AN EMPIRICAL INVESTIGATION OF ECONOMIC CONSEQUENCES OF THE TAX REFORM ACT OF 1986

by

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ABSTRACT

This dissertation investigates the economic impact of the Tax Reform Act of 1986, one of the most far-reaching pieces of tax legislation in American history. The focus is on differential effects of the Act across industries. Event study methodology is used.

A model is created which links tax law provisions, firms' cash flows, and securities returns. Hypotheses are developed for seven industries, based upon analysis of the provisions of the Act and upon reading of contemporaneous expert commentary. The sample consists of firms in those industries trading over-the-counter.

Evidence of an adverse impact for the Act as a whole on the steel and machine tool industries is found. It is concluded that the Tax Reform Act of 1986 caused a shift in economic resources away from those industries, and that shareholders of firms in those industries suffered losses of wealth. In addition, it is determined that the uniform capitalization rules for inventory adversely affected the retailing industry, and that the change in loan loss reserve rules adversely affected large banks. The latter

set of findings emphasizes the substantive importance of tax accounting rules.

With regard to event study methodology, it is found that non-synchronous trading in over-the-counter stocks poses a severe problem when attempting to use the market model. A methodological modification suggested by Dimson is shown to be ineffective in dealing with this problem. Alternatives to the market model are identified, and are used in analysis.

Most significant reactions are found when abnormal returns are pooled over events, supporting an expectations-revision model of market reaction.

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CHAPTER ONE

INTRODUCTION

It is widely recognized that federal fiscal policy has a large and pervasive impact on our economy and society. Wide-spread interest in the nature of this impact is attested to by an extensive literature in the academic, professional, and popular presses. The Tax Reform Act of 1986 (hereafter, TRA) has been described as "...the most sweeping reform in the history of our nation's tax structure" (Condrell, Tierney, and Siegel, 1987, p. 411). As such, it represents a major exercise of federal fiscal policymaking power.

The most dramatic single change in the tax system made in the TRA was a major cut in tax rates, for both individuals and corporations. In order to make the measure "revenue-neutral" (a condition imposed by Congress in response to the sizable federal budget deficit), substantial base-broadening also occurred, especially with

regard to corporations and investors. One pervasive change was the repeal of preferential treatment for long-term capital gains, long a cornerstone of the tax system. Major changes were made in the tax treatment of depreciable assets, most notably the repeal of the investment tax credit and the imposition of less generous depreciation schedules. Numerous changes were made in accounting rules (especially with regard to inventories, long-term contracts, and bad debts), all of which had the effect of deferring taxpayer deductions, and thereby accelerating federal revenue collection.

While the TRA as a whole was forecast to be revenue-neutral to the government, it was by no means neutral with regard to taxpayers. It was expected to cause a sizable reallocation of net tax liabilities from individual taxpayers to corporate taxpayers. Among corporations, the impact varied widely. Much of this variation was expected to be across industry, because many of the TRA provisions were specific to certain industries or groups of industries.

The primary purpose of this study is to explore the economic impact of the TRA, in particular as that impact was revealed by differential effects across industries. Such differential effects can be expected to cause wealth

transfers among shareholders ("distribution effects") and a re-allocation of resources by the market as the TRA alters the relative after-tax return from investment in different industries ("allocation effects").

In this study, evidence about the economic effects of the TRA is sought using an event-study methodology. Selected industries are investigated, based upon an analysis of the provisions of the TRA and a reading of contemporaneous opinions of expert commentators.

Evidence of an adverse impact from the TRA is found, especially on the steel and machine tool industries. It is concluded that the TRA caused a shift in economic resources away from those industries, and that stockholders of firms in those industries suffered losses of wealth. Evidence is also found of an impact of two specific provisions of the TRA in isolation: the uniform capitalization rules for inventory adversely affected the retail industry; and the loan loss reserve rules of §585 adversely affected large banks.

With regard to the event study methodology, it is found that the infrequent trading of many over-the-counter stocks poses a severe problem when attempting to use the standard market model. A methodological modification suggested in the literature is shown to be ineffective in dealing with

this problem. Alternatives to the market model are identified, and are used in analysis.

Chapter Two of the dissertation describes the nature of the economic effects being investigated, and the means of identifying them, in the context of a review of the relevant literature. Chapter Three describes and analyzes the salient provisions of the Tax Reform Act of 1986, and reviews contemporaneous expert forecasts of their economic impact. Ten research hypotheses are developed from this discussion. Chapter Four develops a rationale for using over-the-counter firms in the study. The method of sample selection is described. Statistical methods, abnormal return metrics, and other methodological issues are discussed. Chapter Five presents and discusses the results of the quantitative analyses performed. Chapter Six contains a summary of these results, and conclusions drawn from them. Limitations of the study are identified, and the implications of the findings for future research are explored.

CHAPTER TWO

LITERATURE REVIEW

Taxonomy of Economic Effects

Musgrave and Musgrave (1984, Chapter One) identify three categories of economic effects of fiscal policy: allocation, distribution, and stabilization. "Allocation" refers to the allocation of society's resources between the public and private sectors, and to various uses within each sector. "Distribution" refers to the pattern of distribution of income and wealth among individuals in the society. "Stabilization" refers to maintaining macroeconomic factors at desired levels.

Bittker (1980) identifies two major normative criteria by which tax policy has traditionally been judged: equity and efficiency. In terms of the Musgrave taxonomy, equity is a distributional concern, dealing with the "fairness" of

the social distribution of income and wealth. Efficiency is an allocational concern, dealing with the efficient use of social resources. Feldstein (1976, p. 79), in his classic paper on tax reform, concurs with Bittker in identifying "distributional equity and allocative efficiency" as the key issues in the design of a tax system. In keeping with these traditional concerns for equity and efficiency, this study considers both distributional and allocative effects of the TRA.

Importance of Economic Effects

Distributional effects of tax law arise because, as Feldstein points out, tax laws are not "written de novo on ´a clean sheet of paper´" (1976, p. 77), but involve changes from an existing situation. These changes will have differential impacts across taxpayers, either directly by altering the amount or timing of tax payments, or indirectly by affecting the values of assets or the demand for products. Hence, they will alter the distribution of wealth compared to that obtaining under the status quo.

"The distributional consequences of any proposed tax change are always a central focus of attention and debate

among policy officials and the general public" (Feldstein, 1988, p. 37). A redistribution of income or wealth by means of tax legislation is clearly a matter of great practical importance to those involved. The sheer magnitude of the changes made in the TRA made such considerations of particular importance. The legislative history of the TRA clearly shows the major significance attached to distributional matters in the political process (Birnbaum and Murray, 1987).

Some commentators also view distributional matters as being of considerable ethical importance.[1] There is also evidence that distributional equity (or taxpayer perceptions of it) is related to the extent of taxpayer non-compliance with the law (Alm, 1991; Porcano, 1984). Non-compliance "...appears to be a large and growing problem in the United States," and has a sizable negative impact on the federal revenue (Alm, 1991, p. 577). Distributional effects of tax legislation may therefore have unintended revenue implications for the federal government.

Taxation has allocative effects of two sorts. Its very intent is to cause a reallocation of resources from the

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[1] For examples, see Blinder, 1987; Goode, 1987; Musgrave, 1976.

private to the public sector. This effect is obvious and will not be discussed further.

More subtle is the misallocation of resources within the private sector that results from a system of taxation that causes the tax burden to differ across types of activity, asset, and entity. The differential in the tax burden causes some activities to be undertaken that otherwise would not be, and to make prohibitive some activities that otherwise would be undertaken, resulting in a net loss of social welfare. As economist Robert Eisner (quoted in Tax Notes, September 22, 1986, p. 1125) explains, the tax system tends to subsidize certain forms of investment. Unfortunately, the typical tax-subsidized investment

> is that which was not sufficiently profitable or productive to be undertaken without the tax advantage. It is not likely to be productive. Otherwise it would have been undertaken anyway. And it tends to crowd out other, more productive investment with little or no tax advantage.

In the literature, such misallocation is typically referred to as "inefficiency" (e.g., Fullerton and Mackie, 1989). An analytical study by Diewert (1981) concludes that the level of inefficiency implied by the federal income tax regime is "potentially very large" with respect to the economy as a whole (p. 78).

Madeo and Pincus (1985, p. 408) point out that tax policy is a "social choice problem". It is generally recognized that such problems are not susceptible of unique, rational solutions (Wildavsky, 1979; Stone, 1988; Arrow, 1963). However, neither should they be addressed in an informational vacuum. Bittker (1980, p. 31) laments: "It is hard to see how the normative standards favored by either equity or efficiency theorists can be applied with confidence to existing law while the behavioral consequences of most tax allowances remain terra incognita." The key to mapping the unknown is what a standard text in public policy analysis refers to as "applied research", the goal of which is to "increase the amount and quality of the information the policy-makers have" (Quade, 1989, p. 14).

The statement by Quade is typical of much of the policy analysis literature. It implicitly assumes that providing policy-makers with additional information about the consequences of past policies and the likely consequences of proposed policies is unambiguously good. This assumption rests upon an idealized and rationalistic view of the policy process, in which policy-makers objectively

identify and "solve" social "problems".[2] This view is clearly unrealistic and has frequently been criticized, notably by Banfield (1980), who castigates it as "metaphysical madness". Banfield argues forcefully against the notion of objectivity in policy-making; in reality, he says, policy-makers must necessarily bring their own human foibles, biases, and interests to the policy process. The work of Stone (1988) complements that of Banfield, arriving at similar conclusions from a very different perspective. She points out that "facts" are subject to differing interpretation, depending upon the goals and world-views of the individuals using them. Rather than being used to solve objective social problems, facts are tools/weapons in the policy struggle. Kingdon (1984) provides extensive empirical documentation of the ways in which federal policy is influenced by the self-interest and biases of politicians, bureaucrats, and interest groups.

Recognizing that policy-making is a political process, a struggle of conflicting interests and world-views, raises the possibility that participants may abuse the information provided by analysts. For example, information about the distributive consequences of a policy may encourage

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[2] Wildavsky (1979) explicates and criticizes this view at length.

corruption by facilitating the rewarding of friends or the punishing of enemies.

A study of the legislative process surrounding the enactment of the TRA reveals that both views have merit. While the process was fundamentally political, there were also sincere efforts to promote the public welfare, and sincere searches for information to further those efforts. Because the findings of academic research are essentially a public good, the researcher cannot restrict their use to purposes he considers beneficial. The possibility of use is accompanied by the risk of abuse.

It should also be noted that academic policy analysis can perform a valuable service in a democratic society, by making information about the consequences of government policy publicly available. In the clever metaphor of Downs and Tehranian (1988, p. 1129): "Sound public policy requires an awareness of who is receiving and who is paying for the free lunch ordered by the policymakers".

This study is performed under the assumption that the potential value of its findings in creating socially beneficial policies is worth the risk of potential abuse.

Identifying Distributional Effects

There have been a number of prior empirical studies of the existence and magnitude of distributional effects of federal tax legislation. Tideman and Tucker (1976) investigated the effects on firm tax liabilities of the failure of tax accounting to take inflation into account. Downs and Tehranian (1988) determined the existence of "stock price windfalls" associated with events surrounding the passage of the Economic Recovery Tax Act in 1981. Ayres (1987) and Lyons (1989), examining the checkered history of the investment tax credit, found gains and losses accruing to shareholders of firms affected by the credit. Madeo and Pincus (1985) concluded that bank stocks fell in response to an IRS ruling restricting certain deductions by banks. Schipper and Thompson (1983, 1985) found that several legislative or regulatory actions, including the Tax Reform Act of 1969, were associated with adverse stock market reactions for firms which had been actively conducting mergers in the preceding decade. Manegold and Karlinsky (1988) studied the impact of an element of the Tax Equity and Fiscal Responsibility Act of 1982 (TEFRA) that reduced the tax benefits of "possessions corporations". They determined that there was an adverse

effect on corporations with such subsidiaries, and that the magnitude of the effect was related to the relative importance of the pre-TEFRA tax benefit to the firm.

Several recent econometric studies have attempted to assess the distributional effects of the TRA (Wallace, Wasylenko, and Weiner, 1991; Pechman, 1990; Feldstein, 1988). All of these have focussed narrowly on the distribution of the tax burden on households, using similar methodologies involving imputation of the corporate tax burden to households. All three studies conclude that the TRA had significant distributional effects.

All of the above-mentioned studies (except Tideman and Tucker and the econometric studies of the TRA) look for and find distributional effects evidenced by changes in the value of firms, as reflected in the prices of publicly-traded common stocks. In so doing, they follow a substantial body of literature on the economic consequences of accounting regulation, one of the earliest fields of market-based accounting research.[3] The assumption implicit in these studies is that price changes in individual securities represent net increases or decreases in the

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[3] This literature is reviewed by Beaver, 1981, Chapter Seven, and by Watts and Zimmerman, 1986, Chapters Seven and Twelve.

wealth of the individuals who own the shares at the time of the changes.

This assumption, though plausible, can nonetheless be criticized. For example, May and Sundem (1976) point out that stock price analysis necessarily deals only with aggregates, and cannot speak about costs and benefits at the level of the unidentified individuals involved. Consequently, statements can be made only about shareholders of particular securities in the aggregate. Further, the inability to identify specific shareholders does not permit consideration of the possible mitigating effects of diversification of investment portfolios at the individual level.

It has also been observed that, in an economic regime of incomplete markets, consequences of actions or policies cannot be fully reflected in prices (American Accounting Association, 1977).

Despite these objections, researchers continue to draw distributional conclusions from market-based research.[4] Inferences of relative changes of wealth and well-being from relative changes in stock prices are warranted given the assumption of monotonically-increasing utility of

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[4] An example of a highly-regarded piece of research drawing such conclusions is Chow (1983).

wealth, and given that individuals are rarely, if ever, fully diversified in their investments. However, it must be borne in mind that such conclusions can be of only a general nature. The individuals affected, and the degree to which they are affected, can be determined only imprecisely. With these limitations, market-based research can provide useful information about distributional effects of tax policy.

Identifying Allocation Effects

Jeremias (1979, Chapter One) reviews the economics literature as to misallocations of resources resulting from the tax system. He identifies three types of misallocation: between corporate and non-corporate sectors; among type of assets (buildings, equipment, etc.); among industries. It is the third of these -- allocations of resources across industries -- that will be addressed in this study.

There are several reasons for this choice. First, Atkinson and Stiglitz (1980, p. 159) note that the tax code has a particularly strong tendency to affect the pattern of capital investment across industries, as well as the

overall level of investment in the economy. In part, this tendency is caused by the explicit targeting of tax incentives toward particular industries. Such targeting was pronounced in the TRA. Not only was the TRA's impact of enormous magnitude, adding an estimated $120 billion over five years to the corporate tax burden, but approximately two-thirds of this impact came in the form of industry-specific tax law changes (Tax Notes, December 8, 1986, pp. 879-881). Second, as Cutler (1988, p. 1111) points out, "demand shifts [i.e., changes in demand for a firm's product] from the tax reform would plausibly be concentrated along industry lines". Thus the effects of the TRA on cash flow from operations can be expected to be similar within industry, and to differ across industries. Third, industry effects to some extent subsume Jeremias' second category (misallocations across types of asset). This is true because the asset mix in many industries is similar across firms, due to technology or the nature of operations. For example, a shift of resources away from the real estate and construction industries also represents a shift of resources away from buildings into other types of assets.

The existence of cross-sectional relationships among security prices within industries was first demonstrated by

King (1966). This phenomenon has been "well documented" by subsequent research (Olsen and Dietrich, 1985, p. 144). Empirical market-based research has established the existence of industry effects of legislation. For example, Lenway, Rehbein, and Starks (1990) found abnormal returns to the steel industry from the passage of certain trade legislation. There is also some empirical evidence for the existence of an industry/tax interaction. Harrison (1977) found a modest market reaction to IRS-imposed accounting changes when controlling for industry. Zarowin (1988) studied the impact of unexpected inflation on the depreciation "tax shield", and determined that his model was improved by controlling for industry.

Summary

Concerns about equity and economic efficiency have traditionally held a prominent place in tax policy literature. Equity concerns often focus on the distribution of wealth, and efficiency concerns often focus on the allocation of productive resources within the economy. In keeping with this tradition, this study

examines some distributional and allocation effects of the Tax Reform Act of 1986.

There is reason a priori to believe that the TRA altered the relative incidence of federal income tax across industries, thereby affecting the distribution of wealth, and altering the allocation of resources among different types of economic activity. It is further expected that these effects will reveal themselves in stock price behavior. These expectations are developed more fully in the following chapter.

The use of stock price behavior to investigate the economic consequences of accounting regulation and federal legislation is characteristic of "market-based accounting research" (MBAR). For some twenty years, MBAR has constituted a major part of empirical research in accounting, and has proved to be a productive tool for studying economic consequences of tax policy.[5]

Despite the lack of definitive criteria for social welfare judgments, Lev and Ohlson (1982) argue that MBAR can make a substantial contribution to the social choice process. One aspect of this contribution is to provide

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[5] For recent reviews and evaluations of MBAR, see Bernard (1989) and Watts and Zimmerman (1990). For a discussion of its application to tax policy analysis, see Omer and Shaw (1991).

policymakers and others with empirical evidence of the consequences of past policy choices. Providing such information is the primary goal of this study. It is recognized that providing information to policy-makers carries potential risks as well as benefits, but it is assumed that a legitimate and useful social purpose may be served thereby.

