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Fat: The Displacement of Nonproduction Workers from U.S. Manufacturing Industries

IN THE LONG RUN American industry has relied increasingly on nonproduction staff, yet in the last decade many white-collar employees have been squeezed out of large corporations in the name of increased efficiency. Were exceptional shocks and competitive pressures responsible for inducing corporate weight-loss campaigns? How could administrative "fat" have accumulated in the first place? That large, successful corporations tend to acquire bloated staffs is a commonplace in popular discourse, and many economists give credence to this behavior when they seek (and find) favorable effects on productivity of management buyouts, "refocusing" of diversified enterprises, the excision of layers of supervisory management, and other reorganizations put forth as means to improve productivity. Yet only with caution does one maintain any hypothesis about productivity shortfall or technical inefficiency, lest one seem ignorant of the Law of Cash-Strewn Footpaths: if cost-efficiency could be improved, somebody would already have profited by improving it.

In this paper we investigate the possibility that nonproduction employment in U.S. manufacturing behaves as if fat could be excised by the squeezing. The investigation proceeds through two stages. First, working with disaggregated manufacturing industries observed during 1967–86,

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we ask whether industry-level nonproduction employment was reduced by circumstances that render excess nonproduction employment no longer viable: mergers (distinguished as "related" or "unrelated") and changes in imports' share of U.S. consumption at those times and in the expected sectors. We then examine announcements of corporate downsizings during 1987–91 to observe how the stock market reacted. The results, although qualified, are satisfyingly consistent: by the 1980s bursts of both import competition and changes in corporate control did significantly reduce nonproduction employment, and shareholders came to react positively to downsizings that involved white-collar layoffs and related reorganizations.

For a framework this analysis draws upon the hypothesis that firms—especially successful large firms—are organizational coalitions capable of employing and retaining levels of nonproduction employment in excess of what would maximize their profits. The hypothesis has this implication, central to our statistical test: unanticipated disturbances that shrink the capacities of these coalitions to meet members' reservation demands force reductions in white-collar employment.[1] At most we expect to establish that this framework provides a sufficient explanation for some recent changes in nonproduction employment. To show necessity would require the impossible—ruling out all plausible reasons why (for example) reduced nonproduction employment might be a value-maximizer's efficient submissive response to an upsurge of import competition.

Nonproduction Employment in Manufacturing: Quantitative Patterns

During the prosperous 1980s many white-collar employees discovered that the presumptively secure ground beneath their feet had become shaky. The growth of white-collar employment in large corporations was arrested, and a shift in the distribution of employment toward smaller companies caught public attention. Some economists voiced concern that takeovers and other changes in corporate control among large firms were occasions for breaking long-term employment contracts with workers who had accepted wages less than their marginal products in their younger days in anticipation of excess compensation

1. We use the term "white collar" synonymously with nonproduction labor, the empirical focus of this paper, although they are not exactly congruent.

in their maturity.[2] The recent recession launched an unprecedented assault on white-collar employment; in the year following August 1989, white-collar workers accounted for almost two-thirds of the increase in unemployment.[3] Business gurus lauded the process, urging the breakdown of "functional silos" of bureaucratic authority and the creative use of information technologies.[4]

Changes in nonproduction employment depend, of course, on changes in its use as an input efficiently combined with others as well as on any changes in its excess use. The long-run trend has been upward: the proportion of nonproduction workers in total employment in U.S. manufacturing rose from 28.3 percent in 1973 to 35.4 percent in 1987.[5] Berman, Bound, and Griliches found that this increase resulted both from changes in the composition of manufacturing industries and from increased proportions of white-collar workers in the typical four-digit industry. At least in the 1980s this upgrading of the labor-skill input proceeded in the face of rising relative wages for nonproduction employees. In cross-section Berman, Bound, and Griliches found the complementarity of capital and skill to be statistically significant although not an important factor, and they also linked changes in nonproduction workers' share of employment (1979–87) to industries' rates of investment in computers, their rates of research and development (R&D) activity, and (less formally) to plants' use of new technologies.[6]

We sought evidence on changes in white-collar employment patterns in the *Occupation by Industry* data of the population census, only to find 1990 data not yet available and the analysis of changes during 1970–80 hobbled by a major change in the classification system. The earlier period, however, yielded some evidence on the distribution of patterns among manufacturing industries. We concluded that for fifty-three two- and three-digit manufacturing industries, it was possible to determine the numbers employed in 1970 and 1980 for each category

2. Amanda Bennett, "Laid-Off Managers of Big Firms Increasingly Move to Small Ones . . . ," *Wall Street Journal*, July 25, 1986, p. 23; Shleifer and Summers (1988).
3. Bureau of Labor Statistics data quoted in Michael J. Mandel, "This Time, the Downturn Is Dressed in Pinstripes," *Business Week*, October 1, 1990, p. 130.
4. See Hammer and Champy (1993).
5. Berman, Bound, and Griliches (1993, table 2).
6. Berman, Bound, and Griliches (1993). Notice that their analysis dealt with employment in manufacturing establishments and not with administrative and related establishments, which house an increasing proportion of nonproduction employees.

Table 1. Distribution of Increases of Nonproduction Employment Adjusted for Increase of Real Output, 1970–1980

Percentage

Type of nonproduction employment	*25th percentile*	*Median*	*75th percentile*
Executive, administrative, managerial	36.4	20.6	-3.4
Professional specialty, technical support	10.9	-10.5	-35.2
Administrative support	11.4	-1.6	-29.2
Services	14.7	-4.5	-21.8

Source: Calculated from Bureau of the Census (1972) and Bureau of the Census (1984) (see text).
Note: Each line reports the distribution across 53 manufacturing industries of the percentage increase in the nonproduction employment category minus the percentage increase in real output. As a measure of "excess growth" of nonproduction employment, this difference is biased downward, but comparisons along the columns of the table should be unbiased.

of nonproduction workers shown in table 1.[7] We wanted to determine the relative growth of these employment classes, taking changes in real output into account. We first adjusted crudely for each industry's change in real output by calculating the percentage change in each employment category and subtracting the percentage change in its real output.[8] This adjustment would be correct if the elasticity of nonproduction employment with respect to real output were unity; it is probably smaller (as our regression analysis will suggest), but the discrepancy is unlikely to distort the distribution among industries of "excess" changes for one class of nonproduction employment relative to another class. Results are shown in table 1. In the median industry, employment in executive, administrative, and managerial occupations (including sales) grew far and away the fastest. Professional specialty and technical support personnel—the "knowledge workers" of the white-collar cadre—grew the slowest, with administrative support staff (secretaries,

7. The change in the classification system for occupations and its consequences for comparability over time are described in Bureau of the Census (1989). Our method of reclassifying 1970 employment to the revised 1980 categories was suggested in correspondence by Thomas S. Schopp. The comparison of 1970 to 1980 employment by industry is further complicated by the change in the Standard Industrial Classification in 1972, which rendered some industries noncomparable between 1970 and 1980.

8. For changes in real output, we relied on the data base that supports the Department of Commerce's annual *U.S. Industrial Outlook.* See Bureau of Industrial Economics (1983), pp. A-2–A-19.

for example) and service workers (security personnel and the like) in between. The same differential is evident at the twenty-fifth and seventy-fifth percentiles of the distributions. Because the adjustment for the industry's change in real output is biased upward and the estimated excess growth rates of employment are thus biased downward, the median industry's large excess figure for executive occupations (20.6 percent) is particularly arresting. How this increase in the 1970s was divided between efficient new technologies of organization and squeezable fat is an interesting question.

In the 1980s, when losses of white-collar jobs accelerated, manufacturing employment remained about stationary after the recovery from the 1981–82 recession. The data presented in table 2 show this both in the aggregate and for the major white-collar categories. Since 1979 the Bureau of Labor Statistics has surveyed employees displaced from jobs they had held for three or more years. In general these displacement rates (the proportion of total employees in the category who lost jobs) vary as one would expect with aggregate employment. Table 3 compares displacement rates for the periods 1979–83 (embracing a recession) and 1985–89 (covering prosperous years). Displacement rates were lower in the latter period in the aggregate and in manufacturing (although not all service sectors). Among occupation groups displacement rates are, as expected, lower for white- than for blue-collar workers. In managerial and professional specialty occupations, however, displacements showed no decline between the two periods, and the declines for other white-collar categories were smaller than for blue-collar workers. The same conclusions follow if 1983–87 rather than 1985–89 is compared with 1979–83. In the mid-1980s nonproduction workers evidently found themselves in less firmly tenured positions than before. Apparently, the pattern continued into the recent recession; according to the Bureau of Labor Statistics, of the 485,000 workers added to the unemployed between August 1989 and August 1990, 34.6 percent were managers and professionals, 30.5 percent clerical, 8.0 percent sales and technical personnel, and only 18.6 percent blue-collar (including construction workers).[9]

Also relevant to the question of excised fat is how hard displaced employees find it to regain jobs and how much deterioration of terms

9. Figures quoted in Mandel, *Business Week*, pp. 130–31.

Table 2. Employment of Nonproduction Workers, U.S. Manufacturing Sector, 1983–1987

Thousands

Year		*Executive, administrative, managerial*	*Professional specialty*	*Technicians, related support*	*Sales*	*Administrative support*	*Service*	*All*
1983	D[a]	1,195	1,028	509	271	1,393	204	11,708
	ND	816	491	203	391	1,045	184	8,238
1984	D	1,370	1,083	530	307	1,494	213	12,606
	ND	836	486	209	440	1,051	173	8,389
1985	D	1,419	1,148	540	300	1,464	227	12,586
	ND	869	489	206	415	1,065	173	8,293
1986	D	1,416	1,192	567	295	1,425	210	12,605
	ND	878	510	198	425	1,060	177	8,357
1987	D	1,421	1,166	496	292	1,410	196	12,478
	ND	894	522	202	412	1,060	164	8,456

Source: Bureau of Labor Statistics (1988, table B-13, pp. 655–63).

[a]D indicates durable-goods manufacturing industries, ND nondurables.

Table 3. Displacement Rates for Selected Industries and Occupations, 1979–83 and 1985–89

Percentage

Employee group	*Years* 1979–83	1985–89
Total	8.3	6.4
Industry		
Mining	26.6	22.0
Construction	19.2	12.3
Manufacturing	16.7	11.4
Durable goods	18.4	12.1
Nondurable goods	14.0	10.2
Transportation and public utilities	8.8	6.7
Wholesale and retail trade	8.4	8.7
Finance, insurance, and real estate	2.9	6.6
Services	5.6	4.8
Occupation		
Executive, administrative, and managerial	5.9	5.9
Professional specialty	3.1	3.1
Technicians and related support	7.3	6.2
Sales	7.9	6.5
Administrative support, clerical	5.7	6.0
Service occupations	4.3	3.7
Precision production, craft, and repair	12.7	8.0
Operatives, fabricators, and laborers	16.9	11.3

Source: Bureau of Labor Statistics (1991, table 3, p.4).

of employment they accept. Of displaced employees who were reemployed in January 1990, in the aggregate 43.2 percent reported accepting lower wages or salaries than before. In manufacturing and in transportation and public utilities, these proportions were higher, 49.1 and 51 percent, respectively, and in durable-goods manufacturing they were higher still (50.9 percent overall, 51.7 percent in nonelectrical machinery, and 59.5 percent in transportation equipment). The data are consistent with either quasi-rents (to skills) or rents having been lost more commonly in manufacturing than elsewhere; the data unfortunately are not broken down by occupation.[10] Also relevant is the frequency with

10. These data are taken from Bureau of Labor Statistics (1991, table 5, p. 6). Also see Herz (1990, table 11, p. 31), where data for workers reemployed in January 1988 are supplied for more industries. The proportion reporting lower earnings was 44.6

which employees displaced in the 1980s were reemployed in different lines of work. Although two-thirds of administrative support workers were reemployed in similar jobs, fewer than half of the executives, administrators, and managers found jobs similar to their former ones (17 percent accepted sales jobs, 14 percent administrative-support occupations).[11]

These data do not specifically tie the displacement of white-collar workers to the downsizing of large corporations, but the downsizing itself is readily shown. In 1978, 48.6 percent of all workers were employed in companies with fewer than one hundred workers, but in 1984 the figure had risen to 51 percent, and employment in companies with more than one thousand workers fell from 18.6 to 16.2 percent.[12] These discharges are held to accompany the removal of layers of middle management, shortening lines of communication within large enterprises, and increasing the reaction speeds of those who remain, but the linkage of displacements to such reorganizations has not been quantified.[13]

Why Nonmaximizing Employment of Nonproduction Workers?

That nonmaximizing behavior in large firms is necessary to explain these patterns is not a hypothesis that we maintain. It might explain some of the movements of nonproduction employment, however, depending on the mechanisms that can shield white-collar employment from the reach of the profit-seeking manager. We consider two factors:

percent for all manufacturing, 46.2 percent for durable goods, 63.5 percent for primary metals, 50.5 percent for fabricated metal products, 43.7 percent for nonelectrical machinery, 30 percent for electrical machinery, 50 percent for automobiles, and 62.7 percent for other transportation equipment.

