 5.6% and 7.1% compared with 8.8% and 14.3% for employment. Overall, the main impacts of a base closure appear to be an outflow of population and a somewhat larger reduction in employment. Thus, there is a decline in the employment/population ratio in these counties, which is apparently offset by an increase in income per worker, leaving per-capita personal income little changed.[15] It should

15. This evidence of economic impact is broadly consistent with the findings of Blanchard and Katz (1989) on the dynamics of regional labor markets. It seems that migration is an important equilibrating factor in the adjustment of regions to shocks. Many workers appear inclined to move rather than accept lower wages to remain in the region.

be kept in mind, however, that our employment data are place-of-work, whereas income and population are place-of-residence. Thus it is possible that an increase in the percentage of residents who hold jobs outside the county has occurred.

C. Robustness of Findings

A number of choices with regard to data and the construction of the counterfactual scenario could potentially influence the findings of the paper. In this section, we report on some modifications to the construction of our economic impact measures to examine the sensitivity of our findings to these choices.

First, we replace the wage and salary employment data with total employment, which includes the self-employed in the count, since the share of self-employed workers might change following a base closure. Next, we remove the impact county data from the state data on employment and income when constructing the counterfactual, allowing for the possibility that some closure counties contribute substantially to average outcomes for the state economy, particularly in small states. However, neither of these adjustments had a noticeable impact on our findings.[16]

As a further check on robustness, we modified our counterfactual scenario. Rather than assuming that closure counties would have grown at the same rate as the state in the absence of a closure, we assume that closure counties maintain the same differential growth rate, vis-a-vis the state, observed in the five years before impacts are measured -- i.e., from τ-6 to τ-2. If military base counties historically grew faster than state average growth, then assuming a state average growth baseline would lead us to underestimate the counterfactual growth path and thus the closure impact. This is essentially a differences-in-differences approach.

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16. These results are not reported to save space, but are available upon request.

The results of this exercise are reported in Table 6. Construction of the alternative counterfactual requires deleting closures that took place in the first 6 years, which eliminates 22 observations. The upper two panels of the table report the results using the differences-in-differences construction, while the bottom two panels re-estimate the state-growth baseline regressions using the same, smaller dataset. The 3- and 4-year horizons are not estimated since they entail the loss of several additional observations.

The estimated multipliers are substantially larger than those in Table 2, ranging from .52 to .90 with the τ-1 baseline, and slightly larger than unity with the τ-2 baseline at the one- and two-year horizon. However, the multipliers estimated with the state-growth baseline are virtually identical, suggesting that the differences are due to sample selection rather than a substantive difference from the choice of counterfactual scenario. In addition, neither the variation in sample nor choice of counterfactual scenario provides significant evidence of spillover effects beyond the 1:1 direct loss of military jobs.

5. Summary and Conclusions

In recent years, dozens of military bases have been closed over the often vociferous objections of affected communities and their representatives. This resistance to closures is based on the anticipated economic costs, despite there being almost no hard evidence on the overall effects of past closures. In this paper, we use a newly constructed dataset to analyze the employment and personal income effects on counties from closures that took place in the 1971-1994 period. Our estimated multipliers are mostly less than one, and considerably smaller than those typically used in input-output analyses of the impacts of base closures. Furthermore, we find that -- within a short time after the closure -- the employment costs are mostly limited to the direct job loss associated with military transfers out of the region, and per-capita income in a county is little affected by closures on average. Our results are consistent with Krizan (1998), who finds that recent base closures in California actually improved workers' employment prospects.

While this evidence suggests that, on average, military base closures have not caused significant economic damage to their communities, two factors call for caution in anticipating similar results from future closures. First, the counties have received technical and financial aid in their reuse efforts which may be important. Second, we do not necessarily observe a random sample of base counties; our sample may cover more-adaptable ones. In future work, we intend to examine more micro- data to distinguish the relative roles of market adjustment, government assistance, and self-selection in explaining the economic vitality of closure counties. With these qualifications in mind, the evidence suggests that local economies possess surprising resiliance in the face of external shocks.