CHAPTER THREE

MODEL AND HYPOTHESES

Model

The link between tax law changes and firm valuation is straight-forward under the capital asset pricing model (CAPM) and the efficient markets hypothesis, two widely-maintained assumptions about the way securities markets work.

The CAPM equates the market value of a firm with the present value of the expected future cash flows to the firm. This may be expressed as

$$V_F = \Sigma \left[(E(CFO_t) - E(T_t))/(1 + r_F)^t \right] \quad (1)$$

where

V_F is the value of the firm,

CFO_t is cash flow from operations,

T_t is the amount of taxes paid,

t is the time period, 1..N, where N is the expected life of the firm,

r_F is the firm-specific, risk-adjusted discount rate, and

E is the expectations operator.

Economic models of firm valuation do not yield clear implications regarding the interaction of corporate debt and the income tax (see e.g., Miller, 1977; Benninga and Talmor, 1988). Therefore, debt is exogenous to the model, and interest expense is subsumed under CFO_t.

The expectations operation is defined as

$$E(X) = \Sigma\ p(X_i)v(X_i) \qquad (2)$$

where X is any variable, the X_i are possible states of the world, v is a value function, and p is a probability function. Thus, expected values may change either because of changes in expectations regarding the values (in this case, cash flows) associated with possible states of the world, or because of changes in subjective probabilities of various states occurring.

A change in tax law can affect expected future cash flows in two ways: directly, by changing the amount and/or timing of expected future tax payments (T); and indirectly, by changing economic conditions and thereby expected future cash flows from operations (CFO). For example, a reduction

in tax rates will, ceteris paribus, cause a reduction in expected future tax payments, and thereby increase the value of the firm. On the other hand, the elimination of an incentive provision of the tax law that had stimulated demand for the firm's product may be expected to reduce future sales, and correspondingly future cash flows from operations, thus decreasing the value of the firm.

The efficient markets hypothesis (semi-strong form) holds that "...prices adjust instantaneously to new information and always reflect all publicly available information" (Lev and Ohlson, 1982, p. 284). Thus, one would expect the market to rapidly impound any new information as to the composition of future tax law and the effects of such law on the cash flows of firms.

The creation of any significant amendment to the Internal Revenue Code (IRC) is an extended and generally well-publicized legislative process (Pierce, 1971). Under the Constitution, the bill must originate in the House of Representatives. It must be reported out of the Ways and Means Committee before being voted on by the House. The bill then goes to the Senate, where it must be reported out of the Finance Committee before being voted on. Because the bills passed by the two houses generally differ materially, it is necessary to form a conference committee

of Senators and Representatives to arrive at a compromise bill. This final bill must then be passed by both houses and signed by the President in order to become law. Thus, the legislative process consists of numerous stages, each of which may convey incremental information about the final result of the process.

In the context of equation (2), the state of the world (X_i) that we are concerned with is the eventual passage of an amendment to the IRC. Each legislative event may, but will not necessarily, alter perceptions of $v(X_i)$, $p(X_i)$, or both. For example, the Senate Finance Committee may add to the bill a provision not contained in the House version. If this provision affects firm Z in a way that will alter its future cash flows, it may alter $v(X_i)$ for firm Z. In addition, a legislative event may alter $p(X_i)$, the probability of the amendment eventually becoming law. Since each such event represents an additional hurdle overcome by the bill, it seems quite likely that it will cause observers to revise $p(X_i)$ upward. It should be noted that while $p(X_i)$ is the same across firms, the same is by no means true of $v(X_i)$, because the impact of specific provisions varies across firms. In analyzing the impact of the TRA, Cutler (1988, p. 1107) predicted "substantial cross-firm heterogeneity in the reaction to tax news".

Specific reasons why this is so will be discussed later in this section.

Because each legislative milestone potentially conveys information to the market that may alter V_F, each must be treated as a separate event for purposes of assessing market reaction (Omer and Shaw, 1991). This practice has been followed in a number of previous event studies of tax or other legislative acts.[6] In this study, relevant event dates were identified by following the legislative history of the TRA in the Wall Street Journal and Tax Notes. The former was used to determine the actual calendar dates, because it is considered the most reliable source for event dates in financial studies (Wright and Groff, 1986). Seven major legislative milestones in the passage of the TRA were selected; the final passage of the bill by the House and Senate coincided so closely that they are treated as one event. Event dates are listed in Table 1.

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[6] These include: Schipper and Thompson (1983, 1985); Madeo and Pincus (1985); Ayres (1987); Cutler (1988); Manegold and Karlinsky (1988); and Downs and Tehranian (1988).

TABLE 1

TAX REFORM ACT OF 1986 EVENT DATES AND WINDOWS

1. House Ways and Means Committee reports out original bill
   - Date: Sat., 11/23/85
   - Windows: Fri., 11/22/85 - Wed., 11/27/85
     Thu., 11/21/85 - Thu., 12/5/85

2. House passes bill
   - Date: Tues., 12/17/85
   - Windows: Mon., 12/16/85 - Thu., 12/19/85
     Fri., 12/13/85 - Thu., 12/27/85

3. Senate Finance Committe reports out its version of the bill
   - Date: Weds., 5/7/86
   - Windows: Tues., 5/6/86 - Fri., 5/9/86
     Mon., 5/5/86 - Fri., 5/16/86

4. Senate passes bill
   - Date: Tues., 6/24/86
   - Windows: Mon., 6/23/86 - Thu., 6/26/86
     Fri., 6/20/86 - Thu., 7/3/86

5. Conference Committee reports out final version of the bill
   - Date: Sat., 8/16/86
   - Windows: Fri., 8/15/86 - Weds., 8/20/86
     Thu., 8/14/86 - Weds., 8/27/86

6. House and Senate pass final bill
   - Date: Thu., 9/25/86 and Sat., 9/27/86
   - Windows: Weds., 9/24/86 - Mon., 9/29/86
     Tues., 9/23/86 - Mon., 10/6/86

7. President Reagan signs bill into law
   - Date: Weds., 10/22/86
   - Windows: Tues., 10/21/86 - Fri., 10/24/86
     Mon., 10/20/86 - Fri., 10/31/86

The Tax Reform Act of 1986

In order to select industries for investigation, it was first necessary to examine in detail the provisions of the TRA. The following discussion highlights those aspects of the TRA most relevant to this study. These aspects fall into four broad categories: tax rates, long-lived assets, accounting methods, and tax shelters.

In the view of its creators, the centerpiece of this legislation was tax rate cuts. Prior to the TRA, the tax rates on individuals were progressive, ranging from 11% to 50%. The House proposed to compress this schedule somewhat, to a range of 15% to 38%. The Senate proposed a much more dramatic change to a two-bracket system (15% and 27%). The Senate's plan was adopted in the final bill, with the top rate increased one point to 28%. Thus, tax reform involved a major change in the individual tax rate structure, and a major tax cut to individual taxpayers projected to be in excess of $100 billion over five years.

Prior to the TRA, the corporate income tax was essentially a flat tax at a rate of 46%. The TRA retained this structure, but reduced the rate to 34% (a compromise between the 36% originally passed by the House, and the

Senate's 33%). Thus, from the inception of the reform process, tax reform meant a sizable reduction in income tax for profitable corporations at any given level of taxable income.

However, unlike most individuals, corporations faced numerous changes in the way taxable income is computed. These changes were necessitated by the requirement (imposed by President Reagan and accepted by Congress) that the bill be revenue-neutral. The tax rate cuts had to be paid for by offsetting sources of revenue, which came largely from the business sector.

The most significant changes came in the area of long-lived assets. In 1981, the Economic Recovery Tax Act (ERTA) had instituted very generous depreciation schedules for both real and personal property. The rapid depreciation allowed under this Accelerated Cost Recovery System (ACRS) was valuable to taxpayers, in that it increased the present value of the depreciation tax shield, and was correspondingly costly to the Treasury. The House bill moved severely in the opposite direction. Real property was to be depreciable only on the straight-line method over a thirty year life, as opposed to an accelerated method and nineteen year life under ACRS. Declining balance depreciation was to be retained for other

property, but depreciable lives were to be greatly lengthened (in some cases, two or three times the ACRS length). The Senate accepted the proposal on real estate, which was enacted in the final bill with small changes. On other property, the Senate wished to essentially retain ACRS. The final bill was a compromise, less severe than the House bill, but less favorable to taxpayers than ACRS.[7]

In addition, all versions of the bill contained some form of Alternative Minimum Tax (AMT) for corporations. The basic logic of the AMT is to recalculate taxable income with numerous modifications, including disallowance of accelerated depreciation deductions, and assess a tax on this recomputed income at a flat rate less than the normal statutory rate. This tax is assessed in lieu of the regular tax, if it is higher, and disregarded otherwise. The ramifications of the AMT are numerous and complex. In the context of this discussion, its chief effect is to eliminate any benefit for accelerated depreciation for firms subject to it in a given year.

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[7] The final bill provided straight-line depreciation for realty over lives of 27.5 years (residential rental) or 31.5 years (other). For other depreciable assets, accelerated depreciation was retained. Depreciable lives were lengthened, but moderately (e.g., from three to five, and from five to seven, years).

One of the most significant elements of the TRA was the addition to the Code of §49(a), which repealed the investment tax credit (ITC). The ITC was in effect a subsidy to purchasers of depreciable personalty, and did not apply to most real estate. ITC repeal was a feature of all reform proposals.

Another fruitful source of revenue for the tax reformers was modification of tax accounting rules. The TRA added §460, which reduced the ability to use the completed contract method of accounting for long-term contracts. This change had the effect of accelerating the recognition of income, and therefore tax payments. Also added was §263A, which created the "uniform capitalization" rules (UNICAP). These rules require sellers of goods to inventory certain overhead costs (such as purchasing and storage) that had previously been eligible for expensing. These costs cannot be recovered until the inventory is sold. The effect of UNICAP on a taxpayer is a deferral of deductions, and therefore a relative accleration of tax payments. In the House bill, UNICAP applied only to manufacturers. The Senate extended it to retailers and wholesalers, with an exception for "small" (less than $10 million in sales) firms. The final bill followed the Senate version.

The Senate bill also amended §453 to deny the use of the installment method of accounting for revolving credit plans, such as those used by some retailers. This would result in paying tax on the entire gross profit from credit sales in the year of sale, even though the cash collections are deferred. It also added §453C ("proportionate disallowance"), a complex provision that required the immediate recognition of a portion of installment sales based on the debt level of the taxpayer. The cash flow effects of these rules are similar to those of UNICAP, but were expected to be much smaller in magnitude.[8]

Two other changes were aimed at banks. One of the most controversial provisions of the TRA was an amendment to §585 prohibiting "large" banks (assets greater than $500 million) from using a reserve for bad loans. Affected banks are allowed to take a deduction for a bad loan only when it can be proved that a specific loan cannot be collected. In addition, existing loan loss reserves were required to be taken back into income over four years. The

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[8] The Joint Committee on Taxation estimated the five-year incremental revenues from the Senate's extension of UNICAP to be $4.4 billion, and the incremental revenues from the changes to §453 to be only $600 million (Tax Notes, April 14, 1986, p. 160). The House and Senate estimates were different, but of similar orders of magnitude (1986-3 CB vol. 2 63-77; 1986-3 CB vol. 3 9-27).

TRA also extended §265, dealing with costs related to tax-exempt income, to banks. This change had the effect of disallowing any deduction for a portion of the interest expense incurred by banks that carried municipal bonds in their portfolios.

The TRA also attacked the existence of tax shelters. New §469 created the "passive activity loss" (PAL) rules, which disallowed (with a phase-out) any deduction of passive losses against income from other sources. Losses from most existing tax shelters were defined as PALs under these rules, which were expected to effectively put an end to tax shelters. The PAL rules originated in the Senate. The House had proposed to deal with PALs through the AMT, a much less severe alternative.

Finally, the TRA repealed §1202, which had provided for the exclusion from income of 60% of the long-term capital gains of individuals. Under the new tax regime, capital gains are fully taxable.

Hypotheses

Based upon the foregoing analysis, predictions were made as to which industries' future cash flows were most

likely to be affected, positively or negatively, by the TRA. These predictions were validated by comparison with expert commentary published during or shortly after the legislative process culminating in the passage of the TRA. Three major financial and tax news sources -- Wall Street Journal, Forbes, and Tax Notes -- were reviewed for the period March 1 - October 31, 1986. Also, three major tax practitioner journals -- Journal of Taxation, TAXES, and Taxation for Accountants -- were read for calendar 1987, the period during which they published articles focusing on the TRA. In addition, a contemporaneous analysis of industry effects of the TRA was reviewed (Starcher, 1986).

Greatest emphasis was placed on the Wall Street Journal, which has been described as "the most important disseminator of financial news" (Givoly and Palman, 1985). Because of its timeliness and wide circulation, it was felt to be most representative of information available to the market at the time under study. Those articles on which greatest reliance was placed are listed in Appendix A.

The following paragraphs discuss the major provisions of the TRA from an industry perspective. Those industries about which hypotheses will be developed and tested are underlined for emphasis and ease of reference.

In terms of impact on future tax payments (T in equation (1)), adverse effects of the TRA fell most heavily on capital-intensive manufacturing firms. Commentators were essentially unanimous on this point. Among their disadvantages were loss of ITC and lengthened depreciable lives on newly-acquired productive assets, and addition of excess depreciation on existing assets to the AMT base. Specific industries in this category mentioned most often by commentators were autos and steel. Construction companies suffered these problems, and also possible effects of the change in long-term contract accounting. Real estate firms were faced with greatly reduced annual depreciation deductions on new properties. Interest incurred to carry municipal bonds was no longer deductible by banks, which also faced significant additional current taxes because of the recapture of loan loss reserves. The latter provision applied only to large banks.

Corporations in general enjoyed a substantial reduction in their tax rate. Those not suffering from the adverse consequences enumerated above could be expected to see significant decreases in their future tax liabilities. Major industries mentioned most often by commentators as being in this happy situation were publishing, broadcasting, and retailing. The retailing industry was

subjected to the provisions of the new uniform capitalization rules of Sec. 263A, requiring that some costs, previously allowed to be expensed, in the future be capitalized. While this provision would serve to accelerate the recognition of income and payment of tax, its impact was expected to be more than offset by the rate reduction.

One of the most discussed features of the TRA was its removal or reduction of tax subsidies from major classes of assets. These included machinery and equipment, vehicles, and depreciable real estate. Because the removal of the subsidy would reduce the after-tax return from investment in such assets, demand for them could be expected to fall. This reduced demand would reduce expected future cash flow from operations (CFO in equation (1)) of firms in the business of selling such assets, thus decreasing the value of these firms.

Removal of the subsidies for machinery and equipment could therefore be expected to adversely affect the machine tool and equipment industry. Likewise, removal of the subsidy on vehicles would adversely affect the auto industry. Reduced demand for depreciable real estate, due to both the depreciation and passive loss changes, would negatively impact the real estate, construction, and

building materials industries. Because steel is a component of all of these assets, demand for this product would also be expected to suffer.

On the other hand, demand effects in the retail industry were widely expected to be positive. As one analyst observed: "Retailers win whenever [tax] rates are lowered...because more money would be in the hands of individuals, who would promptly consume it" (Tax Notes, March 31, 1986, p. 1315).

Based on the foregoing discussion, the following hypotheses are tested. "Positive" and "negative" reaction are defined in terms of the sign of significant abnormal returns (see methodology section) in response to events favorable the passage of TRA.

H1: The retailing industry reaction will be positive.

H2: The steel industry reaction will be negative.

H3: The auto industry reaction will be negative.

H4: The machine tool and equipment industry reaction will be negative.

H5: The real estate/construction industry reaction will be negative.

Hypothesis H5 combines the real estate and construction industries. This has been done because many firms are

engaged substantially in both activities, and attempting to assign these firms to one industry or the other would be difficult and ultimately arbitrary. Because the impact of the TRA on both industries is expected to be very much the same, no information is lost by pooling them.

As discussed above, the construction industry generally faces both adverse demand effects and adverse tax effects from the depreciation and ITC changes. One sector of this industry, engineering services, faces only the former effect, because it is not capital-intensive. If both demand and tax effects are operating, it would be expected that engineering services would be less severely impacted than other aspects of the construction industry.

H6a: The engineering services sector reaction will be negative.

H6b: The engineering services sector reaction will be less negative than the remainder of the construction industry.

Banks were expected to be adversely affected by the TRA, but the most important change, dealing with loan loss reserves, applied only to "large" (as defined in the statute) banks. Therefore, the impact on large banks is expected to be greater than on others.

H7: The banking industry reaction will be negative.

H8: The negative reaction for large banks will be greater in magnitude than that for other ("small") banks.

A differential reaction may be found for Events Three (Senate Finance Committee report) and Four (Senate passage of bill) as compared to the other events, especially with regard to H8. As noted above, the damaging loan loss reserve rule, included in the House version of the bill, was dropped by the Finance Committee and full Senate, and later reinstated in the final bill. Thus, the Senate bill was considered much less harmful to banks, and did not differentially impact large and small banks (Tax Notes, 6/30/86, pp. 1337-1340).

Both the broadcasting and publishing industries were expected to benefit substantially from the tax rate cut. These two industries may be pooled for testing, because the impacts are expected to be much the same for both. Also, there is considerable overlap between them, with many firms having substantial operations in both publishing and broadcasting. In the case of book publishers, the benefit of the tax cut could be mitigated somewhat by the UNICAP rules. Therefore, this segment may be expected to have a lesser net benefit.