11. See Herz (1990, p. 31). From the surveys of displaced workers, Farber (1993) developed various conclusions that are complementary to these. He found higher rates of job loss for older and better-educated workers in 1990–91 than occurred during the 1980s, and the difference is not associated with a rate of plant closings higher than in earlier years. He does question whether older and better-educated workers who were displaced recently suffered a significant decrease in the probability of obtaining a new job.

12. Bureau of Labor Statistics data quoted in Bennett, *Wall Street Journal*, p. 23.

13. Carol Hymowitz, "When Firms Cut Out Middle Managers, Those at the Top and Bottom Often Suffer," *Wall Street Journal*, April 5, 1990, pp. B1, B6.

the ease with which managers can measure the revenue productivity of nonproduction employees, and the ways business goals other than profit-maximization might affect the number of nonproduction workers recruited and retained.

Measuring White-Collar Productivity

Nonproduction jobs are diverse, and some of them (such as sales representatives) generate revenue products that probably are as easily measured by managers as those of most production-worker jobs. Nonetheless, many white-collar employees engage in team production, making the output of the individual worker difficult or impossible to observe accurately. That managers grope for efficient organizational structures in the face of prevalent team production is presumed by major lines of the organizational theory of the firm.[14] Furthermore, even when outputs (whether team or individual) can be measured in physical terms (memoranda produced?), it is far from obvious that the physical product can be related to revenue productivity for the firm.

For evidence on this conjecture, we turned to the literature on personnel administration. The views we found there concur that white-collar output can, at best, be measured in forms that cannot be translated into revenue productivity. Caution is urged in the use of such approximate measures; typically they capture imperfectly the tasks that white-collar employees are directed to perform, and their use in incentive and reward schemes can readily distort the allocation of effort. One literature survey flatly states that a broadly acceptable approach to measuring white-collar productivity does not exist. The practitioner literature has turned to finding ways to improve productivity while finessing the problem of how to measure what is being improved.[15]

It seems clear that the would-be value-maximizing manager cannot accurately make the marginal product–wage comparison needed to op-

14. Alchian and Demsetz (1972); Holmstrom (1982); and Holmstrom and Tirole (1989). Winter proposed that the firm's administrative cadre is engaged in producing important ''unconventional assets'' that are not specifically observable outside the firm and unnecessary to current production but that do sustain the continuation value of the firm in the long run. Inputs to this production process are indistinguishable from inputs that represent pure fat. See Sidney G. Winter, ''Routines, Cash Flows, and Unconventional Assets: Corporate Change in the 1980s,'' in Blair (1993, pp. 55–97).

15. See the literature survey of Sumanth, Omachonu, and Beruvides (1990) and the papers contained in Lehrer (1983).

timize white-collar employment. Furthermore, quantification is more elusive for the central managerial hierarchy and its support personnel than it is for specialists and service personnel, whose outputs are less commingled in team production. The situation accords with economists' habit of treating the firm's managerial hierarchy as a cost that is fixed although avoidable upon shutdown, an implicit confession of ignorance as to how this cost varies with the scale and other dimensions of the firm's activities.

Managerial Behavior

If lack of information on white-collar workers' value productivity impedes the precise optimization of actual nonproduction employment, the play of managers' and employees' objectives might lead to excessive white-collar employment, as several lines of analysis suggest. Oliver Williamson's nominees for objectives in the managerial utility function include two that favor excess white-collar employment. First, it is directly inflated by a preference for "staff," assistants who contribute to the ease of or satisfaction derived from top executive jobs. Second, it is indirectly enlarged by a preference for taking decisions of large scope, because staff are presumably needed to evaluate and execute the grand designs that such decisions involve.[16]

The inflation of white-collar employment is also a conditional prediction of what we call the Carnegie approach to the organization and behavior of the firm. That school emphasized not the objectives of the chief executive as "principal" in vertical contracts with the firm's employees, but the preferences of functional specialists whose lateral contracts specify their respective contributions, responsibilities, and expected rewards and thereby define a synthesized objective function for the firm as a whole. In the comparative statics of this model as developed by Cyert and March, an excess of revenue to the firm over the minimum payments demanded by the coalition members represents "slack" that can be absorbed as side payments (either pecuniary or policy payoffs) as well as reported excess profits.[17] This lateral contracts approach is notably consistent with the idea that the ongoing firm operating in an uncertain environment possesses a repertory of team-based

16. Williamson (1963).
17. Cyert and March (1963).

skills that rest on tacit knowledge. Plying these skills in light of revealed opportunities and threats depends on the cooperation of disparate team members and not some chief engineer's master blueprint.[18]

That policy payoffs from slack could lead to expanded white-collar employment was argued indirectly by Niskanen.[19] The government bureaucracy was his prototype, but he remarked that the analysis applies to any component of a firm that is not a profit center but subject to budgetary financing by a central decisionmaker. Not only do regular employees gain personally in various ways when their bureau expands, he argued, but, indeed, their advocacy of expanded projects and responsibilities is necessary to the central authority's process of screening budgetary options. Yet the central authority is asymmetrically ill-informed about the minimum inputs that the bureau needs to achieve any given objective and is thus unable to resist the bureau's desire to absorb slack by expanding, even if the central authority lacks confidence in the average and marginal efficiency of the bureau's production process. This conflation of Niskanen with the Carnegie approach is the most coherent explanation we can find of the emergence of white-collar fat in successful (or *once* successful) enterprises whose viability does not demand cost-minimization.

Notice how Niskanen's bureaucratic expansionism interacts with the difficulty of measuring white-collar productivity. A popular commonplace holds that bureaus tend to create work for each other, as each pushes its own agenda at the expense of the agendas of other bureaus. Bureau *A* expands its tasks by devising new types of information to gather and analyze, causing Bureau *B* to expand its staff in order to provide the information. Bureaus' rates of memo production become strategic complements, and high rates of nominal productivity can correspond to substantive stalemate and inaction for the enterprise.

The hypothesis that organizational fat accumulates in successful business enterprises will surprise no reader of journalistic accounts of the troubles of General Motors, IBM, and the like.[20] A theme that

18. Nelson and Winter (1982, chapter 5) developed this point extensively. Notice the consistency between this approach and the hypothesis that the marginal products of most white-collar staff are effectively unobservable.

19. Niskanen (1971).

20. See, for example, Paul B. Carroll, "Culture Shock: Story of an IBM Unit that Splits Off Shows Difficulties of Change," *Wall Street Journal*, July 23, 1992, pp. A1,

surfaces in the literature on U.S. competitiveness is that American enterprises succumb to foreign competitors because of Niskanen-type bureaucratic insularity and noncooperation within enterprises.[21] Most of the academic evidence that supports the hypothesis comes from investigations of the market for corporate control and will be noted subsequently. Working with British data, Nickell, Wadhwani, and Wall found that high debt-equity ratios favor both levels and growth rates of productivity.[22] Caves and Barton found that the inefficiency (the gap between average and best-practice productivity) of U.S. manufacturing industries in 1977 increased significantly with the extent of "inbound diversification"—control of establishments by enterprises based in other industries—although it was unaffected by the absolute sizes of the largest firms based in the industry in question.[23]

Research Design: Nonproduction Employment and Competitive Disturbances

We first analyze the determinants of changes in nonproduction employment in U.S. manufacturing industries during 1967–86. Specifically, we inquire whether white-collar employment was affected by rent-threatening disturbances—international competition and activity in the market for corporate control—after we control for the principal determinants of changes in equilibrium nonproduction employment.

A5; Bradley A. Stertz, "Importing Solutions: Detroit's New Strategy to Beat Back Japanese Is to Copy Their Ideas," *Wall Street Journal,* October 1, 1992, pp. A1, A12; and David Woodruff, "GM Slices and GM Slashes, But the Flab Survives," *Business Week,* December 23, 1991, p. 27.

21. See Dertouzos, Lester, and Solow (1989, chapter 7). Evidence also appears in case studies such as Rayner (1975).

22. Nickell, Wadhwani, and Wall (1992); see also Geroski (1989).

23. Caves and Barton (1990, pp. 91, 96–99, 127–28). The data did not permit identifying the diversified plants of multi-industry firms as the specific culprits, but they did allow localizing the inefficiency to each industry's larger plants, in which these should be overrepresented. Among the many other influences controlled was oligopolistic behavior, which, indeed, reduces efficiency where levels of seller concentration are moderate or higher. The test of corporate diversification's effect could not be replicated exactly on other industrial countries, but the relationship appears unique to the United States; see Caves and others (1992, chapter 1).

This effect of changes in corporate control on the firm's nonproduction employment has already been studied. It is important, however, to pursue the analysis to the level of the industry:

- The significant downsizing that follows changes in control of the firm might or might not exert a substantial effect at the level of its industry. Downsizings inflicted on particular firms might represent merely part of the constant churning of an industry's distribution of firms by size, whereby some leaders lose their grip and regress to the mean, to be replaced by today's comers. Are industries affected as a whole?
- The disciplinary effect of changes in corporate control is often thought to spill over to onlookers. Whether witnesses are chastened by demonstrations observed in their industries, their cities, or their country clubs is unknown. As a first cut it seems worth testing whether an industry's nonproduction employment decreases with the assets of that industry's firms subject to current and recent changes in control.
- An industry as a whole sometimes faces a major disturbance that shrinks the expected cash flows of its member firms. The major step-ups in import competition that have afflicted numerous oligopolies in U.S. manufacturing are a conspicuous example. When such a disturbance could excise fat from all of an industry's firms (in addition to the employment change directly associated with the induced change in the industry's output), it becomes desirable to test the hypothesis at the industry level and to ignore any incidental reallocation of activity among its member firms.
- Data on nonproduction employment are not available at the level of the firm, but data that include administrative and auxiliary establishments can be constructed for manufacturing industries from published census data.

We estimated a model of the determinants of changes in nonproduction workers in U.S. three-digit manufacturing industries during 1967–86, testing whether they were affected by disturbances that could make excess nonproduction employment less viable. It did not prove feasible to develop a structural model to capture shifts in nonproduction-labor demand and supply that should affect these changes. We do control for the putative determinants of demand changes—changes in real output and relative input prices—in testing whether an industry's white-collar employment declined following increases in its import competition and in its rate of turnover in corporate control. Was the shrinkage greater in sectors that were a priori more likely to run to fat? We set forth the details of the research design in the course of explaining its various features.

Quinquennial Changes 1967–86

The panel structure uses proportional changes over the periods of the successive Censuses of Manufactures, 1967 to 1972, 1972 to 1977, 1977 to 1982, and 1982 to 1986. That is, the dependent variable to be explained will be the logarithm of the number of nonproduction employees in the final year minus the logarithm of the number in the initial year. Five-year changes were selected for investigation. We were not interested in the short-run issues associated with labor hoarding and partial adjustment processes, and we believed that important but slow-acting disturbances to nonproduction employment could be detected from differences among these four quinquennial changes.

The 1967–86 span of the analysis was driven by data considerations. The years 1972 to 1986 provide the core of our data set. The Standard Industrial Classification underwent a moderate change in 1972, limiting the number of three-digit industries that could be traced back to 1967, but we nonetheless made use of 1967–72 as a base period with broadly normal economic conditions; it was not subject to the inflation of the 1970s, and the main force of increased import competition and disciplinary transactions in the market for corporate control was still to come. With regret we closed the analysis in 1986 because of a major overhaul of the Standard Industrial Classification for the 1987 Census of Manufactures and the termination of our data source on import competition. The descriptive evidence cited previously suggests that the squeeze on white-collar employment was strongly felt before 1986, but the process has apparently continued to this day. Indeed, the data for 1986 are cobbled together from 1986 observations on some variables but, for others, from 1987 observations converted to a 1986 basis on the assumption that rates of change were constant between 1982 and 1987.

Production and Administrative Establishments and Industry Classification

We wanted to analyze industries disaggregated into well-defined product markets, which usually means four-digit industries in the Standard Industrial Classification. For this analysis, however, it is vital to include not only the nonproduction employees attached to manufacturing establishments (reported at the four-digit level), but also nonproduction employees in auxiliary establishments (allocated by the Census Bureau to three-digit, but not four-digit, industries). Not only are many nonproduction employees of large companies located in central administrative establishments and other office facilities away from plants, but also the proportion of white-collar employees working at nonfactory locations has risen steadily over the years.[24] When four-digit data are aggregated to the three-digit level, only a modest loss of information occurs. Auxiliary-establishment employees, however, toil for firms whose activities might be spread over many four-digit industries, and this diversification necessarily injects substantial noise. Another relevant (and regretted) factor is the less-than-credible jumps observed in some industries' auxiliary-establishment data from census to census. The reclassification of a few large firms between industries could cause jumps, of course, but doubts begin to gnaw when the jumps occur in data on industries little involved in diversification or when (for example) similar values for 1972 and 1982 surround a divergent value reported for 1977. For these reasons we estimated each model twice, once with the dependent variable based on total nonproduction employees and once on only those based at manufacturing establishments (for which the data seem free of this problem). A statistical relationship significant for the latter could be insignificant for the former because of noisy data rather than a false hypothesis.[25]

24. See Lichtenberg and Siegel (1990). The proportion of nonproduction employees working in auxiliary establishments rose from 2.8 percent in 1954 to 6.7 percent in 1982 (*1982 Census of Manufactures*, Vol. 1, Introduction, p. xx).

