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Government Credit Policy and Industrial Performance

(Japanese Machine Tool Producers, 1963–91)

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In Japan, directed credit policy for the machine tools industry was effective in promoting investment, supporting growing firms, crowding in private funds, and avoiding capture of policy funds by particular firms. But the effectiveness of industrial credit policy in Japan is probably unrepresentative. In countries where special interest lobbies rather than a unified national plan shape such programs, they may do more harm than good.

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Summary findings

Programs to direct credit to industry can be uniquely beneficial if (1) the purpose of government credit is to relax borrowing constraints on firms, as an end in itself (that is, if there is a capital market failure), or (2) other government objectives can best be achieved by relaxing firms' borrowing constraints (in which case, product and factor market externalities motivate government credit programs).

According to Japanese officials, government involvement is warranted when:

- Investment risk is too high for a particular activity (because it is too large-scale or high-tech, or needs long gestation and market development).
- There is a big discrepancy between private and social benefits (when industries or parts of industries may save foreign exchange, for example, and thus relieve the balance of payments constraint on other growth industries).
- Information problems discourage lending to small and medium-scale industries.
- Infant industries face large social set-up costs.

Calomiris and Himmelberg examine the effect of policy-based finance for the period 1963–91 for Japan's machine tool industry, an industry with high potential spillover effects on technological innovation and learning. They found that directed credit may have helped to promote investment among postwar Japanese machine tool producers. Important components of that credit seem to have spurred growth. The government credit programs did not crowd out private funds and did not succeed by providing a permanent lifeline ("credit insurance") to firms.

But Calomiris and Himmelberg do not endorse government interventions in credit markets. For one thing, the effective operation of industrial directed credit in Japan seems to be an unrepresentative case. In many countries, such government intervention has produced large costs: Inefficient borrowers have been funded and public funds have been captured by special interests.

In Japan, directed-credit policy is designed to promote investment, crowd in private funds, and avoid the capture of policy funds by particular firms or industries. The priorities of credit policy are determined as part of a national plan with broad participation (rather than by special-interest lobbying), and once industry-level priorities have been established, firm-level lending decisions by agencies are shielded from political pressure. In political systems that cannot implement such effective plans for distributing industrial credit, government-directed credit programs may create more problems than they solve.

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GOVERNMENT CREDIT POLICY AND INDUSTRIAL PERFORMANCE:

JAPANESE MACHINE TOOL PRODUCERS, 1963-1991

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I. INTRODUCTION

Japanese postwar industrial success coincided with a uniquely Japanese approach to industrial policy and government directed credit, which was part of that policy. Recently, a debate has been joined over whether government "targeting" (including credit policies) was an integral contributor to the Japanese "miracle." The World Bank (1993), and several articles cited therein, take a sanguine view of industrial credit policy. Others (Beason and Weinstein, 1993) argue that, contrary to conventional wisdom, unique institutional features of Japanese development -- most notably, industrial policy and "main" banking -- actually retarded growth.

This paper considers general theoretical arguments that favor directed credit programs, places them in the institutional and political context of postwar Japanese industrialization and directed credit, and confronts theory with evidence. We motivate and summarize the results of an empirical study of industrial directed credit policy in the Japanese machine tool industry over the last thirty years. That period witnessed enormous progress in machine tool productivity and growth in Japan, as well as in the Japanese economy more generally. Our goal is to measure and explain the potential contribution of directed credit to that growth.

II. INDUSTRIAL CAPITAL MARKETS AND POLICY

Chandler (1977) has emphasized large potential economies of scope and scale in production and distribution since the second industrial revolution of the late nineteenth century. Recent analysis of Japanese industrial growth during the postwar period (Baily et al., 1993; Japan Development Bank, 1994) has emphasized that much of Japanese success in boosting productivity in manufacturing rested on the ability to promote efficient, large-scale production.[1] While not all of Japanese postwar manufacturing displayed increasing returns to scale, some of the fastest growing, most R&D intensive manufacturers (notably in electrical machinery) faced scale elasticities of roughly 1.4 (Nadiri and Prucha, 1990, Griliches and Mairesse, 1990). A central question of debate that surrounds recent Japanese success in manufacturing is the extent to which it relied on Japanese industrial policy, and the extent to which government directed credit policies were an important part of industrial policy.

Advocates of the importance of directed credit to industry argue that the potential gains to society from concentrated investment in growing industries may be hard to reap without government intervention. Such investment typically involves substantial uncertainty about the demand for products and the costs of production, particularly in new, growing industries. Large-scale production of new products using new technologies creates special information problems for the financial system. Large-scale production implies greater needs for funds for purposes the merits of which outsiders may be ill-equipped to judge. Similarly, outsiders find it costly to monitor and control the management of large-scale enterprises engaged in complex production and distribution processes.

Financial intermediaries, of course, are designed to process information, raise and allocate funds, and control the use of those funds by firms. Clearly, delegation of monitoring and control to intermediaries is central to the success of mobilizing funds for industrialization. But there are limits to the incentives private intermediaries face to finance all worthy projects.

The monitoring costs of infant industries are front-loaded toward the present, while the profit streams for these industries are back-loaded into the future. As Mayer (1988) has pointed out, if intermediaries are unable to count on a long-term relationship with a firm, then they will be unwilling to postpone collection of monitoring fees until the firm matures. In this case, viable long-run projects will not be financed by private intermediaries. One possible reason why firms and banks cannot credibly commit to a long-term relationship is that the initial bank's decision to grant credit to the firm, and subsequent renegotiations of credit terms with the firm, are publicly observable. Other banks, therefore, may be able to learn about the firm's credit history as it becomes "seasoned" by the initial bank. Thus subsequent lenders can free ride on the initial bank's actions. If banks can compete for an initial bank's customers as the customers mature, then the initial bank cannot internalize all of the benefits from its investments in information. This prevents the initial bank from recouping its front-loaded monitoring costs over time. Mayer (1988) argues that banking systems like Japan's which grant banks greater control over firms may be able to solve this time-inconsistency problem and thus provide greater funding to infant industries. Calomiris (1994) argues that under universal banking (concentrated banking systems in which banks can hold a variety of claims on, and offer many services to, firms) banks may be better able to internalize the benefits of initial investments in monitoring firms.

The failure of private intermediaries to supply adequate credit to industrial borrowers, because of this free-rider problem, can justify government financing of "initial bank" lending. Subsidies to initial banks can compensate for the front-loading of intermediation costs that discourages lending to unseasoned firms. Alternatively, the government can go into the banking business itself, as is the case in Japan, where the Japan Development Bank, the Export-Import Bank, and other government-sponsored authorities, see themselves as providing directed credits as "initial banks." For capital-constrained firms, assistance that relaxes capital constraints directly may have a more potent effect on economic growth than other forms of subsidy.

With respect to externalities across firms, it is often argued that product and factor market externalities may be large in the industrial sector, where technological improvements and worker training can have important spillover effects across firms. Thus in the industrial sector credit constraints can have a wider social cost in addition to their direct effect on constrained entrepreneurs -- they restrict the development of firms that would generate positive externalities for other firms.

To sum up, there are two primary justifications given for government involvement in industrial credit programs, both of which are traceable to a failure of private credit supply to finance worthy projects.[2] Credit programs can have a uniquely beneficial role if (i) the purpose of government credit is to relax borrowing constraints on firms as an end in itself, or (ii) if other government objectives can be best achieved by relaxing firms' borrowing constraints (see Calomiris and Himmelberg, 1994). In the latter instance, product and factor market externalities motivate government assistance to firms. In the former instance, the justification for government credit programs revolves around capital market failure per se.

III. ARE THESE MOTIVATIONS REAL?

Both of these motivations are defensible on a priori grounds, given the widespread evidence of information problems in capital markets, and both have figured prominently in arguments about the benefits of directed credit in Japan.

The experiences of a wide variety of countries over different eras support the notion that asymmetric information places important impediments on firms' abilities to invest, particularly during early high-growth phases of production (for reviews, see Calomiris, Himmelberg, Kahn, and Vittas, 1992, and Calomiris and Himmelberg, 1994). Corporate financing decisions also reflect the importance of asymmetric information as a constraint on firms' sources of funds. Throughout the world, internal finance and inside (bank) debt are the dominant sources of corporate finance.

Several recent empirical studies of Japanese and American corporate finance have emphasized the advantages of banking relationships (see the reviews in Calomiris and Himmelberg, 1994; Calomiris, 1994), particularly under the "main-bank" system in Japan (Sheard, 1989; Hoshi, Kashyap, and Scharfstein (1990a, 1990b, 1991). These studies find that concentration of debt and equity holding, as well as membership in a main bank-run keiretsu, allow firms to invest at lower cost during normal times, and to continue investment and production at much higher levels than other firms during episodes of financial distress.[3]

The stated goals of the Japanese programs have been quite compatible with defensible theoretical objectives, based on the presence of capital constraints. A recent policy statement by the Japanese Overseas Economic Cooperation Fund (OECF, 1991) provides motivations which are closely related to our previous theoretical discussion of the role of government in credit supply. The OECF's list refers specifically to externalities in production, technological development, and factor markets, and to the benefits of relaxing financing constraints faced by growing enterprises. According to the OECF statement, government involvement is warranted in the following cases:

* When the investment risk is too high regarding a particular activity (owing to the need for large scale, long gestation period, high technology and market development).

* When there is significant discrepancy between private and social benefits (e.g., in the case of rural industries that increase job opportunities in rural areas and prevent over-concentration in urban regions).

* In the case of industries that may save foreign exchange and thus relieve the balance of payments constraint on other growth industries.

* In the case of investment for pollution control and environment protection.

* When infant industries face large social set-up costs.

* When information problems discourage lending to small and medium scale industries.

Clearly, there is a close correspondence between the stated rationale for industrial policy intervention and the theoretical motivations discussed above.

A similar list of objectives appears in the Japan Development Bank's (1994) history of postwar Japanese directed credit policy. Interestingly, the JDB places particular emphasis on its role as a "pump primer." That is, it sees itself as an initial lender to unseasoned firms, and looks to step aside once its borrowers' credit records are good enough to attract private funds. From the perspective of Mayer's (1988) time-consistency problem, one could argue that pump priming requires a substantial government investment in monitoring costs, which are recouped through the public benefits of helping firms to develop creditworthiness. The JDB explicitly sees monitoring and "seasoning" as its key role in lending to infant industries, and prides itself on its success in priming the pumps for private creditors. The JDB also prides itself on the rapid technological advancements and high rates of growth that targeted industries have achieved in the post-War era.

Other authors have emphasized that pump priming often occurs immediately, as private lenders join in government-sponsored long-term credit syndications (Horiuchi and Sui, 1993). The alleged "crowding in" of private creditors may be encouraged either by the government agency's willingness to assume the costs of monitoring, or by the long duration of government loans (which provides de facto "senior" status to private credits of shorter duration).

The Japan Development Bank (1994) survey of directed credit and industrial policy in the post-War era also reveals that anticipated product and factor market externalities were central to the sequence of assistance given to different industries. In the 1950s the so-called "basic" industries -- electricity, iron and steel, shipbuilding, and coal mining -- received the bulk of funding because they were seen as industries that supplied basic inputs for growth in other sectors. Once basic industries were developed, the government targeted manufacturing industries -- notably machine tools and automobile parts -- which were viewed as likely to provide spillover benefits to other industries through technological changes and improvements in capital goods. More recently, high-technology electronics firms have been supported for the same reasons.[4]

The fact that one can imagine justifications for government credit policies traceable to capital market imperfections, or that government officials focus on these justifications and on sectoral success stories when explaining their policies, does not mean that such policies are a good idea. First, it is not clear that the assumptions necessary to justify government involvement are met generally. In particular, developed economies with relatively sophisticated private capital markets may offer little opportunity for beneficial government involvement. Even more important, governments do not always "do the right thing," even if they espouse legitimate objectives or possess comparative advantage. For example, Brazilian capital market programs (Gelb et al., 1980) are an extreme (but not unrepresentative) example of how the political economy of government assistance to industry can often result in rent-seeking, corruption, and crowding out, rather than the pursuit of efficient industrial projects. Clearly, empirical support

for directed credit policy must do more than link credit with firm growth. It must show that credit is directed rationally, that permanent capture of government funds is avoided, and that firm performance is ultimately linked to its efficiency as a producer. In addition to statistical work, therefore, it is incumbent on researchers to analyze the political process of directed credit policy.

Furthermore, before reaching firm conclusions about the desirability of industrial credit, directed credit programs must be evaluated from a general equilibrium perspective. Even if a program leads to growth in a particular sector, it may do so by crowding out growth in other sectors. Similarly, if growth is achieved through the granting of special privileges, or the imposition of special government regulations (e.g., consumption taxes in Japan), one must weigh any costs associated with these against any alleged benefits to the targeted industry.

Thus, despite the apparent successes of Japanese development and industrial policy, and the apparent consonance between the theory of welfare-improving government intervention and the stated policies of Japanese government agencies, there has been widespread skepticism about the benefits of Japanese credit programs, both within and outside of Japan (for a review, see Vittas and Wang, 1991). Some of that skepticism revolves around other features of the Japanese economy, which some economists view as mitigating the potential explanatory role of credit policies for industrial growth.

First, even economists sympathetic to the potential constraints posed by capital market imperfections may be skeptical of the importance of such problems for Japanese industry. The Japanese private main bank system may be capable of "internalizing" externalities better than other private banking systems (Sheard, 1989, Mayer, 1988). The main bank system is well suited to capturing the long-run benefits of short-run investments and lending policies, for several reasons. Time-consistency in arrangements between intermediaries and their firms may be improved by cooperation among main banks who participate in multiple lending syndicates. Main banks can use interbank coordination to share benefits and costs to compensate lenders for high initial monitoring costs. Also, the main bank's corporate control devices (especially its power as an equity holder in choosing the Board of Directors) allow it to prevent deviation by firms from prior commitments. Thus it may be particularly hard to justify Japanese government credit programs on the basis of externalities in factor and product markets, or information externalities due to free riding on monitoring costs.

Skeptics also point to other aspects of Japanese industrial policy -- tariffs, Bank of Japan "window policy," and other policies -- which often targeted the same favored sectors that received credit assistance, and offer a competing explanation for high growth rates of credit-subsidized sectors. Clearly, any attempt to argue the merits of directed credit from data on sectoral performance must disentangle the contributions of credit from other government policies favoring particular sectors.

IV. EXISTING EMPIRICAL EVIDENCE

With few exceptions, empirical analysis of Japanese industrial policy and directed credit programs has been done at the sectoral, rather than the firm, level. Kawaura (1992) and Ohno (1992) show that industrial growth and directed credit were positively associated at the sectoral level during the postwar period. This conclusion has been questioned in recent research by Beason and Weinstein (1993), who argue that relatively unproductive industries received large shares of government assistance in a variety of forms (tariff protection, subsidies, favored tax treatment, and credit). While the Beason and Weinstein paper provides an important antidote to uncritical acceptance of the growth benefits of industrial policy, it is not useful for answering the question whether industrial credit policy promoted the growth of infant firms, for two reasons.

First, as is clear in the Japan Development Bank (JDB, 1994) review of industrial policy and directed credit, in many (and perhaps, most) cases, industrial credit policy has not been used to promote highly productive infant firms or industries. The Japanese plan often targeted industries for assistance because of their spillover effects for other industries. The logic of Japanese industrial policy presumes a temporal ordering of development. That does not translate into subsidizing the most productive sectors first, or most. Moreover, in addition to infant-industry motives, the Japanese have sometimes targeted assistance directly to declining industries, as a means to smooth adjustment. Coal mining is a prominent example of a low-productivity industry that received assistance early on because of its perceived spillover potential for initiating industrialization, and then received assistance later to permit smooth, orderly exit of workers. In neither case did the government conceive of, or defend, its support for coal mining on the basis of high productivity. A better test of the success of infant-industry support would focus on the examples of directed credit policy that were explicitly designed to improve productivity in technologically dynamic industries. Prominent examples for postwar Japan include machine tools and automobile parts (JDB, 1994).

Second, sectoral-level analysis is too crude a tool to measure the potential role of credit assistance in supporting "infant" firms. Sectoral-level analysis cannot distinguish the effects of credit policy from other policies. Nor can it help one judge whether the firm-level allocation of credit by the Japan Development Bank (and other government lenders) was effective in promoting growth.

Horiuchi and Sui (1993) provide an important first step toward measuring the effects of directed credit on investment at the firm level. They compare the investment behavior of "medium-size" firms receiving JDB assistance during the period 1964-1988 with other firms of similar size. They find that the year of initial JDB lending was associated with increased investment, and that within three years firms began to move away from a reliance on JDB lending to rely more on private banks. Moreover, they find that the interest expense of firms relying on the JDB was higher than non-JDB firms, which they interpret as evidence that JDB interest subsidies were small. This is consistent with JDB data, which indicate a roughly one percent interest rate subsidy on credit (JDB, 1994). These facts lend support to the JDB's

accounts of "pump priming" and "crowding in" of private credit.[5] Horiuchi and Sui (1993) also find evidence of capital growth (financed by internal funds rather than private bank debt) prior to, as well as following, receipt of JDB credit. Their evidence is consistent with (but does not prove) the view that the JDB targeted capital intensive, relatively productive, credit-constrained firms.