H9: The publishing/broadcasting industry reaction will be positive.

H10: The book publishing segment reaction will be less strongly positive than that of the rest of the industry.

The hypotheses generally do not specify on which of the seven event dates reactions are expected. Neither theory nor prior empirical research provide a sufficient basis to form a priori expectations in this regard. An exception exists for the bank hypotheses, where it is possible to predict differing impacts from differing versions of the bill.

CHAPTER FOUR

DATA AND METHODOLOGY

Use of Over-the-Counter Firms

Cutler (1988) investigated stock market reaction to the TRA. Based upon a general failure to find significant abnormal returns to industry portfolios, he concluded that there is "little evidence that reactions to tax reform fell along industry lines" (p. 1111). Cutler's study used data from only "Fortune 500" firms, and event windows of one, six, and thirty days. A possible explanation for his results, which he does not adequately address, is an information effect resulting from using only very large firms.

There is reason to believe that the market for smaller firms' stock is less efficient than that for very large firms. The latter are followed more closely by analysts,

and are the subject of more detailed study. There are also richer sources of public information about them than for smaller firms. For highly complex events, and for those that unfold over a period of time, the market is able to impound information gradually, rather than intensely on the occurence of a major news event.

Support for this view is provided by Grant (1980), who found that annual earnings announcements of firms traded over-the-counter (OTC) have more information content than those of New York Stock Exchange (NYSE) firms. Grant attributed this finding to the information effect discussed above. Further support is provided by Brown (1988), who studied market reaction to announcements of changes in depreciation method. Brown concluded that the OTC market was less efficient than the NYSE, and took several times longer to fully impound the information contained in the announcement. This is particularly striking in that a change in depreciation method is relatively simple compared to much of the information which the capital markets must absorb.

The TRA was a highly complex piece of legislation. Its political course was protracted and closely followed by lobbyists and others who stood to be affected by it (Birnbaum and Murray, 1987). Therefore, it is plausible to

assert that the NYSE would have impounded its effects gradually over the course of the legislative process to a much greater extent than would have the OTC market. Therefore, the NYSE would be much less inclined to exhibit a strong reaction on the occurrence of a discrete event such as the reporting out of a bill from committee, or its passage by the House or Senate.

The implication of the foregoing for an event study of the TRA is that one is more likely to detect a significant market reaction on the OTC market than on the NYSE. It is not that the reaction has not occurred on the NYSE, but that it has occurred so gradually that the event study methodology lacks power to detect it. In such a case, it may be more fruitful to examine the OTC market in testing for expected consequences of an event.

A second reason for examining OTC firms is that they are generally less diversified than NYSE firms. OTC industry portfolios can therefore be expected to be cleaner, with less noise, and potentially more significant and more readily interpretable results.

For these reasons, this study uses data drawn from the OTC market.

Sample Selection

The procedures followed in selecting firms for the sample and creating industry portfolios were:

1. The initial sample consists of all firms classified in the relevant industries by Moody's Industrial Manual -- OTC for 1986. Firms not covered by Moody's were not selected.
2. Foster (1981) found greater significance in industry effects when "industry" was defined such that there was homogeneity in chief line-of-business (LOB) across firms. Foster classified firms based on LOB footnote disclosures. However, such disclosures are often not available for OTC firms. It is not sufficient to accept at face value published industry classifications. Prior research has disclosed inadequacies in the widely-used federal Standard Industrial Classification (SIC) Manual (Couretas, 1984; Belth, 1984). Preliminary research for this study disclosed numerous classification errors in Moody's Industrial Manual. Therefore, the classification of firms by industry was verified by reading the

detailed description of the corporation's business activities provided in Moody's. Firms that did not appear to be primarily engaged in the specified industry were deleted or reclassified.

3. Firms not covered on the CRSP Daily Returns tape for the entire estimation period and period of legislative activity (June 1, 1985 through October 31, 1986) were deleted.

4. Firms subject to possible confounding events were deleted from the sample for affected event dates. Examples of confounding events include mergers, bankruptcies, earnings announcements, and litigation. Possible confounds were detected by reviewing all news items on the Dow Jones News Service (DJNS) containing reference to any firm in the sample for all event-window days. The DJNS wire was used because it is more comprehensive and more timely than published news sources, including the Wall Street Journal. In addition, disclosures available in Moody's were reviewed. Also, the list of firms receiving relief in the transition rules was reviewed. No firms in the final sample received such relief.

Table 2 lists the industries studied and the number of firms in each industry portfolio. Firms in the sample are listed by industry in Appendix B.

Statistical Methods

In standard event study methodology, the market reaction to an event affecting a given firm is measured by the cumulative abnormal returns (CAR) over the number of trading days (the "window") within which the market can be expected to impound information about the event (Omer and Shaw, 1991; Brown and Warner, 1985). Notationally,

$$CAR_i = \Sigma\, u_{i\,t} \qquad (3)$$

where the summation is over the number of trading days in the window, and the $u_{i\,t}$ are daily abnormal returns.

In this study, hypotheses are stated in terms of industry-wide reactions, requiring the formation of industry portfolios. The relevant statistic for industry I is:

$$CAR_I = (1/N)\ \Sigma\ CAR_i \qquad (4)$$

where N is the number of firms in the industry.

TABLE 2

NUMBER OF SAMPLE FIRMS, BY INDUSTRY

Panel A: Number (N) of firms per portfolio

	Moody	CRSP	(1)	(2)	N
Retailing	102	62	4	3	55
Steel	24	12	2	-	10
Autos	39	22	6	-	16
Machine Tool/Equipment	229	119	27	7	85
Real Estate/Construction					
Engineering Services	47	23	5	1	17
All Other	130	58	14	2	42
Banks	6,322	211	-	14	197
Publishing/Broadcasting					
Book Publishing	27	16	5	1	10
All Other	78	40	9	2	29
Total, All Firms	6,998	563	72	30	461

Legend

Moody: Number of firms listed under industry heading in Moody's Industrial Manual or Moody's Financial Manual

CRSP: Number of firms for which complete data were available on CRSP

(1): Number of firms deleted for not meeting line-of-business criterion

(2): Number of firms deleted because of potential confounding events

TABLE 2

(continued)

Panel B: Number of firms per portfolio, by event

	1	2	3	4	5	6	7
Retailing	41	52	46	53	38	48	50
Steel	10	7	7	10	9	9	8
Autos	16	12	15	14	13	14	8
Machine Tool/ Equipment	78	75	70	82	71	75	65
Real Estate/ Construction							
Eng. Serv.	14	17	13	17	12	17	14
All Other	35	35	31	40	34	39	34
Banks	187	189	179	189	180	182	N/A
Publishing/ Broadcasting							
Books	9	9	9	9	8	10	7
All Other	27	27	24	28	29	26	22

Because all hypotheses are directional, they are tested using the one-sided t-test. The relevant test statistic is:

$$t = CAR_I \; / \; (\sigma_I n^{½}) \qquad (5)$$

where n is the number of trading days in the event window. When comparison of two industries is required, as in hypotheses H6b, H8, and H10, the appropriate test is the pooled t-test:

$$t = (X_1 - X_2) \; / \; \sigma_p \qquad (6)$$

where the X_i are the CARs for the respective samples. The pooled standard deviation is:

$$\sigma_p = (((n_1 - 1)V_1 + (n_2 - 1)V_2)/(n_1 + n_2 - 1))^{½} \qquad (7)$$

where the V_i are the variances of the time series of the respective CARs, and the n_i are the sizes of the respective samples (Ott, 1988).

The use of the t statistic is discussed by Leftwich (1981) and Brown and Warner (1985). It assumes that the CARs are serially and cross-sectionally independent. Brown and Warner conclude that the test is robust to violations of these assumptions. Collins and Dent (1984) point out that cross-sectional dependence in returns, which often exists when industry portfolios are used, can in theory bias variance estimates downward, resulting in inflated t statistics. However, Bernard (1987) concludes that this

problem is not likely to be severe when daily returns are used. Therefore, while the use of industry portfolios in this study may mean a greater likelihood of cross-sectional dependence, it is assumed that any bias in the t statistics will be immaterial.[9]

The t-test formally requires that the test statistic be normally distributed, but is quite robust to violation of this assumption (Kerlinger, 1986). However, to protect against misspecification of tests due to violation of the normality assumption, appropriate non-parametric tests will also be performed for all hypotheses. The non-parametric tests will be performed on CARs from the one-day windows only.[10] The short one-day window is least subject to confounding events, and these returns are therefore the "cleanest" and most reliable.

Whether the parametric or non-parametric tests should be given greater reliance can be determined by comparing the empirical distribution function of the sample data to the normal distribution. This is done by means of the Kolmogorov-Smirnov and Kuiper tests, for which the NPSP

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[9] The variance estimation procedure used in this study is that recommended by Bernard (1987).

[10] See discussion of event windows in following section.

statistical package is used (Pirie, 1990). For these tests, a rejection of the null hypothesis indicates significant deviation from normality in the sample data. Results of performing these tests on all samples and subsamples are reported in Table 3.

For tests of location difference in two populations (such as hypotheses H6b, H8, and H10), the appropriate nonparametric test is the Wilcoxon Rank Sum. For tests of the null hypothesis that the difference in location (median) of two populations is zero, the statistic

$$W = \Sigma R_j \qquad (8)$$

(where R_j is the rank of the jth member of the smaller sample) has a known, tabled distribution (Hollander and Wolfe, 1973). If the size of both samples exceeds ten, then W* (the standardized form of W) has a distribution well-approximated by the standard normal distribution.

The other hypotheses in this study involve tests of a single population, for which the appropriate test is the Wilcoxon Signed Rank. For tests of the null hypothesis that the median of a given population is zero, the statistic

$$T^+ = \Sigma R_i \qquad (9)$$

TABLE 3

GOODNESS-OF-FIT TESTS FOR NORMALITY

INDUSTRY	DNS*		VNS*	
Retail	.886	(.10)	1.572	(.05)
Steel	.848	(.10)	1.524	(.05)
Auto (All)	.679		1.275	
Auto (Primary)	.554		1.104	
Machine Tool	.965	(.025)	1.723	(.01)
Real Est./Const.	.732		1.132	
Construction	.982	(.025)	1.505	(.05)
Engineering Serv.	.508		.934	
Banks (All)	1.665	(.01)	2.521	(.01)
Banks (Large)	1.488	(.01)	2.331	(.01)
Banks (Small)	1.027	(.025)	1.851	(.01)
Publish/Broadcast	.718		1.231	
Book Pub	.986	(.025)	1.412	(.10)

* DNS is the Kolmogorov-Smirnov statistic and VNS is the Kuiper statistic. P-values for rejection of the null hypothesis of normality are given in parentheses; if no p-value is reported, it is greater than .10. All statistics are as calculated by the NPSP statistical package created by Professor Walter Pirie of Virginia Polytechnic Institute and State University.

(where R_i is the rank of the ith member of the sample, if that member has a positive value), has a known, tabled distribution (Hollander and Wolfe, 1973). If the size of the sample exceeds fifteen, then T* (the standardized form of T^+) is well-approximated by the standard normal distribution.

For purposes of this study, the Rank Sum tests are performed using the NPAR1WAY procedure of SAS. The Signed Rank tests are performed using the UNIVARIATE procedure of SAS. SAS performs only the standard normal versions of these tests, using W* and T* (SAS Institute, 1990a, 1990b). The signed rank statistic reported by SAS is not the T of equation (8), but rather T less its expected value ("centered signed rank") (SAS Institute, 1990a, p. 629). The properties of this statistic are identical to those of W, except that its expected value is zero, and the sign of the statistic therefore indicates the direction (positive or negative) of the central tendency of the sample. All p-values used in the relevant tables are those reported by SAS.

Unlike the t-test of equation (5), these non-parametric tests are not performed on the industry CARs (CAR_I), because it is not possible to perform rank-order tests on a single observation. Rather, the observations are the CARs

of the individual firms in the given industry. The interpretation of these tests is therefore somewhat different than that of the t-test. In particular, the "reaction" of the industry is defined in terms of a predominance of positive or negative reactions (weighted by magnitude of the reaction) by the individual firms making up the industry, rather than by a mean reaction over the firms. This difference should be borne in mind when interpreting the results of the tests.

In addition to requiring no assumption about the distribution of the statistic (normality), non-parametric tests have the advantage of being more resistant to outliers, because they define central tendency as median rather than mean (Hollander and Wolfe, 1973). They can therefore provide a valuable supplement to parametric tests, even when an assumption of normality appears warranted.

The parametric test directly corresponding to the signed rank (rank sum) test is the one-sample t-test (pooled t-test) using the firm CARs as the observations. These tests are performed by the SAS UNIVARIATE procedure (SAS Institute, 1990a, 1990b). They consititute only indirect tests of the research hypotheses, in that the null hypothesis is that the firm CARs are drawn from a

population of mean zero, rather than that the industry CAR is zero. They are therefore less relevant than the t-test of equation (5), and are reported primarily for the sake of completeness, and for comparability with the non-parametric tests.

Other Methodological Considerations

The use of multiple event dates may be viewed as providing multiple tests of each hypothesis. In order to avoid misleading inferences, it is therefore necessary to appropriately adjust the alpha level of each individual statistical test. Keppel (1982, pp. 145-146) states that the appropriate α can be found by solving

$$\alpha' = 1 - (1 - \alpha)^c \qquad (10)$$

where α' is the desired familywise probability of a Type I error over all related tests, and c is (in this case) the number of events. In order to reject the null hypothesis of "no reaction" for an industry as a whole, it is necessary that the p-value for at least one event be less than or equal to the α calculated according to equation (10).

In choosing an event window, one must allow sufficient time for the market to react to new information. However, the longer the window, the greater the danger of "event smearing" due to confounding events (Dyckman, Philbrick, and Stephan, 1984). In view of these considerations, the hypotheses are tested using windows of four trading days (from one day before until two days after the event) and one trading day (day of the event). These should be short enough to minimize the noise factor from confounding events. As an additional safeguard against industry-specific confounding events, the general and financial news columns of the Wall Street Journal were reviewed for all dates within the windows.

Given the maintained hypothesis that the OTC market impounds information less rapidly than the NYSE, a four-day window may be insufficient to encompass the market reaction. Therefore, the hypotheses are also tested using a window of ten trading days (from two days before until seven days after the event). The dates encompassed by each event window are given in Table 1.

The hypotheses specify the expected reaction of the market to the overall process of enacting the TRA, but do not specify the timing of the reaction. Testing each event date individually implies that the reaction will occur

forcefully at a particular event. This assumption is commonly made in event studies of tax legislation (Shaw and Omer, 1991). The model used in this study is also consistent with the situation in which the reaction occurs incrementally at various stages of the legislative process. In this case, a reaction may have occurred over time even though none of the individual events has sufficient impact on market expectations to be statistically significant. To detect such a situation, all tests are also performed on pooled CARs (CAR_p). Specifically,

$$CAR_p = (1/N) \Sigma CAR_i \qquad (11)$$

where the CAR_i are summed over all events. The procedure of pooling CARs over events is followed by Ayres (1987) and by Cutler (1988). The adjustment for familywise error rate (equation (8)) is not necessary when pooled CARs are used because only a single test of the hypothesis is being made.

Once the conference committee version of the bill had passed both houses of Congress, it was certain that the TRA would become law in that form. President Reagan had strongly supported the legislation, and was sure to sign it (Birnbaum and Murray, 1987). Therefore, the seventh event (the President's signature) should have conveyed no news to the market. Reactions on this date are not expected, and including the seventh event in the pooled CARs would merely

introduce noise in this statistic. Therefore, the pooling of equation (11) is performed over the first six events. Pooled analysis over all seven events is also presented for sake of completeness.

Metrics

Brown and Warner (1985), in the classic work on event studies using daily returns, identify and investigate three possible metrics for abnormal returns: market model, market-adjusted returns, and mean-adjusted returns. Each of these metrics is used in this study. They are discussed sequentially below.

Market Model

The basic logic of the widely-used market model approach is that returns to a given security are "abnormal" to the extent that they are not accounted for by variations in the daily return to the market as a whole.

The relationship between the returns to an individual security and those to the market as a whole is typically expressed in the market model:

$$R_{it} = \alpha_i + \beta_i R_{Mt} + e_{it} \qquad (12)$$

where R_{it} and R_{Mt} are observed returns to security i and to the market portfolio, respectively, in period t, α_i and β_i are firm-specific parameters, and e is an error term. This relationship can be used to predict expected returns to a security (R^*_{it}) conditional on the return to the market. It is then possible to identify abnormal returns, defined as:

$$u_{it} = R_{it} - R^*_{it} \qquad (13)$$

The market model of equation (12) is estimated over the seventy trading days ending on the sixty-second trading day before the beginning of each ten-day event window. This procedure is adopted in order to allow an intervening period (in this case, 61 days) in which to estimate the standard deviation of the series of abnormal returns for purposes of the t-test. Seventy days is similar in length to the period used by Ayres (1987) and Downs and Tehranian (1988). It was expected to contain enough observations to provide adequate estimates of the parameters.