25. We decided to forswear undertaking any analysis of the determinants of changes of employment in auxiliary establishments alone, because of the noisiness of the data and possible biases caused by substitutability between nonproduction workers at plant locations and those at auxiliary establishments. It would be desirable to analyze the determinants of changes in employment in auxiliary establishments, but only with access to establishment data, as an extension of Lichtenberg and Siegel (1990). We shall, however, draw some tentative conclusions about auxiliary establishments from differ-

Controlling for Demand and Supply Shifts

We should control for other demand and supply factors affecting changes in industries' equilibrium nonproduction employment. Supply factors operating systematically at the industry level are not readily identified, but the determinants of the demand for labor are well worked out in the literature. Hamermesh pointed out that with the assumption of a constant-elasticity-of-substitution production function, the demand for labor in the multifactor case can be written:

$$(1) \qquad \ln L = a_0 + \Sigma b_i(\ln w_i) + a_I(\ln Y) + u,$$

where L represents the number of nonproduction employees, w_i the wages of nonproduction workers and any other inputs deemed substitutable for or complementary to them, and Y real output. We borrow this specification with the variables expressed (as explained previously) as proportional changes over census intervals and the determinants of labor demand entered in additive form.[26]

We took the simple approach of assuming that white-collar workers are substitutable for production workers but neither a substitute for nor a complement to physical capital and purchased inputs. Substitution between production and nonproduction employees has been confirmed statistically.[27] Evidence available when this project was formulated (summarized by Hamermesh) suggested no confirmed empirical relation between nonproduction employment and capital. Unfortunately, recent evidence from Berndt, Morrison, and Rosenblum and Berman, Bound, and Griliches indicates a significant complementarity between capital and skill, and Brynjolfsson and Hitt concluded that computer capital and related labor have recently been more productive than other inputs (presumably substituting for them).[28] The cost of capital accordingly is

ences in the determinants of changes in plant-based and total nonproduction employment.

26. See Hamermesh (1986). Berman, Bound, and Griliches (1993) omitted wage variables from their similar cross-section analysis on the ground that sectoral wage variations are likely to be endogenous. We include them partly because our analysis has a time-series dimension, partly because we are concerned not with unbiased estimates of wage effects but with the omission of substantial influences on employment of nonproduction workers.

27. See Freeman and Medoff (1982).

28. Berndt, Morrison, and Rosenblum (1992); Berman, Bound, and Griliches (1993); and Bryniolfsson and Hitt (1993).

an omitted variable in our analysis, although we take slight comfort in the judgment of Berman, Bound, and Griliches that capital-skill complementarity is a small effect. To control for changes in relative factor prices, we include only the difference between the proportional changes in salaries per nonproduction worker and wages per production worker during each five-year period.[29]

Sources of Disturbance: Changes in Corporate Control

To test the effect of major shifts in competitive conditions on nonproduction employment, we focused on two factors, the changing volume of activity in the market for corporate control and the changes (largely increases) in the share of U.S. supplies of manufactured products that are imported. The evidence on how changes in corporate control affect efficiency has been accumulating rapidly. Ravenscraft and Scherer showed that those businesses of the four hundred largest enterprises that had undergone control changes before 1973–77—presumably in the wave of conglomerate mergers in the 1960s—were suffering subnormal performance that deteriorated up to the time of their divestment.[30] Overall, however, changes in control have been found to increase productivity at the establishment level in both the United States and Canada, and Lichtenberg and Siegel estimated that during 1977–82 the growth of employment in auxiliary establishments of manufacturing enterprises subject to changes in control was 15.7 percent less than in such establishments that did not experience changes in control.[31] For large mergers in the 1980s, the subsequent improve-

29. Somewhat similar functions were estimated for production and nonproduction employees separately in a study using a panel of annual data, 1970–79, for Canadian manufacturing industries (Caves, 1990). It was found that the demand models for both nonproduction and production workers behave rather similarly, and tariff reductions induced cuts in the use of both types of labor. For nonproduction labor, however, the great bulk of the variance in the panel data was interindustry, not intertemporal, and increases in real output in the 1970s typically involved little, if any, expansion of nonproduction employment.

30. See Ravenscraft and Scherer (1987).

31. Lichtenberg and Siegel (1990, p. 397). Lichtenberg and Siegel (1987) found that control changes, on average, are productive when observed across the board for all manufacturing establishments. Baldwin and Caves (1991) obtained the same result for Canada but associated the productivity gains from control changes not with reduced labor inputs (nonproduction or other), but with the redeployment of multiuse assets. We conjecture that the difference between the two results from the focus of Lichtenberg and

ment in (industry-normalized) profitability has been associated with better use of assets and with reduced employment, especially of white-collar labor.[32]

Arguably, these results are all consistent: conglomerate mergers in the 1960s depressed efficiency and productivity, while control changes in the late 1970s and 1980s performed a salutary disciplinary function. We organized our test to permit the detection of this pattern. We built up (laboriously) a set of data on the proportions of assets classified to each industry that were subject to changes in control in each year from 1965 to 1986, distinguishing between acquisitions that consolidated related activities and those that involved unrelated activities. For 1979 and before, we used the well-known series collected by the Federal Trade Commission (FTC), treating its "conglomerate" category as unrelated and all others as related. For the years after the FTC data terminated (1980–86), we sought to replicate the FTC's data-assembly procedure, using information from Securities Data Co., Compustat, M&A Roster, Moody's, and annual reports to identify and confirm changes of control and the target's base industry. These data suffer many shortcomings: mergers for which the value of the acquired assets is unknown are missed, and both the FTC and subsequent data are surely incomplete in other ways as well. Because of the massive investment that would be needed to effect major improvements, however, we can only place our faith in the randomness of the errors and omissions.[33]

Thus, the hypothesis that we test holds that an industry's use of nonproduction labor was shifted by the incidence (proportion of assets involved) of related and unrelated mergers. A two-year lag was introduced: the 1967–72 change in nonproduction employment is related to the summed proportion of industry assets affected during 1966–70. Whether the proportional volume of activity in the corporate-control market is better related to the change or the level of its nonproduction

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Siegel (1990) on auxiliary establishments belonging to large enterprises, whereas the Canadian analysis covered all establishments and embraced few large and diversified independent enterprises.

32. See Bhagat, Shleifer, and Vishny (1990); and Healy, Palepu, and Ruback (1992).

33. In assembling a similar data base, Blair, Lane, and Schary (1991, pp. 7-13) made the distressing discovery that the overlap between the 1979 transactions identified by the Federal Trade Commission and in the ADP data base is only half of the FTC total count and one-fifth of the ADP total.

employment is open to debate, but a prominent consideration is that most of the variance in corporate-control activity is interindustry rather than intertemporal. Notice the important difference between the hypothesis formulated in our study and those of the predecessors. We observe the assets subject to control changes not in isolation, but only as a component of their three-digit industry. The sizes of any effects that we observe therefore will depend not just on changes within the business units directly involved (documented in previous studies), but also the responses of competing firms. If, after disciplinary control changes, the excess employees were dispersed to the more efficient competitors of the taken-over firms, we would observe no effect on industry employment. If instead the takeover of competitors causes sinning rivals to repent and undertake their own reforms before the raider strikes, we would find effects that go beyond those measured by Lichtenberg and Siegel.

Sources of Disturbance: Import Competition

The other source of disturbance expected to shift an industry's use of nonproduction labor is import competition. The effect in question here is not the competing down of domestic producers, which is already controlled through inclusion of the industry's real output. Rather, we seek to determine whether changes in international competition alter the effective pressure on producers to minimize the costs of whatever output they offer.

Previous evidence suggests that toughened international competition increases the pressure for cost minimization. International competition reduces the rents obtained by producers in concentrated industries, after control for the degree to which oligopolies' elevated prices themselves attract the import competition.[34] The compressed rents could be an incentive to increase efficiency in the use of nonproduction workers (and other ways), although the relationship between import competition and the rents earned in concentrated industries tells nothing directly about the effect of import competition on efficiency (indeed, it is consistent with consequent reductions in efficiency). Evidence for Canada

34. See Pugel (1980). Domowitz, Hubbard, and Petersen (1986) found that increased import competition was one source of the collapse in the 1970s of the (cross-section) relation between price-cost margins and concentration in U.S. manufacturing.

showed that tariff protection increases the prices set by concentrated Canadian producers relative to their U.S. competitors but not their profit margins, suggesting that protection nurtures inefficiency.[35] Studies of productive efficiency (the gap between average and best-practice productivity in an industry's plants) in six countries found in every case either a positive association of efficiency with import competition or a negative association with restrictions on import competition.[36]

Because the efficient use of nonproduction workers seems particularly problematical for large firms, an apt question is whether import competition tends to take a greater toll on the large or the small competitors in any given industry. This can be inferred from the way in which seller concentration changes with import competition once the change in domestic industry output is controlled. On average the smaller producers take the worse hit (that is, concentration increases). That pattern is mitigated or reversed, however, in industries that are intensive in skilled labor, physical capital, and sales-promotion activities. The induced reductions in concentration in these industries appear to result from changes in the relative sizes of large and small companies more than from changes in the numbers of companies or establishments. Industries with these activity structures are probably the most susceptible to the inefficient use of nonproduction labor.[37]

The pressure for cost-cutting brought by import competition might be evident in patterns of employee compensation, with wages constricted through the effects of import competition on employees' rents or quasi-rents. During the 1980s wages became responsive to industry-level demand shifts associated with international competition. The effects can be explained by shifts in sectors' real outputs, however, and we know of no evidence that distinguishes between output changes and intensified incentives to minimize the cost of producing any given output.[38]

Obtaining data on imports matched to production is problematical for the United States, because the trade statistics (classified by com-

35. See Bloch (1974).

36. See Caves and others (1992).

37. See Caves (1988). Long ago Delehanty (1968) observed positive correlations between these structural attributes of industries and their proportions of nonproduction employees.

38. See Katz and Murphy (1992); Murphy and Welch (1991); and Revenga (1992).

modity) are not readily matched to the production statistics (recorded on an establishment basis). The match on which we relied, prepared by the U.S. International Trade Administration, is available for the years 1972–86.[39]

To recapitulate, the basic regression model takes the form

$$NPRA = a_0 + a_1QR + a_2WDIFR + a_3IMPR + a_4MERGR + a_5MERGU + u,$$

where

NPRA = logarithm of final-year nonproduction worker employment minus logarithm of initial-year nonproduction worker employment, including employment in auxiliary establishments;

QR = logarithm of final-year real output minus logarithm of initial-year real output;

WDIFR = (logarithm of final-year salary per nonproduction worker minus logarithm of initial-year salary per nonproduction worker) minus (logarithm of final-year average annual wage per production worker minus logarithm of initial-year average annual wage per production worker);

IMPR = ratio of value of competing imports to total supply (imports plus production) in final year minus ratio of value of imports to total supply in initial year;

MERGR = proportion of industry assets absorbed in related mergers between initial and final years, lagged two years;

MERGU = proportion of industry assets absorbed in unrelated mergers between initial and final years, lagged two years.

A dummy variable is included for each time period (D77 designates 1972–77 observations, and so on). Including the dummies is particularly important because the census years fall at diverse points in the business cycle. Dummy variables (not reported in the tables) are also included for eighteen of the twenty two-digit manufacturing industries

39. Unpublished tabular material provided by the U.S. International Trade Administration.

(each of the two omitted industries, tobacco and petroleum, is represented by a single three-digit industry); with at most four observations in the time dimension, a fixed effect for each three-digit industry was likely to leave little variance for the substantive regressors.

NPRA and *QR* can clearly be affected by common disturbances, and the substantive framework of this investigation hardly rejects the possibility that *WDIFR* might be causally affected by *NPRA*. Initially, we hoped to use instrumental variables to avoid the biased and inconsistent estimates that ordinary least squares would yield in such circumstances. Experiments at instrumenting *QR* did not work well, however, and no approach to instrumenting *WDIFR* seemed attractive even *ex ante*.[40] Our concern is not with estimating a demand function for nonproduction labor, however, but only with determining whether major shocks changed its employment level. Ordinary least squares recovers the best predictor of the effects of these shocks on the conditional mean of the dependent variable and so should suffice for our main purpose.