Interestingly, Horiuchi and Sui (1993) also find (for a very small sample of firms) that firms with main bank affiliations that received JDB credit did not display the same reactions in their investment behavior as other firms. Specifically, main bank-affiliated firms receiving directed credit did not show the same increase in investment after JDB intervention. That fact is consistent with Mayer's (1988), Sheard's (1989), and Hoshi, Kashyap, and Scharfstein's (1990b) arguments about the role of the main bank system as a device for resolving free-rider problems. If main banks serve that useful function, and if directed credit is an alternative means to resolve free riding, then one would expect directed credit to have relatively little effect on main bank-affiliated firms. Despite their small sample, and their mostly informal empirical analysis, Horiuchi and Sui (1993) provide the first systematic quantitative study of directed credit at the firm level. Their results are broadly consistent with the stated objectives and performance claims of the JDB, and with the theoretical motivations for directed credits that rely on the free rider problem.[6] The empirical work we discuss below, like that of Horiuchi and Sui (1993), is designed to address the questions of the sources and consequences of government directed credit, and to understand the mechanism through which directed credit promotes growth, by focusing on firm-level data.

V. AN OVERVIEW OF POSTWAR INDUSTRIAL CREDIT POLICIES IN JAPAN

Industrial directed credit programs in Japan, like other forms of government assistance to industry, trace their intellectual origins to the aftermath of World War II. The Japanese economy was devastated by the War, and lacked concentrations of financial wealth to finance the rebuilding of the economy. The Japan Development Bank (1994) describes the history of early attempts to organize directed credit programs during this period, which culminated in the establishment of the Export-Import Bank and the Japan Development Bank as the two most important vehicles for providing credit assistance to industry.[7] Credit assistance has always been guided by the priorities set by the government's five-year plan. The plan outlines sectors on which credit assistance should be focused, and credit is only one means of government industrial policy toward various sectors. Tariff policy, subsidies, and other government interventions combine with credit policy to meet the overall objectives of the government's plan.

The details of sectoral resource allocation plans are achieved through an elaborate consultative process which involves "deliberative councils" and other advisory bodies composed of industrialists, workers, academics, bankers, politicians and bureaucrats. Participation by virtually the entire industrial sector in this process is compulsory. Representatives of various sectoral interests must make a case for prioritizing their needs in this national forum for debate. In contrast to the American political process, where Congressional committees are primarily influenced by special lobbying interests, competing interest groups in Japan are forced to obtain

national consensus for targeting their industry for assistance. Once the broad guidelines are set in place, firms applying for credit must meet the credit standards of the individual lending agencies, who pride themselves on independence from government pressure in determining which borrowers are worthy of credit. Loans from government banks take the form of standard loan contracts, and JDB officials view themselves as playing the same role as a private bank in administering the loan and monitoring the firm (JDB, 1994). Projects financed by directed credit sometimes are financed through a syndicated loan involving private banks (Horiuchi and Sui, 1993, p. 447).[8]

Consistent with the lack of "capture" in Japan's directed credit programs and the independence of its largest public lenders, the industries receiving assistance have changed over short periods of time and the default record for firms receiving assistance has been remarkably good. As shown by the Japan Development Bank (1994), despite the fact that directed credit has sometimes been the largest component of some industries' outstanding credit, and has taken the form of very long-term lending (typically in excess of 10 years), it has resulted in very few loan defaults. Indeed, the Japan Development Bank's default record of 0.01 percent for the high industrial lending period of 1956-1965 is superior to that of private commercial banks whose lending exposure was often less and whose loan durations were typically under 3 years. The Japan Development Bank's loan interest rates were also quite high, ranging typically between 1 and 2 percent below private rates. Thus, at least from the standpoints of direct costs from subsidies and defaults on government directed credit, Japan's credit programs have not suffered the extreme costs typical of many other countries.

Even if one were convinced that Japanese directed credit avoided capture and high costs of default, however, that does not prove that it caused industrial growth, or that in doing so it provided a net social benefit. Even for those who are convinced that directed credit promoted growth in targeted firms, the mechanism through which credit operated is unclear. Did credit help firms because of its direct effect on firms' immediate access to funds, or did it help by convincing private lenders that the government was "insuring" the future of the recipient firm, thus reducing the credit risk faced by private lenders? Some commentators on JDB policy (Higano, 1986) have stressed the importance of the JDB's credible commitment to act as an initial bank -- monitoring and controlling the firm to protect its long-term debt. Others (Sato, 1990) view JDB assistance as a signal that the government will provide credit insurance to private banks if the firm falls into financial distress. If the latter explanation were important, that would raise the possibility that costly implicit bailouts and moral hazard problems were a price for industrial growth fueled by directed credit.

Only a microeconomic analysis of the supply of directed credit, and the relative marginal contributions of government and private credit supply to capital accumulation, can provide a convincing assessment of the contribution of directed credit to Japanese post-World War II industrial growth. As we have argued, because it is difficult to isolate the effects of credit assistance at the industry level, firm level data are needed to test the effects of credit policy. With firm-level data one can control for industrial policies affecting whole industries, and examine within-industry connections between government credit and economic performance

across firms. In cooperation with the Japanese government, the Japan Development Bank, and the World Bank, Calomiris, Himmelberg, Kahn, and Vittas (1992) outlined an agenda for collecting and analyzing panel data on Japanese firms. That proposal emphasized the importance of collecting detailed information at the firm level, including all sources of credit, to arrive at conclusions about the effectiveness and potential costs of directed credit. The data collection effort that grew out of that proposal underlies the empirical work that follows.[9]

VI. A PANEL STUDY OF MACHINE TOOL PRODUCERS

We have collected data on government credit and firm characteristics for the period 1963-1991 for machine tool producers (defined as firms producing general machinery, electrical machinery, precision instruments, and transportation industry parts, which correspond to JDB industry codes 25, 27, 31, and 29). These data are from publicly available annual corporate reports. Our principal source for these balance sheet and income data is the "JDB tape" (available for purchase from the JDB), supplemented for years prior to 1982 with financial data collected by hand from individual corporate annual reports, which were made available through a joint effort by the World Bank and the Japan Development Bank. Despite the unique richness of this data set -- particularly, the identification of outstanding balances to individual lenders -- it has the important limitation of selection bias. Only survivors are included in the JDB tape. Firms that exited the industry prior to 1991 are nowhere present in the data. This may make the identification of the characteristics of recipients and the effects of government credit more difficult, particularly during the consolidation phase of the 1960s and 1970s when less productive firms exited or were acquired by other firms.

Consistent with our emphasis on industries and firms in which infant-industry concerns motivated directed credit, we chose to focus our initial investigation on the experience of machinery manufactures. These firms were among the primary targets of industrial policy plans during the 1960s and 1970s. Machine tool producers underwent substantial consolidation during the 1960s and 1970s, which was associated with the introduction of new technologies and the achievement of unusually large economies of scale (Nadiri and Prucha, 1990). The technological dynamism of these industries also suggests the possible importance of front-loaded monitoring costs, which may lead to free-rider problems among intermediaries discussed above.

These industries may also have served a central role in promoting industrial growth in other sectors. DeLong and Summers (1992) find that machine-tool investment is strongly correlated with long-term economic growth (see also Rosenberg, 1972). That raises the intriguing questions of whether product and capital market externalities were present in these industries, whether firms likely to create positive externalities were targeted by government finance, and whether this may have contributed to the dramatic growth of industry in post-War Japan. Although measuring such spillovers is not the focus of our study, potential technological spillovers add to the interest in studying the history of machine tool producers.

Our analysis of directed credit divides into seven related parts. We begin by (1) describing the composition of our dataset and (2) outlining broad trends in the machine tool

industry and its sources of credit during our sample period. We then (3) examine the changing concentration of directed credit over time within the industry, and (4) the frequency and duration of the receipt of directed credit for the firms that received it.

(5) Using probit models, we analyze the characteristics of firms that predict which firms became targeted for directed credit by government agencies. We compare the characteristics of targeted firms across government agencies and over time, and compare the lending patterns of the main government lenders (the Japan Development Bank and the Export-Import Bank) with private long-term creditors (the Industrial Bank of Japan and the Long-Term Credit Bank). These regressions help us to identify economic characteristics of firms that made them more likely to be chosen by the JDB and the Export-Import. They also allow us to ask whether firms that received a first round of directed credit were more likely to be bailed out by their government creditors if they experienced financial distress (the possible "moral hazard" problem referred to above).

(6) Using panel vector autoregressions relating sales, earnings, investment, and various sources of credit, we analyze the effect of an increase of government credit on fixed capital investment, and compare the effects on investment of increases in government and private credit. Finally, (7) we use the same method to estimate the effects of government credit on private credit, to determine whether government credit "crowds out" or "crowds in" privately supplied funds.

Composition of Panel and Industry Trends

Our data cover the period 1963-1991, and include 8,156 firm-year observations. General machinery accounts for 3,561 observations, electrical machinery for 3,327, precision instruments for 734, and transportation parts for 624. After subtracting observations with missing values and extreme outliers, we retained roughly 80 percent of our observations. The percentage of usable observations was roughly the same across the various sub-industry classifications. The percentage of usable observations, however, was larger for later years. For the period 1963-1971, 61 percent of observations were usable, compared to 81 percent for 1972-1981 and 85 percent for 1982-1991. Despite the apparently large number of observations within each of the four sub-industries for the sample as a whole, for our purposes it was necessary to aggregate across the sub-industries to obtain sufficient numbers of observations for various categories (e.g., recipient of JDB credit in the period 1963-1971). In cases where it was possible to test for behavioral differences across sub-industries, we found that our results were not very sensitive to the inclusion or exclusion of a particular sub-industry.

As shown in Tables 1-3, Machine tool producers in the various sub-industries experienced similar trends and cycles over the period 1963-1991, as is evident in their average investment rates, earnings-to-capital ratios, and sales-to-capital ratios. For industries 25, 27, and 31, the sales-to-capital ratio peaks in 1970, then rises to a second smaller peak in 1981. For these same industries, earnings-to-capital follows the same time path, while investment-to-capital peaks in 1970 and rises only slightly in the early 1980s. For industry 29, the sales-to-capital and

earnings-to-capital peaks come in 1967-1968, with smaller significant rises in 1974 and 1977; investment-to-capital peaks in 1968 and rebounds in 1974 before continuing its declining trend. For all three variables and all four industries, the overall trends are declining after the early 1970s.

Machine Tool Industry Credit Trends

As Table 4 shows, sub-industries varied in the relative importance of long-term credit and its components, but the sub-industry averages were similar, and the trends broadly the same. Figures 1-8 plot the annual average for our sample of the investment-to-capital ratio from 1964 to 1991 against various debt-to-capital ratios. Debt-to-capital ratios are defined for total debt, government debt, and private long-term and short-term debt, and for the principal components of these categories. In our discussion we emphasize patterns in long-term credit (debts with greater than one year maturity), which one would expect to be closely linked to the financing of fixed capital.

Mirroring the downward trend of investment, the trends in credit and its components are declining over the sample period. From 1965 to 1974, investment divided by capital averaged 27 percent; from 1975 to 1991, it averaged 10 percent. Over time there is also a decline in the ratio of long-term debt relative to capital. Total long-term credit relative to capital fell from an average of 41 percent prior to 1975 to 26 percent afterwards. This likely reflects, in part, the maturing of the industry and the substitution of internal funds for debt -- operating income over capital fell over time as well, but by far less (from an average of 19.5 percent for the period 1965-1974 to an average of 18 percent); thus the slower capital accumulation of the later sub-period could be more easily financed without resort to debt.

Directed credit was a small proportion of total long-term credit. Total government-agency credit fell from an average 3 percent of capital prior to 1978 to one percent after the mid-1980s. The percentage decline in total government credit relative to capital was similar to the decline in total private credit relative to capital. The decline in government credit relative to capital reflected both a decline in the percentage of firms receiving assistance, and in the government debt-to-capital ratios of borrowers, except in industry 25 where the decline was almost entirely due to the declining fraction of firms receiving government credit. Overall, for 1963-1971 79 percent of firm-year observations showed positive government credit, which fell to 77 percent for 1972-1981, and 50 percent for 1982-1991.

Within the category of government credit, the relative roles of the principal creditors also changed over time. Relative to one another, the role of the Export-Import Bank (XMB) waxed as that of the JDB waned. The percentage of firm-years with positive JDB credit declined markedly from 39 percent in the period 1963-1971, to 24 percent in the period 1972-1981, and 12 percent in the period 1982-1991.[10] For recipients of JDB credit, the ratio of JDB credit to capital fell from 4.1 for 1963-1971 to 2.5 percent for 1972-1981 and 1.6 for 1982-1991. The Export-Import bank maintained positive balances for 6, 15, and 7 percent of the observations, for the three consecutive sub-periods, respectively. Not only did the percentage of XMB

borrowing relationships rise over time relative to the JDB's, the ratio of XMB credit to capital (for firms borrowing from the Export-Import Bank) rose from 1.6 percent for 1963-1971 to 3.4 percent for 1972-1981 and 3.1 percent for 1982-1991.

One interesting common feature of all long-term credit providers, including the JDB, the XMB, and private long-term creditors -- notably the Industrial Bank of Japan (IBJ) and the Long-Term Credit Bank (LTCB) -- is the relative importance of the same small number of years for originating long-term borrowing relationships. We define "spells" of borrowing from a particular lender as episodes in which the balance owed goes from zero to positive and then returns to zero. By this definition, the six years from 1970 to 1975 account for 192 of the 438 spell beginnings for long-term lending in our dataset (from 1964 to 1991) for the aggregate of JDB, XMB, IBJ, and LTCB spells. The proportion of spells begun in those years is roughly the same for each of the four major long-term creditors. From the perspective of the history of the industry, this coincided with a time of rapid growth and investment which began in 1968.

An earlier period of rapid investment, prior to 1966, also coincided with a disproportionate number of spell beginnings for the JDB, IBJ, LTCB, and (to a lesser extent) the XMB. During 1964 and 1965 alone, 28 of the JDB's 112 spells began, compared to 31 of 131 for the IBJ, 20 of 115 for the LTCB, and 5 of 80 for the XMB.

The amount of long-term credit and the timing of long-term credit spell beginnings shows a clear link between fixed capital investment and long-term credit, both public and private. These data also show that, in terms of the timing of long-term credit supply, the principal government and private providers were quite similar. These patterns are especially visible in the lending by the JDB, IBJ, and LTCB. It is worth emphasizing that these data indicate no long-term "capture" of government funds by the machine tool industry. The timing and changing degree of government credit were closely related to economic fundamentals in the industry, and mirrored patterns of private credit suppliers.

Despite the common secular movements of long-term credit and investment, from year to year there is little apparent association between the average investment ratio and any of the government debt ratio averages. This does not imply, however, that variation in the supply of government credit was unimportant, for four reasons. First, these ratios are not time series aggregates; they are simple averages across firms, and are not weighted by the size of firms. If government credit were targeted to a few large firms, concentrated assistance to these firms could conceivably have a large effect on investment for the sector as a whole. Second, because government credit was always a small component of total credit, total investment for the industry as a whole varied largely independently of government credit. Nevertheless, government credit supplied at crucial moments to individual firms might have had important effects in stimulating new technologies or promoting consolidations which had lasting importance. Third, the number of firms for which we have usable data increases over time, as noted above; variation in the time series behavior of the averages may reflect changes in the characteristics of firms that populate the sample. Fourth, government credit supply may respond with a variable and distributed lag to newly demonstrated opportunities at the industry or firm level. There is no reason to presume

that effective directed credit must precede or coincide with moments of greatest increase in private investment (in aggregate or at the firm level). For example, directed credit may simply help to speed implementation of an investment plan already under way.[11] Clearly, there are limitations to what can be learned from aggregate statistics. We now turn to firm level data to take a closer look at the nature of the borrowing relationship between government lenders and firms, at the economic determinants of access to credit, and at the consequences of having received directed credit.

Frequency and Duration of Government Credit "Spells"

Here we review statistics on the frequency and duration of government credit spells for firms that received directed credit from the Japan Development Bank or the Export-Import Bank during our sample period. We compute the duration of credit spells without including "truncated" spells, for which beginning and end dates are unknown -- those which begin before 1964 and end after 1991.

Most firms that received credit from the JDB or the XMB only received it once. The mean number of credit spells for JDB or XMB borrowing are 1.19 and 1.21. For JDB borrowers, 80.7 percent received only one spell, 18.8 percent received two spells, and 0.6 percent received three spells. For XMB borrowers, 81.6 percent received one spell, 15.8 percent received two spells, and 2.6 percent received three spells. These data are similar to those for private long-term credit spells from the IBJ and LTCB. For these lenders, the number of spells averaged 1.12 and 1.15; the frequency of one, two, and three spells were 88.9, 10.6, and 0.6 percent for the IBJ, and 86.8, 11.2, and 2.0 percent for the LTCB.

The duration of 63 non-truncated credit spells for the JDB averaged 7.6 years, with a standard deviation of 4.1 years. The median spell length was 7 years, the maximum and minimum spell lengths were 17 years and one year, and the 25th and 75th percentiles of the distribution were 5 and 10 year spells. For the XMB, 66 non-truncated credit spells averaged 6.2 years, with a standard deviation of 3.2 years. The median spell length was 6 years, the maximum and minimum were 17 years and one year, and the 25th and 75th percentiles of the distribution were 4 and 8 years.