The market model is estimated anew for each event because recent research calls into question the standard assumption of beta stationarity over relatively long intervals (Ingram, 1991). Estimating beta at a time close to the event can be expected to provide a more accurate expected return, and therefore a more reliable measure of abnormal returns. The method used (re-estimation for each event) does require the stationarity assumption, but over a shorter time period.

Returns for day t are calculated as dividends (D) plus the change in price (P) over previous day's price:

$$R_{it} = (D + (P_t - P_{t-1})) / P_{t-1} \qquad (14)$$

These returns are available for each firm on the daily NASDAQ returns tape of the Center for Research in Security Prices (CRSP). The customary practice of defining "price" for OTC stocks as the closing bid is followed (Grant, 1980; Keim and Stambaugh, 1984).

The use of daily returns creates a potential econometric problem when some securities are traded only infrequently (Roll, 1981). As pointed out by Scholes and Williams (1977, p. 309), when prices are

> reported only at distinct, random intervals, completely accurate calculation of returns over any fixed sequence of periods is virtually impossible. In turn this introduces into the market model the econometric problem of errors in

> variables. With daily data this problem appears particularly severe.

Scholes and Williams demonstrate mathematically that the ordinary least squares (OLS) parameters calculated from "non-synchronous" data are biased and inconsistent. The implication is that analyses based on the standard market model are unreliable. Both Scholes/Williams and Dimson (1979) have proposed methods for correcting for this problem, as discussed below.

Brown and Warner (1985) argue that biased betas do not necessarily imply misspecification of the market model in an event study. Given stationarity, a bias in beta is offset by a countervailing bias in alpha, so that the OLS model remains an unbiased predictor of expected returns. On the other hand, they point out that the use of nonsynchronous data adds noise to the calculation of abnormal returns, and may therefore impair the power of tests. However, based on the results of a simulation study, they determine that the methods of Scholes/Williams and Dimson "convey no clear-cut benefit" in terms of increased power (p. 18). They conclude that the use of OLS with daily data is appropriate.

Brown and Warner drew their data solely from the NYSE and the AMEX; no OTC firms were included. Because OTC

firms are generally much more thinly traded than NYSE/AMEX firms, it is plausible that the nonsynchronous data problem is more severe in the OTC market, and that therefore the Brown and Warner conclusion does not hold for this market. There is apparently no literature that provides evidence on this question. Therefore, the market model is estimated both in the standard form of equation (12) and with a method that attempts to correct for the non-synchronous trading problem.

The method used is the "aggregated coefficients" method of Dimson (1979). This method has been used in a number of prior studies, including Richardson, Sefcik, and Thompson (1986). Its advantage over the Scholes/Williams method lies largely in making more efficient use of available observations. The latter method discards observations on days for which there is no trade, while Dimson does not.

Dimson points out that nonsynchronous data induce positive serial correlation in returns. He deals with this by incorporating lead and lag terms in the regression. The number of lead/lag periods is a matter of judgment. Dimson uses five in his original paper, while Brown and Warner (1985) use three and Richardson, Sefcik, and Thompson (1986) use one. In this study, three are used in order to enhance comparability with the Brown and Warner results.

The estimation equation (with i subscript suppressed for economy of presentation) is:

$$R_t = \alpha + \beta^{-3} R_{Mt-3} + \beta^{-2} R_{Mt-2} + \beta^{-1} R_{Mt-1} + \beta^{0} R_{Mt} + \beta^{+1} R_{Mt+1} + \beta^{+2} R_{Mt+2} + \beta^{+3} R_{Mt+3} + e_t \quad (15)$$

Expected returns are calculated as:

$$R^*_t = \alpha^* + \beta^* R_{Mt} \quad (16)$$

where

$$\beta^* = \Sigma \beta^k \qquad (k = -3 \ldots +3) \quad (17)$$

Market-adjusted Returns

An alternative to the market model is provided by the market-adjusted return methodology. Abnormal returns are defined as the difference between the actual return to a security on day t and the rate of return to the market on that day. Notationally,

$$u_{it} = R_{it} - R_{Mt} \quad (18)$$

where R_{Mt} is the CRSP equal-weighted NASDAQ index for day t (Brown and Warner, 1985).

It can readily be seen that this model is equivalent to a no-intercept form of the market model, with beta implicitly equal to one for all firms. The assumption is that, in the absence of firm-specific events, each firm will earn the market rate of return. This is clearly an over-simplification, in particular because it ignores the risk differential among firms which is central to the market model. This model is therefore relatively unsophisticated conceptually.

The advantage of this model is that it does not require estimation of the market model. It is therefore more parsimonious in its data requirements, and eliminates the need for assumptions about the stationarity of the market model parameters.

A more sophisticated version of the market-adjusted return metric was introduced by Foster, Olsen, and Shevlin (1984; hereafter, FOS). The FOS version is based on the size effect first documented by Banz (1981) and Reinganum (1981). This size effect takes the form of an inverse relationship between stock returns and firm size (as measured by market value). This relationship, though not adequately explained in theory, is well established empirically. Disregard of the size effect is a weakness of

both the market model and the model of equation (18) (Bernard and Thomas, 1989).

The FOS model, instead of assuming that each firm will earn the market return, assumes that the firm will earn the level of return of firms of similar size. Specifically,

$$u_{it} = R_{it} - R_{Pt} \qquad (19)$$

where R_{Pt} is the equally-weighted mean return for day t for the decile portfolio of which firm i is a member. The size portfolios are formed of all firms listed on the CRSP daily returns NASDAQ tape as of December 31, 1985. "Size" is measured by the market value of outstanding common shares on that date, and sample firms are assigned to portfolios on that basis. Descriptive statistics for the size portfolios are reported in Table 4.

An indication that the size effect may be operating on the OTC market during the time period of this study is provided in Table 5. Mean daily returns to each size portfolio were tabulated over a 71-day period prior to and including the first event window. An ANOVA for differences in the portfolio means was highly significant ($F=7.08$, $p=.0001$). As shown in the table, the effect of firm size on returns is virtually monotonic. Interestingly, however, returns increase with firm size, opposite of the situation found by Banz (1981) and Reinganum (1981). A possible

explanation for this anomaly is that Banz and Reinganum studied risk-adjusted returns, while the analysis in Table 5 uses raw returns. It should also be noted that Banz and Reinganum did not include OTC firms in their studies. It is not clear why either of these differences should result in a reversal of the phenomenon.

In this study, the FOS metric of equation (19), hereafter referred to as "size-adjusted returns" (SAR), is used for all non-bank firms. It has the advantages of the market-adjusted return as discussed above, and in addition controls for the size effect. Because one of the hypotheses (H8) relative to the banking industry predicts a size effect, it is not appropriate to control for a size effect in the abnormal return metric.[11] Therefore, for banking firms, the definition of equation (18) is used.

For purposes of the t-test of equation (5), the standard deviation of the abnormal returns is estimated over the same 61-day period used for the market model.

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[11] Failure to control for a Banz/Reinganum size effect may bias the results of tests of H8 in the hypothesized direction. On the other hand, Table 5 indicates that any size effect in this instance may be in the opposite direction, making the test more conservative. Thus, the nature of any bias from failure to control for size is indeterminate.

TABLE 4

MARKET VALUES OF SIZE PORTFOLIOS

(thousands of dollars)

Decile	N	Mean	Min	Max	NS*
1	386	1443	19	2505	12
2	386	3931	2511	5314	18
3	386	6923	5318	8669	32
4	386	11200	8682	13684	26
5	386	17394	13700	21365	24
6	386	26894	21375	32724	33
7	386	40964	32734	51605	22
8	386	68128	51680	88966	37
9	386	129419	89152	189960	31
10	383	470617	190000	4610656	29

* Number of sample firms in each portfolio

TABLE 5

MEAN DAILY RETURNS TO SIZE PORTFOLIOS

MEAN RETURN	PORTFOLIO
.001758	9
.001690	10
.001376	8
.001205	7
.000866	5
.000840	6
.000150	4
-.000566	3
-.001030	2
-.002712	1

Note: Mean returns are calculated for the 71 trading days beginning 8/26/85 and ending 12/5/85.

Mean-adjusted Returns

The two metrics previously discussed are similar in that they define "abnormal" returns as those that differ from returns to the market (as variously defined). An alternative way of looking at the meaning of "abnormal" returns is to view them as returns that differ from the "normal" level of return for a specific firm. Notationally,

$$u_{it} = R_{it} - R_{iM} \qquad (20)$$

where R_{iM} is the mean daily rate of return for firm i over some time period prior to or surrounding the event date (Brown and Warner, 1985). Conceptually, the mean-adjusted abnormal returns (hereafter, MAR) are returns that are atypical given recent performance of the firm's stock.

In this study, the estimation period for the "typical" mean daily return is 280 trading days, similar in length to the 240 days used by Brown and Warner.[12] The estimation period contains all trading days from May 1, 1985 to the

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[12] The standard deviation of the series of mean-adjusted returns is estimated over this same 280-day period, resulting in 279 degrees of freedom for t-tests.

beginning of the first ten-day event window, and all intervals of at least twenty trading days between windows thereafter.[13] This procedure differs from that used by Brown and Warner, in that they used only a pre-event estimation period. The reason for using the different procedure is that the length of time from first to last event is considerable (almost one year), and one may question whether the "typical" return to the firm would remain stable over such an extended period. The procedure actually used should provide a more reliable estimator of expected firm-specific returns over the relevant period. It would not be acceptable to re-estimate the expected return for each event because the estimation period for some events would include other event dates, thus potentially biasing the expected return (Leftwich, 1981).

A potential problem with the MAR metric was identified in the simulation study performed by Brown and Warner. The problem arises in the case of event clustering (event dates are the same for all firms) such as exists in this study. In this case, they found that the alpha level for tests using the MAR metric is highly inflated (about 25% in their

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[13] The estimation period contains all trading days in the following intervals: 5/1/85 - 11/20/85; 12/30/85 - 5/2/86; 7/7/86 - 8/13/86.

study). Therefore, there is a considerable probability of a Type I error or "false positive", i.e., the finding of abnormal performance where none exists. An adjustment is available to bring the alpha level under control, but Brown and Warner found that this adjustment so reduced the power of the test as to make it virtually unusable.[14]

Another potential problem with the MAR metric is that it does not control for the effect of market-wide events on returns to individual securities. An indication of the possible magnitude of such a problem is given in Table 6, which shows return to "the market" (equal-weighted NASDAQ index from the CRSP daily returns tape) for the seven event windows. Several of the daily or multi-day returns are quite large, although most are small.

The effect of failing to control for the market is indeterminate a priori. In the case where the firm and the market move in the same direction, it may create spurious "reactions", while in the case where the firm and the market move in opposite directions, it may serve to mask a real reaction. In this regard, it is interesting to note that the market reactions on the days of the first six

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[14] Brown and Warner found that the problem of uncontrolled alpha was immaterial for market-adjusted returns, and in all cases where there was no event clustering.

TABLE 6

RETURNS TO THE OVER-THE-COUNTER MARKET

EVENT	CUMULATIVE TEN-DAY	CUMULATIVE FOUR-DAY	ONE-DAY
1	.030435**	.012927*	-.001373
2	.007841	-.005891	-.006138*
3	.009146	.010511*	-.000504
4	.018742*	.004316	.002203
5	.004408	.000122	-.001066
6	.000992	-.009301	-.004901
7	.015347	.006890	.001197

** Different from zero at p=.01 (two-tailed t-test)

* Different from zero at p=.10 (two-tailed t-test)

events are almost uniformly negative. This may have the effect of reducing the power of the MAR statistic to detect positive reactions, while raising the possibility of spurious findings of negative returns.

The MAR metric is used in this study, because it provides an alternative and potentially informative and useful definition of "abnormal return". However, the results of tests using this statistic must be read in light of the considerations discussed above.

The Influence of Risk

The beta from the market model of equation (12) is customarily viewed as a measure of firm-specific risk (Watts and Zimmerman, 1986). Thus, analysis using the market model explicitly controls for the effect of firm-specific risk on returns. A possible disadvantage of using metrics (such as SAR and MAR) not derived from the market model is that risk is not controlled for. In order to determine whether this omission may bias the results of tests using MAR and SAR, tests are performed to ascertain the relationship between these measures of abnormal return and a measure of risk. If it can be shown that they are

essentially unrelated, then no serious problem is posed by failure to control for risk.

It has long been known that market risk (beta) is correlated with various accounting measures of risk (Beaver, Kettler, and Scholes, 1970). For purposes of this test, an accounting risk measure (debt/equity ratio) therefore surrogates for market risk. Debt/equity ratio is a widely-recognized measure of risk, and has been used as a risk measure in previous market-based studies.[15]

It has recently been pointed out (Harris and Raviv, 1990) that the relationship between debt and risk may be more ambiguous than has been thought. Using a model based on the agency theory of Jensen and Meckling (1976), Harris and Raviv argue that debt is a rich source of information about various aspects of a firm, including the quality of its underlying operations. A possible implication of their model is that high levels of debt may be associated with low, rather than high, risk, because high-quality, low-risk firms may be better able to borrow. Thus, debt may provide an ambiguous signal about the riskiness of a firm. As Harris and Raviv point out, this line of thought has not

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[15] This ratio is discussed in a standard text on financial statement analysis (Bernstein, 1974). An example of its use in a market-based study is Zmijewski and Hagerman (1981).

been well explored, and their work is tentative. Therefore, this study will rely on the established theoretical and empirical linkage in using debt/equity ratio as a proxy for risk.

The operational definition of debt/equity ratio (as used by Moody's in its analytical segments) is: all liabilities (other than current liabilities and deferred income taxes) divided by total assets. The ratios were calculated from the most recent balance sheet data provided in the 1986 edition of Moody's, and were expressed as a four-place decimal.

The analyses are performed separately for each of the seven event periods, using the abnormal returns for each firm included in the non-bank sample for each period.[16] Six different measures of abnormal return (both SAR and MAR, for the ten, four, and one-day windows) are used. Two types of analysis are performed, the first being simple correlation between the abnormal returns and the debt/equity ratio. These are computed using the SAS CORR procedure, with Pearson and Spearman options. This

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[16] Banks are excluded from the analysis because their balance sheets are inherently very different from those of non-financial companies, and debt/equity ratio may not be a useful measure of risk in this specialized industry.

statistic can detect whether there is a systematic relationship between abnormal returns and risk at the firm level.

The second type of analysis is OLS regression, using the SAS REG procedure. A systematic relationship is inferred from a significant regression coefficient for the debt/equity variable. The MAR are regressed against debt/equity ratio, firm size (measured as described above), and a series of industry dummy variables. This procedure refines the correlation analysis by looking for a systematic relationship between abnormal returns and risk, while controlling for the effect of industry and firm size. In the regressions for the SAR, firm size is deleted from the independent variables, because the SAR metric is designed to control for size effects. To protect against misspecification of the regression model due to non-normality in the data, the regression are re-run using logarithmic transformations of the non-dummy variables.

CHAPTER FIVE

ANALYSIS OF RESULTS

Market Model

Use of the market model proved to be unproductive. Because of the unforeseen extent of the non-trading problem, most market model regressions were not significant.

Summary results of the market model regressions for non-bank firms are presented in Table 7. Panel A presents results for the regressions for Event Five, which is considered representative of all periods, and includes 214 firms. The results for Event One were similar, and are not separately reported.[17]

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[17] Given the lack of results from the regressions for Events One and Five, it was felt that it would be unproductive to run the regressions for the other five events, and this accordingly was not done.

TABLE 7

SUMMARY OF MARKET MODEL REGRESSIONS

PANEL A: EVENT FIVE (70 observations)

	Simple Regression	Dimson Regression
Percent significant regressions:		
Alpha = .05	20.0	8.4
Alpha = .10	30.7	15.3
Percent of adjusted R-squared exceeding:		
.05	16.3	21.9
Zero	54.0	50.7

PANEL B: FULL DATA SET (381 observations)

	Simple Regression	Dimson Regression
Percent significant regressions:		
Alpha = .05	61.7	47.3
Alpha = .10	70.5	55.3
Percent of adjusted R-squared exceeding:		
.05	17.8	20.8
Zero	84.5	78.8

As shown in Table 7, when using the standard (simple regression) form of the market model (equation (12)), only 20% of the firms produced regressions with F-statistics significant at the .05 level (30.7% at the .10 level). This outcome indicates that, for the great majority of firms, return to the market is not a meaningful predictor of returns to the firm over the 70-day time horizon used. This conclusion is reinforced by the fact that, for over half of the regressions, the adjusted R-squared was negative, indicating that the regression has no value in prediction.

To test for the possibility that these outcomes were due to an insufficient number of observations, the market model was also run on the full data set available: all 264 non-bank firms with 381 observations each. These results are reported in Panel B of Table 7. While substantial improvement is noted, more than one-third of the firms still lack significant regressions, and almost one-sixth have negative adjusted R-squared. Thus, even when one expands the number of observations well beyond that which is normal in market studies, the problem remains.

It seems likely that the root of the problem lies in extensive non-trading. On days when a firm's stock does not trade, the return is zero. Given the mathematics of

ordinary-least-squares regression, a relatively large number of zero values for the dependent variable forces the regression line to the horizontal, leading to a statistically insignificant regression (i.e., one cannot reject the null hypothesis that the slope of the regression line is zero). Thus, one would expect a strong association between frequency of non-trading and insignificant regressions.