Deficiencies of the data cause us to report several versions of each model. First, although the change in nonproduction employment is in principle better measured with administrative establishments included, the diversification of large enterprises and disturbing discontinuities in the data (mentioned previously) make it possible that noise in these establishments' data could obscure significant relationships. Therefore we also estimated each model on the change in nonproduction employees working in production establishments (the dependent variable is then designated *NPR* rather than *NPRA*). (The relative compensation variable *WDIFR* is measured as a weighted average of average compensation data for production and administrative establishments when *NPRA* serves as the dependent variable, only for manufacturing establishments when the dependent variable is *NPR*.[41]) Second, observations

40. An instrumental-variables approach might work if the data set were expanded from quinquennial to annual changes, but the quinquennial changes accord with both the available data and the putatively slow working of disturbances from import competition and changes in corporate control.

41. The denominator of *WDIFR* is always average annual compensation per production worker in manufacturing establishments. The numerator is either a weighted average of average annual compensation in auxiliary and production establishments or the average for nonproduction workers in production establishments only. Incidentally, we noticed that (as expected) the compensation of nonproduction workers in manufacturing and administrative units is strongly correlated among industries and that auxiliary-

Table 4. Determinants of Changes in Nonproduction Employment: Basic Model

Exogenous variable	Endogenous variable			
	NRPA	*NRP*	*NRPA*	*NRP*
Constant	0.141	0.075	0.199	0.118
	(1.31)	(1.51)	(1.49)	(2.11)
QR	0.456	0.543	0.382	0.481
	(8.81)	(14.08)	(7.01)	(11.06)
WDIFR	−0.149	−0.373	−0.186	−0.425
	(0.83)	(3.28)	(0.93)	(3.31)
MERGR	−0.0007	−0.0007	−0.0012	−0.0007
	(0.82)	(0.93)	(1.15)	(0.70)
MERGU	0.0002	0.0006	0.0017	0.0008
	(0.38)	(1.71)	(1.44)	(1.09)
IMPR			−0.0010	−0.0004
			(0.84	(0.53)
D77	0.019	0.027		
	(0.82)	(1.66)		
D82	0.076	0.067	0.046	0.029
	(3.11)	(3.83)	(2.10)	(1.78)
D86	−0.128	−0.075	−0.134	−0.097
	(3.51)	(3.85)	(3.84)	(4.74)
	+ 18 dummies	+ 18 dummies	+ 18 dummies	+ 18 dummies
$\bar{R}^2$	0.320	0.551	0.292	0.524
Number of observations	351	434	282	342

Source: Authors' calculations; see text for definitions. *t*-statistics appear in parentheses.

on *IMPR* (change in import competition) are unavailable before 1972, so we estimate each model with and without *IMPR* (and the dummy for the 1972–77 period) included.

Statistical Results: Determinants of Change in Nonproduction Employment

To preview the flavor of our conclusions, the effects of the corporate-control market and of import competition differ among types of industries and periods of time. After the core results in table 4 are noted, we turn to the pursuit of slope shifts that expose these differential effects.

establishment compensation is typically a little higher than in the same industry's manufacturing plants.

Heteroskedasticity-consistent standard errors are used to calculate the reported t-statistics.

To take the control variables first, the growth of nonproduction employment is closely associated with the growth rates of industries' real outputs. Employees in auxiliary establishments are more likely to perform truly overhead functions than are nonproduction workers based at manufacturing locations, and, accordingly, the estimated elasticities with respect to output are higher by 15 to 20 percent when the former are excluded. The coefficient of the change in the nonproduction-to-production worker wage ratio is correctly negative. It is significant when central-office employees are omitted but not when they are included. Several reasons for the divergent significance levels suggest themselves. First, opportunities for substituting between nonproduction and production workers might be concentrated in establishments where both are employed. Second, industry-level wage differentials are probably measured with less error in manufacturing plants than in central-office establishments. And, third, nonproduction employment in central offices might be less sensitive to labor-cost variations, or employee compensation might contain a larger endogenous component. A positive intercept shift is observed for the 1977–82 period, a negative one for 1982–86. The pattern conforms to the impression that a recent squeeze-out followed an earlier buildup, but, of course, the recession-year status of 1982 is a sufficient explanation.

In table 4 the measures of activity in the market for corporate control are not particularly significant. Related mergers apparently reduce nonproduction employment and unrelated ones increase it, but at most the coefficients achieve 10-percent significance in a two-tail test. Similarly, the sign of the effect of changes in import competition is correct, but it is not significant. Our principal hypotheses about disciplinary forces thus are not accepted for all sectors and time periods, but they might prevail in *a priori* congenial times and industrial settings.

Variations over Time

That the key hypotheses fail to win support for the whole time period is not a big surprise. Import competition struck U.S. industries at diverse times but clearly stepped up over the period of analysis. Mergers, unrelated ones in particular, surely varied in their consequences between the go-go conglomerates of the late 1960s and the bust-up takeovers of the 1980s. The findings of Blair and Schary support the impression that efficiency-increasing reorganizations accelerated greatly in the 1980s—a phenomenon that Blair and Schary tied to the encroachment of high real interest rates on free cash flows.[42]

Table 5 reports models that allow for slope shifts over time in the model's various coefficients; this treatment is applied to one variable at a time, to avoid clutter. Each regressor was multiplied by dummy variables set equal to one for 1972–77, 1977–82, and 1982–86 in turn (only the latter two periods for *IMPR*). Each independent variable's slope shifts are reported in a separate pair of equations in table 5. For *QR* the slope shifts in equations 1 and 2 are negative and generally significant, although 1982–86 shows no significant shift. These results need to be considered in relation to the intercept shifts. Together, they indicate that nonproduction employment grew during 1972-82 but in ways unrelated to changes in industries' real outputs. For the change in relative compensation levels (*WDIFR*), the significant negative effect found in table 4 is evident in manufacturing establishments from 1972 on but not previously. We conjecture that the pattern results from the greater variance of *WDIFR* observed in the inflationary conditions of 1972–82, and the data partly support the conjecture.[43]

When slope shifts are added for related mergers, none is significant, but the negative effect in manufacturing establishments overall (that is, the base coefficient of *MERGR*) becomes significant at 10 percent (not shown in table 5). Related mergers would seem generally to economize on white-collar employees, but they did not propel the squeeze-out during the 1980s.[44] The finding accords with the view that such mergers chiefly involve the redeployment of firms' lumpy and intangible assets rather than serving a disciplinary role in corporate governance. For unrelated mergers the results are more dramatic. Equations 5 and 6 both suggest that unrelated mergers were associated with increasing white-

42. Margaret M. Blair and Martha A. Schary, "Industry-Level Pressures to Restructure." In Blair (1993, pp. 149-203).

43. Compared with 1967–72 the standard deviation of *WDIFR* increased in 1972–82 by one-third for all nonproduction employees and by nearly one-half for those in manufacturing plants. In 1982–86, however, it rose by one-fourth more in both groups.

44. The coefficient's magnitude implies that when 5 percent of an industry's assets change hands in related mergers during a five-year period (roughly the sample mean), its nonproduction employment falls by nearly 1 percent.

Table 5. Stability over Time of Coefficients of Determinants of Changes in Nonproduction Employment

	Endogenous variable							
Exogenous variable	*NPRA (1)*	*NPR (2)*	*NPRA (3)*	*NPR (4)*	*NPRA (5)*	*NPR (6)*	*NPRA (7)*	*NPR (8)*
Constant	0.124	0.066	0.153	0.088	0.136	0.074	0.182	0.112
	(1.14)	(1.27)	(1.42)	(1.67)	(1.30)	(1.46)	(1.37)	(1.98)
QR	0.592	0.652	0.460	0.551	0.456	0.546	0.403	0.484
	(7.23)	(13.34)	(8.75)	(14.16)	(8.92)	(14.12)	(7.87)	(11.44)
WDIFR	−0.134	−0.335	0.110	0.174	−0.214	−0.378	−0.177	−0.404
	(0.76)	(2.98)	(0.28)	(0.78)	(1.44)	(3.35)	(0.90)	(3.20)
MERGR	−0.0008	−0.0008	−0.0006	−0.0006	−0.0006	−0.0008	−0.0011	−0.0006
	(0.91)	(1.05)	(0.67)	(0.77)	(0.69)	(0.98)	(1.15)	(0.66)
MERGU	0.0003	0.0006	0.0004	0.0008	−0.0005	−0.0007	−0.002	−0.0009
	(0.51)	(1.37)	(0.61)	(2.12)	(0.71)	(1.77)	(1.76)	(1.15)
D77	0.043	0.049	0.008	0.008	0.013	0.025		
	(1.66)	(2.64)	(0.30)	(0.43)	(0.51)	(1.51)		
D82	0.090	0.075	0.066	0.042	0.065	0.063	0.043	0.025
	(3.95)	(4.51)	(2.73)	(2.05)	(2.51)	(3.44)	(2.05)	(1.59)
D86	−0.114	−0.075	−0.145	−0.099	−0.106	−0.072	−0.087	−0.081
	(3.17)	(3.65)	(4.23)	(5.06)	(3.00)	(3.62)	(2.47)	(3.97)
QRD77	−0.188	−0.208						
	(1.23)	(2.14)						
QRD82	−0.208	−0.233						
	(2.18)	(3.46)						

QRD86	−0.078	−0.057						
	(0.33)	(0.59)						
WDIFD77			−0.653	−0.775				
			(1.39)	(2.50)				
WDIFD82			−0.326	−0.717				
			(0.77)	(2.74)				
WDIFD86			0.049	−0.377				
			(0.11)	(1.40)				
MGUD77					0.002	0.0006		
					(1.76)	(0.53)		
MGUD82					0.003	0.002		
					(1.40)	(1.54)		
MGUD86					−0.023	−0.0001		
					(2.48)	(2.05)		
IMPR							−0.0007	0.0006
							(1.62)	(0.73)
IMPD82							0.0023	0.0020
							(1.59)	(1.62)
IMPD86							−0.007	−0.024
							(5.24)	(1.91)
	+ 18 dummies	+ 18 dummies	+ 18 dummies	+ 18 dummies	+ 18 dummies	+ 18 dummies	+ 18 dummies	+ 18 dummies
$\bar{R}^2$	0.322	0.568	0.330	0.562	0.328	0.550	0.326	0.548
Number of observations	351	434	351	434	351	434	282	342

Source: Authors' calculations; see text for definitions. *t*-statistics appear in parentheses.

collar employment during 1972–82 (weak statistical significance) but with decreasing white-collar employment during the 1980s (significant). This pattern is consistent with unsuccessful, fat-promoting diversified mergers in the 1960s and 1970s that give way to bust-up (fat-shedding) takeovers in the 1980s.[45] The commonplace interpretation of a bust-up role for unrelated acquisitions in the 1980s is thus confirmed. Although these results are not surprising when regarded as effects of control changes on target firms, it is significant that they prevail at the three-digit industry level. They are not washed out in the churning of firm sizes (employment) within an industry, and they are amplified by contagion. The effects of both related and unrelated mergers, although subject to great uncertainty, appear to be quantitatively substantial.

The effect of *IMPR* appears initially to be positive (weakly significant) during 1977–82 but grows significantly negative after 1982. During 1982–86 an increase of five percentage points in an industry's ratio of imports to total supply apparently caused a 1 to 4 percent decline in nonproduction employment (the higher figure estimated when administrative establishments are included). The effect cannot be attributed to the substitution of production for nonproduction labor, because Berman, Bound, and Griliches showed that increases in imports' market share were associated with the upgrading of an industry's skill mix (their analysis covered 1979–87). The result is consistent with MacDonald's conclusions about the effects of import competition on productivity.[46] Finally, the result can be contrasted to the findings of Scherer and Huh about R&D activities of U.S. manufacturers in the face of international competition—initially a submissive reaction, followed by a provoked one.[47]

Variations among Sectors and Settings

If disciplining effects can be localized in time, can they also be localized by sectors with certain market structures? If surplus staff accumulates solely because of the preferences of poorly monitored man-

45. The coefficient for *NPRA* in 1982–86 implies that the turnover of 5 percent of an industry's assets in a five-year period would lead to a 12 percent reduction in nonproduction employment. The figure is too high to be credible, especially in light of other coefficients on unrelated mergers, but it does suggest a substantial effect.

46. MacDonald (1992).

47. Scherer and Huh (1992).

agers, it should be randomly distributed among market structures (once we control for the prevalence of the corporate form of organization). The Carnegie-Niskanen approach, however, suggests that some market structures might be more congenial to business corpulence—those where white-collar tasks are important in firms' activities and where these tasks require the collaboration of diverse nonproduction specialists and skills.