The durations of spells for the IBJ and LTCB were similar on average to those for the XMB and JDB, but the private long-term bank spell duration distributions were more skewed. For the IBJ, the mean duration of 58 non-truncated spells was 7.1 years, with a standard deviation of 5.5 years. The median was 5 years, the maximum and minimum were 24 years and one year, and the 25th and 75th percentiles were 4 and 10 years. The mean for 65 non-truncated LTCB spells was 7.2 years, with a standard deviation of 4.5 years. The median was 6 years, the maximum and minimum were 19 years and one year, and the 25th and 75th percentiles were 3 and 11 years.

We also calculated statistics for "joint" directed-credit spells involving borrowing from one or both of the two main government agencies. For these calculations, a spell is defined as

beginning with the first positive loan balance from either bank, and ending with both banks returning to a zero balance. The mean number of spells for firms receiving directed credit from one or both intermediaries was 1.3. 72.8 percent of firms received only one spell, 25.7 received two spells, and 1.6 percent received three. The duration of 91 non-truncated spells averaged 7.3 years, with a standard deviation of 4.2 years. The median spell length was 7 years, the maximum and minimum spell lengths were 17 and one year, and the 25th and 75th percentiles were 4 and 10 years. The close correspondence between the data on the frequency and duration of directed-credit spells for the JDB or XMB alone, and the frequency and duration of joint spells, indicates that firms receiving credit from both lenders typically received that credit within a single spell, and that the interaction of the two lenders did not lead to a significant prolonging of the duration of spells.

In our previous discussion of sectoral trends in directed credit, we showed that there was no long-term "capture" of government funds by machine tool producers as an industry. The data on the frequency and duration of spells show that this conclusion also holds at the level of individual firms' access to government funds. Directed credit (whether by the JDB, the XMB, or both) was usually provided to a firm only once and it lasted for a brief period. Relative to the behavior of private long-term creditors, the XMB and JDB did not tend to lend to the same firm more frequently or for longer periods. To the extent to which the JDB and XMB lent to the same firm, that cooperation did not tend to have a large impact on the duration or frequency of directed-credit spells.

It is important to note that the exclusion of truncated spells for computing the duration of borrowing spells biases all the estimated duration lengths downward substantially. Including truncated spells (which were typically much longer than non-truncated spells) raises the average and median spell lengths for all intermediaries, as well as the standard deviations of spell lengths. Including truncated spells, however, does not substantially change the data for directed credit duration reported above. Moreover, truncated spells of the IBJ and LTCB were much longer on average than those for the JDB and XMB, and much more common. Thus, adding truncated spells to our comparisons simply strengthens our conclusion that directed-credit spells were not long in comparison to private long-term credit spells.

Who Receives Government Credit?

In this section, we examine the quantifiable characteristics of firms that received directed credit, based on data from balance sheets and income statements. While we will argue that it is difficult to derive conclusive interpretations about the goals of government policy from probit estimates of recipient characteristics, our results have two broad applications. First, by establishing the characteristics of recipients of directed credit, we provide basic quantitative facts which must be reconciled with qualitative interpretations of the intentions of directed credit policy, whether sanguine or jaundiced. Second, we are able to examine Sato's (1990) proposition that government creditors provide insurance to private creditors by being willing to bail out private creditors of firms which have borrowed from government creditors. Our probit evidence is inconsistent with that proposition, at least as applied to the machine tool industry.

We examine the question of who receives government credit by asking whether potentially interesting balance sheet and income statement measures (1) preceded the onset of a directed-credit spell; (2) preceded observations of positive outstanding balances of government credit; and (3) preceded decreases or increases in directed credit (using an ordered probit model). In answering each of these questions, where possible we examined the sensitivity of our results to specific periods and specific intermediaries by dividing our sample into three sub-periods (1963-1971, 1972-1981, 1982-1991), and by investigating differences among the JDB, the XMB, and other government creditors in their selection criteria. Throughout, we use the IBJ and LTCB as benchmarks for comparison and contrast, to highlight features of directed credit targeting that differed from private long-term credit targeting.

Candidate measures of firm characteristics we considered include: firm size (the natural log of sales), the investment rate (fixed investment divided by fixed capital), the ratio of sales to fixed capital, the ratio of operating income to fixed capital, and the growth rate of sales (the log difference of sales). In all the probits, these variables are lagged relative to the period for which dependent variables are defined.

Given the descriptions of the use of directed credit to machine tool producers to promote technological innovation, we wanted to include expenditures on research and development in our measures of firm characteristics, but we found that the data reported by firms on this variable were not reliable. In many cases, firms reported zero expenditures on research and development, and the pattern of these zero observations over time suggests that most of the variation observed within the panel is the result of changes in accounting over time. It seems that only by the mid-1980s did most firms make a serious attempt to report R&D expenditures. Reported R&D expenditures are zero for all firms until 1969. Reported R&D expenditures in aggregate tripled from 1974 to 1980, doubled from 1980 to 1983, then doubled again from 1983 to 1985. Clearly, given the importance of R&D for machine tool producers prior to the 1980s, these data reflect improved reporting practices over time rather than changes in economic fundamentals. Regrettably, this means the data cannot be used as consistent measures for our panel. Judging from the rate of change of aggregate expenditures, reporting changes dominate the variation within the panel until the period after 1986.

Economic interpretations of significant positive or negative effects from each of our five measures of firm characteristics are not straightforward. For example, large firm size could be interpreted (from a sanguine perspective) as evidence that government credit was used to promote efficient consolidation, economies of scale, and technological externalities in machine tool production (which was exactly what the government explicitly claimed it was doing). Alternatively, (from a jaundiced perspective) large firm size could be viewed as a measure of the political influence of the borrower, or an indicator of excessively conservative bureaucratic behavior (lend only to well-established, low-risk firms whose prospects are well known).

The same difficulties arise with each of the other four measures. A high sales growth rate or high investment rate can be viewed either as a measure of a firm's progressive technology, or of it's low credit risk. High earnings may measure high private returns to

investment$_t$ (which may be positively correlated with public returns to investment), or alternatively, may measure low risk to the lender. A low sales-to-capital ratio may indicate a more capital-intensive production process, or a more mature stage of growth (after the firm has had a chance to catch up to increasing sales).

Even if the economic meaning of these five variables were unambiguous, other considerations make it difficult to evaluate policy on the basis of the observed characteristics of borrowers. Do these measures indicate "demand-side" characteristics of directed-credit firms (high sales growth implies high demand for funds from all sources), or do they reflect government decisions about which firms should receive funds? Would a wise and beneficent government lender trying to spur consolidation lend to firms at the beginning of the process or after the firms have demonstrated a commitment to it by devoting some of their own resources to the strategy?

Rather than agonize over every possible interpretation of the various probit specifications, our strategy in reporting our results will be to summarize them briefly, discuss their robustness across lenders and over time, and emphasize conclusions that can be drawn most easily from the results.

Our first set of results address the question of what firm characteristics are associated with the onset of a credit spell. We will refer to these probits as the START regressions. For these regressions, our sample excludes all "left-truncated" firms -- those that began the sample period with positive balances of credit from the relevant government lender. Obviously, for these firms, we cannot analyze the factors associated with the beginning of a government credit spell. To economize on language, here and elsewhere, we will refer to results with levels of significance of 5 percent or less without any qualification; coefficients with significance levels of between 5 and 20 percent will be referred to as weak; other effects will be described as "zero." Details are presented in the tables.

Our data on outstanding loan balances of firms to individual intermediaries for the period prior to 1982 were collected by hand. To make this task manageable, we confined ourselves to collecting data on only four intermediaries -- the JDB, XMB, IBJ, and LTCB. It is important to note that the number of JDB and XMB spells used in the probits is much smaller than the number of actual spells. Only 50 JDB spells and 60 XMB spells were used; outlier rules for discarding observations led to the elimination of nearly half the directed-credit spells. Clearly, this was not a random phenomenon. One possibility is that unusual events (like mergers or acquisitions) associated with dramatic changes in balance sheet or income measures tended to precede government credit spells. This is an extremely interesting possibility, but one which our current data source does not allow us to examine. The reduction in usable observations of spells made it impossible to estimate START regressions for the JDB and XMB for the three sub-periods separately.

Table 5 reports START probits for our entire sample period for each of the four intermediaries we analyze separately, and for "JD-XM" spells -- those involving either the JDB,

the XMB, or both. For the JDB, the one-year lag of the sales-to-capital ratio enters negatively as a predictor of spells, the one-year lag of the investment rate enters positively, and lagged sales growth and firm size both enter weakly positively. For the XMB, the results are similar, but the levels of significance vary between the two. The XMB START probits give similar weight to investment rates, less importance to the sales-to-capital ratio and sales growth rate, and more to firm size. The JD-XM START probits tend to reflect the JDB results more, with a weak negative effect from the sales-to-capital ratio, and positive effects from the investment rate and sales growth. Overall, these results indicate that growing, large, capital-intensive firms with high rates of investment tend to be the most likely recipients of directed credit.

Interestingly, the results for the IBJ and LTCB are quite different. Although these lenders also targeted investment-intensive firms with high sales growth, their new borrowers tend to be smaller firms with higher sales-to-capital ratios and lower earnings-to-capital ratios. One interpretation of these results is that IBJ and LTCB firms are at an earlier stage of growth and investment, in which capital has not yet caught up to sales and size is still small. The negative earnings effects may also indicate a greater need for credit to support investment for these borrowers.

Our next set of probits define the dependent variable as taking a value of one if the current firm-year observation shows positive outstanding credit from a particular intermediary or set of intermediaries; zero otherwise. In contrast to the START probits, the definition of the dependent variable in these HAVE probits results in many observations where the dependent variable is unity. That follows from the fact that there are more periods in which balances are positive than there are beginnings of spells, and from the fact that we are able to include "left-truncated" firms in the HAVE probits. The large number of firm-year observations for which the HAVE indicator is one allows us to measure the sensitivity of our results across our three sub-periods. Also, we are able to report HAVE probit results for government credit as a whole, in addition to results for the JDB and XMB individually.

There is a natural interpretation of differences or similarities between START and HAVE probits. To the extent the results are the same, that indicates that the identifiable characteristics of firms receiving government credit were observable prior to the receipt of government credit. To the extent the START and HAVE probits differ, that suggests that the receipt of government credit was associated with changes in the characteristics of borrowers.

Tables 6-8 report HAVE probits for the JDB, the XMB, government credit as a whole, the IBJ, and the LTCB. The HAVE probits for the JDB and XMB in Table 6 for the entire period are very similar to the START probits. The HAVE results, however, show significantly larger coefficients (in absolute value) for the negative sales-to-capital ratio effect and the positive size effect. In the case of the JDB, the positive investment rate coefficient is also much larger. Earnings-to-capital ratio coefficients (which were insignificant before) are now much larger and more significant, and of opposite sign for the JDB and XMB. On the whole, these results suggest that the characteristics that gave rise to directed credit spells may have been reinforced by the receipt of directed credit. From the standpoint of the sanguine view of directed credit,

this is consistent with the interpretation that directed credit reinforced the process of consolidation, investment, and technological change of targeted firms.

Interestingly, government credit as a whole from all sources was not significantly associated with firm size. This is not surprising, since some government credit providers targeted firms using criteria very different from the JDB or XMB. For example, directed credit for small businesses obviously would not have been available to large firms. Sub-period breakdowns for government credit, XMB credit, and JDB credit HAVE probits reported in Table 7 occasionally show significant differences in coefficient size or statistical significance. Overall, however, the strongest results for the whole period tend to be qualitatively robust to period-by-period breakdowns.

The 1980s, however, display some important differences relative to earlier periods, as shown in Table 7. The coefficient on the lagged investment rate is less positive and possibly negative, the positive sales-to-capital ratio effect is muted, and the positive sales growth coefficient is larger. These patterns are visible for all the HAVE probits for government credit for the 1980s. This suggests a possible change in government credit policy during the latter part of our sample.[12]

Results of HAVE probits for the LTCB and IBJ in Tables 6 and 8 are different in important respects from those of government providers, and the comparisons between START and HAVE probits are also quite different for the LTCB and IBJ. In general, the coefficients for the IBJ and LTCB HAVE probits are not larger in absolute value than the comparable START coefficients. Indeed, in several cases, the coefficients are significantly smaller in absolute value, or of reverse sign. Firms receiving LTCB or IBJ credit saw reductions in their sales-to-capital ratios upon receiving credit. Most interesting, however, is the reversal in the size effect. While firms receiving credit from the IBJ and LTCB were smaller than average (controlling for other effects), during their credit spells they were larger than average. These differences in coefficient sign and magnitude for size and sales-to-capital ratio effects between the START and HAVE probits lend support to the possibility that the LTCB and IBJ were willing to provide long-term credit at an earlier stage of the process of expansion/consolidation than were the JDB and XMB.

The period-by-period breakdowns for the IBJ and LTCB HAVE probits in Table 8 show substantial changes across periods in the coefficients on investment rates and sales growth. Like the results for government credit, the investment rate coefficient switches from positive to negative in the 1980s. A positive sales growth effect is largely confined to the IBJ in the period 1963-1971; otherwise sales growth effects are either zero or negative.

We also estimated, but do not report, a set of ordered probit regressions to investigate firm characteristics associated with changes in the amount of outstanding government credit qualitatively. Here the dependent variable takes a value of one if a credit balance increases, a value of zero if it remains constant, and a value of negative one if it falls. In these probits we sought to measure how fast credit responded to changes in firm characteristics, and thus included

contemporaneous observations and one, two, and three year lags of sales-to-capital ratios, earnings-to-capital ratios, and investment rates. Given the multiple sales-to-capital regressors, sales growth would have been redundant, and thus was excluded. Firm size (log sales) was also excluded for similar reasons.

For the most part, results for the ordered probits on changes in directed credit were qualitatively similar to the HAVE probits. The most interesting finding from the ordered probits for government credit was the importance of distant lags in determining increases or decreases in credit. For the JDB and XMB, the largest and most significant positive effects associated with investment rates were for lags of two and three years, and these effects tended to be larger and more significant in the first two sub-periods. Interestingly, we found that investment rate effects in ordered probits for the IBJ and LTCB also showed greatest sensitivity to the most distant lags, and this effect was concentrated in the first two sub-periods. Once again, these results are supportive of the conclusion that directed credit for machine tool producers reinforced a process of fixed capital investment that was largely prior to the receipt of government credit, although in this respect the ordered probit results provide less contrast between the behavior of private and government providers of long-term credit than the HAVE probits. Period-by-period breakdowns echo those of the HAVE probits. The 1980s showed weaker effects from lagged investment or sales-to-capital ratios than for earlier periods.

Our analysis thus far of the characteristics of firms beginning spells, receiving positive credit, or receiving changes in credit, provide interesting, if somewhat inconclusive, insights into the behavior of government intermediaries during our period, and the differences between government and private long-term credit supply. The most fundamental and clear findings are that (1) quantifiable economic characteristics of firms were associated with the likelihood of receiving government credit; (2) both in an absolute sense, and relative to the behavior of private long-term lenders, long-term directed credit tended to provide assistance only after firms had already undertaken substantial investment and consolidation; (3) directed credit policy during the 1980s was different from earlier periods, as indicated by muted or zero effects of low lagged sales-to-capital ratios or high lagged investment rates in making credit or an increase in credit more likely.

We turn now to Sato's (1990) hypothesis about the effects of directed credit on private credit risks, and its implicit assumptions about government lending policy. Sato's view is that government lenders provide implicit protection to private lenders. Once a firm has received directed credit, private lenders expect that if the firm experiences financial distress, government creditors will infuse the firm with new loans, effectively protecting private lenders.

Our earlier discussion of the descriptive statistics of government credit cast doubt on this view. We found that roughly 80 percent of firms received directed credit only once, and that the duration of credit was typically short relative to private long-term credit. At least in the case of the machine tool industry, there seems to be little evidence of the government singling out a few firms for repeated access to funds.

We can test the "credit-insurance" view of directed credit more directly by asking whether firms receiving a directed credit spell from a lender for the second or third time are forced to meet the same, or lower, standards of credit than first-time borrowers. This question is addressed in the probit results reported in Table 9. After controlling for firm characteristics (discussed above), which affect the likelihood of receiving government credit, firms that have already received financing from the JDB are less likely to receive it again from the JDB, while firms that have received credit from the XMB are equally likely to receive it again. In neither case are government lenders willing to relax credit standards for previous borrowers. Given the coefficients on the firm characteristic regressors, (1) firms experiencing declining sales or declining investment are always less likely to receive credit from the JDB or XMB, and (2) the criteria for second- and third-time borrowers are even more stringent (in the case of the JDB) or equally stringent (in the case of the XMB).

Interestingly, the cross-intermediary effects are significant and positive, both for the JDB and the XMB. For both government lenders, borrowers that have received credit previously from the other government lender are more likely to receive a new loan, all other things equal. But this is not because one intermediary is bailing out the other one's distressed firms. As shown in the JD-XM probit, if one looks at joint credit spells, the effects of previous borrowings from either intermediary are negative or zero. This is inconsistent with the "Sato conjecture," but quite consistent with what we already saw in the statistics for the frequency and duration of joint JD-XM spells. They were mainly simultaneous targetings of firms (resulting from similar targeting objectives), not sequential spells. Recall that the average length of joint non-truncated spells was 7.3 years (slightly shorter than the average length of JDB spells) and that most firms received only one joint spell.