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Table 1: Jobs and Per Capita Income Lost
Relative to State Growth Rate, Two Years After Closure

A. τ-1 baseline

	Employment		Income	
	In Year 2	**Through year 2**	**In Year 2**	**Through year 2**
min	-22.19%	-37.81%	-15.40%	-32.29%
max	38.07	84.38	15.56	32.65
mean	5.39	13.95	-.75	-1.05
median	3.48	8.76	-.42	-.89

B. τ-2 baseline

	Employment		Income	
	In Year 2	**Through year 2**	**In Year 2**	**Through year 2**
min	-16.62%	-37.03%	-22.76%	-55.32%
max	40.72	103.79	30.52	73.81
mean	7.18	20.71	-.34	.54
median	4.73	14.37	-.11	.58

Notes: "In Year 2" is the additional stock of jobs or per capita personal income that the county would have had, if it had grown at the state's rate, in the second year after the closure. "Through Year 2" is the *sum* of the additional flows of jobs or income in years 0, 1, and 2 (τ-1 base year case) and years -1, 0, 1 and 2 (τ-2 base year case). That is, for the without-anticipation effect case, In Year 0 + In Year 1 + In Year 2 = Through Year 2.

Table 2: Simple Regressions of Jobs and Per Capita Income Lost on Size of Closure Shock, at Different Horizons

A. Employment Regressions

1. τ-1 baseline

Dependent Variable:	*JL*(0)*	*JL*(1)*	*JL*(2)*	*JL*(3)*	*JL*(4)*
shock	.40	.57	.57	.56	.53
	(8.84)	(7.15)	(5.39)	(3.94)	(3.34)
R^2	.52	.42	.28	.29	.22
ser	4.18	7.35	9.77	10.78	12.20

2. τ-2 baseline

Dependent Variable:	*JL**(0)*	*JL**(1)*	*JL**(2)*	*JL**(3)*	*JL**(4)*
shock	.69	.90	.93	.97	.96
	(12.32)	(11.56)	(8.98)	(7.30)	(6.17)
R^2	.63	.63	.49	.57	.46
ser	4.85	6.77	9.01	9.50	11.15

B. Per Capita Income Regressions

1. τ-1 baseline

Dependent Variable:	*IL*(0)*	*IL*(1)*	*IL*(2)*	*IL*(3)*	*IL*(4)*
shock	.01	-.07	-.09	-.15	-.29
	(.25)	(-1.60)	(-1.45)	(-1.53)	(-2.69)
R^2	.01	.03	.02	.04	.11
ser	3.88	4.22	5.56	7.65	8.24

2. τ-2 baseline

Dependent Variable:	*IL**(0)*	*IL**(1)*	*IL**(2)*	*IL**(3)*	*IL**(4)*
shock	.10	.01	-.01	-.06	-.22
	(1.69)	(.17)	(-.08)	(-.53)	(-1.48)
R^2	.03	.00	.00	.01	.04
ser	5.15	5.69	8.20	8.19	10.77

Notes: Shock is the sum of military and civilian jobs lost in the closure as a percentage of county employment in the reference year before the closure. Regressions at horizons 0-2 years use full 57-closure dataset; 3- and 4-year horizon regressions use 36 observations (21 closures took place in 1993 and '94). *t*-statistics in parentheses; R^2 is adjusted; ser represents standard error of the regression.

Table 3A: Multiple Regressions of Jobs Lost on Closure Shocks

Dependent Variable is *JL**(2)*: stock of jobs lost at two years after closure, τ-2 baseline case

shock	1.29	1.09	.60		
	(4.05)	(3.44)	(4.22)		
shock^2	-.01				
	(-1.18)				
shock*Drur		-.17			
		(-.52)			
shock*DAFB			.60		
			(3.11)		
civ				.15	.26
				(.36)	(.60)
civ*DAFB					.02
					(.02)
mil				1.24	.79
				(6.54)	(3.05)
mil*DAFB					.70
					(1.57)
R^2	.44	.48	.56	.54	.57
ser	8.98	9.07	8.38	8.80	8.43

Notes: *t*-statistics in parentheses; R^2 is adjusted. Shock is as in Table 2; civ and mil are the civilian and military jobs lost in the closure as a percentage of county employment two years before the closure. Drur is a dummy for rural counties, defined as having less than 75,000 population in 1970 and lacking proximity to a large metropolitan area. DAFB is a dummy for Air Force Base closures, which are overrepresented in outliers. *t*-statistic for mil and civ coefficients equal is 1.93, *p*-value = .06.