We related a group of market-structure traits that might distinguish sectoral environments having these attributes (a single observation on each industry centered in our time period):

R = total R&D outlays of the industry, divided by value of shipments and outward transfers, 1977;

A = total media advertising outlays and other sales-promotion outlays, divided by value of shipments and outward transfers, 1977;

K = for each quinquennial census period, the sum of nominal capital expenditures at establishments classified to the industry, divided by the sum of (nominal) values of industry shipments in the same years;

C = four-firm producer concentration ratio for the industry in 1977 (industry-shipments-weighted average of ratios for four-digit industries classified to each three-digit industry);

S = combined size (value of shipments) of the four leading firms in each four-digit industry, converted to a weighted average for the three-digit industry using industry shipments as weight.

Employing the simplest possible approach, we calculated the median value for each of these variables, formed a dummy variable (Di) set equal to one if the industry ranks above the sample median, zero otherwise. The product of Di and one of the regressors embodying disturbances then serves to test the hypothesis that the disturbance's effect differs significantly between the industries ranked low and high on the ith structural attribute (the dummy is also entered to allow an intercept shift).

Table 6 presents reestimations of the basic model (table 4) to test

Table 6. Dependence of Effect of Shifters of Nonproduction Employment on Characteristics of Industry Structure

Dependent variable	*Interactive exogenous variables*			$\bar{R}^2$	*Number of observations*
1. *NPRA*	−0.0019 *MERGR*	+ 0.0046 *DRMERGR*	− 0.039 *DR*	0.320	348
	(1.95)	(2.58)	(1.53)		
2. *NPR*	−0.0028 *MERGR*	+ 0.0040 *DRMERGR*	− 0.039 *DR*	0.563	431
	(3.19)	(3.66)	(2.40)		
3. *NPRA*	−0.0014 *MERGR*	+ 0.0023 *DAMERGR*	− 0.027 *DA*	0.313	348
	(1.60)	(1.38)	(1.02)		
4. *NPR*	−0.0012 *MERGR*	+ 0.0013 *DAMERGR*	− 0.013 *DA*	0.547	431
	(1.27)	(0.83)	(0.78)		
5. *NPRA*	−0.0017 *MERGR*	+ 0.0010 *DKMERGR*	+ 0.034 *DK*	0.321	351
	(1.10)	(0.54)	(1.67)		
6. *NPR*	0.0006 *MERGR*	− 0.0026 *DKMERGR*	+ 0.025 *DK*	0.557	434
	(0.66)	(2.21)	(0.23)		
7. *NPRA*	−0.0025 *MERGR*	+ 0.0043 *DCMERGR*	− 0.030 *DC*	0.327	351
	(2.66)	(3.26)	(3.53)		
8. *NRP*	−0.0016 *MERGR*	+ 0.0027 *DCMERGR*	− 0.019 *DC*	0.556	434
	(1.65)	(2.02)	(1.26)		

9. *NPRA*	−0.0016 *MERGR*	+	0.0025 *DSMERGR*	−	0.043 *DS*	0.323	351
	(1.85)		(1.54)		(0.22)		
10. *NPR*	−0.0016 *MERGR*	+	0.0021 *DSMERGR*	−	0.045 *DS*	0.560	434
	(1.66)		(1.77)		(2.88)		
11. *NPRA*	−0.0002 *MERGU*	+	0.0028 *DCMERGU*	−	0.019 *DC*	0.318	351
	(0.27)		(1.73)		(0.82)		
12. *NPR*	−0.0004 *MERGU*	+	0.0028 *DCMERGU*	−	0.011 *DC*	0.551	434
	(0.97)		(1.43)		(0.75)		
13. *NPRA*	0.0012 *IMPR*	−	0.0037 *DCIMPR*	−	0.008 *DC*	0.293	282
	(1.00)		(1.40)		(0.30)		
14. *NPR*	−0.0013 *IMPR*	+	0.0018 *DCIMPR*	−	0.010 *DC*	0.528	342
	(1.53)		(1.44)		(0.60)		
15. *NPRA*	0.0002 *IMPR*	−	0.0038 *DSIMPR*	−	0.026 *DS*	0.302	282
	(0.13)		(1.61)		(0.96)		
16. *NPR*	−0.0002 *IMPR*	−	0.0013 *DSIMPR*	−	0.026 *DS*	0.527	342
	(0.23)		(0.59)		(1.49)		

Source: Authors' calculation; see text for definitions. t-statistics appear in parentheses.

these structural shifts, with only the coefficients directly involved reported (other coefficients were not substantially changed from table 4). Equations 1 and 2 show that related mergers in industries with low research intensities are associated with decreased use of white-collar labor, while the effect shifts to positive in industries with above-median R&D levels. Similar patterns appear for advertising (equations 3, 4) and for investment intensity as it affects central office employees (equation 5) but not those in manufacturing plants (equation 6). We expect R, A, and K all to be positively correlated with producer concentration, and indeed equations 7 and 8 show that related mergers are also associated with higher white-collar employment in the more concentrated industries. The average size of an industry's leading firms has a similar effect, with related mergers tending to increase white-collar employment in industries with large leading firms (weak statistical significance in equations 9 and 10). In contrast to these findings on related mergers, differences in industry structure do not alter the effects of unrelated mergers in any significant or even regular way. The only exception (equations 11 and 12) is that unrelated mergers have tended to sustain increased white-collar employment when they take place in concentrated industries (weakly significant).

Do the results on mergers and mediating structural conditions tell a coherent story? The difference between the patterns for related and unrelated mergers shows that the effects of mergers on white-collar employment are associated with the redeployment of assets and activities that are expected to be associated with related mergers. Related mergers economize on nonproduction labor inputs in activities where that input is less important in the first place, but they can augment it where it is important. Thus, the normative implications of the positive effect of related mergers on white-collar employment are ambiguous: it seems desirable in research-intensive industries but not in concentrated industries. Conversely, whatever the typical effects of unrelated mergers, they are independent of the industry structures and activity patterns that in turn govern the payout from asset redeployments associated with mergers. (As table 5 showed, however, the effects of unrelated mergers have varied substantially over time.) That unrelated mergers have not compressed white-collar employment in concentrated industries (equation 11) seems anomalous, but other (negative) results on slope shifts for *MERGU* (and equations 5 and 6 of table 5) incline us to put this result aside.

The mediating effect of structural conditions on the slope coefficient for *IMPR* can be considered more briefly. As equations 13–16 of table 6 show, import competition seems to squeeze employment in central-office establishments, as indicated by negative effects on *NPRA* in industries with large firms. These effects are not observed when the dependent variable reflects only plant-based nonproduction employment (*NPR*). The result is consistent with Niskanen's bureaucracy hypothesis and with the doleful tales of corporate downsizing heard in the 1980s.

The evidence of table 6 suggests that a firm's susceptibility to inflated white-collar employment might depend on the activities mandated by its industry's structure. The linkages are quite explicable in the case of mergers. But they are not strong statistically and leave room for the hypothesis that corporate governance matters chiefly for efficiency, not for the firm's structure of activities. In the next section we get another shot at testing whether proneness to corporate obesity varies with the industry's structure.

Downsizing Employment: A Firm-Level Analysis

Overall, this inquiry has provided some support for the hypothesis that some U.S. corporations accumulated excess nonproduction employees that they were forced to disgorge by exogenously increased pressures to minimize costs. The statistical effects occurred at times and (to a modest extent) in sectors where they might have been expected. We are thus inclined to reject the null hypothesis about accumulated organizational slack, although data limitations qualify the results, and value-maximizing explanations for these statistical patterns cannot be ruled out.

This retrospective analysis suggests that at least some corporate downsizing should raise expected profits. We now address that question directly by measuring and analyzing stock-market reactions to the announcements of corporate downsizings. Have positive reactions been common? Have they occurred in settings where the downsizing might likely remove corporate fat rather than acknowledge corporate misfortune?

Possible Reactions to Announcements of Downsizing

How the market reacts to downsizing announcements might be explained in these two ways:

ASYMMETRICAL INFORMATION. If managers are value maximizers and they and shareholders are equally well-informed, downsizings will occur at optimal times, and shareholders should have no systematic reactions to their announcements (as distinguished from the adverse shocks that induced the downsizings). Keep the assumption that managers are value maximizers able to choose and sustain optimal levels of nonproduction employment. Suppose, however, that managers have better information than the general public about the firm's future profit prospects and that states of nature in which the firm's (flow) profits will be reduced are highly correlated with circumstances in which its optimal employment level is lowered.[48] The downsizing announcement then serves to reveal to the market bad news that management has already received and is acting upon. The market's reaction to the downsizing announcement should be negative if this ''bad news'' effect dominates.

EXCISION OF SLACK. Assume that white-collar employment and cooperating resources can be inflated in a successful firm in the manner described previously. Assume that the existence of the excess cost is known to owners of the firm's equity and is capitalized (negatively) into the value of the shares. A downsizing announcement then can raise the value of the firm's shares by revealing that some coalition-breaking force has dislodged the unproductive resources. This ''bite-the-bullet'' effect could arise from disgorging resources other than white-collar employees, such as unprofitable activities retained for the utility they give to managers.[49]

These opposed sign predictions leave us with no prior expectations

48. One can think of exceptions, such as when the demand curve is rendered less elastic, but, overall, the assumption seems reasonable.

49. Analyses of cases of financial distress indicate that they provide an occasion for managers to reverse committed policies of the firm that have proven unsuccessful. See Wruck (1990) and Shefrin and Statman (1985).

about the mean value of the excess returns associated with announcements of corporate downsizings.[50] The preceding part of this paper indicated that some firms are forced to bite the bullet, but these cases need not account for many, or even a large proportion of, downsizings. Furthermore, a downsizing judged by shareholders to have bite-the-bullet significance could at the same time reveal bad news and elicit a negative market reaction. Therefore, the mean value of excess returns associated with downsizing announcements is unlikely to discriminate between the hypotheses. All owners can be assumed to share a common reaction to a given announcement, so we expect the variance of the excess returns to reflect the differing situations of the announcing firms. In the balance of this section we report the first phase of this investigation.

Research Design and Core Results

We collected a sample of announcements of corporate downsizings appearing in the *Wall Street Journal* between 1987 and 1991 by searching the ABI/Inform data base for stories reporting layoffs and retaining all announcements that mentioned specific quantitative layoff targets. This process yielded a total of 513 announcements of downsizings by U.S. corporations whose excess returns are available on data tapes prepared and distributed by the University of Chicago's Center for Research in Securities Prices. These excess returns were obtained for the day of the announcement (*XR*0) and for three trading days before (*XRM*1–*XRM*3) and three trading days subsequently (*XRP*1–*XRP*3). Their means and standard errors are shown in table 7. The average downsizing announcement brings a loss of 0.63 percent of the company's value on the announcement date, anticipated by losses on the two previous trading days that bring the total to 1.65 percent. The mean return on each of these three days differs significantly from zero at the 5 percent level. The concentration of significant excess returns on the announcement date and the two preceding days agrees with the pattern found by Blackwell, Marr, and Spivey, and the sum of excess returns

50. Blackwell, Marr, and Spivey (1990) investigated the bad-news effect in a sample of announcements of permanent plant-closings. They observed significant negative excess returns, and the firms' accounting returns on equity had underperformed their three-digit SIC industries in the preceding two years. Worrell, Davidson, and Sharma (1991) also reported significant negative market reactions to announcements of layoffs.

Table 7. Excess Returns Associated with Corporations' Announced Decisions to Downsize and Properties of Announcements

Percentage

Variable	Share of cases	Mean value of excess return: With stated property	Mean value of excess return: Without stated property
Excess returns			
*XRM*3	n.a.	0.061 (0.39)	n.a.
*XRM*2	n.a.	−0.348 (2.19)	n.a.
*XRM*1	n.a.	−0.681 (3.14)	n.a.
*XR*0	n.a.	−0.625 (2.72)	n.a.
*XRP*1	n.a.	0.430 (1.94)	n.a.
*XRP*2	n.a.	−0.228 (1.41)	n.a.
*XRP*3	n.a.	−0.024 (0.17)	n.a.
Features of announcements			
Charge against earnings	19.1	−2.963	−1.321
Earnings announced	2.7	−0.173	−1.104
Loss announced	11.1	−6.164	
Separations voluntary	20.7	−0.607	−1.900
Separations temporary	11.1	−1.152	−1.692
Previous merger	5.7	−1.123	−1.662
Plant closure	29.0	−1.224	−1.799
Reorganization announced	24.8	−1.414	−1.703
White-collar layoffs	30.2	−0.726	−2.026

Source: Authors' calculation; see text for definitions. *t*-statistics for mean excess returns appear in parentheses.
n.a. = not applicable.

over this three-day "window" will be the dependent variable that we seek to explain.[51] About 60 percent of the three-day returns are negative (mean = −5.1 percent), while 40 percent are positive (mean = 3.7 percent). The enlarged standard errors at the time of announcement are consistent with the perspective offered above: the different situations of individual companies could elicit widely varying market reactions to announcements of downsizings.