Government Credit and Investment

In Calomiris and Himmelberg (1994), we reported preliminary findings for the period 1982-1991 of the effects of government directed credit flows on fixed investment and private long-term credit. That study found large and statistically significant positive effects from directed credit on fixed investment (in a reduced form, VAR model), and large "crowding in" of long-term private credit by government lending. Our results here share some features with those reported before, but differ in important ways.

Our definitions of fixed investment and total government and private long-term credit flows are the same as before. We define "net" fixed investment as gross fixed investment net of asset sales. Long-term private credit includes all long-term credit from commercial banks and long-term credit banks (which includes the IBJ and LTCB). All credit flow measures are defined as changes in outstanding credit, and credit flows and investment flows are all normalized by the lagged stock of fixed capital.

Our earlier study did not examine data prior to 1982, and did not distinguish effects from government credit according to the lender making the loan. The results from the VARs we report here examine the period 1963-1991 -- and the three sub-periods 1963-1971, 1972-1981,

and 1982-1991 -- and compare the effects of individual providers of government and private long-term credit.

Our earlier study used the GMM fixed-effects estimator suggested by Holtz-Eakin, Newey and Rosen (1988). This estimator was designed to take account of firm fixed effects by differencing them out of the regression analysis. Because such differencing induces correlation between the error term and the regressors (and hence biases estimated coefficients), Holtz-Eaken, Newey and Rosen (1988) suggest the use of distant lags of regressors (in levels) as instruments for the differenced regressors.

Since writing our first paper, we have changed our view of the relative merits of using the Holtz-Eakin, Newey and Rosen estimator for our purposes. Our change in method reflections econometric, as well as economic, concerns. From an econometric standpoint, Himmelberg (1994) has shown using Monte Carlo simulations that the Holtz-Eakin, Newey and Rosen estimator, while asymptotically efficient, is prone to large errors in small samples when compared to Himmelberg's alternative GMM estimator (which imposes additional restrictions on the GMM weighting matrix) or to two-stage least squares instrumenting.

From a theoretical standpoint, there are also problems with estimating the effects of instrumented government credit to infer the effects of government credit. Instrumented government credit captures only the predictable component of credit policy. One might expect, under rational expectations, that the unpredictable component of credit policy would have larger effects, since firms known to be more likely to receive government assistance may be able to attract some private credit in anticipation of government funds.

These considerations suggest that there may be large costs associated with trying to control for firm fixed effects through differencing and instrumenting. Furthermore, the benefits of controlling for fixed effects by differencing and instrumenting may be small. Lagged endogenous variables should be highly correlated with firm fixed effects, and should capture much of the influence of the fixed effect. The most important cost of not eliminating fixed effects is the inability to estimate long-term effects, because fixed effects influence estimated coefficients on lagged endogenous variables. Because fixed effects and lagged endogenous variables are positively correlated, estimated coefficients on lagged endogenous variables will be too large when the fixed effect is not eliminated, and thus will exaggerate the long-term effect on the endogenous variable of changes in the regressors.

One way to test for the potential importance of fixed effects is to compare estimated lagged endogenous coefficients between "level" and "instrumented difference" specifications. Using two-stage least squares for instrumenting, we found that fixed effects did significantly increase estimated lagged endogenous coefficients in the simple level regressions. Estimates of lagged endogenous coefficients after differencing out fixed effects were negative, ranging between zero and -0.5, while those estimated without controlling for fixed effects ranged between 0.1 and 0.8. This suggests that fixed effects are important, and that the lagged endogenous variables reported in our regression results should not be used to calculate long-term

effects. For our purposes, however, we are most interested in detecting whether there are large, significant effects among government credit, investment, and private credit; precise measures of long-term effects are less important. For these reasons, in Tables 10-15 we report results from simple panel VARs without attempting to control for fixed effects through differencing and instrumenting.

In our investment regressions, reported in Tables 10 and 11, fixed investment is the dependent variable. Dependent and independent variables are all normalized by the lagged stock of fixed capital. Independent variables include current and lagged sales and earnings, as well as a host of current and lagged credit flow measures. Credit flows are divided into three broad categories: (long-term) government credit, long-term private credit, and short-term private credit.

We report some specifications that include credit flows from individual government lenders (the JDB and XMB) and long-term private lenders (the IBJ and LTCB), as well as the three broad categories of credit flows. Earnings and sales effects are included as control variables to isolate the effects of changes in credit flows unrelated to these fundamentals. Private credit flows are included for comparison to examine whether positive effects of government credit on investment are large relative to comparable innovations in private credit. Including current as well as lagged credit flows implicitly assumes that credit flows are causally prior to investment. This is an unrealistically extreme assumption, of course, but it is a useful one for our purposes. This assumption allows us to ask whether government and private credit flows have different strengths of association with current-year and next-year investment, after controlling for earnings and sales. For example, a finding of a relatively large effect from government credit would be consistent with viewing government credit as relaxing borrowing constraints and private credit as mainly responding passively to the fundamental determinants of investment opportunities (sales, earnings, and lagged investment).

When individual intermediaries are included, their coefficients measure the extent to which that intermediary differs from other lenders in the same broad class. Thus if the lagged coefficient on broad government credit were 0.5 and the comparable coefficients on the JDB and XMB were zero that would mean that the predictive effect of government credit on investment was 0.5 for the JDB and XMB, as well as for other government lenders.

For the entire period, and each sub-period, we report two different specifications. The first (reported in Table 10) includes only the broad credit flow measures. The second (reported in Table 11) includes additional estimates of effects associated with specific intermediaries. Where necessary, we discuss (but do not report) additional regressions that involve slight changes in specification from those reported here.

The results reported in Table 10 for the entire sample period show a large positive effect on investment from government directed credit. The sum of the two coefficients is 0.6, which is much larger than the analogous coefficients for private long-term credit (0.14) or private short-term credit (0.01). The sub-period breakdowns reveal large differences in the sizes of

these coefficients and their statistical significance across periods. For two of the sub-periods -- 1963-1971 and 1982-1991 -- the sum of coefficients for government credit is much smaller. It remains larger than for private long-term credit, but only slightly, and not significantly. Indeed, from the standpoint of statistical significance, private long-term credit is clearly significantly positive, while the same cannot be said of government credit.

These findings provide no conclusive evidence of any effect of directed credit as a whole for the 1980s (given the large standard errors, the effects could be as large as 0.8 or as small as 0), but do indicate a large positive effect during the earlier two sub-periods.

The results in Table 11, which distinguish patterns for individual lenders from those of broad categories of credit, show that the declining impact of government credit was a feature of only some government credit programs. In addition to the regression coefficients reported in Table 11, Table 16 summarizes total effects by period and by government lender and reports relevant standard errors. The effect of non-JDB and non-XMB credit is large and statistically significant only in the early period. The coefficients for the JDB indicate that credit from this source (although estimated with large standard errors in the early period) had a large effect in all periods (with a point estimate of 1.5 for the effect from JDB lending for the sample as a whole). In contrast, the coefficients for the XMB indicate a net effect of roughly 0.2, which is insignificantly different from zero. The effect of non-XMB government credit, particularly when it came for the JDB, is larger than the estimated effects of private long-term credit or its components (which sum to 0.24 for the IBJ and 0.15 for the LTCB).

The differences over time in the relative magnitudes of the effects of credit from different government intermediaries warrant emphasis. For 1963-1971, the overall government credit effect is large (likely between 1 and 2), and the additional coefficients for the JDB and XMB are insignificantly different from the overall government credit effect. We conclude that government credit from many sources had similarly large estimated positive effects on investment during the first sub-period. For the second and third sub-periods, the effects of non-JDB credit are much smaller, while the effects of JDB lending remain large and statistically significant.

To sum up, a close look at the relationships between directed and private credit, on the one hand, and investment, on the other hand, confirm that some government credit had significant, positive effects on investment during the entire sample period. Credit from a variety of government agencies was responsible for the overall effect for the period 1963-1971; during the period 1972-1981, the JDB's effect was likely larger than the average effect for all government credit; and by the end of the sample period, 1982-1991, only government credit through the JDB had an effect, and that effect remained large and statistically significant.

What explains the dramatic decline in the estimated impact of non-JDB credit from the 1960s to the 1980s? One possible explanation is that the JDB was more selective than other government lenders in its targeting during the later period (when, according to the JDB, 1994, its goals shifted from targeting consolidation and rationalization of the industry to targeting high-

tech producers). According to this explanation, firms receiving credit from the JDB in the later periods had greater growth potential than the typical firm receiving credit from other government agencies. Similar reasoning can explain the importance of non-JDB, non-XMB government credit in the early sub-period. If, as is commonly alleged, the non-JDB, non-XMB government creditors were given greater responsibility during the 1960s for selective targeting of growing firms, then this could account for the greater impact of their credit flows during that period.

Government Credit and Private Credit

If government creditors financing fixed capital investment simply underbid private lenders to secure the best clients -- a strategy that might appeal to a conservative bureaucrat -- then government lending would have little beneficial effect, even though it might be associated with the large, positive predictive effects on investment found above. If government lenders were simply picking obvious winners, and if the receipt of government credit did not truly relax borrowing constraints on firms, then government credit flows should be negatively related to private credit flows. A simple test, therefore, of whether government credit is effective is to ask whether it crowds out private funds.

Tables 12 and 13 report regressions with the (normalized) flow of private long-term credit as the dependent variable. The independent variables are the same as in Tables 10 and 11, with the exception that private long-term credit and its components are no longer included. Below, we also discuss effects of government credit on short-term private credit.

For the sample period as a whole, and for specifications that only include aggregate credit categories (reported in Table 12), we find significant crowding in of private long-term credit by government credit, with a positive magnitude of roughly 0.3. This may reflect the "pump priming" effect referred to by the JDB (1994) and Horiuchi and Sui (1993), and it is inconsistent with the view that government lenders simply pick, and bid for, obvious winners. "Crowding-in" coefficients for long-term private credit by government credit are larger than the coefficients on short-term credit.

The effects of aggregate government credit (Table 12) for the first two sub-periods are similar in magnitude to that for the period as a whole, while the estimates for the period 1982-1991 indicate no crowding in or crowding out of long-term private credit. These sub-period breakdowns mirror the effects of government credit on investment shown in Table 10.

As before, allowing government credit effects to vary across lenders and sub-periods identifies interesting differences. As reported in Tables 13 and 16, non-JDB, non-XMB government credit may have crowded in long-term private credit in the early period, and by the late period, may have crowded out long-term private credit. The crowding in effects of JDB and XMB credit are relatively large and statistically significant in the 1980s. These results confirm the increase in the relative importance of the JDB over time in promoting investment. The only surprising result is that XMB credit during the 1980s appears to be a source of crowding in of private credit in Table 13, while in Table 11, loans from the XMB had no

significant effect on investment.

Tables 14 and 15 report analogous regressions to those of Tables 12 and 13, with private short-term credit taking the place of private long-term credit as the dependent variable, and private long-term credit appearing as an independent variable. As before, Table 16 summarizes the coefficient effects. Generally, we find small and statistically insignificant effects of government credit on short-term private credit. The exceptions -- both during the 1963-1971 period -- are the significant crowding in of short-term credit by the XMB, and the crowding out by non-JDB, non-XMB government credit.

Overall -- with the possible exception of the negative effects of non-JDB, non-XMB credit on long-term private credit in the 1980s, and on short-term private credit in the 1960s -- government credit tended to have either positive or zero predictive power for private credit. Where the JDB and XMB are concerned there is no evidence that their decision to target a particular firm resulted in the crowding out of private funds. The only consistent crowding in effects are related to JDB credit.

Summary of Probit and VAR Results

We have reported findings of important positive effects from government credit for investment and private credit, as well as important differences in the magnitudes of these effects across time and across sources of government credit. Explanations for observed differences have not been explored fully here, as our goal is first to summarize the evidence and point to broad conclusions.

We have argued against explanations of the positive effects of directed credit on investment that rely on government creditors simply picking winners, or on the insurance of credit risk. The lack of crowding out of private funds (at least for important categories of government directed credit) is inconsistent with a policy of simply picking winners. The probit results reported in Table 9, and the related discussion in the text, show that government creditors did not tend to bail out troubled firms which had previously borrowed from them. Firms experiencing falling sales and profits would always have been unlikely to receive government credit, and having received it before only made their chances worse.

While these arguments and findings are generally consistent with the view that government directed credit promoted investment and private funding for targeted firms, there is still an alternative explanation for our findings that we are unable to refute. It is possible that government interventions coordinated with directed credit -- at the *firm* level -- may have made directed credit to firms seem to produce growth, when in fact other interventions were responsible. Of course, interventions that affected all firms in the industry could not explain our observed cross-sectional differences; but it is possible that some important interventions by MITI were targeted to specific machine tool producers (or producers with certain common traits) who were also receiving directed credit. Ideally, we would like to be able to control for other effective subsidies (particularly, tariffs and import licenses) when measuring the effects of

directed credit, but we are currently unable to do so for lack of data.

Based on our reading of the history of industrial policy toward machine tool producers, we think that controlling for subsidies specifically directed to individual firms is unlikely to overturn our results. Import licenses granted to individual firms were important in the 1950s and early 1960s (Baily et al., 1993, JDB, 1994, pp. 190-203), but with the passing of time and the adoption of more free-market trade policies, firm-specific import licensing ceased to be a factor. For virtually all of our sample period, therefore, other firm-specific interventions seem an unlikely explanation of the apparent importance of directed credit.

There is still, however, the problem of accounting for the effects of categorical subsidies or tariffs which may have had different effects on different firms. It is possible that the same group of firms that benefitted most from a particular tariff were also targeted as recipients of credit.

While future researchers may be able to construct alternative explanations of our results, on the whole our findings provide evidence that industrial credit market interventions in the Japanese machine tool sector have accomplished the stated goals of policy. Our results on the characteristics of firms receiving credit show that firms were not able to permanently capture credit assistance, and that directed credit was associated with identifiable economic characteristics of firms. Government credit was withdrawn quickly from targeted firms, was targeted toward growing, capital-intensive firms, and was associated with substantial increases in investment and access to private credit.

VII. CONCLUSION

We have discussed the theoretical justifications for government directed credit programs for industry, and have argued that in the case of postwar Japanese machine tool producers, directed credit may have helped to promote investment. In theory, government can help to overcome problems due to free riding on investment in monitoring, or externalities in product or factor markets. In practice, important components of Japanese directed credit seem to have spurred growth. These government credit programs did not crowd out private funds, and did not succeed because they provided a permanent lifeline ("credit insurance") to firms.

Our findings may shed light on the "gross" benefits of directed credit, but we have not measured its benefits net of the opportunity costs of directing funds away from other firms, or away from consumers. We have analyzed firms within one industry group, and have not measured the social costs of depriving other sectors of funds, or of the macroeconomic policies that underlay the low cost of industrial finance more generally. Clearly, industrial policy and directed credit policies must be evaluated ultimately from a general equilibrium perspective, which is beyond the scope of this paper.

It is also important to keep in mind that industrial credit to infant industries represents only a small fraction of Japanese directed credit interventions. Other interventions to smooth

industrial decline, to sponsor public works, abate pollution, etc., may have very different social costs and benefits. Moreover, the political process that gives rise to credit support for construction, infrastructure, and public health projects may be different from, and possibly more subject to political manipulation than, industrial credit to infant industries.

For all these reasons, our arguments do not constitute an endorsement of government interventions into credit markets, in Japan or elsewhere. Moreover, it is worth noting that the effective operation of industrial directed credit in Japan seems to be an unrepresentative case. In many countries, government interventions have produced large costs through the funding of inefficient borrowers and the capture of public funds by special interests. A key feature of Japanese industrial directed credit is the institutional mechanism through which policy objectives are translated into government programs. Policy is designed to minimize problems of private crowding out, and to minimize capture of taxpayer resources by individual firms or particular industries. In Japan, the priorities of credit policy are determined as part of a national plan with broad participation (rather than by special-interest lobbying), and once industry-level priorities have been established, firm-level lending decisions by agencies are shielded from political pressures. In political systems that lack the ability to produce and implement effective plans for the distribution of industrial credit, government directed credit programs may create more problems than they solve.

Future work should build on this study, and that of Horiuchi and Sui (1993), by (1) collecting additional data on other government interventions at the firm level to measure more precisely the contribution of directed credit to targeted firms' growth, (2) investigating further the meaning of the economic characteristics of firms likely to receive credit, (3) establishing whether changes in targeting policy underlie the observed decreasing importance of non-JDB credit over our period, and (4) examining the links between the efficacy of directed credit and a firm's ability to establish strong, long-term "main bank" relationships. If the Japanese government truly has served as a vehicle for solving free rider problems among banks and firms, then, as Horiuchi and Sui (1993) argue, firms with main bank relationships should be less likely to receive credit assistance from the government, and government credit should have a smaller effect on their behavior.

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NOTES

1. This movement toward increased scale was a matter of conscious policy. As the Japan Development Bank (1994, p. 200) writes of policy circa 1965:

> The goal in upgrading plant and equipment was to establish a mass production system by expanding the size of firms and specializing production. To this end, the [machine tool] industry was designated as outside the purview of the Antimonopoly Law, emphasis was placed on such joint activities as rationalization cartels, and restrictions were imposed on product standards.

2. One can also make arguments for industrial credit assistance to mature, declining industries. Such arguments depend on macroeconomic costs of adjustment in labor markets or "coordination failure" among firms. For example, recent theoretical models (Ghemawat and Nalebuff, 1985) identify efficiency gains from coordinated capacity reductions. One can view government subsidies as a means to overcome an inefficient "prisoner's dilemma." Without coordinated reduction, firms would maintain inefficiently high capacity as part of a competitive dynamic strategy to maintain market share in a declining industry.