Table 3B: Multiple Regressions of Income Lost on Closure Shocks

Dependent Variable is *IL**(2)*: per-capita personal income lost at two years after closure, τ-2 baseline case

shock	.14 (.48)	-.19 (-.64)	.04 (.29)		
shock^2	-.01 (-.53)				
shock*Drur		.20 (.66)			
shock*DAFB			-.09 (-.46)		
civ				.37 (.95)	.41 (.97)
civ*DAFB					-.38 (-.32)
mil				-.16 (-.88)	-.16 (-.61)
mil*DAFB					.08 (.19)
R^2	-.01	-.01	-.01	.00	-.03
ser	8.25	8.24	8.26	8.20	8.35

See notes to Table 3A. t-statistic for mil and civ coefficients equal is 1.00, p-value = .32.

Table 4: Simple Regressions of Jobs Lost on Size of Civilian Shock – Employment Excluding Military

τ-1 baseline case

Dependent Variable:	*JL*(0)*	*JL*(1)*	*JL*(2)*	*JL*(3)*	*JL*(4)*
civ	.33	.35	.24	.30	.13
	(2.87)	(1.73)	(.83)	(.75)	(.27)
R^2	.12	.07	.03	.05	.03
ser	3.04	5.34	7.67	9.30	10.70

τ-2 baseline case

Dependent Variable:	*JL**(0)*	*JL**(1)*	*JL**(2)*	*JL**(3)*	*JL**(4)*
civ	.64	.69	.61	.74	.58
	(3.75)	(2.88)	(1.83)	(1.61)	(1.09)
R^2	.15	.11	.05	.08	.04
ser	4.53	6.34	8.74	10.48	12.10

Notes: Regressions at horizons 0-2 years use full 57-closure dataset; 3- and 4-year horizon regressions use 36 observations (21 closures took place in 1993 and '94). *t*-statistics in parentheses; R^2 is adjusted; ser represents standard error of the regression. The shock measure is civilian jobs lost to the closure as a percentage of non-military jobs in the reference year before the closure, and the responses similarly exclude military employment.

Table 5: Population Lost Relative to State Growth Rate, Two Years after Closure

A. τ-1 baseline case

	In Year 2	Through year 2
min	-6.75%	-14.41%
max	20.39	61.22
mean	4.16	10.06
median	2.39	5.61

B. τ-2 baseline case

	In Year 2	Through year 2
min	-8.98%	-22.87%
max	22.77	71.12
mean	4.89	12.88
median	3.43	7.14

See notes to Table 1.

Table 6: Simple Regressions of Jobs Lost on Size of Closure Shock – Different Counterfactual Trends

A. Counterfactual Trend is State-County Differential in 5 years Before Closure

1. τ-1 baseline case

Dependent Variable:	*JL*(0)*	*JL*(1)*	*JL*(2)*
shock	.53	.86	.91
	(10.99)	(8.94)	(6.43)
R^2	.72	.61	.43
ser	2.64	5.29	7.78

2. τ-2 baseline case

Dependent Variable:	*JL**(0)*	*JL**(1)*	*JL**(2)*
shock	.67	1.03	1.09
	(11.04)	(10.33)	(7.60)
R^2	.67	.65	.50
ser	3.31	5.41	7.83

B. Counterfactual Trend is State Growth Rate

1. τ-1 baseline case

Dependent Variable:	*JL*(0)*	*JL*(1)*	*JL*(2)*
shock	.52	.85	.90
	(11.71)	(9.84)	(7.12)
R^2	.74	.66	.49
ser	2.95	5.78	8.47

2. τ-2 baseline case

Dependent Variable:	*JL**(0)*	*JL**(1)*	*JL**(2)*
shock	.66	1.01	1.08
	(9.94)	(10.28)	(7.94)
R^2	.63	.65	.52
ser	4.35	6.48	8.94

Notes: The results in part B are computed as in Table 2, excluding the observations that are lost in constructing the differences-in-differences counterfactual. That counterfactual projects employment to increase at the state's rate less the average state-county differential over τ-6 to τ-2, where τ is the closure year. The 22 closures that took place before 1976 are thus excluded; regressions are only done at 0-2 year horizons because an additional 21 would be lost going to the 4-year horizon. *t*-statistics in parentheses; R^2 is adjusted; ser represents standard error of the regression.