We recorded whether several attributes were present in or missing from the announcements of downsizings. The attributes were picked to shed light on the prevalence of bad-news and bite-the-bullet effects. The proportions of announcements including each attribute are reported in table 7, along with means of the three-day returns for observations with and without them. Consider first the features that indicate the

51. Excess returns on other trading days are insignificant and do not warrant attention (the positive value for *XRP*1 is strongly influenced by one huge outlier).

magnitude of the adverse shock and should be associated with lower (more negative) excess returns. The announcement indicated that a charge would be taken against earnings to cover the costs of the downsizing in 19 percent of the cases, and these charges resulted in losses of value of 2.96 percent, while firms not announcing charges lost only 1.32 percent. In 14 percent of the news stories, earnings were also announced, and negative announced earnings brought a market-value loss of 6.16 percent, while positive earnings slightly mitigated the mean loss of 1.10 percent in cases accompanied by no earnings announcement. Announcement that the employment attrition would be voluntary or temporary brought smaller losses, presumably because of smaller reductions in the expected present value of the firm's earnings. We expected that downsizings following mergers would entail smaller losses for having been anticipated, but the mean difference is small. These differences largely confirm that the varying badness of the news accounts for part of the variance of the excess returns.

The market's responses to other attributes seem to reveal the bite-the-bullet effect. Announcement that the downsizing would involve the closure of a plant should have a depressant effect as new information, but it entailed smaller mean losses (1.22 percent) than when no closure was announced (1.80 percent). The loss is slightly smaller when the layoffs were announced as part of a plan to reorganize, refocus, or consolidate the firm or change its strategy, 1.41 percent rather than 1.70 percent. Most striking, the announcement that white-collar layoffs would be involved produced a smaller loss (0.73 percent) than otherwise (2.03 percent).

We also measured the proportion of the work force to be laid off. It ranges from 0.01 percent to 53 percent, with a mean of 5.6 percent. This variable is taken from the *Wall Street Journal* when reported there as a proportion. When it is reported as an absolute number, we converted it to a proportion by using as a divisor the total employment figure reported for the previous year-end in Standard and Poor's *Compustat PC Plus* data base. The distribution of observations on the proportion laid off (hereafter L) is roughly half-normal, with the mode close to zero. Also, we had reason to expect it to be conditional on the sizes of companies. If (as is commonly assumed) the adjustment costs of reducing a firm's employment are convex in the (absolute) number of employees laid off, we expect the proportional sizes of layoffs to

Table 8. Regression Models of Determinants of Excess Returns Associated with Corporate Downsizing Announcements

	Equation number				
Exogenous variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*
Constant	−0.011	−0.012	−0.008	−0.008	−0.005
	(1.97)	(2.23)	(1.64)	(1.94)	(1.05)
Fraction laid off (*L*)	−0.182	−0.179	−0.225	−0.253	−0.219
	(2.07)	(2.07)	(2.39)	(2.49)	(2.39)
Charge against earnings	−0.004	−0.004	−0.005	−0.005	−0.007
	(0.37)	(0.34)	(0.42)	(0.43)	(0.58)
Loss announced	0.043	−0.043	−0.043	−0.046	−0.040
	(2.17)	(2.17)	(2.17)	(2.32)	(2.05)
Separations voluntary	0.011	0.011	0.010	0.012	0.010
	(1.62)	(1.69)	(1.58)	(1.80)	(1.53)
Separations temporary	−0.005				
	(0.56)				
Previous merger	−0.004	−0.004	−0.005	−0.004	−0.005
	(0.60)	(0.57)	(0.72)	(0.46)	(0.72)
Plant closure	0.010	0.009	0.007		0.007
	(1.27)	(1.28)	(1.01)	(0.97)	
Reorganization announced	0.009	0.010	0.007	0.009	
	(0.96)	(1.05)	(0.77)	(0.93)	
White-collar layoffs	0.011	0.012		0.011	
	(1.64)	(1.73)		(1.59)	
*L** white-collar			0.289		
			(2.79)		
*L** plant closure				0.278	
				(2.35)	
*L** white-collar*reorganization					0.384
					(3.15)
$\bar{R}^2$	0.075	0.077	0.091	0.099	0.092

Source: Authors' calculation; see text for definitions. *t*-statistics appear in parentheses. Each model is estimated from 512 observations.

have a wider dispersion for small companies than for large. Even without this factor, the *Wall Street Journal* presumably reports small downsizings by large firms and large downsizings by small ones, but not small downsizings by small firms. Because bite-the-bullet effects might be more common among large firms, we were concerned about the interaction between L and firms' employment size (hereafter S). We first regressed three-day excess returns on dummy variables indicating ranges of L, in order to observe the shape of this relation. The regression coefficients and the mean values of S for each tranche of L are:

Proportion laid off (L)	*Regression coefficient (and* t-*statistic)*	*Mean* S	*Number of observations*
$0 < L \leq 0.01$	omitted class	209,230	177
$0.01 < L \leq 0.02$	−0.0097 (1.61)	121,875	69
$0.02 < L \leq 0.03$	0.0017 (0.23)	120,506	38
$0.03 < L \leq 0.06$	−0.0004 (0.05)	84,806	77
$0.06 < L \leq 0.10$	−0.0192 (1.77)	40,129	69
$0.10 < L \leq 0.18$	−0.0410 (2.04)	16,638	50
$0.18 < L$	−0.0530 (2.01)	7,631	33

Announcements of larger layoffs cause more negative reactions but apparently have no regular effect on market value until they reach a threshold of around 6 percent. A simple linear relation between three-day excess returns and L will turn out to fit the data fairly well, but the preceding regression result shows that it is determined by the larger layoffs announced by the smaller firms in the sample. We investigated whether three-day excess returns are related to S for individual tranches of L but found no significant relationships.

Determinants of Excess Returns: Regression Analysis

A regression analysis of the determinants of three-day excess returns ($XR0 + XRM1 + XRM2$) yielded the results shown in table 8.[52] The regressors include those listed in table 7 plus the fraction of employees to be laid off. Equation 2 differs from equation 1 only in excluding the dummy for separations that are temporary, which is never at all significant and is highly collinear with S (because the giant auto firms announce many temporary separations—more on this subsequently). In equation 2 all signs are correct, and layoffs and the dummy for reported losses are significant, as are the dummies for voluntary separations and

52. It is based on 512 observations because of one missing excess-return value.

for nonproduction-worker layoffs if one-tail tests are deemed appropriate. The occurrence of a charge against earnings is insignificant because it is strongly collinear with other variables, especially negative earnings. This is quite plausible because the charge represents purely an accounting decision that should be conditional on the resource-allocation decisions registered by the other regressors.

Equation 3 interacts the fraction laid off with the dummy indicating white-collar discharges. The interaction's positive coefficient is significant, and its magnitude more than offsets the negative coefficient of the laid-off fraction itself. This finding confirms the hypothesis that the market's positive reactions to downsizing announcements are associated with white-collar separations. In equation 4 the fraction laid off is interacted with the dummy indicating plant closure. The coefficient of the interaction term is again significant and large enough to offset the coefficient of L. This interaction test was performed with the dummy indicating that a reorganization was announced, yielding an insignificant coefficient (not shown). In equation 5 the dummy indicating an announced reorganization is shifted from an additive regressor to one multiplied by L and the nonproduction-workers dummy; compared with equation 3, the t-statistic on the interaction and the equation's F-statistic increase.[53] If the sample is subdivided into cases with and without reorganizations announced, the positive coefficient of the dummy indicating white-collar layoffs is significant only when reorganization takes place. The same result occurs when the cases of voluntary and involuntary separations are distinguished: shareholders applaud reduced-white collar employment (significantly) only when actual layoffs are involved.

The results so far support the hypothesis that separations of nonproduction workers are sometimes viewed as creating value for shareholders, but the explanatory power of table 8's models is quite low. Could we increase it by identifying *a priori* a subsample of firms most likely to indulge in excess white-collar employment? In contrast to the ap-

53. Worrell, Davidson, and Sharma (1991, p. 668) reported that excess returns attributable to layoff announcements were not significantly different from zero when reorganization and consolidation were also announced but were significantly negative (-2.46 percent) when the layoffs occurred simply because the firm was running losses. Statman and Sepe (1989) observed positive excess returns to announcements of project terminations in cases where shareholders already had information on the project's prospects for success.

Table 9. Regression Models of Determinants of Excess Returns with Companies Distinguished by Importance of Overhead Activities

	SGA per employee	
Exogenous variable	*Below median*	*Above median*
Constant	−0.011	−0.049
	(1.06)	(3.48)
Fraction laid off (*L*)	−0.083	0.436
	(0.48)	(1.89)
Charge against earnings	−0.020	−0.027
	(1.46)	(1.32)
Loss announced	0.002	−0.073
	(0.09)	(2.42)
Separations voluntary	0.012	0.018
	(1.15)	(1.80)
Previous merger	0.005	−0.010
	(0.28)	(0.90)
Plant closure	−0.006	0.030
	(0.63)	(1.87)
Reorganization announced	0.001	−0.027
	(0.06)	(1.42)
White-collar layoffs	0.004	0.039
	(0.46)	(2.61)
Layoffs squared (L^2)	0.127	−1.858
	(0.31)	(2.72)
Previous announcements	0.010	0.020
	(1.06)	(1.36)
$\bar{R}^2$	−0.006	0.193

Source: Authors' calculations; see text for definitions. *t*-statistics appear in parentheses. Each model is estimated from 162 observations (*SGA* is available for 324 firms).

proach in table 6, we selected an indicator based on the firm itself: selling, general, and administrative expenses per employee (*SGA*) as a measure of the intensity of overhead costs and the potential for Niskanen-type behavior. We ranked the observations for which this variable is available (only 324 of 513), split the sample into firms below and above median *SGA*, and estimated various models on the subsamples separately.

Table 9 illustrates the useful conclusions yielded by this exercise. First, the explanatory power of the model is (for such cross-sections) rather good for the high-*SGA* subsample but nonexistent for the downsizings by low-*SGA* firms. By implication, much of the consequence of scale changes for expected profit is bound up in administrative and organizational choices for the former group, other factors for the latter group. The plant-closure dummy is significant for high-*SGA* firms although not for the sample as a whole.

Second, table 9's model for the firms with high overhead allows us to investigate the positive values of the three-day returns in a way that is infeasible with table 8's model. In table 9 the complex relation between excess returns and *L* is successfully represented by a quadratic relationship—a maneuver that does not work (that is, does not improve statistically on a linear representation of *L*) for the whole sample. We can calculate the range of fractions laid off for which predicted excess returns are positive, conditional on values of the other regressors, as follows:

1. Set all the dummy variables (including the indicator of white-collar layoffs) equal to zero. Predicted three-day excess returns then are negative for all values of *L*.
2. Set the dummy indicating white-collar layoffs equal to one but all the other dummies equal to zero. Predicted excess returns are then positive for all fractions laid off where *L* is greater than 2.3 percent but less than 20.9 percent.
3. Set the dummy indicating the announcement of a reorganization equal to one (in addition to the white-collar dummy), but the others equal to zero. Predicted excess returns are then positive for all values of *L* less than 27 percent.[54]

In these overhead-intensive firms, it takes the bad news of a very large downsizing to offset the gains that shareholders expect from reducing the white-collar cadre (with or without formal reorganization).

Further Experiments

Several other experiments that were performed with the data base can be summarized briefly.

54. Brickley and Van Drunen (1990, p. 265) analyzed market valuations of announcements of internal corporate reorganizations, finding significant positive returns (for the more conspicuous events) of 0.69 to 1.15 percent. The small size of the gain might reflect (they note) the fact that the reorganizations commonly affect only a division or other small proportion of a company. In general they found that liquidations of divisions or subsidiaries get negative market reactions, other reorganizations positive reactions. They also observed that firms reorganizing to increase efficiency or cut costs had previously exhibited stock-market performance worse than their industry, consistent with the bite-the-bullet hypothesis of our own study.

First, a chronic problem with studies of this type is that the "event" does not represent a clean injection of completely new information. For example, an announced downsizing might follow upon earlier downsizings that caused shareholders to anticipate that the observed announcement would take place; the excess return then values only the difference between the terms actually announced and those that the market expected (our 512 announcements emanate from only 240 firms). Blackwell, Marr, and Spivey found that significant negative returns were set off by a firm's first announcement of a plant closing (in their data base) but not by subsequent announcements.[55] In table 9 we added a dummy indicating layoff announcements successive to the firm's first in our data base (of course, not necessarily its first in a sequence); the coefficient is not significant in either equation, but it suggests that subsequent downsizings yield 1–2 percent higher excess returns.[56]

Second, a variable that we collected is the length of time over which the announced downsizing was projected to occur. On the assumption of convex adjustment costs, a given downsizing should have a stronger negative effect the shorter the time horizon over which it is implemented. We assumed that this duration is a decision variable for the firm chosen to minimize the adjustment cost of the necessary layoffs but subject to the consideration that dire circumstances might compel swifter action. Therefore, we regressed the length of the announced downsizing period on the (absolute) number of employees to be laid off (with 421 observations the t-statistic equals 7.91). We entered the residual as an exogenous variable in the model, expecting a positive coefficient (a hasty downsizing elicits a more negative market reaction). The coefficient is indeed positive but only weakly significant (t equals 1.54).