3. Recent financial innovations and regulatory changes (see Hoshi, Kashyap, and Scharfstein, 1990a) have enhanced the desirability of financing outside keiretsus, and adverse shocks to Japanese bank capital have reduced the advantages of membership.

4. In addition to providing assistance for growing firms, the Japanese government has provided credit to declining industries. More recently, credit has been provided as part of a government program to encourage capacity reduction. Unlike many countries, where declining industries receiving credit never seem to disappear, in Japan declining industries like coal mining have been forced to shrink in a smooth but steady manner as their workers are retrained for other occupations.

5. Horiuchi and Sui (1993) point out that, while joint loan syndications between the JDB and private banks can account for gross crowding in of some commercial bank lending, syndication, per se, does not explain net crowding in. Instead, net crowding in is associated with an increased willingness of private banks to lend to firms, conditional on their receiving government credit.

6. Weinstein and Yafeh (1994) argue that main banks do not perform the beneficial function described above, and see them instead as extortionists that extract rent from borrowers. Their evidence for this interpretation, however -- that main banks seem to charge higher interest, and that the profit rates of main bank-affiliated firms are lower on average -- could easily be interpreted as proving rather than rejecting the standard view of main banking as an effective long-term contract. If main banks relax borrowing constraints, they should allow firms to grow and possibly thereby reduce their average profitability. (Consistent with this interpretation is the fact that main bank-affiliated firms have higher capital ratios.) Moreover, if main banks are

able to resolve free rider problems, that may allow them to collect higher interest fees, the credible payment of which allows efficient intermediation, and is in the interest of the borrower as well as the banker.

7. Other important policy makers in directed credit include the Small Business Finance (SBFC) Corporation and the Bank of Japan (BOJ). Unlike the others, the BOJ does not lend directly to firms. Instead, it operates through "window" policies that encourage private banks to lend to specific industries. Window policy could be a source of measurement error of government credit in that some privately supplied credit is effectively a "pass through" from the central bank. Window guidance, however, is not a problem for the results reported below, or those found in Calomiris and Himmelberg (1994). First, window guidance was abandoned from 1982 to 1989 (Hoshi, Scharfstein, and Singleton, 1993), and our results are broadly the same for this and other periods. Second, window guidance operated at the industry level. The BOJ favored banks with loan concentrations in particular industries at particular points in time, but did not require loans to specific firms. Thus, window guidance should not matter for studies of firm-level data that control for industry and time effects.

8. Note that even in syndications, because the JDB's loan is of much longer term than those by private commercial banks (roughly 10 years versus 1-2 years), the JDB maintains "junior" status, since it is the last to be repaid. This may provide an important incentive for it to take the initiative in monitoring firms and enforcing behavior by borrowers. Thus, even when the JDB acts as the lead bank in a syndicate, it may still be effectively acting as the "initial" bank in the sense described above.

9. The research proposal also outlined an approach for testing for technological spillover effects across firms, as in Henderson (1994), to panel data. Pursuing this line of research has not been possible because, as we discuss below, Japanese data on research and development expenditure (which was to be the focus of the analysis of spillovers) seem not to be useful for performing panel data analysis. The coverage in the data is uneven across firms and across time.

10. The decline in JDB credit was especially pronounced in the early 1980s for industry 31. For that industry, from 1981 to 1986, only one firm out of 236 received JDB credit. After 1986, no sample firms in industry 31 received JDB credit.

11. This view is consistent with the probit results discussed below, which show that government credit assistance is closely associated with lagged investment rates of recipient firms.

12. As we noted before, the survivorship bias in the JDB tape may have a greater effect during the 1960s because of greater consolidation and exit during that period. One potential problem our analysis does *not* suffer from is the use of incorrect retrospective data in characterizing firms' balance sheets and income statements. As we understand the JDB's method of constructing the data, firms that merged were also merged in the data base retroactively for the years prior to the merger. To prevent this retrospective bias from influencing our results, before including firms in our sample we checked their annual reports to make sure that their actual balance sheet levels matched those in our data.

Table 1: Investment-to-Capital Ratios. By Industry over Time.

	3-Digit Industry Code			
Year	025	027	029	031
1965	0.204	0.299	0.288	0.202
1966	0.141	0.193	0.191	0.146
1967	0.189	0.242	0.282	0.180
1968	0.274	0.295	0.454	0.259
1969	0.350	0.368	0.320	0.245
1970	0.403	0.438	0.354	0.401
1971	0.347	0.298	0.291	0.367
1972	0.203	0.192	0.205	0.229
1973	0.185	0.230	0.275	0.299
1974	0.255	0.298	0.346	0.265
1975	0.232	0.167	0.170	0.205
1976	0.143	0.133	0.141	0.139
1977	0.107	0.135	0.180	0.155
1978	0.050	0.072	0.094	0.102
1979	0.055	0.089	0.097	0.085
1980	0.087	0.095	0.095	0.117
1981	0.092	0.131	0.124	0.137
1982	0.097	0.148	0.094	0.133
1983	0.080	0.120	0.096	0.085
1984	0.061	0.152	0.084	0.135
1985	0.095	0.154	0.073	0.140
1986	0.066	0.114	0.087	0.102
1987	0.043	0.092	0.070	0.080
1988	0.044	0.068	0.060	0.073
1989	0.065	0.090	0.096	0.114
1990	0.108	0.094	0.094	0.088
1991	0.116	0.114	0.104	0.105

Table 2: Operating Income-to-Capital Ratios, By Industry over Time.

	3-Digit Industry Code			
Year	025	027	029	031
1965	0.224	0.307	0.201	0.286
1966	0.172	0.261	0.204	0.224
1967	0.233	0.326	0.241	0.298
1968	0.358	0.397	0.242	0.352
1969	0.428	0.428	0.208	0.366
1970	0.415	0.423	0.201	0.430
1971	0.356	0.313	0.162	0.310
1972	0.228	0.255	0.141	0.189
1973	0.210	0.298	0.175	0.247
1974	0.276	0.335	0.191	0.309
1975	0.265	0.185	0.161	0.274
1976	0.166	0.158	0.154	0.222
1977	0.162	0.223	0.199	0.260
1978	0.129	0.198	0.147	0.236
1979	0.172	0.233	0.144	0.263
1980	0.232	0.278	0.153	0.319
1981	0.291	0.305	0.166	0.354
1982	0.251	0.275	0.127	0.220
1983	0.163	0.218	0.094	0.209
1984	0.126	0.228	0.095	0.226
1985	0.141	0.226	0.104	0.265
1986	0.094	0.148	0.083	0.172
1987	0.036	0.098	0.061	0.058
1988	0.076	0.128	0.079	0.070
1989	0.154	0.160	0.099	0.136
1990	0.186	0.166	0.111	0.112
1991	0.226	0.162	0.111	0.134

Table 3: Sales-to-Capital Ratios, By Industry over Time.

	3-Digit Industry Code			
Year	025	027	029	031
1965	2.618	3.455	2.655	2.821
1966	2.352	3.145	2.702	2.703
1967	2.687	3.593	3.069	2.872
1968	3.343	3.852	3.038	3.231
1969	3.669	4.230	2.710	3.360
1970	3.831	4.382	2.737	3.854
1971	3.621	3.869	2.629	3.510
1972	3.103	3.546	2.325	3.021
1973	3.180	3.741	2.653	3.336
1974	3.682	4.264	3.086	3.782
1975	3.693	3.673	2.716	3.777
1976	3.227	3.497	2.581	3.632
1977	3.148	3.915	2.910	3.951
1978	3.161	3.786	2.701	3.986
1979	3.366	3.888	2.761	3.883
1980	3.572	4.067	2.784	4.106
1981	3.859	4.326	2.714	4.314
1982	3.613	4.072	2.391	3.832
1983	3.264	3.780	2.192	3.402
1984	3.072	3.951	2.146	3.411
1985	3.179	3.852	2.184	3.505
1986	2.877	3.315	2.085	3.166
1987	2.492	3.056	1.875	2.705
1988	2.598	3.147	1.833	2.638
1989	2.934	3.309	1.962	2.863
1990	3.146	3.208	2.001	2.684
1991	3.159	3.223	1.981	2.833

Table 4A: Debt-to-Capital Ratio, Loans f :n Japan Development Bank.

	3-Digit Industry Code			
Year	025	027	029	031
1955	0.053	0.050	0.051	0.049
1966	0.041	0.042	0.038	0.038
1967	0.040	0.047	0.041	0.037
1968	0.042	0.041	0.067	0.039
1969	0.037	0.037	0.060	0.027
1970	0.035	0.034	0.058	0.025
1971	0.030	0.026	0.038	0.026
1972	0.028	0.020	0.034	0.013
1973	0.024	0.016	0.038	0.016
1974	0.023	0.026	0.048	0.034
1975	0.021	0.024	0.037	0.047
1976	0.024	0.022	0.031	0.015
1977	0.031	0.022	0.031	0.017
1978	0.025	0.026	0.043	0.022
1979	0.017	0.024	0.030	0.021
1980	0.021	0.021	0.027	0.026
1981	0.026	0.028	0.023	0.034
1982	0.022	0.026	0.031	0.019
1983	0.024	0.020	0.026	0.014
1984	0.018	0.017	0.023	0.007
1985	0.010	0.014	0.016	0.004
1986	0.008	0.011	0.012	0.001
1987	0.009	0.009	0.011	.
1988	0.009	0.016	0.006	.
1989	0.011	0.017	0.012	.
1990	0.013	0.019	0.011	.
1991	0.027	0.016	0.010	.

Table 4B: Debt-to-Capital Ratio. Loans from Export-Import Bank.

	3-Digit Industry Code			
Year	025	027	029	031
1965	0.009	0.018	.	.
1966	0.006	0.012	.	.
1967	0.009	0.009	.	.
1968	0.006	0.024	.	.
1969	0.004	0.025	.	.
1970	0.017	0.027	.	.
1971	0.030	0.022	.	.
1972	0.060	0.018	.	.
1973	0.040	0.020	.	.
1974	0.048	0.025	0.347	0.012
1975	0.030	0.026	0.088	0.022
1976	0.031	0.045	0.085	0.018
1977	0.036	0.040	0.085	0.020
1978	0.038	0.030	0.081	0.013
1979	0.036	0.021	0.060	0.009
1980	0.038	0.019	0.046	0.006
1981	0.059	0.023	0.013	0.004
1982	0.050	0.030	0.008	.
1983	0.046	0.026	0.033	.
1984	0.037	0.022	0.032	.
1985	0.013	0.018	0.020	.
1986	0.036	0.014	0.020	.
1987	0.113	0.012	0.014	.
1988	0.199	0.009	0.001	.
1989	0.083	0.009	0.003	.
1990	0.076	0.007	0.025	.
1991	0.072	0.009	0.025	.

Year	3-Digit Industry Code			
	025	027	029	031
1965	0.061	0.068	0.125	0.127
1966	0.072	0.075	0.096	0.112
1967	0.058	0.084	0.098	0.093
1968	0.068	0.080	0.098	0.077
1969	0.089	0.085	0.091	0.095
1970	0.078	0.078	0.078	0.208
1971	0.071	0.076	0.074	0.190
1972	0.060	0.070	0.088	0.118
1973	0.061	0.066	0.100	0.113
1974	0.079	0.071	0.075	0.134
1975	0.083	0.077	0.073	0.118
1976	0.087	0.070	0.074	0.133
1977	0.077	0.066	0.066	0.134
1978	0.073	0.060	0.059	0.120
1979	0.062	0.054	0.069	0.112
1980	0.057	0.054	0.073	0.108
1981	0.058	0.051	0.063	0.103
1982	0.066	0.041	0.067	0.106
1983	0.057	0.044	0.064	0.106
1984	0.059	0.035	0.053	0.108
1985	0.059	0.027	0.047	0.100
1986	0.048	0.024	0.042	0.079
1987	0.042	0.022	0.041	0.093
1988	0.040	0.019	0.039	0.085
1989	0.058	0.019	0.028	0.097
1990	0.044	0.018	0.026	0.086
1991	0.052	0.015	0.022	0.088

Year	3-Digit Industry Code			
	025	027	029	031
1965	0.075	0.085	0.037	0.061
1966	0.082	0.084	0.053	0.044
1967	0.074	0.069	0.041	0.032
1968	0.077	0.077	0.080	0.030
1969	0.072	0.079	0.074	0.048
1970	0.076	0.077	0.075	0.070
1971	0.064	0.069	0.073	0.061
1972	0.060	0.070	0.062	0.053
1973	0.053	0.067	0.065	0.034
1974	0.050	0.066	0.076	0.043
1975	0.048	0.066	0.082	0.057
1976	0.055	0.060	0.068	0.063
1977	0.054	0.059	0.061	0.074
1978	0.051	0.055	0.055	0.065
1979	0.043	0.059	0.074	0.058
1980	0.043	0.055	0.060	0.062
1981	0.043	0.053	0.073	0.045
1982	0.048	0.048	0.069	0.055
1983	0.042	0.053	0.070	0.041
1984	0.042	0.027	0.056	0.015
1985	0.041	0.030	0.018	0.011
1986	0.043	0.042	0.017	0.019
1987	0.045	0.042	0.022	0.016
1988	0.068	0.034	0.109	0.016
1989	0.039	0.032	0.106	0.014
1990	0.033	0.019	0.077	0.012
1991	0.034	0.017	0.070	0.009

Table 5: "Start" Probits, by Bank.

	Bank				
	JDB	XMB	JDB or XMB	IBJ	LTCB
Constant	-3.015 (0.463)	-3.208 (0.449)	-2.567 (0.435)	-2.101 (0.515)	-1.189 (0.532)
$(S/K)_{t-1}$	-0.133 (0.068)	-0.029 (0.042)	-0.035 (0.041)	0.040 (0.049)	0.111 (0.031)
$(OI/K)_{t-1}$	0.523 (0.440)	-0.272 (0.412)	0.041 (0.371)	-0.725 (0.421)	-0.662 (0.378)
$(I/K)_{t-1}$	1.250 (0.340)	0.976 (0.346)	0.923 (0.314)	0.981 (0.359)	0.774 (0.350)
$\%\Delta S_t$	0.190 (0.452)	0.845 (0.402)	0.532 (0.383)	0.447 (0.404)	0.832 (0.388)
$\log(S_t)$	0.065 (0.042)	0.071 (0.040)	0.013 (0.040)	-0.056 (0.050)	-0.181 (0.056)
loglikelihood	-206.302	-232.145	-256.860	-188.173	-190.637

Footnotes: Standard errors appear in parenthesis. Variable abbreviations: (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (NI/K) = the ratio of net investment to capital; %Δ(Sales) = the growth rate of sales; log(Sales) = the log of sales.

Table 6: "Have" Probits -- Full Time Period, 1963-1991.

	Bank Lender				
Regressors	JDB	XMB	All Gov't	IBJ	LTCB
Constant	-1.649	-5.542	-2.008	-1.879	-2.554
	(0.159)	(0.217)	(0.256)	(0.144)	(0.149)
(S/K)	-0.259	-0.083	-0.321	-0.053	0.066
	(0.020)	(0.021)	(0.029)	(0.013)	(0.013)
(OI/K)	0.280	-0.564	0.318	0.308	-0.866
	(0.164)	(0.219)	(0.249)	(0.131)	(0.138)
(NI/K)	1.875	0.941	1.476	0.219	0.842
	(0.154)	(0.204)	(0.226)	(0.139)	(0.143)
%Δ(Sales)	0.438	0.435	0.031	0.806	0.348
	(0.153)	(0.213)	(0.201)	(0.135)	(0.141)
log(Sales)	0.132	0.428	0.223	0.175	0.185
	(0.015)	(0.019)	(0.026)	(0.014)	(0.014)
Total Obs.	4451	4451	4451	4451	4451
# Obs=1	1048	491	2985	1894	1427
Log Likelihood	-2206	-1244	-2734	-2947	-2667

Footnotes: Standard errors appear in parentheses. Variable abbreviations: (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (NI/K) = the ratio of net investment to capital; %Δ(Sales) = the growth rate of sales; log(Sales) = the log of sales.

Table 7a: "Have" Probits -- Japan Development Bank, by Time Period.

	Time Period			
Regressors	Full Sampie	63-71	72-81	82-91
Constant	-1.649	-1.266	-2.008	-2.755
	(0.159)	(0.461)	(0.256)	(0.258)
(S/K)	-0.259	-0.325	-0.321	-0.198
	(0.020)	(0.058)	(0.029)	(0.031)
(OI/K)	0.280	-0.338	0.318	0.185
	(0.164)	(0.422)	(0.249)	(0.287)
(NI/K)	1.875	1.696	1.476	0.376
	(0.154)	(0.348)	(0.226)	(0.390)
%Δ(Sales)	0.438	0.662	0.031	0.380
	(0.153)	(0.479)	(0.201)	(0.307)
log(Sales)	0.132	0.163	0.223	0.208
	(0.015)	(0.050)	(0.026)	(0.024)
Total Obs.	4451	474	1842	2135
# Obs=1	1048	185	542	321
Log Likelihood	-2206	-276	-985	-840

Footnotes: Standard errors appear in parentheses. Variable abbreviations: (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (NI/K) = the ratio of net investment to capital; %Δ(Sales) = the growth rate of sales; log(Sales) = the log of sales.