Empirical support for this proposition is provided in Table 8. There is a very high (.4352 Pearson, .5748 Spearman) and significant (p = .0001) correlation between the frequency of non-trading and the p-value which indicates the significance of the regression. Frequent non-trading is associated with high p-values (non-significant regression).

A strong size effect is also indicated in Table 8. There is a large (-.2155 Pearson, -.5599 Spearman) and significant (p = .0004, .0001) negative correlation between firm size (as measured by market value) and p-value. Thus, larger firms tend to have smaller p-values, and are more likely to have significant regressions. This is logical given the negative correlation between firm size and non-trading, reflecting the well-known fact that small firms are more thinly-traded than large firms.

TABLE 8

CORRELATIONS AMONG MEASURES OF NON-TRADING,

FIRM SIZE, AND REGRESSION SIGNIFICANCE

PANEL A: PEARSON CORRELATIONS

	PS	PD	MV
NT	.4352	.3696	-.2623
	(.0001)	(.0001)	(.0001)
PS		.7246	-.2155
		(.0001)	(.0004)
PD			-.2948
			(.0001)

PANEL B: SPEARMAN CORRELATIONS

	PS	PD	MV
NT	.5748	.5276	-.5535
	(.0001)	(.0001)	(.0001)
PS		.8955	-.5599
		(.0001)	(.0001)
PD			-.5521
			(.0001)

Variables: NT = percent of non-trading days (zero volume)
PS = p-value of simple regression
PD = p-value of Dimson regression
MV = firm size as measured by market value

Note: parenthetical figures are p-values for test of
H_o : Rho = 0

The correlations reported in Table 8 differ very slightly across industry. There is no noteworthy industry effect.

The extent of the non-trading problem is illustrated by Table 9. The proportion of non-trading days does not differ significantly across industry (Chi-square (20) = 18.02, p = .5860).

Regressions were run only for non-bank firms. Because of the large size of the data set, it was not possible to run regressions for all 461 sample firms at once. It was initially planned to run the bank regressions after the non-bank firms. However, given the results described above, it was not considered necessary to run the bank regressions. As shown in Table 10, the pattern of non-trading for banks is similar to that for non-bank firms, and one would therefore expect similar results (or non-results) from the regressions.[18]

In summary, the use of the market model was not feasible, primarily because of the unanticipated impact of non-trading. As discussed more fully below, the Dimson method did not fulfill its intended function of controlling

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[18] The "full sample" and "reduced sample" terminology is explained below in the context of testing the bank hypotheses.

TABLE 9

FREQUENCY OF NON-TRADING IN NON-BANK STOCKS

PERCENTAGE OF NON-TRADING DAYS	NO. OF FIRMS	PERCENT
None	96	36.4
0 - 10%	73	27.6
10 - 20%	30	11.4
20 - 50%	41	15.5
> 50%	24	9.1

TABLE 10

FREQUENCY OF NON-TRADING IN BANK STOCKS,

BY SIZE OF BANK (ALL EVENTS)

	NUMBER OF BANKS			
	FULL SAMPLE		REDUCED SAMPLE	
PERCENTAGE OF NON-TRADING DAYS	SMALL	LARGE	SMALL	LARGE
None	1	88	1	86
0 - 10%	5	35	4	32
10 - 20%	4	12	4	11
20 - 50%	21	20	15	16
> 50%	6	5	5	5

for the effects of non-trading. Thus, the market model regressions for most firms were unusable, in that they lacked reliability as predictors of expected returns. Using the market model to compute abnormal returns would have resulted in a drastic and unacceptable decrease in sample size.

Use of such a restricted sample also would have biased the sample in favor of larger firms. Such a bias would have had especially adverse consequences for the banking industry, for which a size-related hypothesis (H8) is tested. As indicated in Table 10, there is a very strong size/non-trading relationship for banks.[19] Thus, use of the market model for banks would have resulted in a very small sample of "small" banks.

As an alternative to abandoning the market model, it would have been possible to expand the estimation period beyond the initial 70 days. This alternative was rejected for several reasons. As shown in Panel B of Table 7, even

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[19] Chi-square statistics (4 df) are 57.14 ($p < .0001$) for the full sample and 48.48 ($p < .0001$) for the reduced sample. It should be noted that, while a "size" effect exists for both banks and non-banks, size is defined in terms of assets for the former and market value for the latter. This difference is immaterial in light of the high correlation (.94 Pearson, .92 Spearman) between bank assets and bank market value.

when the estimation period is expanded to 381 days, more than one-third of the firms are lost. The firms thus lost would be primarily small firms, thereby introducing a bias in the sample selection. Also, a great expansion of the estimation period would have resulted in a further attrition of sample firms because of incomplete data. Such an expansion would also have resulted in a huge and unwieldy data set with an inconsiderable offsetting advantage for the increased cost and effort.

As a result of the above considerations, no hypothesis testing is performed using abnormal returns estimated with the market model technique.

The Dimson Method

As discussed in the metrics section, the Dimson method (equation (15)) is intended to control for the effects of non-synchronous trading. Its use in this study did not achieve the desired effect.

Clearly, the most important problem posed by non-synchronous trading in the context of this study is the lack of significant regressions. The most valuable service that the Dimson method could perform would be to control

this problem to a degree, so that more significant regressions could be obtained for the market model.

In actuality, the contrary result was obtained. As shown in Table 7, the Dimson method produced a smaller proportion of significant regressions than the standard market model, in both the large n and small n cases. Thus, not only was there no improvement, but the results were worse using this method. The intuitive explanation for this unexpected outcome is that the Dimson averaging procedure, instead of diluting the effect of non-trading days by averaging them with trading days, tainted the trading days by averaging them with non-trading days. For firms with any substantial amount of non-trading, the effect would be to force the regression line closer to the horizontal, making significance of the regression harder to achieve.

The Dimson method is not uniformly worse than simple regression. As indicated in Table 11, the Dimson method produced p-values equal to or less than the p-values of the simple regression 28.4% of the time (32.6% for the large n case). As might be expected, the greater the p-value of the simple regression, the greater the likelihood that Dimson will be as good or better. This relationship is essentially monotonic, and highly statistically

TABLE 11

DIFFERENCES IN P-VALUES WHEN DIMSON METHOD IS USED

PANEL A: EVENT FIVE

P-value of simple regression	(A) Dimson p-value is: <	(B) Dimson p-value is: >	(C) %<
< .05	3	40	7.0
.05 - .10	3	20	13.0
.10 - .50	18	54	25.0
> .50	37	40	48.1
Total	61	154	28.4

PANEL B: FULL DATA SET

P-value of simple regression	(A) Dimson p-value is: <	(B) Dimson p-value is: >	(C) %<
< .05	51	112	31.3
.05 - .10	4	19	17.4
.10 - .50	12	36	25.0
> .50	19	11	63.3
Total	82	178	32.6

significant.[20] However, the improvement offered by this method is typically not sufficient to be useful. In those cases where the p-value of the simple regression exceeded .10, the Dimson p-value was less than or equal to .10 only 12.7% of the time (25.8% for the large n case).

In summary, whatever its theoretical merits, the Dimson method is empirically unable to deal with the non-trading problem at the severe level at which it is often encountered on the OTC market. Some other solution must be sought if the market model is to be used effectively in OTC studies.

Tests of the Retail Industry Hypothesis

Hypothesis H1 predicts a postive reaction to the TRA by the market with regard to the retail industry.

Modest support for this hypothesis is found in Table 12. Using the SAR metric, a significant positive reaction is found in the four-day window around the sixth event, the

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[20] For Panel A, chi-square (3) = 27.4 ($p < .0001$).
For Panel B, chi-square (3) = 16.7 ($p < .001$).

final passage of the TRA by both houses of Congress.[21] The timing of the reaction appears plausible, in that the sixth event represented the final hurdle to be passed by the bill, President Reagan's signature being assured (Birnbaum and Murray, 1987). Thus, the market reacted positively to news that the changes embodied in the TRA, including the major tax rate cuts, were assured.

This result is not obtained using the MAR metric. The four-day CAR for the sixth event is positive, but not large enough to be significant. Interestingly, the CARs in Panel B tend to be negative, although small, especially in the one-day window. The non-parametric signed rank test reported in Panel B of Table 13 casts further light on this phenomenon. While the ten and four-day windows show no particular pattern, the one-day CARs are uniformly negative. The p-values are typically quite small, although none are small enough to be considered significant using a family-wise alpha of .05.[22] When the data are pooled, however, a highly-significant negative result is obtained.[23]

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[21] T-statistic significant at .05, after making the adjustment suggested by Keppel (equation (8)).

[22] The required p-value is .0073.

[23] The pooled statistic is CAR_P of equation (11).

TABLE 12

RETAIL INDUSTRY

PARAMETRIC TESTS OF H1

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	-.01651	-1.192	-.01044	-1.191	-.00326	- .744
2	-.00362	- .314	.00171	.235	.00533	1.465
3	.00443	.349	-.00507	- .631	-.00423	-1.052
4	.00911	.632	.00692	.760	-.00715	-1.569
5	-.02964	-1.700	-.01048	- .950	.00180	.326
6	.01589	.945	.02753	2.589*	.00372	.670
7	.00200	.098	-.00277	- .213	.00169	.260
ALL(6)	-.02034	- .575	.01018	.455	-.00378	- .338
ALL(7)	-.01833	- .452	.00741	.289	-.00210	- .164
PANEL B: MAR						
1	.01053	.524	-.00125	- .098	-.00567	- .892
2	-.00043	- .021	-.00656	- .516	-.00220	- .346
3	.00532	.265	.00220	.173	-.00593	- .934
4	.02240	1.115	.00957	.753	.00099	.155
5	-.02983	-1.485	-.01107	- .871	-.00047	- .074
6	.04478	2.229	.01404	1.105	-.00361	- .568
7	.00761	.379	.00007	.006	.00224	.352
ALL(6)	.05276	1.072	.00694	.223	-.01689	-1.085
ALL(7)	.06037	1.136	.00701	.209	-.01466	- .872

* Significantly different from zero, $\alpha = .05$ (one-tailed)

TABLE 13

RETAIL INDUSTRY

SIGNED RANK TESTS OF H1

Event	t	p	T*	p
PANEL A: SAR				
1	- .993	.3266	- 60.5	.4398
2	.955	.3443	99	.3724
3	- .913	.3659	-170.5	.0617
4	-2.229	.0301	-230.5	.0401
5	.403	.6895	17.5	.8035
6	.587	.5598	25	.8007
7	.376	.7085	- 48.5	.6443
ALL(6)	- .812	.4203	-102	.3977
ALL(7)	- .475	.6368	- 57	.6373
PANEL B: MAR				
1	-1.806	.0785	-163.5	.0162
2	- .391	.6975	-199	.0348
3	-1.263	.2130	-185.5	.0207
4	-1.546	.1283	-155.5	.0855
5	- .106	.9164	- 40.5	.2820
6	- .567	.5736	-215	.0129
7	.508	.6135	-59.5	.2855
ALL(6)	-2.239	.0293	-314	.0073
ALL(7)	-1.972	.0538	-254	.0320

Note: T* is the centered signed rank statistic

The interpretation of this statistic is that a disproportionately large number of retailing firms had negative CARs when pooled over all event dates. This would seem to indicate an overall negative reaction on the dates of TRA-related events.

This anomalous result appears to be due to a deficiency in the MAR metric. The MAR does not control for the effect of market-wide events (return to the market) on firm returns. As reported in Table 6, returns to the OTC market as a whole were negative, in some cases quite substantially so, for most event dates associated with the TRA. Thus, it may be that non-TRA market-wide factors drove down the raw returns, and hence the MARs, of a large number of retailing firms. This supposition is supported by Panel A of Table 13. The signs of the SARs are mixed, and the pooled signed rank statistic, while negative, is insignificantly small. Thus, the apparent anomaly appears to be an artifact of the MAR metric.

Of even greater interest in Panel A of Table 13 are the signed rank statistics for the third and fourth events (passage of the bill by the Senate Finance Committee, and by the full Senate, respectively). These do not indicate a negative reaction to the TRA as a whole, but they demonstrate quite strongly (α = .0617 and .0401,

respectively) that there was a negative market reaction in retail stocks in response to the third and fourth events taken individually. The SAR CARs are negative over these one-day windows (Table 12, Panel A), but the t-statistics are not significant. The signed rank test may be more powerful in this situation because the retail industry data are not distributed normally (see Table 3).

While not hypothesized, these reactions are not anomalous. The consensus of the commentators was that the net impact of the TRA on retailers was favorable, but this does not deny the existence of individually unfavorable provisions. One of the "last minute amendments" to the bill made by the Finance Committee was the expansion of the uniform capitalization (UNICAP) rules to retailers (Tax Notes, May 12, 1986, p. 553). As explained in Chapter Three, UNICAP was unambiguously unfavorable to affected taxpayers, in that it operated to defer deductions. Thus, it could be expected to adversely impact retailers. The fact that it was added unexpectedly to the Senate version of the bill made it a surprise to the market. As discussed in Chapter Three, the Senate amendment to §453 (installment sales) also was expected to adversely affect retailers, though to a lesser degree.

The behavior of the CARs for the third and fourth event is consistent with the market having previously impounded the positive implications of the TRA for the retailing industry, and then reacting negatively to a surprise amendment unfavorable to that industry. This finding also establishes that UNICAP was expected by the market to have a material impact on firms, despite the lack of interest in the subject by commentators.[24]

In summary, there is modest evidence that the hypothesized positive market reaction in retailing occurred, and that it occurred at the passage of the final version of the TRA by both houses of Congress. In addition, there is evidence that the market reacted negatively to the surprise extension of the UNICAP rules to retailers by the Senate.

Tests of the Steel Industry Hypothesis

Hypothesis H2 predicts a negative reaction to the TRA by the market with regard to the steel industry.

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[24] UNICAP was little noticed by commentators. For example, there is no mention of the subject in the 300-page history of the enactment of the TRA by Birnbaum and Murray (1987).

The evidence supporting this hypothesis is quite persuasive. As shown in Table 14, the CARs for this industry are generally negative across event dates, especially for the one-day window. The individual events do not show statistically significant reactions, but the pooled MAR statistic is significantly negative. The same pattern appears in Table 15. The signed rank statistics are generally negative across events, but none are particularly large. The MAR pooled statistics are significant, and the SAR nearly so. The signed rank test is important for this industry because of indications of non-normality in the sample data (see Table 3).

It is not surprising that, in both tables, the results are stronger for the MAR, given that this metric does not adjust for the negative return to the market as a whole. However, when market-adjusted returns (SAR) are used, the pattern of signs on the CARs is essentially the same. The pooled CARs are no longer significant, but have very low p-values. Thus, there is some indication that the negative CARs are industry-specific, not entirely market-driven.

The pattern of response exhibited by the steel industry is consistent with the model of equation (2), in which successive events may cause revision of subjective probabilities of the eventual passage of legislation.

TABLE 14

STEEL INDUSTRY

PARAMETRIC TESTS OF H2

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	.00959	.427	.00029	.021	-.00036	- .051
2	-.01159	- .446	-.00866	- .526	-.00488	- .593
3	-.02088	- .588	-.01643	- .732	-.00897	- .799
4	-.01223	- .477	.00692	.427	-.00498	- .615
5	-.00697	- .246	.00182	.101	-.00976	-1.088
6	-.01271	- .527	-.01169	- .766	.00416	.545
7	-.03924	-1.391	-.02135	-1.196	-.00128	- .143
ALL(6)	-.05479	- .828	-.02776	- .663	-.02480	-1.185
ALL(7)	-.09404	-1.307	-.04911	-1.080	-.02607	-1.146
PANEL B: MAR						
1	.03447	1.164	.00826	.441	-.00279	- .298
2	-.00826	- .279	-.01904	-1.016	-.01283	-1.369
3	-.01800	- .608	-.00995	- .531	-.00971	-1.037
4	.00235	.080	.01029	.549	-.01693	-1.808
5	-.00774	- .261	.00155	.083	-.01105	-1.180
6	-.01279	- .432	-.02382	-1.271	-.00175	- .187
7	-.02556	- .863	-.01611	- .860	.00026	.028
ALL(6)	-.00997	- .137	-.03271	- .713	-.05506	-2.400#
ALL(7)	-.03553	- .453	-.04882	- .985	-.05480	-2.212*

* Significantly different from zero, $\alpha = .05$ (one-tailed)

\# Significantly different from zero, $\alpha = .01$ (one-tailed)

TABLE 15

STEEL INDUSTRY

SIGNED RANK TESTS OF H2

Event	t	p	T-	p
PANEL A: SAR				
1	- .147	.8859	22	.6880
2	- .975	.3671	20	.1880
3	-1.290	.2444	22	.1090
4	-1.389	.1983	36	.2160
5	-1.567	.1558	40	.0200
6	.499	.6313	19	.6330
7	- .144	.8895	22	.3200
ALL(6)	-1.566	.1518	38	.1610
ALL(7)	-1.243	.2453	37	.1880
PANEL B: MAR				
1	-1.094	.3024	37	.1880
2	-2.591	.0411	25	.0390
3	-1.393	.2130	22	.1090
4	-1.076	.3098	41	.0970
5	-1.680	.1314	35	.0820
6	- .214	.8360	29	.2480
7	.028	.9783	22	.3200
ALL(6)	-2.392	.0404	54	.0020
ALL(7)	-1.801	.1052	45	.0420

Note: The test statistic used is T- (Wilcoxon signed rank statistic) rather than T* (SAS centered signed rank statistic). The small sample sizes make the SAS approximate test inappropriate. The analysis was performed using the NPSP statistical package.