Third, a question sometimes treated in event studies is whether different or more predictable market reactions occur when more informa-

55. Blackwell, Marr, and Spivey (1990). See also Worrell, Davidson, and Sharman 1991).

56. A sufficient reason for the insignificance of this dummy is that our hypotheses embrace expectations of both positive and negative excess returns. If the market values a strategic change chiefly upon its first announcement, the announcement of subsequent steps will tend to bring reactions that are smaller in absolute but not necessarily in algebraic value. The force of this consideration is seen when we regress the squared value of three-day excess returns on the variable indicating subsequent announcements: the coefficient is negative, with t equal to 2.97.

tion is announced (indicated by the length of the *Wall Street Journal* story). Brickley and Van Drunen found much more statistical significance in reactions to stories longer than the median in their sample.[57] Our results are different. When the sample is split around the median-length story, a somewhat better fit is actually preserved for the short than for the long stories. Story length is correlated with company size (it was not for Brickley and Van Drunen), and splitting the sample around median company size yields a parallel result: better explanatory power for small firms. The concern that these findings might arouse is greatly reduced by the results of table 9 (*SGA* is uncorrelated with company size).

Fourth, could we have obscured important behavior by summing excess returns over the *Wall Street Journal* publication date and the two days preceding it? We replaced the three-day return by the individual-day returns and reestimated the model. The models for days *XR*0 and *XRM*2 closely resemble the three-day model, but that for *XRM*1 (the day on which many of the announcements were first made public) is somewhat different. The dummy indicating an announced reorganization is significantly positive, but the fraction laid off and the dummy for white-collar discharges are not significant.

Fifth, the automobile industry was a conspicuous downsizer during 1987–91, the source of no less than 63 of our 513 announcements. Because some of these represent the routinized temporary plant closings that are common in the auto industry, we were concerned that these observations might somehow be distorting our regression results. Fortunately, when the auto company observations are deleted the basic model (table 8) is essentially unchanged. This industry's distinctive pattern did, however, account for our early decision to drop the dummy for temporary closings from the analysis.

In conclusion, with the qualification that some levels of statistical significance are marginal, we find that the data consistently support the hypothesis developed previously in the paper: one cannot rule out the hypothesis that nonproduction-worker cadres are overinflated in successful corporations, necessitating negative shocks to trigger a value-increasing reorganization. This analysis is just the first step of investigating the situations of these downsizing companies. We hope to track

57. Brickley and Van Drunen (1990).

their situations back in time, taking account of their market structures and governance situations, to ascertain what circumstances brought them to the point where a substantial reduction in the resources that they employed was mandated.

Summary and Conclusions

The large reductions of white-collar employment in major U.S. companies during the past decade or so raise the question of why this apparent fat accumulates and what shocks promote its removal. The question is underlined by various results of scholarly research, such as the negative association of the productive efficiency of an industry's plants with the prevalence of "inbound" diversification and the productivity gains associated with changes in corporate control and with corporate "refocusing" strategies during the 1980s. Cadres of nonproduction employees could be inflated by various mechanisms, including managerial preferences in firms poorly monitored by their shareholders. The mechanism on which we focus is Niskanen's version of the lateral contract within a firm that employs diverse groups of nonproduction workers as functional specialists. This mechanism yields predictions about both where (and when) the inflation of white-collar employment should occur and what sorts of disturbance would excise it.

We investigated whether the nonproduction labor used by three-digit U.S. manufacturing industries was reduced by competitive disturbances in their product markets (increases in imports' market share) and in the market for corporate control (turnover of assets in their industry through related and unrelated mergers). Import competition exerted this effect significantly, to a degree that increased through the 1970s to a high level in the 1980s. The story is more complex for changes in control. Consistent with the conventional wisdom, unrelated acquisitions tended to inflate white-collar employment in the 1970s but had the reverse effect in the 1980s. Related acquisitions, more likely to involve the transfer of business assets into hands that can use them better, increase nonproduction employment in overhead-intensive industries (but tend weakly to reduce overhead otherwise). The analysis was applied to total nonproduction employees located at manufacturing establishments and at central offices and other administrative establishments, and to plant-based nonproduction workers separately. In general the results indicate that employees in administrative establishments are more vulnerable to fat-excising shocks (especially in the 1980s), but differences in the results between counts of nonproduction workers excluding and including the administrative establishments (and irregularities in data for the latter) are cautionary. Another major qualification is that we do not jointly test hypotheses about sources of downsizing based on value-maximizing responses to disturbances. Possible examples of these are changes in the technology of organization or in the feasibility of contracting out white-collar tasks formerly performed in-house.

If these adverse shocks forced profit-increasing white-collar layoffs on some firms, the stock market should have reacted positively to some layoff announcements, and so we analyzed stock market reactions to announcements of corporate downsizings made during 1987–91. Two factors could affect the market's reactions to these downsizings: the negative information effect of the bad news that the announcement reveals to shareholders, and the positive reaction of informed shareholders who welcome an indication of decisive action against corporate inefficiency. The mean excess returns are significantly negative, although with a large minority of positive reactions. The associations between the excess returns and traits of the announcement imply that reactions to the announcements reflect a mixture of "bad news" and "bite-the-bullet" components. In particular, market valuations of downsizing announcements tend to be positive when white-collar discharges are involved, an effect strengthened when a reorganization is also announced. Plant closures also offset the negative effect of layoffs.

The analytical perspective of this paper suggests that the risk of corporate obesity is greatest when the firm is successful and when its industrial base mandates extensive reliance on the services of diverse nonproduction workers. We got rather indecisive results in testing whether the downsizing effects of adverse shocks vary with industries' market-structure traits. When firms were sorted by the magnitude of their overhead intensities per employee, however, it turned out that the stock market's reaction to downsizing announcements is strongly predictable in firms with high overhead, unpredictable in firms with low overhead.

This finding about efficiency and overhead intensity is important for relating our analysis to the views on efficiency and corporate governance that are standard in the finance literature.[58] The two approaches are complementary and broadly consistent, but their policy implications might carry an important difference. The finance literature concludes (to put it starkly) that there is nothing wrong with U.S. industry that an orgy of hostile takeovers can't fix. Our findings raise the possibility that the efficiency level that optimal boardroom arrangements can achieve is importantly qualified by bureaucratic dynamics in the internal organization of large, successful firms engaged in complex tasks.

58. Jensen (forthcoming) provides a forceful statement.

Comments and Discussion

Comment by Michelle J. White: This paper looks for evidence supporting the hypothesis that large enterprises tend to accumulate excess nonproduction workers (''fat'') but to cut back nonproduction employment differentially in response to ''unanticipated disturbances.'' The hypothesis is based on the assumption that it is difficult to maximize profit with respect to nonproduction employment—particularly executive employment—because the output of individuals cannot be measured. Therefore, the level of nonproduction employment is determined by considerations other than profit maximization. The paper is motivated by evidence suggesting that nonproduction employment grew faster than output during the 1970s but that managers were more likely to lose their jobs during the 1980s than either blue-collar workers or other types of nonproduction workers. Caves and Krepps hypothesize further that the source of the unanticipated disturbances in the 1980s was increased competition in the form of either greater import penetration or increased takeover activity. They test for associations between the level of nonproduction employment and the levels of import penetration and takeover activity by industry.

The question is an interesting one, with implications not only for private corporations, but also for the feasibility of reducing bloated staffing levels in the public sector. The hypothesis is quite vague, however. Caves and Krepps appeal to work by Williamson, the Carnegie group, and Niskanen to support their general approach. But these theories mainly concern levels of executive and managerial staffing, while in their empirical work Caves and Krepps explain the level of nonproduction employment—a much broader category. Nonproduction workers include not only executives, their assistants, and secretaries, but also sales and purchasing agents, lower-level managers, research and development personnel, and the clerical employees that issue invoices, pay salaries, run benefit programs, file reports with the Census Bureau, and so on. It is not clear whether or why the hypothesis of excess staffing applies to many of these categories. In addition, the Williamson, Carnegie, and Niskanen theories were developed specifically for large enterprises, but the data set that Caves and Krepps use to test their hypotheses is not restricted to large firms.

An additional issue concerning Caves and Krepps' hypothesis is how to define the unanticipated disturbance that causes firms to begin shedding excess nonproduction employment. They hypothesize that the unanticipated disturbance is either increased import penetration or increases in merger activity and do not consider any other possibilities. The Williamson, Carnegie, and Niskanen models, however, assume that it is enterprises with high profit levels that accumulate excess staff, so any factor that reduces the level of profits could trigger firms to reduce their staffing. This suggests that it might have been reasonable to look at profit levels directly as the determinant of the level of nonproduction employment. Also, other factors in addition to the levels of import penetration and merger activity could have affected profits and therefore nonproduction employment. Among these might be tax rates, business cycle considerations, and the level of domestic (rather than foreign) competition.

Turn now to the data used by Caves and Krepps. For various reasons, the data set covers only 1967–86, thus missing the period of the most drastic cuts in nonproduction employment, which occurred after 1986. Furthermore, individual observations are for three-digit industries in the SIC, so that whether industries have mainly large versus small firms is obscured. There is also substantial noise in the data for "auxiliary personnel," who are white-collar employees working at nonmanufacturing locations. This forces Caves and Krepps to rely mainly on regressions estimated for nonproduction workers who work at manufacturing sites—but these presumably exclude the executives and other headquarters staff who were the main motivation for the study in the first place. There are also problems with the data on import penetration levels and on merger activity and problems in matching up these series with the years and industries for which the other data are available. As a result, the data set is quite noisy.

Caves and Krepps rely on estimating reduced form equations, presumably because of their view that the level of nonproduction employment is not subject to profit-maximizing behavior. A theoretical model would nonetheless have been useful, particularly in providing an explicit story for where the error term comes from. A very simple story would be the following. Suppose we assume that firms' production function is Cobb-Douglas, or:

$$Q = L_p^{\alpha} L_n^{\beta} K^{1-\alpha-\beta} e^{\epsilon}.$$

Here Q is output, L_p and L_n are production and nonproduction labor, respectively, K is capital, and ϵ is an error term. For simplicity, assume that $E(e^{\epsilon}) = 0$, so that $E(Q) = L_p^{\alpha} L_n^{\beta} K^{1-\alpha-\beta}$. The story behind the error term is that the firm hires capital, production workers, and nonproduction workers at time 1 and uses them to produce output in time 2. But how much output these inputs will produce next period is uncertain. Normally, the uncertainty results from such factors as the possibility of a strike, but in Caves and Krepps' context, it can be thought of as resulting from the difficulties of measuring the contribution of nonproduction workers to output. Therefore, the output that a given number of these workers will produce is uncertain.

Expected profit at time 1 is

$$E(\pi) = PE(Q) - w_n L_n - w_p L_p - rK,$$

where P is the price of output, w_p and w_n are the wages of production and nonproduction workers, respectively, and r is the interest rate. If we solve for the first order condition for nonproduction workers, substitute for $E(Q)$, solve for L_n, and take logs, we get

$$\ln L_n = \ln\beta - \ln w_n + \ln(PQ) - \epsilon.$$

This is similar to the Caves and Krepps specification, except that they use first differences. We know, however, that the error term ϵ is related to Q. This would pose no problem if Q were on the left-hand side of the equation being estimated. But when the output is on the right-hand side, OLS results will be biased because an independent variable is correlated with the error term. Caves and Krepps ignore this bias.

If larger firms tend to be more profitable and are therefore more likely to have excess nonproduction workers, then the output and error terms will be negatively correlated. In that case, the estimated coefficient of output in the regressions will be biased downward. How much this bias will affect the coefficients of the import penetration and merger variables depends on how strongly these variables are correlated with output.

Taking first differences, we get

$$[\ln L_{n2} - \ln L_{n1}] = - [\ln w_{n2} - \ln w_{n1}] + [\ln(PQ)_2 - \ln(PQ)_1] - [\epsilon_2 - \epsilon_1].$$

This equation is similar to those estimated by Caves and Krepps, except that they add a term measuring the change in the log of production workers' wages (restricted to have the same coefficient as the change in the log of nonproduction workers' wages), and variables measuring import penetration, takeover activity, and time dummies. They also ignore the theoretical predictions that the coefficients are unity (not all of which would hold in a more general production function).