Table 7b: "Have" Probits -- Export-Import Bank, by Time Period.

Regressors	Time Period			
	Full Sample	63-71	72-81	82-91
Constant	-5.542	-6.436	-5.711	-7.757
	(0.218)	(0.794)	(0.326)	(0.427)
(S/K)	-0.084	-0.114	-0.121	-0.087
	(0.021)	(0.093)	(0.030)	(0.038)
(OI/K)	-0.563	-2.004	-0.745	-0.656
	(0.219)	(0.894)	(0.298)	(0.459)
(NI/K)	0.941	0.423	0.433	-0.771
	(0.204)	(0.582)	(0.285)	(0.646)
%Δ(Sales)	0.435	0.162	0.183	0.368
	(0.213)	(0.880)	(0.256)	(0.502)
log(Sales)	0.428	0.609	0.511	0.596
	(0.019)	(0.081)	(0.032)	(0.037)
Total Obs.	4451	474	1842	2135
# Obs=1	491	38	299	154
Log Likelihood	-1244	-89	-651	-367

Footnotes: Standard errors appear in parentheses. Variable abbreviations: (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (NI/K) = the ratio of net investment to capital; %Δ(Sales) = the growth rate of sales; log(Sales) = the log of sales.

Table 7c: Directed Credit "Have" Probits -- All Government Banks, by Time Period.

	Time Period			
Regressors	Full Sample	63-71	72-81	82-91
Constant	-0.001	-2.252	-0.527	-1.737
	(0.146)	(0.610)	(0.276)	(0.213)
(S/K)	-0.056	-0.167	-0.121	-0.027
	(0.013)	(0.048)	(0.022)	(0.018)
(OI/K)	-0.681	0.038	-1.082	-0.824
	(0.133)	(0.421)	(0.213)	(0.204)
(NI/K)	1.553	0.842	1.348	-0.105
	(0.158)	(0.381)	(0.261)	(0.295)
%Δ(Sales)	0.540	0.904	-0.051	0.537
	(0.140)	(0.552)	(0.210)	(0.231)
log(Sales)	0.049	0.363	0.163	0.130
	(0.014)	(0.072)	(0.029)	(0.020)
Total Obs.	4451	474	1842	2135
# Obs=1	2985	379	1469	1137
Log Likelihood	-2733	-210	-860	-1441

Footnotes: Standard errors appear in parentheses. Variable abbreviations: (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (NI/K) = the ratio of net investment to capital; %Δ(Sales) = the growth rate of sales; log(Sales) = the log of sales.

Table 8a: Directed Credit "Have" Probits -- Industrial Bank of Japan, by Time Period.

	Time Period			
Regressors	Full Sample	63-71	72-81	82-91
Constant	-1.879	-2.584	-2.511	-2.051
	(0.144)	(0.460)	(0.245)	(0.219)
(S/K)	-0.052	-0.142	-0.070	-0.029
	(0.013)	(0.045)	(0.020)	(0.018)
(OI/K)	0.308	0.512	0.276	0.166
	(0.131)	(0.385)	(0.198)	(0.208)
(NI/K)	0.219	0.049	0.030	-0.882
	(0.140)	(0.315)	(0.212)	(0.313)
%Δ(Sales)	0.086	0.353	-0.352	0.079
	(0.134)	(0.462)	(0.183)	(0.237)
log(Sales)	0.174	0.264	0.270	0.181
	(0.014)	(0.051)	(0.025)	(0.021)
Total Obs.	4451	474	1842	2135
# Obs=1	1894	211	863	820
Log Likelihood	-2948	-300	-1206	-1381

Footnotes: Standard errors appear in parentheses. Variable abbreviations: (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (NI/K) = the ratio of net investment to capital; %Δ(Sales) = the growth rate of sales; log(Sales) = the log of sales.

Table 8b: Directed Credit "Have" Probits -- Long Term Credit Bank of Japan, by Time Period.

	Time Period			
Regressors	Full Sample	63-71	72-81	82-91
Constant	-2.554	-4.648	-3.388	-2.566
	(0.149)	(0.535)	(0.251)	(0.230)
(S/K)	0.066	-0.051	0.040	0.108
	(0.013)	(0.049)	(0.020)	(0.019)
(OI/K)	-0.866	-1.404	-0.975	-0.930
	(0.138)	(0.435)	(0.205)	(0.229)
(NI/K)	0.842	0.218	0.615	-0.336
	(0.143)	(0.342)	(0.215)	(0.327)
%Δ(Sales)	0.348	0.362	0.152	0.024
	(0.141)	(0.493)	(0.188)	(0.254)
log(Sales)	0.184	0.529	0.307	0.166
	(0.014)	(0.060)	(0.025)	(0.022)
Total Obs.	4451	474	1842	2135
# Obs=1	1427	172	703	552
Log Likelihood	-2667	-254	-1133	-1170

Footnotes: Standard errors appear in parentheses. Variable abbreviations: (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (NI/K) = the ratio of net investment to capital; %Δ(Sales) = the growth rate of sales; log(Sales) = the log of sales.

Table 9: "Initial Loan" Probits. Conditional on Previous Access to Government Credit

	Bank		
Regressors	JDB	XMB	JDB or XMB
Constant	-2.630	-3.291	-2.959
	(0.476)	(0.436)	(0.418)
(S/K)	-0.113	-0.027	-0.076
	(0.048)	(0.039)	(0.038)
(OI/K)	0.211	-0.028	0.162
	(0.352)	(0.341)	(0.309)
(NI/K)	1.039	0.825	0.713
	(0.282)	(0.276)	(0.262)
%Δ(Sales)	0.645	0.375	0.682
	(0.348)	(0.324)	(0.300)
log(Sales)	0.036	0.085	0.056
	(0.041)	(0.037)	(0.056)
Previous JDB Loan Dummy	-0.168	0.209	-0.135
	(0.114)	(0.114)	(0.099)
Previous XMB Loan Dummy	0.241	-0.092	-0.111
	(0.142)	(0.138)	(0.139)
Total Obs.	5999	5939	5999
# Obs=1	50	60	69
Log Likelihood	-272	-323	-364

Footnotes: Standard errors appear in parentheses. Variable abbreviations: (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (NI/K) = the ratio of net investment to capital; %Δ(Sales) = the growth rate of sales; log(Sales) = the log of sales.

Table 10: Investment Regressions. Directed Credit Aggregated over All Lenders.

Regressors	Time Period			
	Full Sample	63-71	72-81	82-91
$(NI/K)_{-1}$	0.376	0.476	0.342	0.324
	(0.015)	(0.050)	(0.022)	(0.023)
(S/K)	0.041	0.067	0.032	0.038
	(0.004)	(0.022)	(0.006)	(0.005)
$(S/K)_{-1}$	-0.043	-0.077	-0.034	-0.039
	(0.004)	(0.023)	(0.006)	(0.005)
(OI/K)	0.102	0.193	0.124	0.027
	(0.018)	(0.010)	(0.025)	(0.026)
$(OI/K)_{-1}$	0.081	0.059	0.052	0.142
	(0.019)	(0.107)	(0.025)	(0.024)
(SF/K)	0.055	0.039	0.051	0.058
	(0.013)	(0.067)	(0.018)	(0.017)
$(SF/K)_{-1}$	-0.043	0.001	-0.058	-0.031
	(0.013)	(0.064)	(0.018)	(0.017)
(PF/K)	0.160	0.289	0.152	0.085
	(0.017)	(0.071)	(0.024)	(0.024)
$(PF/K)_{-1}$	-0.022	-0.020	-0.008	-0.040
	(0.017)	(0.080)	(0.023)	(0.025)
(GF/K)	0.915	1.090	0.988	0.303
	(0.122)	(0.439)	(0.150)	(0.233)
$(GF/K)_{-1}$	-0.307	-0.074	-0.393	-0.249
	(0.123)	(0.431)	(0.164)	(0.201)
R^2	0.261	0.323	0.277	0.201
Obs	4470	477	1841	2150

Footnotes: Year dummies included but not reported. Standard errors appear in parentheses. Variable abbreviations: (NI/K) = the ratio of net investment to capital; (S/K) = the ratio of sales to capital; (OI/K) = the ratio of operating income to capital; (SF/K) = the ratio of short term bank financing to capital; (PF/K) = the ratio of private long term bank financing to capital; (GF/K) = the ratio of government long term bank financing to capital; (IF/K) = the ration of IBJ financing to capital; (LF/K) = the ratio of LTCB financing to capital; (JF/K) = the ratio of JDB financing to capital; (XF/K) = the ratio of XMB financing to capital.

Table 11: Investment Regressions. Directed Credit Disaggregated by Lender.

Regressors	Time Period			
	Full Sample	63-71	72-81	82-91
$(NI/K)_{-1}$	0.375	0.460	0.345	0.328
	(0.015)	(0.051)	(0.022)	(0.023)
(S/K)	0.041	0.065	0.033	0.039
	(0.004)	(0.022)	(0.006)	(0.005)
$(S/K)_{-1}$	-0.043	-0.074	-0.034	-0.040
	(0.004)	(0.023)	(0.006)	(0.005)
(OI/K)	0.102	0.216	0.120	0.026
	(0.019)	(0.101)	(0.025)	(0.026)
$(OI/K)_{-1}$	0.081	0.029	0.053	0.141
	(0.019)	(0.108)	(0.025)	(0.024)
(SF/K)	0.055	0.048	0.050	0.056
	(0.013)	(0.684)	(0.018)	(0.017)
$(SF/K)_{-1}$	-0.043	0.013	-0.057	-0.033
	(0.013)	(0.065)	(0.018)	(0.017)
(PF/K)	0.146	0.222	0.160	0.083
	(0.019)	(0.078)	(0.025)	(0.027)
$(PF/K)_{-1}$	-0.021	-0.024	0.007	-0.053
	(0.018)	(0.086)	(0.024)	(0.027)
(GF/K)	0.927	1.565	1.199	-0.087
	(0.207)	(0.784)	(0.274)	(0.320)
$(GF/K)_{-1}$	-0.248	0.796	-0.700	-0.351
	(0.205)	(0.739)	(0.280)	(0.311)
(IF/K)	0.208	0.594	-0.122	0.313
	(0.096)	(0.308)	(0.126)	(0.180)
$(IF/K)_{-1}$	-0.164	-0.006	-0.292	-0.140
	(0.09[illegible])	(0.314)	(0.115)	(0.172)
(LF/K)	0.112	0.708	0.019	-0.374
	(0.122)	(0.459)	(0.149)	(0.232)
$(LF/K)_{-1}$	-0.172	-1.201	0.253	0.559
	(0.281)	(0.897)	(0.384)	(0.520)
(JF/K)	0.380	-0.703	0.481	0.864
	(0.290)	(0.967)	(0.390)	(0.546)
$(JF/K)_{-1}$	0.170	0.288	-0.104	0.768
	(0.120)	(0.464)	(0.145)	(0.230)
(XF/K)	-0.735	1.565	-1.271	0.644
	(0.329)	(1.982)	(0.385)	(0.725)
$(XF/K)_{-1}$	0.215	-4.524	1.173	-0.144
	(0.348)	(2.971)	(0.444)	(0.493)
R^2	0.263	0.327	0.288	0.207
Obs.	4470	477	1841	2150

Footnotes: Year dummies included but not reported. Standard errors appear in parentheses. For variable definitions, see footnotes to Table 10.

Table 12: Long-Term Financing Regressions (Flow of Private Bank Debt). Directed Credit Aggregated over All Lenders.

	Time Period			
Regressors	Full Sample	63-71	72-81	82-91
$(PF/K)_{-1}$	0.134	0.082	0.197	0.046
	(0.015)	(0.005)	(0.022)	(0.022)
(S/K)	-0.008	0.007	-0.008	-0.011
	(0.003)	(0.014)	(0.005)	(0.004)
$(S/K)_{-1}$	0.009	-0.009	0.012	0.011
	(0.003)	(0.014)	(0.006)	(0.004)
(OI/K)	-0.040	0.017	-0.046	-0.059
	(0.016)	(0.066)	(0.025)	(0.022)
$(OI/K)_{-1}$	0.024	0.019	-0.002	0.053
	(0.016)	(0.071)	(0.025)	(0.022)
(SF/K)	0.086	0.121	0.106	0.052
	(0.011)	(0.044)	(0.018)	(0.015)
$(SF/K)_{-1}$	0.062	0.122	0.044	0.062
	(0.011)	(0.042)	(0.018)	(0.015)
(GF/K)	0.293	0.412	0.217	0.301
	(0.105)	(0.286)	(0.148)	(0.206)
$(GF/K)_{-1}$	-0.010	-0.055	0.229	-0.322
	(0.106)	(0.284)	(0.160)	(0.178)
R^2	0.061	0.042	0.097	0.036
Obs	4470	477	1841	2150

Footnotes: Year dummies included but not reported. Standard errors appear in parentheses. For variable definitions, see footnotes to Table 10.

Table 13: Long-Term Financing Regressions (Flow of Private Bank Debt), Directed Credit Disaggregated by Lender.

	Time Period			
Regressors	Full Sample	63-71	72-81	82-91
$(PF/K)_{-1}$	0.136	0.080	0.195	0.047
	(0.015)	(0.054)	(0.022)	(0.023)
(S/K)	-0.008	0.007	-0.009	-0.011
	(0.003)	(0.014)	(0.005)	(0.004)
$(S/K)_{-1}$	0.009	-0.009	0.012	0.012
	(0.003)	(0.014)	(0.006)	(0.004)
(OI/K)	-0.040	0.018	-0.045	-0.056
	(0.016)	(0.066)	(0.025)	(0.022)
$(OI/K)_{-1}$	0.024	0.018	-0.002	0.048
	(0.016)	(0.071)	(0.025)	(0.022)
(SF/K)	0.085	0.130	0.106	0.053
	(0.011)	(0.044)	(0.018)	(0.015)
$(SF/K)_{-1}$	0.062	0.122	0.045	0.063
	(0.011)	(0.042)	(0.018)	(0.015)
(GF/K)	0.126	0.685	0.139	-0.374
	(0.179)	(0.514)	(0.273)	(0.281)
$(GF/K)_{-1}$	0.035	-0.048	0.389	-0.527
	(0.120)	(0.290)	(0.183)	(0.216)
(JF/K)	0.367	-0.261	0.186	1.442
	(0.250)	(0.635)	(0.385)	(0.481)
$(JF/K)_{-1}$	-0.056	-0.081	-0.018	-0.062
	(0.104)	(0.302)	(0.143)	(0.202)
(XF/K)	0.138	-2.174	0.140	1.535
	(0.285)	(1.297)	(0.382)	(0.639)
$(XF/K)_{-1}$	-0.241	1.879	-0.711	0.499
	(0.270)	(1.918)	(0.390)	(0.395)
R^2	0.062	0.040	0.097	0.040
Obs.	4470	477	1841	2150

Footnotes: Year dummies included but not reported. Standard errors appear in parentheses. For variable definitions, see footnotes to Table 10.

Table 14: Short-Term Financing Regressions (Flow of Private Bank Debt), Directed Credit Aggregated over All Lenders.

	Time Period			
Regressors	Full Sample	63-71	72-81	82-91
$(SF/K)_{-1}$	0.078	0.160	0.072	0.06
	(0.015)	(0.044)	(0.023)	(0.022)
(S/K)	0.022	0.045	0.012	0.027
	(0.004)	(0.014)	(0.007)	(0.006)
$(S/K)_{-1}$	-0.011	-0.038	-0.005	-0.013
	(0.004)	(0.015)	(0.007)	(0.006)
(OI/K)	-0.212	-0.286	-0.186	-0.215
	(0.021)	(0.068)	(0.032)	(0.032)
$(OI/K)_{-1}$	0.185	0.299	0.198	0.142
	(0.021)	(0.074)	(0.033)	(0.031)
(PF/K)	0.150	0.135	0.184	0.110
	(0.019)	(0.048)	(0.030)	(0.031)
$(PF/K)_{-1}$	-0.049	0.247	0.023	-0.001
	(0.020)	(0.053)	(0.029)	(0.032)
(GF/K)	-0.030	-0.302	0.029	0.158
	(0.139)	(0.302)	(0.195)	(0.299)
$(GF/K)_{-1}$	0.230	0.208	0.082	0.437
	(0.140)	(0.299)	(0.211)	(0.258)
R^2	0.072	0.140	0.075	0.057
Obs	4470	477	1841	2150

Footnotes: Year dummies included but not reported. Standard errors appear in parentheses. For variable definitions, see footnotes to Table 10.

Table 15: Short-Term Financing Regressions (Flow of Private Bank Debt), Directed Credit Disaggregated by Lender.