While no single event is important enough to yield a very large response, the cumulative effect is strong. These findings are consistent with the market gradually impounding the negative implications of the TRA for the steel industry as the bill progressed toward eventual enactment.

Tests of the Auto Industry Hypothesis

Hypothesis H3 predicts a negative reaction to the TRA by the market with regard to the auto industry.

There is no support for this hypothesis. Tables 16 and 17 present the results of parametric and non-parametric tests for all auto industry firms in the sample. The CARs are generally small over all windows, without noticable pattern as to sign or magnitude. None of the tests yield statistically significant results.

A possible explanation for the failure to find the hypothesized reaction is the high degree of heterogeneity of the sample. The sixteen firms selected as belonging to "auto industry" consist of three vehicle manufacturers, six manufacturers chiefly supplying parts to auto and truck manufacturers, and seven firms serving chiefly the

secondary auto market (makers and retailers of replacement auto parts). Although all were classified as "automotive" by Moody's, it is apparent that there are substantial differences in the markets served by these firms. These differences suggest that not all sixteen firms may be expected to respond as hypothesized to the TRA.

In particular, it would appear that the seven firms serving the secondary auto market would not suffer from the adverse demand effects generally predicted for the auto industry. Therefore, a better test of H3 can be obtained by examining only the nine firms in the primary market. Results of tests for this subsample are presented in Tables 18 and 19. Again, there is a general failure to obtain any significant result. A higher proportion of the CARs are negative than in Table 16, but they are generally small in magnitude. The signed rank test does not indicate a disproportionate number of firms with negative CARs, but the test lacks power with such a small number of firms.[25]

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[25] The asymptotic signed rank test performed by the SAS UNIVARIATE procedure is technically not appropriate when n = 9, but the pattern of the data makes it highly unlikely that meaningfully different results would be obtained from the small n test.

TABLE 16

AUTO INDUSTRY

PARAMETRIC TESTS OF H3

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	-.00217	- .085	-.01077	- .664	.00540	.666
2	-.00568	- .216	-.00152	- .091	-.00011	- .013
3	.01356	.581	.01226	.830	.01623	2.198
4	.02951	1.243	-.00785	- .523	-.00674	- .898
5	-.00057	- .021	-.00009	- .005	-.00674	- .774
6	.01553	.581	-.00137	- .081	.00611	.723
7	-.04077	-1.404	-.00578	- .315	-.00000	- .000
ALL(6)	.05019	.802	-.00936	- .236	.01416	.716
ALL(7)	.00941	.137	-.01514	- .347	.01416	.650
PANEL B: MAR						
1	.02904	1.033	.00091	.051	.00363	.408
2	.00267	.095	-.00687	- .386	-.00635	- .714
3	.01864	.663	.02334	1.313	.01528	1.718
4	.04391	1.562	-.00551	- .310	.01559	1.754
5	.00827	.294	.00239	.134	-.00872	- .981
6	.01662	.591	-.01144	- .644	.00081	.091
7	-.03130	-1.113	-.00137	- .077	.00189	.213
ALL(6)	.11914	1.730	.00283	.065	.02023	.929
ALL(7)	.08785	1.181	.00145	.031	.02212	.940

TABLE 17

AUTO INDUSTRY

SIGNED RANK TESTS OF H3

Event	t	p	T*	p
PANEL A: SAR				
1	.777	.4495	22	.2688
2	- .032	.9754	3	.8350
3	1.464	.1654	25	.1643
4	-1.168	.2639	- 27.5	.0874
5	- .647	.5299	- 9.5	.5417
6	.911	.3792	13.5	.4263
7	.000	.9999	- 6	.4453
ALL(6)	1.072	.3006	11	.5966
ALL(7)	.998	.3343	8	.7057
PANEL B: MAR				
1	.531	.6035	10	.6322
2	-1.739	.1099	- 21	.1084
3	1.337	.2025	15.5	.3972
4	- .726	.4806	- 23	.1577
5	- .840	.4174	- 14.5	.3396
6	.123	.9041	- 2.5	.9032
7	.165	.8735	- 5	.5469
ALL(6)	.330	.7462	- 5	.8209
ALL(7)	.365	.7204	- 3	.8999

Note: T* is the centered signed rank statistic.

TABLE 18

AUTO INDUSTRY (PRIMARY)

PARAMETRIC TESTS OF H3

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	-.00514	- .191	-.00083	- .049	.00712	.839
2	-.02172	- .746	-.00096	- .052	-.00244	- .265
3	.01939	.738	-.01017	- .612	.00120	.144
4	.00692	.238	-.00518	- .282	-.00315	- .342
5	-.00470	- .173	-.02201	-1.281	-.01266	-1.474
6	.01046	.379	-.00514	- .295	.00977	1.119
7	-.00963	- .244	-.00057	- .023	.00627	.502
ALL(6)	.00521	.077	-.04429	-1.033	-.00016	- .008
ALL(7)	-.00442	- .057	-.04485	- .913	.00610	.248
PANEL B: MAR						
1	.02342	.810	.00964	.527	.00492	.538
2	-.01626	- .562	-.00728	- .398	-.00870	- .951
3	.02075	.717	-.00063	- .034	.00015	.017
4	.02050	.709	-.00295	- .161	-.00256	- .280
5	.00372	.129	-.02013	-1.100	-.01508	-1.648
6	.00745	.258	-.01737	- .949	.00395	.432
7	-.02234	- .772	.00195	.107	.00786	.859
ALL(6)	.05959	.841	-.03871	- .864	-.01732	- .773
ALL(7)	.03726	.487	-.03676	- .759	-.00946	- .391

TABLE 19

AUTO INDUSTRY (PRIMARY)

SIGNED RANK TESTS OF H3

Event	t	p	T*	p
PANEL A: SAR				
1	.989	.3519	10.5	.2500
2	- .485	.6447	- 1	.9375
3	.189	.8554	- 3	.7422
4	- .851	.4231	- 9	.2500
5	- .849	.4348	- 2.5	.6875
6	1.366	.2141	9	.2500
7	.360	.7372	- .5	.9999
ALL(6)	.563	.5891	3.5	.7344
ALL(7)	.684	.5135	2.5	.8203
PANEL B: MAR				
1	.695	.5065	4.5	.6523
2	-1.599	.1609	- 8.5	.1719
3	.027	.9796	- 6	.4609
4	- .255	.8063	- 8	.3125
5	-1.014	.3571	- 3.5	.5625
6	.557	.5947	3	.7422
7	.434	.6868	- .5	.9999
ALL(6)	- .546	.6003	- 5.5	.5703
ALL(7)	- .427	.6805	- 3.5	.7344

Note: T* is the centered signed rank statistic.

Even this subsample may not provide a good test of H3, in that it remains fairly heterogeneous. The sample firms include two heavy truck makers, a manufacturer of light trucks and ambulances, and six parts manufacturers with a great variety of products and markets. It may be that the sample fails to represent that group of firms which the commentators had in mind when speaking of the "auto industry". However, the logic used in developing H3 would seem to apply to these firms, both in terms of tax liability effects and demand effects.

It should also be noted that the relatively small size of the subsample reduces the power of the tests to detect a reaction.

Bearing in mind these caveats about the adequacy of the sample, it appears that the market did not believe that the TRA would have a materially adverse impact on the automotive industry.

Tests of the Machine Tool Industry Hypothesis

Hypothesis H4 predicts a negative reaction to the TRA by the market with regard to the machine tool industry.

There is persuasive evidence to support this hypothesis. The CARs reported in Table 20 tend to be negative across event dates, especially for the shorter four and one-day windows. The six-event pooled CARs for the one-day window are large and negative, as predicted. The results of the signed rank test are strong (Table 21), especially for the MAR, for which four individual events are significant, and the pooled test is highly significant. The strongest reaction appears at the sixth event, the final passage of the bill by both houses of Congress. Considerable reliance may be placed on the signed rank test because of strong indications of non-normality in the machine tool industry data (see Table 3).

It is not surprising that, in both tables, the results are stronger for the MAR, given that this metric does not adjust for the negative return to the market as a whole. However, when market-adjusted returns (SAR) are used, the pattern of signs on the CARs is essentially the same, and the pooled signed rank statistic is still significant. Thus, the negative CARs are industry-specific, not entirely market-driven.

These findings are consistent with the market gradually impounding the negative implications of the TRA for the machine tool industry as the bill progressed toward

TABLE 20

MACHINE TOOL INDUSTRY

PARAMETRIC TESTS OF H4

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	-.01365	- .973	-.00743	- .837	-.00618	-1.394
2	-.01859	-1.403	-.01068	-1.275	-.00303	- .723
3	.00396	.328	-.00017	- .023	.00106	.277
4	-.01682	-1.434	-.00502	- .677	-.00258	- .695
5	.00348	.283	-.01097	-1.410	-.00584	-1.500
6	.00097	.066	.00267	.287	-.00291	- .624
7	.00279	.166	-.00432	- .407	.00409	.770
ALL(6)	-.04064	-1.274	-.03160	-1.567	-.01947	-1.930*
ALL(7)	-.03785	-1.055	-.03592	-1.583	-.01538	-1.356
PANEL B: MAR						
1	.01387	.816	.00220	.205	-.00783	-1.457
2	-.01401	- .824	-.01782	-1.658	-.00951	-1.769
3	.01002	.589	.00896	.833	.00004	.006
4	-.00170	- .100	-.00245	- .228	-.00688	-1.281
5	.00482	.283	-.01224	-1.138	-.00707	-1.315
6	.00046	.027	-.00859	- .799	-.00871	-1.619
7	.01277	.751	-.00023	- .022	.00493	.916
ALL(6)	.01346	.323	-.02993	-1.136	-.03996	-3.035#
ALL(7)	.02622	.583	-.03016	-1.060	-.03504	-2.464#

* Significantly different from zero, $\alpha = .05$ (one-tailed)

\# Significantly different from zero, $\alpha = .01$ (one-tailed)

TABLE 21

MACHINE TOOL INDUSTRY

SIGNED RANK TESTS OF H4

Event	t	p	T*	p
PANEL A: SAR				
1	-2.139	.0356	-226.5	.2618
2	- .680	.4986	145	.4475
3	.221	.8259	164.5	.3393
4	- .880	.3813	-223.5	.3043
5	-1.254	.2140	-165	.3480
6	- .895	.3736	- 77	.6871
7	.905	.3689	121.5	.4314
ALL(6)	-2.142	.0351	-487.5	.0318
ALL(7)	-1.591	.1154	-319.5	.1628
PANEL B: MAR				
1	-2.735	.0077	-419.5	.0358
2	-2.159	.0341	-440	.0191
3	.049	.9608	83.5	.6211
4	- .482	.6310	-163.5	.4531
5	-1.527	.1312	-321	.0655
6	-2.700	.0086	-558	.0026
7	1.073	.2875	26.5	.8641
ALL(6)	-3.681	.0004	-851.5	.0001
ALL(7)	-2.954	.0041	-654.5	.0036

Note: T* is the centered signed rank statistic.

eventual enactment. This behavior is consistent with the expectations model of equation (2).

Tests of the Real Estate/Construction Industry Hypotheses

Hypothesis H5 predicts a negative reaction to the TRA by the market with regard to the real estate/construction industry.

Little support exists for this hypothesis. The CARs shown in Table 22 are generally small, with no apparent pattern in magnitude or sign. None of the t statistics begin to approach significance. The centered signed rank statistics for the MAR (Table 23, Panel B) are all negative (though non-significant), but this appears to be due to a failure to control for the market return. This pattern disappears when the SAR metric is used (Panel A). Indeed, the larger signed rank statistics for the SAR, as well as the statistic for the pooled test, are positive rather than negative. The statistic for the second event is quite large ($T^+ = 249$, $p = .0218$) and positive, but is not significant at a familywise alpha of .05. The pooled statistic is also not significant, although the p-value is

TABLE 22

REAL ESTATE/CONSTRUCTION INDUSTRY

PARAMETRIC TESTS OF H5

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	-.01114	- .640	-.00050	- .046	-.00420	- .763
2	.00944	.530	.00845	.750	.00658	1.170
3	.01104	.667	.00162	.155	.00603	1.153
4	-.02931	-1.742	-.00057	- .054	-.00515	- .967
5	.01526	.769	-.00967	- .771	.00836	1.332
6	.00782	.382	.02336	1.801	.00017	.026
7	-.01028	- .471	.01258	.911	-.00182	- .263
ALL(6)	.00311	.070	.02268	.806	.01180	.839
ALL(7)	-.00718	- .145	.03526	1.128	.00998	.639
PANEL B: MAR						
1	.01234	.635	.00722	.596	-.00607	-1.002
2	.01083	.565	.00023	.019	-.00013	- .021
3	.01387	.724	.00994	.820	.00439	.724
4	-.01955	-1.020	-.00040	- .033	.00087	.144
5	.00913	.476	-.01329	-1.097	.00646	1.067
6	.00378	.197	.01185	.978	-.00547	-.902
7	-.00414	- .216	.01496	1.234	-.00119	-.197
ALL(6)	.03039	.648	.01556	.524	.00006	.004
ALL(7)	.02626	.518	.03052	.952	-.00114	-.071

TABLE 23

REAL ESTATE/CONSTRUCTION INDUSTRY

SIGNED RANK TESTS OF H5

Event	t	p	T*	p
PANEL A: SAR				
1	- .748	.4582	- 28.5	.7801
2	1.540	.1297	249	.0218
3	.988	.3286	27	.7567
4	-1.385	.1717	-122.5	.3348
5	1.870	.0681	143.5	.1180
6	.037	.9705	91	.4629
7	- .350	.7279	-133	.1751
ALL(6)	.917	.3631	212	.1102
ALL(7)	.635	.5283	161.5	.2258
PANEL B: MAR				
1	-1.071	.2896	-116.5	.2506
2	- .029	.9768	-115	.2994
3	.732	.4681	-114	.1865
4	-1.041	.3024	- 54.5	.6689
5	1.453	.1533	13.5	.8846
6	-1.195	.2373	-138	.2640
7	- .228	.8211	-116	.2381
ALL(6)	- .606	.5471	- 66	.6225
ALL(7)	- .608	.5458	- 80	.5505

Note: T* is the centered signed rank statistic.

fairly low (.11). While the non-parametric tests using the SAR metric are insufficient to support a finding of an anomalous positive reaction, they clearly have a tendency in the direction opposite to that hypothesized.

A possible reason for the failure to find the expected results is the extreme heterogeneity of the sample. While all firms appear on the basis of available evidence to be primarily engaged in construction or real estate-related activities, they are nonetheless very diverse. They range from heavy industrial construction companies to home builders, road builders, military contractors, engineering consultants, and real estate developers and managers. Many are engaged in two or more of these activities. In addition, many of the firms have substantial other lines of business, as diverse as computer software, bowling alleys, advertising, railroads, and horse racing. It is likely that this heterogeneity introduces considerable noise into the CARs for this "industry". It is not surprising that the tests reported in Tables 22 and 23 are unable to produce order from this chaos.

In an attempt to reduce the noise level, a subsample was created of firms which may be described as being largely in the field of "construction". Given the available data and the amount of diversification

encountered, it was difficult to operationalize the "construction industry" concept effectively, which may account for the lack of results, as reported in Tables 24 and 25. The only glimmer of statistical significance appears in the signed rank tests for the fourth event using both the MAR and SAR. This provides some indication of a negative reaction for the construction industry in response to the initial Senate passage of the TRA. The use of the signed rank test is especially appropriate because of indications of non-normality in the construction industry data (see Table 3). However, this reaction is not at a level that would support an overall finding of negative reaction in confirmation of H5.

It is instructive to note that refining the sample, thereby reducing the noise, resulted in a generally negative shift (i.e., in the hypothesized direction) in the CARs and the centered signed rank statistics. While this was not sufficient to support the hypothesis, it may indicate that the lack of statistical confirmation is due to "noise" caused by inaccuracy in assigning firms to the industry portfolio.

Hypothesis H6a predicts a negative reaction to the TRA by the market with regard to the engineering services segment of the real estate/construction industry. H6b

TABLE 24

CONSTRUCTION SEGMENT

PARAMETRIC TESTS OF H5

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	-.02058	- .861	.00719	.476	-.00196	- .259
2	-.01376	- .590	-.00994	-.673	.00303	.410
3	.02075	.861	-.00798	-.524	.00356	.467
4	-.03524	-1.821	-.00342	-.279	-.00971	-1.586
5	.02190	.810	-.01679	-.982	.00240	.280
6	.00595	.211	.00465	.260	.00440	.493
7	-.03756	-1.273	-.00543	-.291	-.00373	- .400
ALL(6)	-.02097	- .352	-.02630	-.698	.00172	.091
ALL(7)	-.05853	- .882	-.03173	-.757	-.00201	- .096
PANEL B: MAR						
1	.00427	.174	.01510	.976	-.00396	- .511
2	-.01135	- .464	-.01808	-1.168	-.00362	- .468
3	.02559	1.046	.00094	.061	.00190	.245
4	-.02443	- .999	-.00270	-.174	-.00196	- .254
5	.01673	.684	-.01936	-1.251	.00105	.136
6	.00361	.147	-.00635	-.410	-.00097	- .125
7	-.02654	-1.085	-.00137	-.088	-.00308	- .397
ALL(6)	.01440	.240	-.03044	-.803	-.00756	- .399
ALL(7)	-.01214	- .187	-.03182	-.777	-.01063	- .519

TABLE 25

CONSTRUCTION SEGMENT

SIGNED RANK TESTS OF H5

Event	t	p	T*	p
PANEL A: SAR				
1	- .554	.5871	- 1.5	.9540
2	.181	.8586	19.5	.3714
3	.852	.4084	17	.3517
4	-2.319	.0317	-57	.0328
5	.271	.7898	.5	.9908
6	- .319	.7536	1	.9782
7	- .647	.5283	-17	.3511
ALL(6)	- .643	.5275	- 7.5	.8015
ALL(7)	- .858	.4009	-16.5	.5789
PANEL B: MAR				
1	- .912	.3752	-17.5	.4307
2	- .797	.4369	-26.5	.2247
3	.775	.4511	- 8	.6788
4	-2.024	.0572	-49	.0696
5	.036	.9719	-16.5	.4586
6	-1.061	.3021	-27	.3300
7	- .594	.5621	-11	.5614
ALL(6)	-1.625	.1198	-45.5	.1157
ALL(7)	-1.761	.0935	-45.5	.1157

Note: T* is the centered signed rank statistic.

predicts that this reaction will be less in magnitude than the reaction of the remainder of the industry.