Figure 1: Direct Job Loss in Base Closures

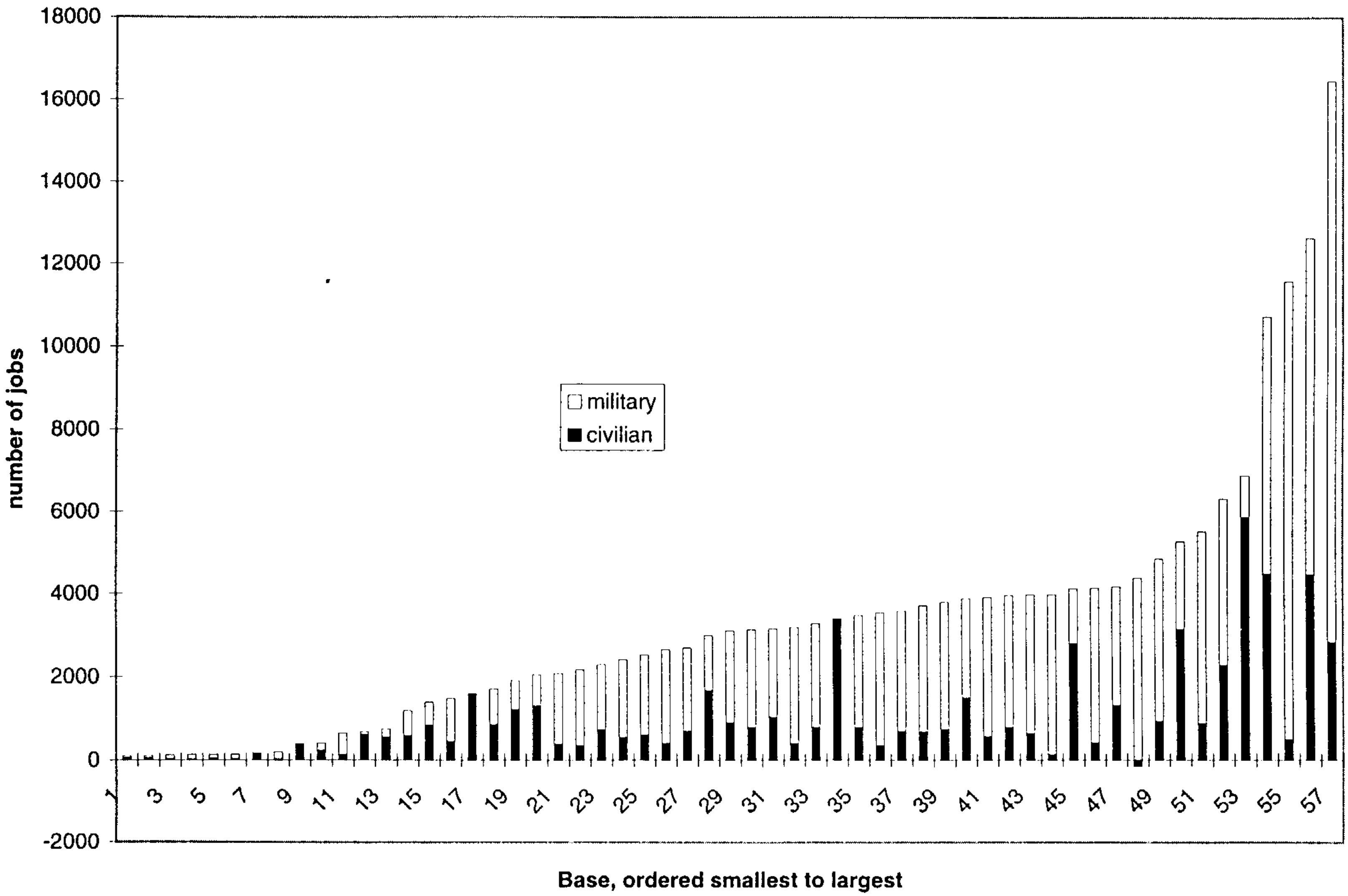

Figure 2: Direct Job Loss in Base Closures as a Percentage of County Employment