	Time Period			
Regressors	Full Sample	63-71	72-81	82-91
$(SF/K)_{-1}$	0.076	0.144	0.074	0.058
	(0.015)	(0.044)	(0.023)	(0.022)
(S/K)	0.022	0.041	0.013	0.029
	(0.004)	(0.014)	(0.007)	(0.006)
$(S/K)_{-1}$	-0.011	-0.034	-0.006	-0.014
	(0.004)	(0.015)	(0.007)	(0.006)
(OI/K)	-0.212	-0.283	-0.189	-0.218
	(0.021)	(0.068)	(0.032)	(0.032)
$(OI/K)_{-1}$	0.184	0.298	0.201	0.145
	(0.021)	(0.073)	(0.033)	(0.031)
(PF/K)	0.134	0.159	0.153	0.113
	(0.021)	(0.053)	(0.033)	(0.034)
$(PF/K)_{-1}$	0.029	0.219	0.017	-0.045
	(0.021)	(0.058)	(0.031)	(0.035)
(GF/K)	-0.228	-2.048	0.353	-0.021
	(0.237)	(0.531)	(0.360)	(0.411)
$(GF/K)_{-1}$	0.162	0.053	0.034	0.338
	(0.235)	(0.507)	(0.365)	(0.399)
(IF/K)	0.118	-0.055	0.239	-0.003
	(0.110)	(0.211)	(0.165)	(0.230)
$(IF/K)_{-1}$	0.240	0.308	0.008	0.685
	(0.104)	(0.214)	(0.151)	(0.221)
(LF/K)	0.289	-0.224	0.538	0.101
	(0.140)	(0.316)	(0.195)	(0.297)
$(LF/K)_{-1}$	0.161	-0.107	0.183	0.891
	(0.322)	(0.617)	(0.503)	(0.667)
(JF/K)	0.305	2.259	-0.443	0.115
	(0.333)	(0.655)	(0.510)	(0.701)
$(JF/K)_{-1}$	0.195	0.262	0.033	0.456
	(0.138)	(0.318)	(0.190)	(0.294)
(XF/K)	0.254	4.308	-0.671	0.897
	(0.377)	(1.341)	(0.505)	(0.931)
$(XF/K)_{-1}$	-0.172	0.972	0.030	-0.666
	(0.399)	(2.038)	(0.581)	(0.633)
R^2	0.075	0.166	0.076	0.062
Obs.	4470	477	1841	2150

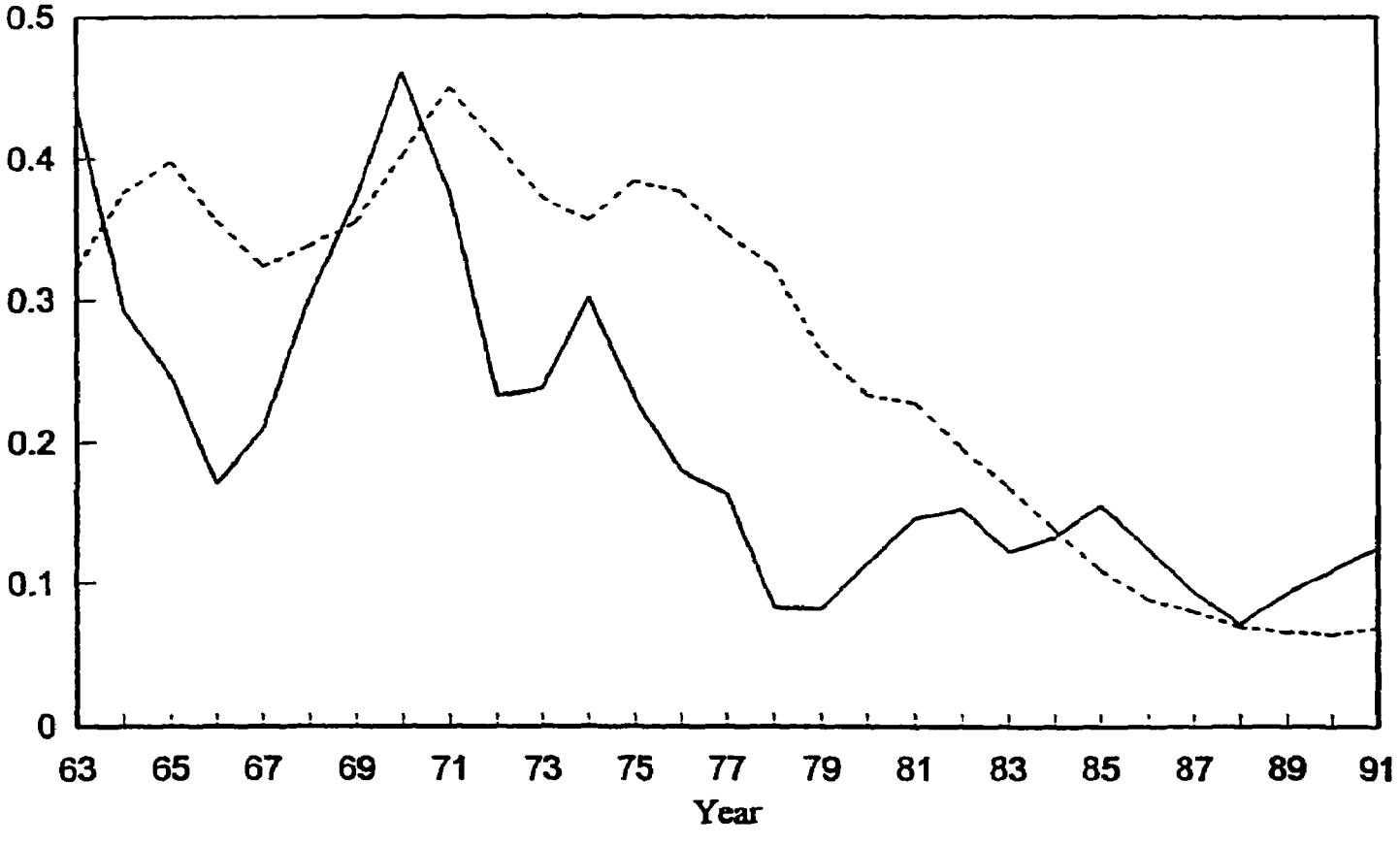
Footnotes: Year dummies included but not reported. Standard errors appear in parentheses. For variable definitions, see footnotes to Table 10.

Table 16: Summary of *Total* Directed Credit Effects, by Lender.

This table reports the *total* effect of the respective government lenders by adding the incremental effect of directed credit from the Japan Development Bank and the Export-Import Bank to the baseline "government bank" effect, and then summing this effect over both years. These estimates are taken from Tables 11, 13, and 15. Thus, for example, the entry below in the first row of the first column indicates the sum of both the current and lagged coefficients on GF/K and JF/K, and reports standard errors that account for the covariance among those estimates.

	Time Period			
	Full Sample	63-71	72-81	82-91
Investment Regressions (see Table 11)				
Japan Develop-. ment Bank	1.478 (0.231)	1.151 (0.723)	1.576 (0.298)	1.545 (0.503)
Export-Import Bank	0.159 (0.320)	-0.598 (2.422)	0.406 (0.369)	0.063 (0.653)
Other Gov't Banks	0.679 (0.292)	2.361 (1.081)	0.503 (0.383)	-0.438 (0.464)
Long-Term Financing Regressions (see Table 13)				
Japan Develop-. ment Bank	0.442 (0.197)	0.326 (0.467)	0.358 (0.292)	0.955 (0.442)
Export-Import Bank	0.058 (0.277)	0.310 (1.602)	-0.033 (0.366)	1.135 (0.577)
Other Gov't Banks	0.089 (0.252)	0.985 (0.710)	0.207 (0.380)	-0.736 (0.408)
Short-Term Financing Regressions (see Table 15)				
Japan Develop-. ment Bank	0.272 (0.264)	0.473 (0.490)	-0.057 (0.390)	0.550 (0.646)
Export-Import Bank	0.016 (0.367)	3.285 (1.659)	-0.254 (0.484)	0.548 (0.838)
Other Gov't Banks	-0.066 (0.334)	-1.995 (0.737)	0.387 (0.501)	0.317 (0.595)

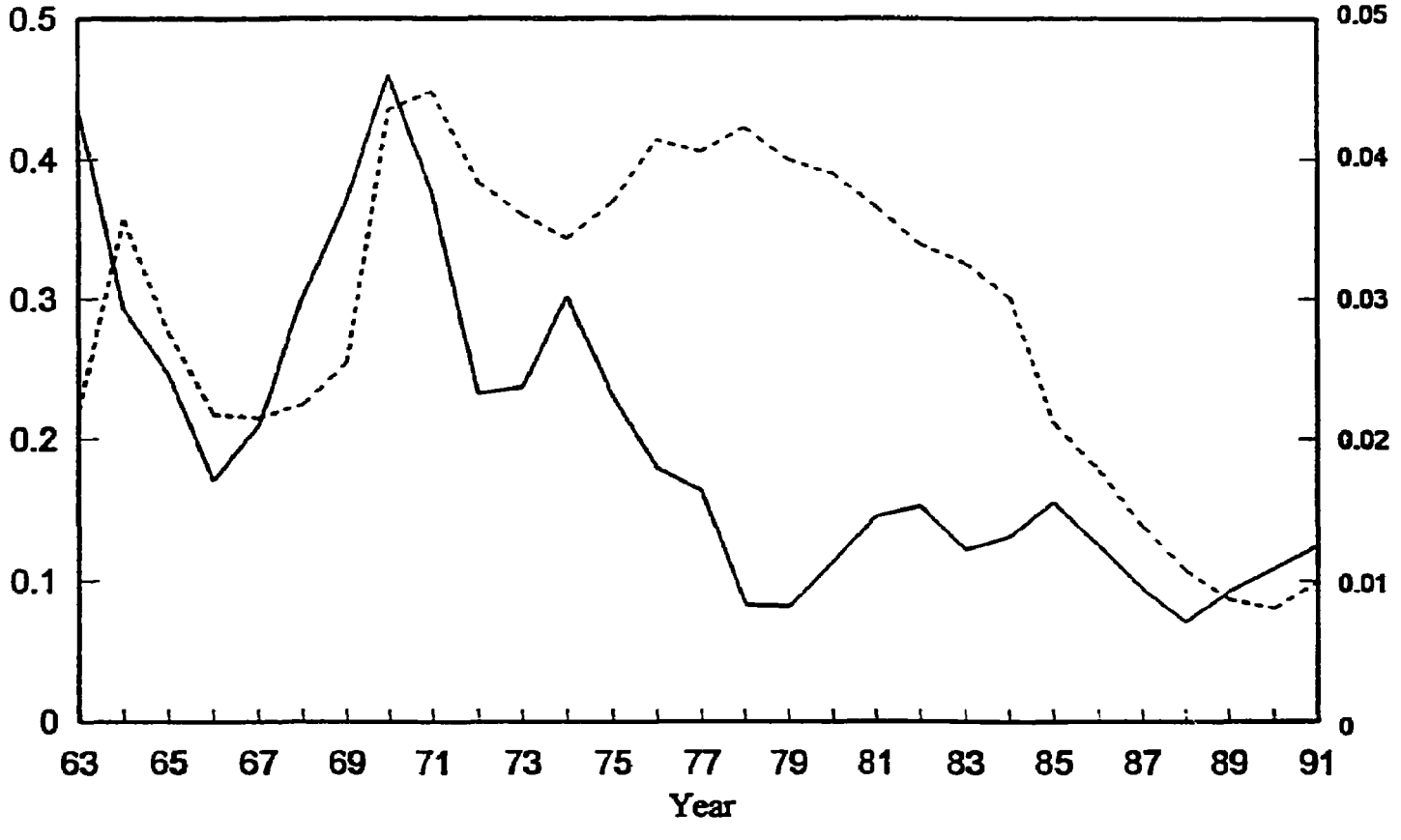
Figure 1
Investment vs. Long Term Bank Debt



Investment to Capital Ratio

Long-Term Bank Debt to Capital Ratio

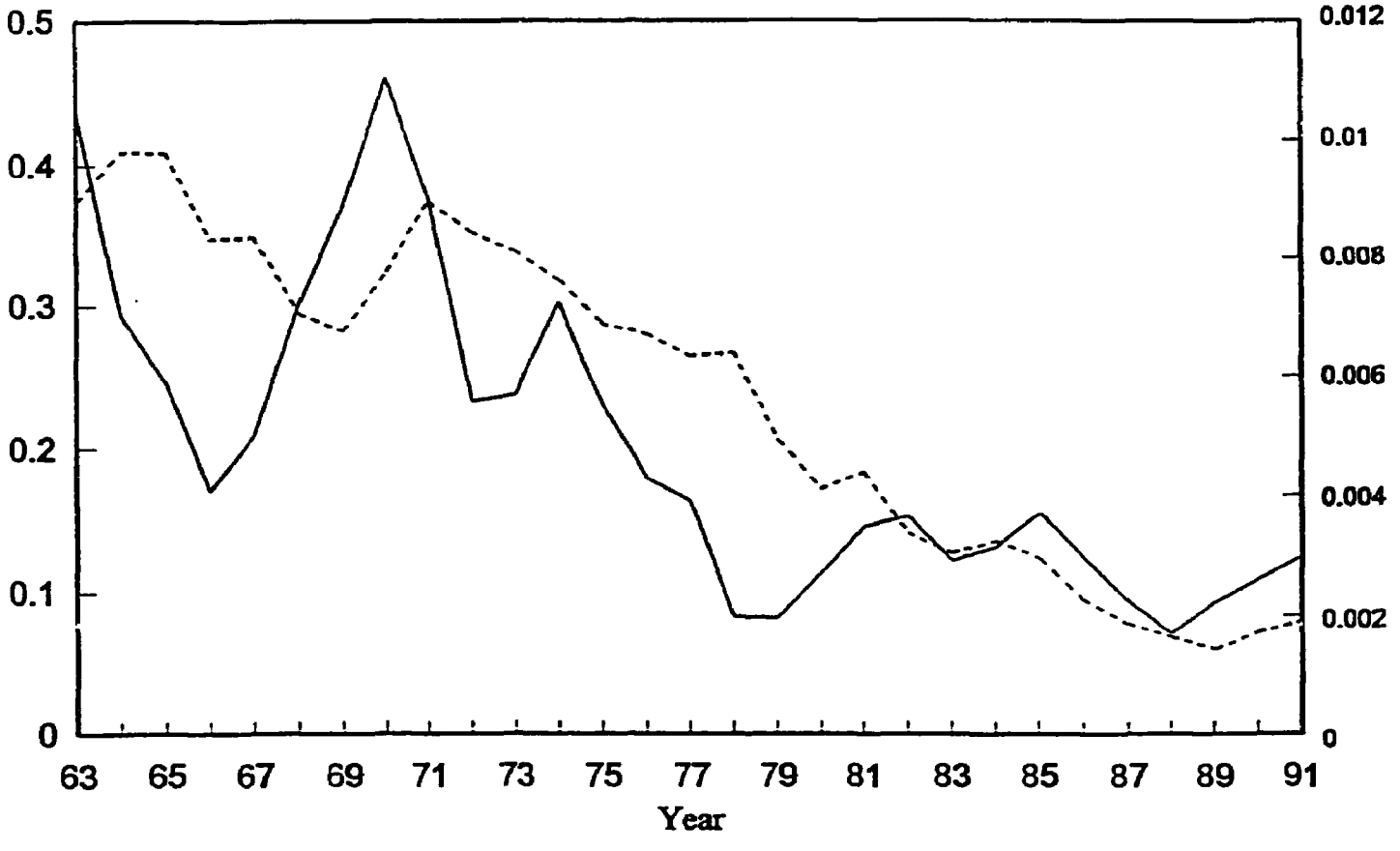
Figure 2
Investment vs. Total Government Debt