An additional problem with the specification is that it assumes implicitly that the production function is the same for all industries and that it remains the same over time, except for the error term. Given the rapid changes in technology caused by computerization, this seems problematic. Increasing computerization of services reduces the number of production workers needed to produce a fixed amount of output, but it also changes the number of nonproduction workers needed. Increasing globalization of production also changes the nature of the production function. If U.S. firms move production offshore, for example, but keep their nonproduction workers at home, then output per nonproduction worker will appear to fall drastically if the data capture only output produced in the United States. The opposite would appear to be true for foreign firms that produce in the United States but keep their nonproduction activities at home. The extent to which particular industries are affected by these trends varies. Caves and Krepps use time and sometimes industry dummy variables, which capture these effects crudely, but some of them might have been measured directly.

Given the various problems, it is not surprising that the results are inconclusive. The effects of merger activity and import penetration are found to be significant only for the 1980s, and even these effects are quite small.

The second study in the paper—the event study of announcements of corporate downsizings—is more successful. Here Caves and Krepps look for whether the value of firms announcing downsizings rises more (falls less) when firms also announce white-collar layoffs. An advantage of the event study is its simplicity. By examining the immediate effect of downsizing announcements on firm value, many other factors are held constant. Also, by including only announcements that occurred during the relatively short period of 1987–91, Caves and Krepps hold underlying conditions fairly constant. They find that downsizing announcements that include an announcement of white-collar layoffs cause the value of the firm to fall by less than it would have if white-collar layoffs were not announced. The effect of announcing white-collar layoffs is also greater for firms whose overhead is higher than the median in the sample. These results provide support for the hypothesis that at least some firms have excess nonproduction workers. But the event study cannot answer the question of what triggers firms to shed their excess nonproduction workers.

Comment by Henry Farber: The ability of managers of large enterprises to pursue goals other than profit maximization is a potentially important factor in determining productivity levels. Building on the work of Cyert and March, Williamson, Niskanen, Nelson and Winter, and others, Caves and Krepps argue that managerial deviation from the single-minded pursuit of profit maximization can take the form of excess employment of nonproduction workers.[1]

As I understand it, the basis for their argument has two pieces. The first is what Caves and Krepps call the Carnegie approach (as developed by Cyert and March). This emphasizes the lateral relationships within firms that are central to the synthesis of disparate activities into coherent operation of the firm as a whole. To the extent that survival of the firm does not require strict adherence to profit maximization (due to imperfect product markets), excess profits can be shared laterally across functional groups in the form of expansion of the various domains. The

1. Cyert and March (1963); Williamson (1964); Niskanen (1971); and Nelson and Winter (1982).

second piece is based on Niskanen's model of bureaucratic behavior. In this model the central management is ill-informed about the operational details of the various subgroups that make up the firm. This incomplete information makes it difficult for central management to resist budgetary and personnel requests of the subgroups. The informational asymmetries are argued to be particularly acute for nonproduction subgroups.

The argument that there is organizational slack and that this slack might result in excess employment of nonproduction workers has some face validity. The focus on nonproduction workers is questionable since the same organizational slack might also result in excess employment of production workers. Work rules negotiated by unions and management that have the effect of increasing labor-output ratios are one institutionalized example of this.

The empirical analysis begins with some summary statistics, gleaned from published sources, suggesting, first, that nonproduction employment as a fraction of total employment rose between the late 1960s and the late 1980s and, second, that executive, administrative, and managerial employment in manufacturing grew more rapidly between 1970 and 1980 than employment of other white-collar workers.

Additional published tabulations, from the Displaced Workers Survey (although the source is not stated), compare job loss rates among blue- and white-collar workers in the early and late 1980s. It is noted that job loss rates are lower for white-collar workers than for blue-collar workers over the entire decade. Caves and Krepps argue that the early period (1979–83) covers a recession, while the later period (1985–89) covers more prosperous years. The facts (in table 3) are that rates of job loss are higher in the slack years, but the difference between the slack and strong years is much larger for blue-collar workers than for white-collar workers.

Here is where important issues of interpretation are raised. Caves and Krepps want to interpret the lack of substantial decline in white-collar job loss between the early and late 1980s as preliminary evidence that ''fat'' was being excised from firms in the late 1980s as pressures from increased import competition mounted and as mergers and acquisitions resulted in managerial tightening. A longer view is needed, however, before such a claim can be accepted. It is almost certainly the case that white-collar employment, in addition to having lower rates of job loss generally, is much less sensitive cyclically than is blue-collar employment.[2] Thus, it is not surprising that the decrease in the rate of job loss for white-collar workers was much smaller between the early and late 1980s than it was for blue-collar workers, and this may have nothing to do with "excision of fat."

This brings us to the core of the empirical analysis. The key insight is that the ability to pursue objectives other than profit maximization, in this case increased nonproduction employment, depends on imperfections both in firms' product markets and in the control of managers by firm owners.[3] By implication changes in competitiveness or control will yield commensurate changes in overemployment of nonproduction workers. Operationally, Caves and Krepps use changes in industry-level import penetration to indicate changes in competitiveness of domestic markets and industry-level merger activity to indicate changes in corporate control that could restrict management's ability to overemploy nonproduction workers.

At this point some simple tabulations and summary statistics on import penetration, merger activity, and production and nonproduction employment would have been useful. That would allow readers to determine whether there were strong relationships in the data "screaming" to make themselves heard. How are these variables trending over time? Are they moving together? What does interindustry variation look like? Unfortunately, no such simple statistics are presented, and readers are left to their own devices in evaluating the first-order properties of the data.

With regard to estimation, Caves and Krepps estimate labor demand functions for nonproduction workers in U.S. three-digit manufacturing industries during 1967–86 using data from various Censuses of Manufactures. Roughly speaking, there are industry-level observations every

2. Reasons for this might include such factors as more specific investment in white-collar employees that makes firms less willing to lose these workers in temporary downturns and implicit contracts with white-collar workers (perhaps for reasons related to specific capital) that promise relatively stable employment.

3. The market imperfection idea is the same idea used by Becker (1957) in his analysis of discrimination in labor markets. Becker argued that employers could exercise a taste for discrimination only if the product market was not perfectly competitive. Otherwise, nondiscriminating employers would drive discriminating employers out of business.

five years. These labor demand functions are augmented with measures of import penetration and merger activity.

It is worth writing down carefully the general form of the late demand function that is estimated. This is

$$\Delta L^n_{it} = \alpha_0 + \alpha_1 \Delta Q_{it} + \alpha_2 (\Delta \ln W^n_{it} - \Delta \ln W^p_{it}) + \alpha_3 \Delta I_{it} + \alpha_4 M_{i(t-2)} + \epsilon_{it},$$

where ΔL^n_{it} is the five-year change (to period t) in industry i nonproduction employment, ΔQ_{it} is the change in industry i real output, $\Delta \ln W^n_{it}$ is the industry change in industry i wages for nonproduction workers, $\Delta \ln W^p_{it}$ is the change in industry i for production workers, ΔI_{it} is the change in industry i import penetration, and M_{it} is the fraction of industry assets absorbed in merger activity in industry i in the five-year period ending at year $t-2$.[4] The model also includes a set of time dummies.

This empirical representation is meant to be the differenced form of a demand function derived from a simple Cobb-Douglas production function augmented by variables (ΔI and M) that represent changes in the firms' ability to deviate from optimal resource allocation. One obvious, untested, and perhaps unnatural restriction implicit in this specification is that the elasticity of demand for nonproduction workers with respect to the nonproduction wage is assumed to be equal in magnitude and opposite in sign to the elasticity of demand for nonproduction workers with respect to the production wage. I do not see why this should be the case. It would be straightforward to relax this restriction. It is also true that these elasticities are likely to differ significantly across industries. The lack of a sufficiently long time series, however, precludes estimation of industry-level demand functions with these data.

The key results of this analysis are presented in table 4. There seems to be no significant relationship between the change in nonproduction employment and either merger activity or import penetration. Further estimation, in table 5, allows for changes over time in the effects of the key variables. The strongest result found here is that changes in import penetration are significantly negatively related to changes in nonproduction employment after 1982. The results regarding merger activity

4. The two-year lag on merger activity is arbitrary, and it would have been useful to explore the sensitivity of the results to this assumption.

are less clear. One has to be concerned in this analysis about the rationale for breaking up the sample by time period. If variables such as changes in import penetration and merger activity are important determinants of changes in the scope for pursuit of objectives other than profit maximization, then the correlations should not be particularly sensitive to time period. It is hard to conclude that finding a relationship in only some subperiod is strong evidence for a general theoretical proposition. Overall, I do not find the results of the labor demand analysis compelling with regard to the central question.

The final section of the paper contains potentially the most interesting results. Here the authors carry out event studies to investigate the effect of announcements of reductions in force on the market values of the firms involved. Two plausible hypotheses are contrasted. First, market value may fall if the announcement of a reduction in force signals to the market that the firm is in worse shape than was thought. Alternatively, market value may rise if the market perceives that the reduction in forces signals a tightening of control in the firm and a return to profit maximization as the central objective. Of course, these hypotheses are not mutually exclusive.

The evidence suggests that, on average, firm value declines relative to the market on announcement of a reduction in force. This would seem to support the view that the market takes the reduction as a signal of ill health. An intriguing exception to this rule supports the alternative hypothesis, however. When the reduction in force involves white-collar employees, the market value of the firm does not decline significantly relative to the market. This is what is expected if the market perceives the white-collar reduction in force as an improvement in firm-level efficiency.

In summary this paper is about a very interesting problem, but the empirical analysis provides preliminary evidence at best. The estimates of the labor demand functions are insufficiently precise to draw firm conclusions. The tantalizing results from the event studies, where preliminary evidence is found that layoffs of white-collar workers do not decrease market value, suggests that further work along this line is warranted.

General Discussion: Several participants suggested alternative ways to explain the forces driving nonproduction layoffs. Martin Baily conceded that anecdotal evidence argues for the existence of excess nonproduction employment, but he suggested that the "fat" cutting triggered by import competition, which the authors uncovered, may actually occur because such competition forces domestic producers to abandon particular markets, making them shed employment as they go. Margaret Blair contended that white-collar employees who had once been employed under profit-maximizing conditions began to experience layoffs in the 1980s as a result of a rise in the hurdle rate.

Henry Ergas suggested that computerization may explain some portion of the white-collar layoffs. According to Ergas, the Carnegie model assumes that certain groups of workers are able to bargain for more jobs and job security or higher pay levels than they would receive under conditions of perfect competition. The power of a group to secure such rents depends partly on the extent of its firm-specific skills. Computerization, Ergas surmised, has probably standardized a broad range of administrative tasks, such as order management and invoicing, thereby reducing the firm-specific nature of such tasks and the firm-specific skills involved in performing them. In turn, the bargaining power of the groups that carry out such tasks has been reduced. Consequently, Ergas argued, their ability to secure rents in the form of higher employment levels has decreased.

Several participants commented on methodological and measurement issues. Ernst Berndt praised the authors for trying to include white-collar employment at central administration sites in their data, noting that the exclusion of these workers has been a flaw in previous literature. He questioned, however, the authors' treatment of import ratios as an exogenous variable in their model. Eric Bartelsman said that he had converted industry data from 1987 and more recent years to conform to the old (1972) SIC standards and suggested that the authors use this 1987 data instead of those from 1986, because the 1986 data come from an ASM panel that is biased by entering firms. He also suggested that the outsourcing of business services has grown in recent years, as evidenced by a tightening relationship between changes in business service employment and cyclical changes in manufacturing output. He suggested that to understand changes in white-collar employment within firms, one has to recognize the increasing importance of outsourcing.

Given the allegedly nonoptimizing nature of firm nonproduction employment, Peter Reiss suggested that more work needs to be done on how and when a firm actually decides that it has excess labor. He also wondered why corporate restructurings are usually identified, at least in the press, only when there are massive layoffs. Presumably, he said, downsizings also take place on a more gradual basis.

Robert Hall questioned the intellectual underpinnings of the division of workers into production and nonproduction categories. Noting that in the airline industry, pilots and reservationists are classified as nonproduction workers, while copilots and maintenance employees are classified as production workers, he argued that such divisions do not correspond to serious economic distinctions. Hall asserted that, because of the replacement of labor by technology and the movement of physical production abroad, "production" in the U.S. economy today is largely concerned with tasks that are formally classified as nonproduction, such as tracking down missing invoices.

Frank Lichtenberg said that although the authors do not find strong evidence that changes in corporate control have affected white-collar employment at the industry level, his own work from census data has found such evidence at the plant and firm levels. He also noted that the authors find that white-collar layoffs are associated with such returns. Because changes in control are known to be associated with such returns, this suggests that white-collar layoffs should be positively correlated with changes in control, Lichtenberg said.

Robert Lawrence argued that a close look at the data shows that the proportion of nonproduction employment in manufacturing has actually continued to grow steadily, with little evidence of a white-collar shakeout. In contrast, a shakeout has been occurring in service industries, which Lawrence continued, may support a hypothesis that excessive white-collar management has been partially responsible for the low productivity growth in that sector.

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