There is no support for H6a. Indeed, when CARs are pooled over all seven events, there is an indication of a positive reaction (see Table 26). This finding occurs using both metrics, and is especially strong over the four-day window. However, it appears to be due to noise introduced by the seventh event; the six-event pooled CARs, while positive, are not large enough to be significant. The evidence does not support a finding of an anomalous positive reaction, but the tendency of the data is clearly contrary to H6a.

CARs for the event day are small and mixed in sign, and provide no sign of reaction (Tables 26 and 27). The signed rank tests in Table 27 show no significant result. This is not surprising, given the pattern of one-day CARs shown in Table 26. It should also be noted that this test lacks power when the data are normally distributed, which appears to be the case for this industry segment (see Table 3).

Hypothesis H6b appears supported by a strong positive difference between the CARs of the engineering services and construction segments. This difference is statistically significant over the four-day window (Table 28). It is strongest for the CAR pooled over six or seven events; this

TABLE 26

ENGINEERING SERVICES SEGMENT

PARAMETRIC TESTS OF H6a

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	.02333	.672	-.00740	- .337	-.01126	-1.026
2	.04562	1.296	.02108	.947	.01316	1.182
3	.01766	.559	.01616	.808	.00167	.167
4	-.03667	-1.152	-.00445	- .221	-.00313	- .311
5	.01707	.485	-.00358	- .161	.01424	1.281
6	.00958	.266	.04630	2.036	-.00481	- .423
7	.02685	.626	.04646	1.712	.00190	.140
ALL(6)	.07659	.917	.06811	1.290	.00988	.374
ALL(7)	.10344	1.106	.11457	1.937*	.01178	.398
PANEL B: MAR						
1	.04758	1.493	.00125	.062	-.01253	-1.243
2	.04751	1.491	.01367	.678	.00662	.657
3	.01728	.542	.02454	1.217	-.00043	- .042
4	-.02778	- .872	-.00510	- .253	.00707	.701
5	.00783	.245	-.00941	- .467	.01115	1.106
6	.00461	.116	.03537	1.755	-.01046	-1.038
7	.02853	.895	.04726	2.345	.00202	.201
ALL(6)	.09704	1.243	.06033	1.222	.00142	.057
ALL(7)	.12556	1.489	.10759	2.017*	.00344	.129

* Significantly different than zero, α = .01 (two-tailed)

TABLE 27

ENGINEERING SERVICES SEGMENT

SIGNED RANK TESTS OF H6a

Event	t	p	T*	p
PANEL A: SAR				
1	- .658	.5219	-10.5	.5314
2	1.264	.2245	22.5	.3060
3	.172	.8662	- 6.5	.6848
4	- .448	.6604	- 3.5	.8808
5	1.504	.1608	16	.2256
6	- .481	.6373	6.5	.7730
7	.122	.9048	-20.5	.2166
ALL(6)	.272	.7894	8.5	.7119
ALL(7)	- .183	.8569	-11.5	.6112
PANEL B: MAR				
1	- .721	.4836	-13.5	.4263
2	.621	.5431	7.5	.7467
3	- .045	.9647	-14.5	.3396
4	- .183	.8570	1.5	.9632
5	1.175	.2647	2	.9097
6	-1.045	.3117	-10.5	.6441
7	.128	.8999	-17.5	.2958
ALL(6)	- .323	.7507	-11.5	.6112
ALL(7)	- .183	.8569	-11.5	.6112

Note: T* is the centered signed rank statistic.

TABLE 28

COMPARISON OF CONSTRUCTION AND

ENGINEERING SERVICES SEGMENTS

PARAMETRIC TESTS OF H6b

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	.02575	.804	-.02225	-1.098	-.00780	- .770
2	.08615	2.647x	.04475	2.174	.01190	1.157
3	-.02776	-1.279	.02262	1.211	-.00837	- .896
4	.01600	.563	.00359	.199	.01396	1.553
5	.00149	.039	.02180	.911	.01202	1.004
6	-.00808	- .223	.03796	1.659	-.00257	- .225
7	.06298	1.516	.04812	1.831	.00649	.494
ALL(6)	.08356	1.041	.10846	2.137x	.01914	.754
ALL(7)	.14654	1.628	.15658	2.750#	.02564	.901
PANEL B: MAR						
1	.02609	.886	-.02192	-1.178	-.00688	- .739
2	.08672	2.923x	.04574	2.437	.01225	1.305
3	-.02831	- .960	.02194	1.176	-.00923	- .990
4	.01612	.547	.00322	.173	.00099	.106
5	-.00151	- .052	.01990	1.076	.01086	1.174
6	-.00696	- .236	.06385	3.425#	-.00275	- .295
7	.05410	1.829	.04520	2.416	.00638	.682
ALL(6)	.09215	1.277	.13272	2.907#	.00524	.230
ALL(7)	.14625	1.875	.17792	3.606*	.01162	.471

* Significantly different from zero, α = .001 (one-tailed)

\# Significantly different from zero, α = .01 (one-tailed)

x Significantly different from zero, α = .05 (one-tailed)

TABLE 29

COMPARISON OF CONSTRUCTION AND

ENGINEERING SERVICES SEGMENTS

RANK SUM TESTS OF H6b

	SAR		MAR	
Event	z	p	z	p
1	- .735	.4626	- .377	.7061
2	- .482	.6295	- .792	.4282
3	- .854	.3933	-1.244	.2136
4	1.418	.1562	1.265	.2060
5	.998	.3181	.554	.5799
6	.412	.6806	.381	.7032
7	- .502	.6154	- .720	.4715
ALL(6)	.440	.6597	.440	.6597
ALL(7)	.675	.4995	- .294	.2691

result is robust to differences in metric. The result also occurs at the sixth event with the MAR. It seems clear that, among construction firms, those whose business was concentrated in the engineering services sector fared better under the TRA than did the others.

The construction industry in general was expected to suffer from the depreciation and ITC provisions of the TRA in two ways: higher tax liability from lower deductions and credits on its own equipment, and reduced demand for its services. On the other hand, lower tax rates would result in lower tax liability, ceteris paribus, and would be a positive influence. Commentators clearly expected the former to outweigh the latter for the entire industry. However, the engineering services segment, being non-capital-intensive, would suffer chiefly from the demand effects; tax liabilities would probably go down as a result of the rate cuts. The evidence provided by Tables 26 - 29 is an indication that, for the engineering services sector, the benefit from the tax cuts equalled or outweighed any expected loss of business from lowered demand. It is an interesting piece of evidence, although hardly conclusive, that the market did not expect the decline in demand for industrial plant to be as severe as did some commentators. The finding of a significant difference between engineering

service and other construction firms is also evidence of the economic effect of the changes in depreciation methods and repeal of the ITC, because these were the only provisions to differentially impact the two segments.

In summary, there is virtually no evidence to support a finding of a negative reaction to the TRA with regard to the real estate/construction industry or any segment thereof. Indeed, there is a mild appearance of a positive reaction for the engineering services segment. The failure to find a reaction may be due to the heterogeneity of the sample and sub-samples. The pattern of results may also be construed as evidence, albeit inconclusive, that the negative impact of the TRA on this industry, particularly in terms of demand effects, was not perceived by the market to be as severe as commentators forecast. The finding of a differential reaction between engineering service and other construction firms is an indication that the depreciation and ITC provisions of the TRA did have a negative economic impact on affected firms.

Tests of the Banking Industry Hypotheses

Hypothesis H7 predicts a negative reaction to the TRA by the market with regard to the banking industry. Hypothesis H8 predicts that (apart from Events Three and Four), the negative reaction of large banks will be greater in magnitude than that of small banks.

In order to test H8, it was necessary to operationalize the construct of "large" and "small" banks. The definition of "large bank" in §585 is one with total assets having adjusted basis of at least $500 million. Because information on the adjusted basis of firms' assets is not publicly available, it was necessary to use a surrogate. The accounting concept corresponding to adjusted basis is book value, and therefore book value of total assets, as reported in Moody's, was used. Banks with total assets of $500 million or more were classified as "large", and all others were classified as "small".

Because of book/tax accounting differences, book value is not necessarily equal to adjusted basis of total assets. In order to provide a margin of error in classification, a reduced sample was created, in which large banks were defined as those having total assets of at least $600 million, and small banks as those having total assets of

$400 million or less. Those in the intermediate range were considered most subject to measurement error and were discarded. All tests were performed on the reduced sample as well as on the full sample. The results for the reduced sample did not differ materially in any way from those for the full sample, and therefore are not reported.

The evidence regarding H7 is mixed. Interestingly, the different metrics tell somewhat different stories.

The MAR generally support the hypothesis. The CARs for the crucial second and sixth events (House bill and final bill) are negative, but not significant, across all windows (Table 30, Panel B). There is an indication of a positive reaction to the fourth event (Senate bill), consistent with the discussion in Chapter Three, although this appears only in the ten-day window.

Because of the non-normal distribution of the data (see Table 3), greater reliance should be placed on the signed rank tests (Table 31, Panel B). These tests strongly support H7. The banking industry as a whole exhibits negative reactions for the second, fifth, and sixth events, and a positive reaction to the fourth event. The statistic for the pooled data is negative as expected, but only

TABLE 30

ALL BANKS

PARAMETRIC TESTS OF H7

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: MARKET-ADJUSTED						
1	-.00018	- .016	.00551	.789	.00481	1.379
2	.00155	.141	.00823	1.179	.00508	1.455
3	.01394	1.264	.00780	1.140	.00319	.914
4	.02481	2.249	.01042	1.494	.00245	.702
5	.01341	1.215	.00479	.687	.00068	.194
6	.00451	.409	.00776	1.113	.00109	.312
ALL	.05803	2.148*	.04467	2.614#	.01729	2.024*
PANEL B: MAR						
1	.01207	1.094	.00925	1.326	.00142	.408
2	-.00550	- .499	-.00362	- .519	-.00255	- .731
3	.00855	.775	.01265	1.813	.00123	.353
4	.02901	2.629*	.00892	1.278	.00320	.917
5	.00296	.269	-.00103	- .147	-.00187	- .537
6	-.00848	- .768	-.00747	-1.071	-.00530	-1.518
ALL	.03861	1.429	.01871	1.094	-.00387	- .453

Significantly different from zero, α = .01 (two-tailed)

* Significantly different from zero, α = .05 (two-tailed)

TABLE 31

ALL BANKS

SIGNED RANK TESTS OF H7

Event	t	p	T*	p
PANEL A: MARKET-ADJUSTED				
1	3.552	.0005	3195	.0001
2	3.634	.0004	2993.5	.0001
3	2.043	.0425	1611.5	.0193
4	1.601	.1111	1224.5	.1034
5	.419	.6759	914	.1886
6	.805	.4220	713.5	.3159
ALL	5.125	.0001	3832.5	.0001
PANEL B: MAR				
1	1.413	.1593	553.5	.4567
2	-1.824	.0697	-2072	.0056
3	.790	.4308	- 745	.2845
4	2.116	.0356	1855	.0134
5	-1.149	.2519	-2477	.0003
6	-3.893	.0001	-3005.5	.0001
ALL	- .930	.3538	-1268.5	.1136

Note: T* is the centered signed rank statistic.

marginally significant because of the countervailing influence of the fourth event.

It is clear from Tables 32 and 33 that these results are driven by the large banks. The pattern of reaction of large banks (Panel B) is essentially the same as for the industry as a whole. This is not surprising, given that about 80% of the banking firms in the sample as classified as "large". On the other hand, the small banks (Panel D) exhibit no reaction whatsoever. Thus, while Tables 30 and 31 appear to support H7 with regard to the entire banking industry, it is more accurate to say that the results apply only to large banks.

The pattern evident in Tables 32 and 33 provides indirect support for H8. Tables 34 and 35 report the results of direct tests of the hypothesis. The CARs are computed as the difference between the small bank and large bank CARs, and are hypothesized to be positive. The evidence in Table 34 (Panel B) supports H8. Significant positive differences are found for the second and sixth events, and for the pooled CARs. This is confirmed by the rank sum tests of Table 35 (Panel B). While the pooled-t statistic is not significant for Event Six in the one-day window, the rank sum statistic is significant. The greater

TABLE 32

BANKS, BY SIZE

PARAMETRIC TESTS OF H7

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: MARKET-ADJUSTED, LARGE BANKS						
1	-.00138	- .113	.00391	.506	.00427	1.105
2	-.00011	- .009	.00656	.850	.00316	.817
3	.01656	1.356	.01035	1.340	.00301	.780
4	.02724	2.231	.01279	1.655	.00264	.684
5	.01351	1.107	.00427	.553	-.00096	- .249
6	.00190	.155	.00261	.337	-.00028	- .073
ALL	.05772	1.929	.04049	2.140*	.01184	1.251
PANEL B: MAR, LARGE BANKS						
1	.01118	.915	.00778	1.007	.00142	.369
2	-.00659	- .540	-.00506	- .655	-.00442	-1.143
3	.01141	.934	.01515	1.960	.00108	.279
4	.03179	2.603*	.01143	1.479	.00343	.887
5	.00346	.283	-.00139	- .180	-.00347	- .899
6	-.01077	- .881	-.01250	-1.618	-.00663	-1.717
ALL	.04048	1.353	.01541	.814	-.00859	- .908
PANEL C: MARKET-ADJUSTED, SMALL BANKS						
1	.00503	.322	.01243	1.259	.00717	1.452
2	.00912	.584	.01581	1.600	.01383	2.801*
3	.00277	.177	-.00226	- .229	.00395	.799
4	.01481	.948	.00070	.071	.00166	.337
5	.01296	.830	.00695	.704	.00745	1.509
6	.01631	1.045	.03106	3.144x	.00727	1.471
ALL	.06100	1.595	.06469	2.674x	.04134	3.417#

TABLE 32

(Continued)

PANEL D: MAR, SMALL BANKS

1	.01593	1.020	.01564	1.583	.00416	.843
2	-.00054	- .034	.00291	.295	.00594	1.203
3	-.00366	- .234	.00202	.204	.00189	.382
4	.01757	1.125	-.00137	- .139	.00227	.460
5	.00091	.058	.00049	.050	.00474	.960
6	.00185	.119	.01524	1.542	.00074	.149
ALL	.03207	.838	.03492	1.443	.01974	1.632

Significantly different from zero, $\alpha = .001$ (two-tailed)

x Significantly different from zero, $\alpha = .01$ (two-tailed)

* Significantly different from zero, $\alpha = .05$ (two-tailed)

TABLE 33

BANKS, BY SIZE

SIGNED RANK TESTS OF H7

Event	t	p	T*	p
PANEL A: MARKET-ADJUSTED, LARGE BANKS				
1	3.237	.0015	2072	.0001
2	2.164	.0320	1509	.0064
3	1.875	.0628	1029	.0412
4	1.785	.0762	985	.0694
5	- .762	.4471	402.5	.4249
6	- .195	.8459	- 28.5	.9570
ALL	3.384	.0009	1887	.0011
PANEL B: MAR, LARGE BANKS				
1	1.397	.1645	523.5	.3420
2	-2.882	.0045	-2026	.0003
3	.685	.4945	- 295.5	.5655
4	1.684	.0943	1406	.0100
5	-1.402	.1629	-1771.5	.0004
6	-4.607	.0001	-2487.5	.0001
ALL	-1.967	.0509	-1678.5	.0043
PANEL C: MARKET-ADJUSTED, SMALL BANKS				
1	1.608	.1170	139	.0205
2	3.732	.0007	195.5	.0003
3	.858	.3971	87.5	.1368
4	.335	.7398	15.5	.8188
5	1.158	.2548	84	.1723
6	2.029	.0509	126.5	.0213
ALL	4.680	.0001	255.5	.0001

TABLE 33

(Continued)

PANEL D: MAR, SMALL BANKS

1	.391	.6982	-	25.5	.6695
2	1.433	.1616		50.5	.3750
3	.391	.6982	-	78.5	.1640
4	1.303	.2011		34	.6003
5	.035	.9724	-	63.5	.2842
6	.182	.8570	-	14	.7982
ALL	1.712	.0957		120	.0582

Note: T* is the centered signed rank statistic.