Investment to Capital Ratio

Total Government Debt to Capital Ratio

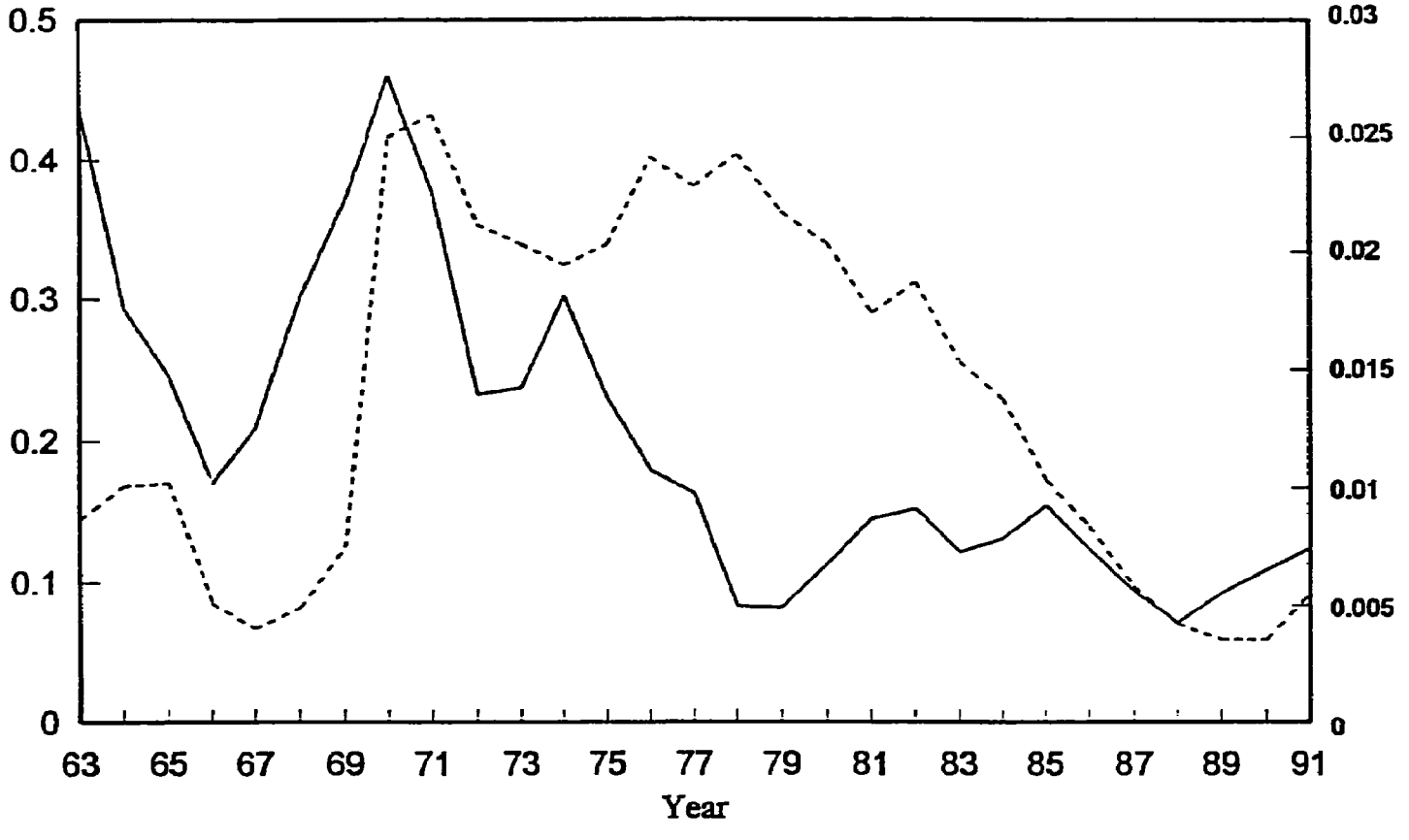
Figure 3
Investment vs. JDB Debt



Investment to Capital Ratio

JDB Debt to Capital Ratio

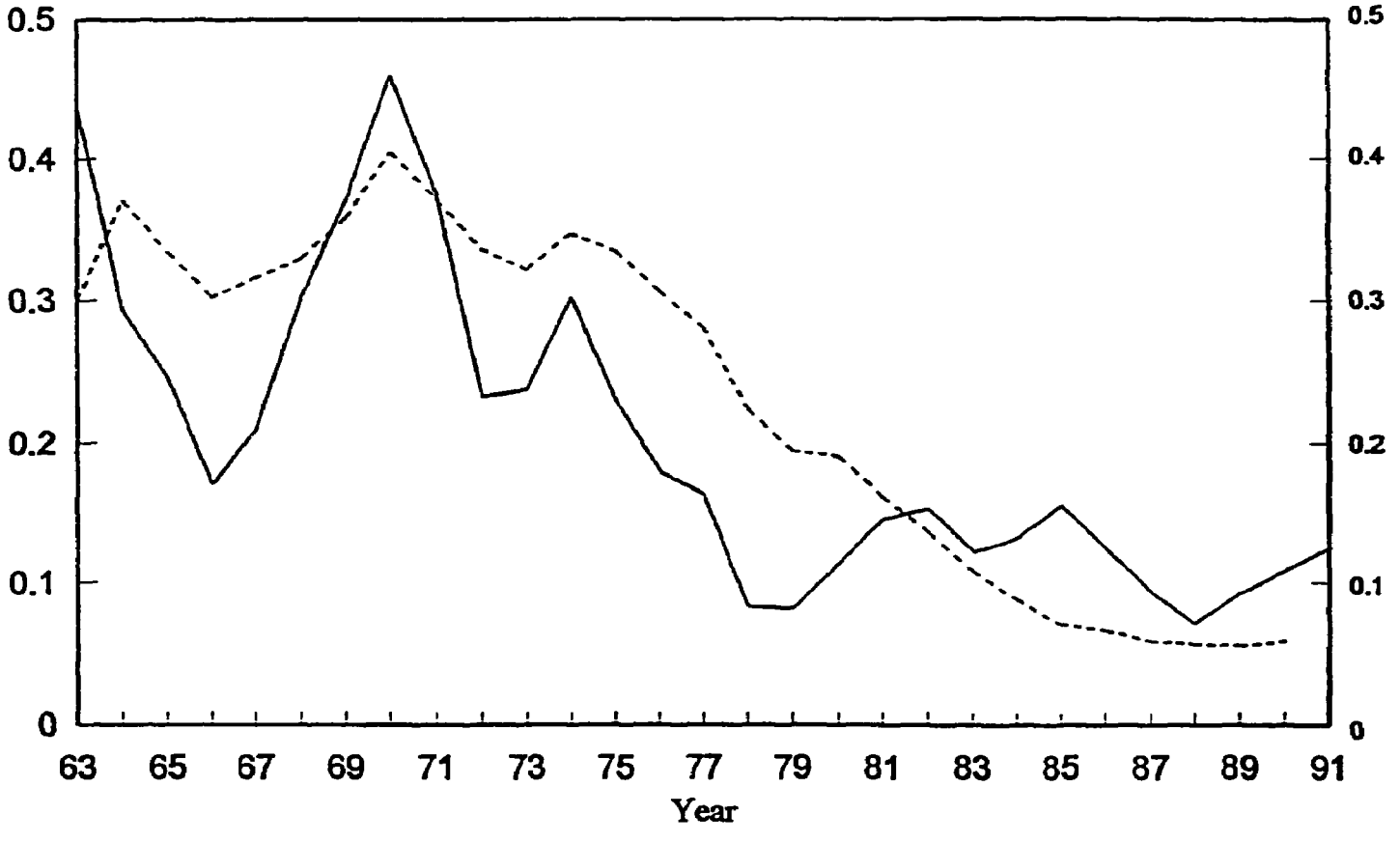
Figure 4
Investment vs. XMB Debt



Investment to Capital Ratio

XMB Debt to Capital Ratio

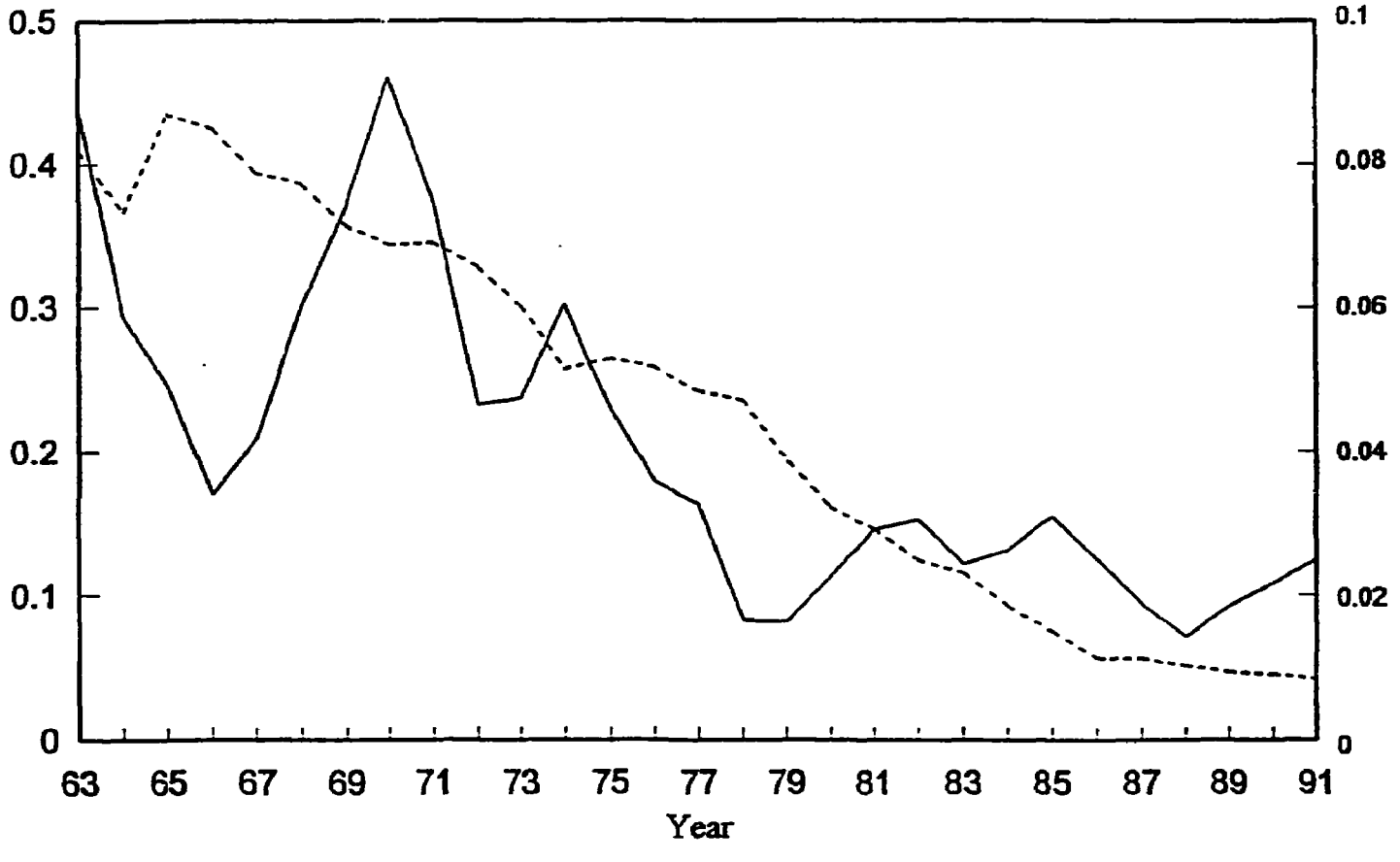
Figure 5
Investment vs. Long Term Commercial Bank Debt



Investment to Capital Ratio

Long-Term Commercial Bank Debt to Capital Ratio

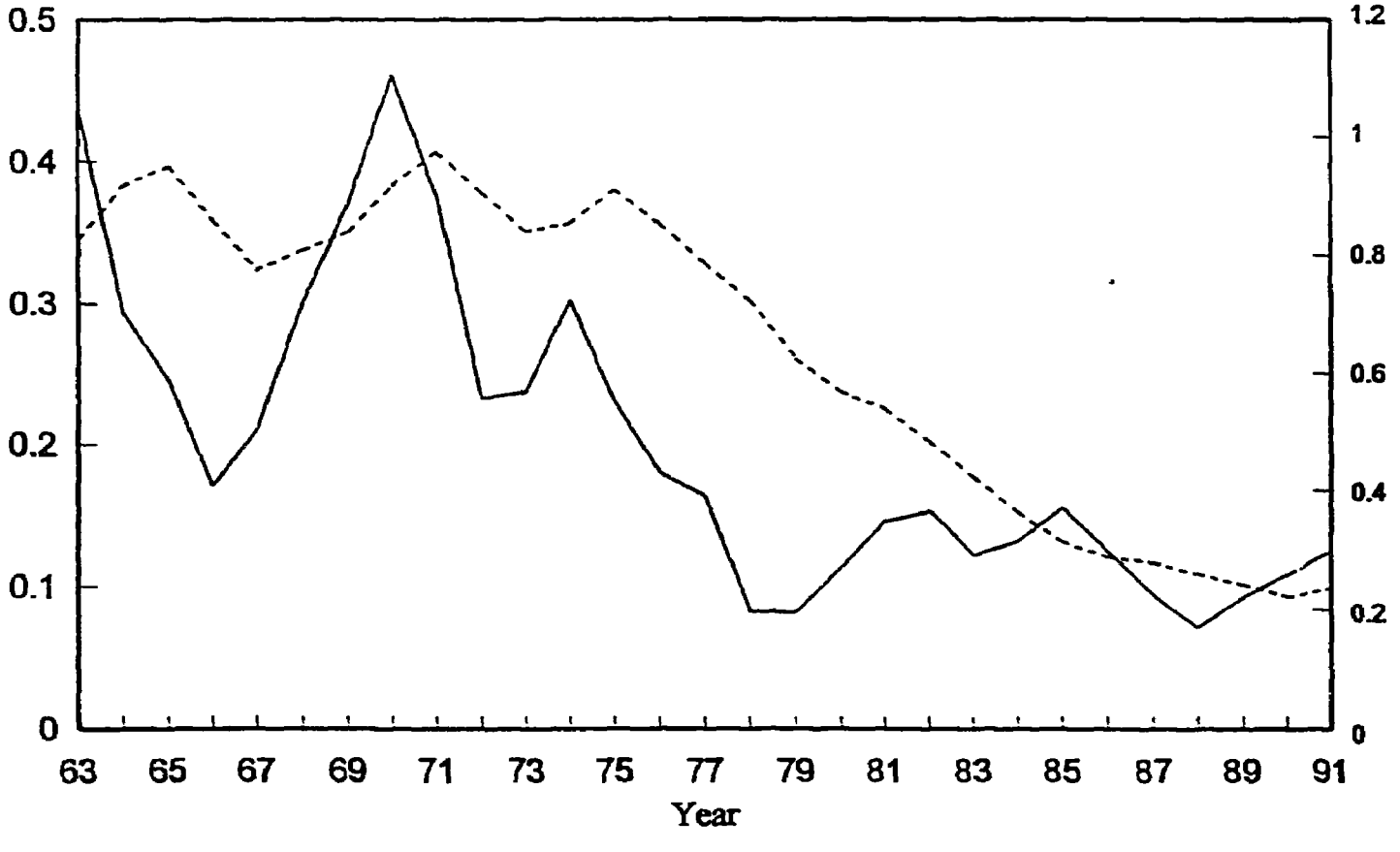
Figure 6
Investment vs. IBJ plus LTCB Debt



Investment to Capital Ratio

IBJ plus LTCB Debt to Capital Ratio

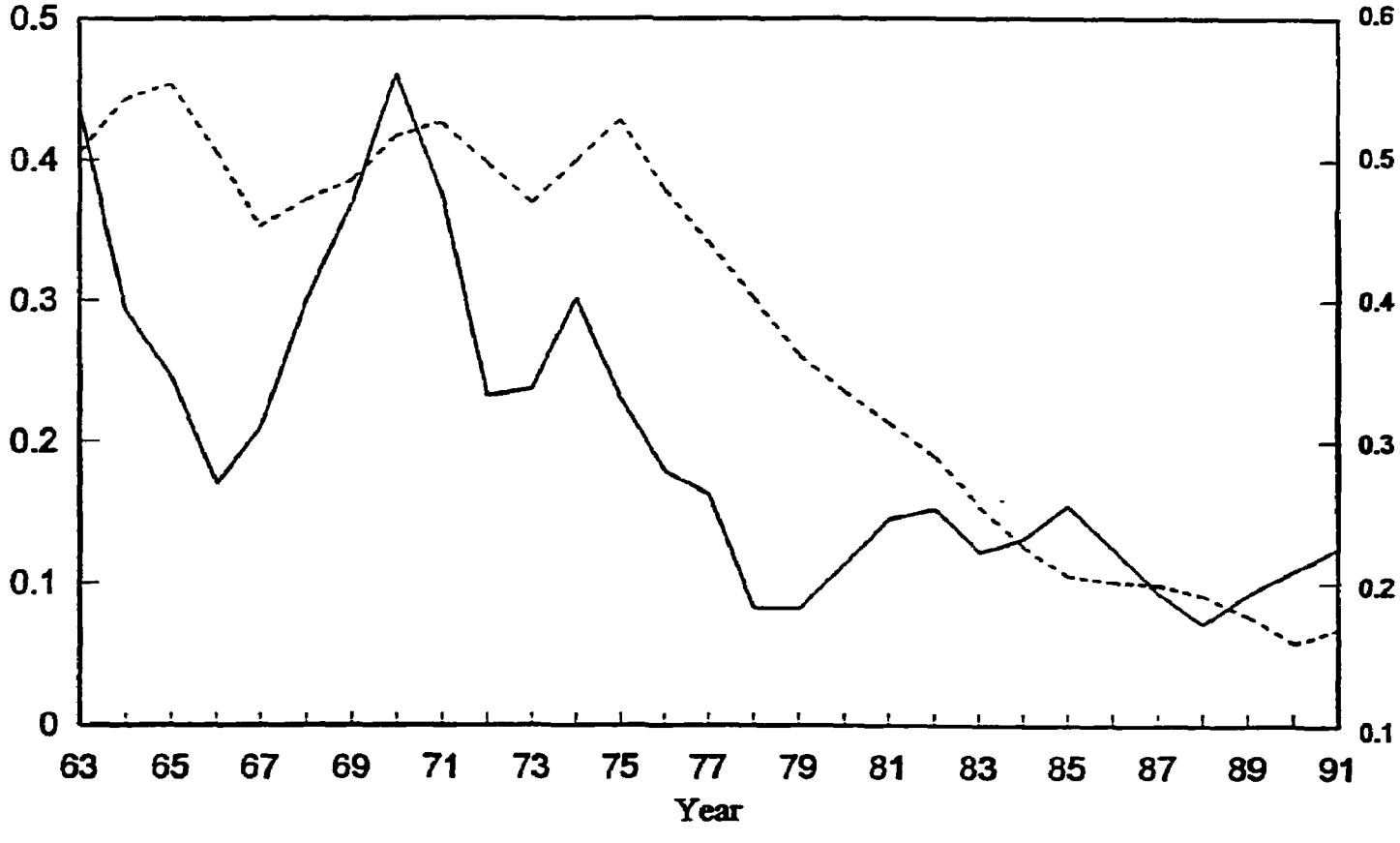
Figure 7
Investment vs. Total Bank Debt



Investment to Capital Ratio

Total Bank Debt to Capital Ratio

Figure 8
Investment vs. Short Term Bank Debt



Investment to Capital Ratio

Short Term Bank Debt to Capital Ratio

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