TABLE 34

COMPARISON OF BANK SEGMENTS

PARAMETRIC TESTS OF H8

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: MARKET-ADJUSTED						
1	.00641	.497	.00852	1.044	.00290	.711
2	.00923	.717	.00924	1.135	.01068	2.622*
3	-.01379	-1.068	-.01261	-1.544	.00094	.229
4	-.01243	- .961	-.01208	-1.477	-.00098	- .239
5	-.00055	- .043	.00268	.327	.00841	2.057
6	.01442	1.119	.02845	3.492#	.00755	1.852
ALL	.00329	.104	.02420	1.210	.02950	2.950#
PANEL B: MAR						
1	.00475	.368	.00786	.963	.00274	.671
2	.00605	.470	.00797	.978	.01036	2.543*
3	-.01507	-1.166	-.01313	-1.607	.00081	.198
4	-.01422	-1.099	-.01280	-1.564	-.00116	- .283
5	-.00255	- .197	.00188	.230	.00821	2.008
6	.01262	.979	.02773	3.403*	.00736	1.808
ALL	-.00841	- .266	.01952	.976	.02833	2.832#

Significantly different from zero, $\alpha = .01$ (one-tailed)

* Significantly different from zero, $\alpha = .05$ (one-tailed)

TABLE 35

COMPARISON OF BANK SEGMENTS

RANK SUM TESTS OF H8

	M*		MAR	
Event	z	p	z	p
1	- .039	.9691	- .652	.5147
2	2.776	.0055	2.138	.0325
3	- .487	.6260	- .846	.3974
4	- .441	.6594	- .347	.7285
5	.558	.5766	- .155	.8766
6	2.258	.0240	2.022	.0432
ALL	3.224	.0013	2.951	.0032

Note: M* is market-adjusted return.

power of the latter test is consistent with the non-normal distribution of the data.

The MAR metric supports both H7 and H8, with some modification. It depicts large banks reacting negatively to both the original House bill (Event Two) and the final passage of the bill (Event Six), as expected. In general, the cumulative reaction over all events is also negative. There is also some evidence that large banks reacted positively to the deletion of the loan loss reserve rules from the Senate bill (Event Four). On the other hand, the MAR metric depicts the TRA as a non-event for small banks, which exhibit no reaction. Thus, H7 is confirmed for large banks, but not for the entire industry. The expected differential reaction between large and small banks is also found.

The use of market-adjusted returns portrays a different picture: that the TRA was "good news" to the banking industry. Panel A of Table 30 shows CARs that are almost uniformly positive. While none of the individual events exhibits a significant reaction, the pooled CARs are significant across all windows. The pooled signed rank statistic for the one-day window is also highly significant (Table 31, Panel A). In addition, the signed rank

statistics for the first two events are highly significant, the third less so, and the fourth marginally.

As with the MARs, the pattern of response for large banks (Panel A, Tables 32 and 33) mirrors that of the full industry, because the large banks are so numerically dominant. The large bank t-statistics are generally not significant, with two marginal exceptions, possibly because of the non-normal data. The more powerful signed rank test finds significant reactions to the first four events, but with monotonically decreasing signed rank statistics. This pattern of reaction may be explained by the market substantially impounding the good news from the TRA at the time of the first two events (associated with the House bill). The deletion of the loan loss reserve rules by the Senate then provided additional good news to the market, though of lesser importance, with regard to large banks. The final bill was ambiguous, providing conclusive evidence that the bill would pass (good news) but restoring the loan loss reserve rules (bad news); the outcome was an ambiguous response by the market.

In contrast with the MARs, there is strong evidence of a positive reaction by small banks (Panel C, Tables 32 and 33). In particular, the cumulative abnormal return over all six events is highly significant (both parametric and

non-parametric tests). The t-test finds the most important events to be the second (one-day window) and sixth (four-day). These findings are confirmed by the signed rank tests.

This pattern of differential response by large and small banks provides indirect support for H8. The results of the direct tests of this hypothesis (Tables 34 and 35) also clearly support H8. The abnormal returns to small banks were significantly greater than those to large banks, when pooled over all six events. The same is true at the level of two individual events (Two and Six). The parametric and non-parametric tests are in agreement, and the results are essentially the same for both metrics (although they are slightly stronger for market-adjusted returns).

The overall picture of the TRA given by the market-adjusted metric is one of good news to the banking industry. For small banks, this good news was unalloyed; for large banks, it was tempered by the unfavorable loan loss reserve rules. The positive reactions of the market were exhibited on the relevant event dates, and cumulatively over events.

With regard to H8, the findings are unambiguous, and robust to changes in the definition of "abnormal return".

An examination of Tables 34 and 35 reveals essentially the same pattern of response for both metrics, and for both parametric and non-parametric tests. Abnormal returns to small banks were clearly greater than those to large banks on the crucial second and sixth event dates, and also over all six events cumulatively. In contrast, no difference was found at the fourth event date, consistent with the fact that the Senate bill did not differentiate between large and small banks. Thus H8, and the reasoning underlying it, is strongly supported. It is clear that the market expected that the new §585, forbidding large banks to take deductions for additions to estimated loan loss reserves, and requiring that existing reserves be taken into income, would, taken by itself, have a materially adverse impact on affected banks. It is important to note that the tests of H8 allow isolation of the expected effect of this provision of the TRA, because it was the only material difference between the treatment of large and small banks in the bill.

With regard to H7, statistically significant results are obtained using both metrics. There is some, but not complete, agreement as to the timing of the reactions; it is clear that the passage of the House bill (Event Two) was important, and that the Senate bill (Event Four) was

important for large banks. However, the two yield opposite conclusions as to the nature of the TRA's impact: use of mean-adjusted returns finds the TRA negative for large banks (as hypothesized), and neutral for small banks; use of market-adjusted returns finds the TRA positive for large banks and highly positive for small banks. Which is "correct"?

Both are correct, given their definition of "abnormal return". As noted in Chapter Four, the two metrics define this construct differently, and thus provide different information about market reactions. The negative returns calculated using the MAR metric indicate that, in response to TRA-related events, large banks tended to generate raw returns smaller than those they had been producing in the pre-TRA period. In particular, the strong results obtained using the signed rank test on this metric indicates that this was true either of a highly disproportionate number of banks, or of a disproportionate number of those banks with relatively large (in absolute value) returns. On the other hand, the lack of response for small banks shows that they tended to earn returns similar to those of the pre-TRA period.

The positive returns calculated using the market-adjusted method indicate that banks earned higher raw

returns than the market as a whole in response to the TRA. It does not necessarily imply that these returns were large in absolute magnitude, since the market returns were generally negative over the relevant periods (see Table 6). Thus, the TRA was good news for banks compared to the market as a whole.

In summary, there are mixed findings regarding H7. The hypothesis is supported, in that returns to large banks in response to TRA-related events were generally lower than the "normal" returns to those banks. However, this finding is mitigated by the conclusion that bank returns in response to those events were generally higher than those to the market as a whole. Banks (especially large banks) fared poorly compared to prior performance, but well compared to other firms. H8 is strongly supported. New §585, apart from other provisions of the TRA, had an adverse impact on affected banks.

Tests of the Publishing/Broadcasting Industry Hypotheses

Hypothesis H9 predicts a positive reaction to the TRA by the market with regard to the publishing/broadcasting industry.

There is no support for this hypothesis. The CARs reported in Table 36 are generally small in magnitude, and mixed in sign. None are statistically significant, except for the MAR ten-day CAR accumulated over all seven events. This CAR is positive as predicted. However, it appears to be due to noise introduced by the seventh event; the CAR pooled over six events is insignificantly small.

The signed rank tests (Table 37) shed no particular light on the question. These tests lack power, because it is likely that the sample data are normally distributed (see Table 3).

As noted in Chapter Three, this industry is very diverse. "Publishing" includes producers of books, magazines, newspapers, and information services. "Broadcasting", according to Moody's, includes radio, television, cable TV, and video production. These business areas differ in important respects, such as reliance on capital equipment, carrying of inventories, and other factors that could result in differential impact of the TRA. It is not practicable to carve out segments of this industry, other than book publishing, because most firms in the sample operate more than one line of business, often several. Many of the firms are diversified communications companies that both publish newspapers or magazines, and

TABLE 36

PUBLISHING/BROADCASTING INDUSTRY

PARAMETRIC TESTS OF H9

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	-.00359	- .219	.00168	.162	.00283	.545
2	.03779	1.769	.02844	2.104	.00645	.954
3	-.02627	-1.564	-.00466	- .439	.00421	.793
4	.03833	2.018	.02396	1.994	-.00211	- .350
5	-.00410	- .162	.00647	.407	.00642	.809
6	.00840	.352	-.01632	-1.081	-.00335	- .444
7	.04201	1.498	.01029	.580	.00330	.372
ALL(6)	.05058	1.012	.03956	1.251	.01445	.914
ALL(7)	.09260	1.627	.04985	1.385	.01775	.987
PANEL B: MAR						
1	.01287	.589	.00683	.494	-.00039	- .056
2	.03750	1.715	.01865	1.349	-.00044	- .063
3	-.02679	-1.225	.00318	.230	.00240	.348
4	.04962	2.270	.02564	1.854	-.00142	- .205
5	-.00768	- .351	.00434	.314	.00474	.686
6	.00265	.121	-.02996	-2.167	-.00858	-1.241
7	.04802	2.197	.01330	.962	.00416	.602
ALL(6)	.06817	1.273	.02867	.846	-.00368	- .217
ALL(7)	.11619	2.009*	.04197	1.147	.00048	.026

* Significantly different than zero, α = .05 (one-tailed)

TABLE 37

PUBLISHING/BROADCASTING INDUSTRY

SIGNED RANK TESTS OF H9

Event	t	p	T*	p
PANEL A: SAR				
1	.686	.4975	40	.5373
2	1.849	.0729	114	.0727
3	.534	.5972	2.5	.9652
4	- .234	.8166	- 99.5	.1353
5	1.336	.1898	43.5	.5191
6	- .540	.5923	15	.8175
7	.442	.6617	- 35.5	.4524
ALL(6)	1.222	.2293	54	.4583
ALL(7)	1.298	.2023	64	.3787
PANEL B: MAR				
1	- .095	.9252	- 33	.6111
2	- .121	.9044	- 30	.6440
3	.303	.7636	- 48.5	.3944
4	- .143	.8870	- 89.5	.1804
5	.986	.3308	44.5	.5095
6	-1.416	.1655	- 72	.2638
7	.553	.5849	- 27.5	.5613
ALL(6)	- .161	.8728	- 95	.1885
ALL(7)	- .083	.9344	- 61	.4016

Note: T* is the centered signed rank statistic.

own television and radio stations. Thus, the sample is inherently heterogeneous, which may account for the inability to find stronger indication of the anticipated reaction.

Hypothesis H10 predicts that the reaction with regard to the book publishing segment will be less strongly positive than that of the remainder of the industry.

There is mixed evidence relative to this issue. Table 38 presents the results of pooled-t tests comparing the book publishing segment with the remainder of the industry. The CARs are the differences between the CARs of the rest of the industry and book publishing, and are hypothesized to be positive. Both the MAR and SAR metrics find the predicted reaction occurring in response to Event Four. These statistics are significant at a familywise error rate of .05, sufficient to support a finding of an overall positive reaction to the TRA, as hypothesized. It should be noted that this finding is made only over the longer (four and ten day) event windows. This may be due to a delayed reaction on the part of the market. It also raises the possibility of undetected confounding events.

Both metrics also find strong negative CARs (meaning that book publishing abnormal returns were substantially higher than those for the rest of the industry) for the

TABLE 38

BOOK PUBLISHING SEGMENT

PARAMETRIC TESTS OF H10

EVENT	TEN-DAY		FOUR-DAY		ONE-DAY	
	CAR	t	CAR	t	CAR	t
PANEL A: SAR						
1	-.00903	- .432	.02150	1.626	.00599	.905
2	.05156	1.965	.02827	1.703	-.00419	- .504
3	.00240	.111	.00335	.246	.00174	.255
4	.07419	3.187*	.04865	3.304*	-.00481	- .653
5	-.04340	-1.441	.02398	1.259	.00804	.844
6	-.06908	-2.190	-.04938	-2.475	-.03574	-3.582#
7	.00850	.233	-.00810	- .350	.00624	.539
ALL(6)	.00663	.106	.07636	1.925	-.02898	-1.461
ALL(7)	.01514	.211	.06827	1.502	-.02274	-1.000
PANEL B: MAR						
1	-.00530	- .200	.02266	1.357	.00553	.663
2	.05402	2.047	.03019	1.808	-.00299	- .358
3	.00577	.219	.00618	.372	.00117	.140
4	.08196	3.101*	.05118	3.062*	.00490	.586
5	-.03736	-1.407	.02542	1.513	.00882	1.050
6	-.06600	-2.513	-.04666	-2.809*	-.03452	-4.156#
7	.01177	.445	-.00555	- .332	.00700	.837
ALL(6)	.03310	.512	.08897	2.176*	-.01709	- .836
ALL(7)	.04487	.642	.08341	1.888	-.01008	- .457

* Significantly different than zero, $\alpha = .05$ (two-tailed)

\# Significantly different than zero, $\alpha = .01$ (two-tailed)

TABLE 39

BOOK PUBLISHING SEGMENT

RANK SUM TESTS OF H10

	SAR		MAR	
Event	z	p	z	p
1	- .677	.4987	- .731	.4650
2	.585	.5583	.877	.3806
3	1.417	.1565	1.516	.1296
4	1.240	.2151	.903	.3667
5	- .499	.6178	- .719	.4719
6	2.581	.0099	2.490	.0128
7	0	.9999	0	.9999
ALL(6)	1.978	.0479	1.978	.0479
ALL(7)	1.785	.0743	1.656	.0976

sixth event (passage of the final bill) over all windows. This finding is the reverse of that which was hypothesized. There is no apparent cause for it in any distinctive feature of the final bill, nor were any industry-wide confounding events detected in the Wall Steet Journal. It is interesting to note, however, that the book publishing segment is quite small (ten firms for the sixth event), and thus the CARs and parametric tests are sensitive to outliers. A review of the underlying returns shows that over 60% of the book segment CAR was accounted for by two firms: Thomas Nelson, and Wiley and Sons. A supplemental extended search was conducted relative to these two firms, in order to determine whether their large returns were due to previously undetected confounding events.[26] No such events were disclosed, but it remains possible that the unexpected positive returns to the book publishing segment are an artifact of some unknown firm-specific events. That this is so is rendered less likely by the fact that the

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[26] Procedures followed were: detailed review of Wall Street Journal for all days in the four-day window; search of Forbes index for all issues in August, September, and October, and scanning of those issues for industry-specific articles; reading of most recent Value Line report on Wiley before and after the event date (Nelson is not covered by Value Line).

same result was obtained using the rank sum tests (Table 39), which are less sensitive to outliers.

In summary, there is no support for H9, a positive reaction for the publishing/broadcasting. The inability to find support for this hypothesis may be due to the heterogeneity of the sample. There are mixed results for H10, book publishing abnormal returns less than the rest of the industry. Some support for this hypothesis is provided by the behavior of returns over longer windows around the fourth event. On the other hand, there is strong contrary behavior associated with the sixth event. At this point, the latter is an unexplained anomaly.

Tests of the Risk/Return Relationship

To test for a systematic relationship between measures of abnormal return and risk, two types of analysis were performed: simple correlation, and multiple linear regression. Table 40 summarizes the results of the correlation analysis. For clarity of presentation, the table shows only the signs of the Pearson correlation coefficients. The Spearman correlation coefficients

TABLE 40

SIGNS OF CORRELATIONS BETWEEN MEASURES OF ABNORMAL RETURN AND RISK

		Event						
Industry	Metric	1	2	3	4	5	6	7
Retail	SAR	-	-	+	-	+	+*	+
	MAR	-	-	+	+	+*	-	+
Steel	SAR	+	+	-	-	+	-	-
	MAR	+	+	-*	+	+	+	+
Auto (All)	SAR	+*	-	+	-	-	-	+
	MAR	+*	-	+	-	-	-	+
Auto (Primary)	SAR	+	-	-	-	+	-*	+
	MAR	+	-	+	+	-	+	+
Machine Tool	SAR	+	+	+	-	-	-	-
	MAR	+*	-	+	-	-	+	-
RE/Const	SAR	-	-	-	-	-	+	-
	MAR	-	-	-	-	+	-	-
Construction	SAR	+	-	+	-	+	-	-
	MAR	+	-	+	+	-	-	-
Eng Services	SAR	-	-	+	-	-	+	-
	MAR	-	-	+	-	+	-	-
Publ/Bro	SAR	+	-	+	-	-	+	-
	MAR	+	-	+	-	+	-	-
Book Publ	SAR	-	+*	+	-	-	+*	+
	MAR	-	-	+	-	+	+*	+

* Correlation coefficient is different from zero, $\alpha = .10$.

Note: All correlations are measured by the Pearson correlation coefficient.

generally have the same sign as the Pearson, and show no materially different pattern of significance; therefore, they add no additional information, and are not presented.

No systematic relationship can be inferred from the information in Table 40. The number of significant correlations does not rise above the level of chance. No industry or segment has a significant correlation for a given metric in more than one event period (with one exception, where there are two). There is generally no consistency in the signs from period to period, an indication that the correlations are the product of random variation.

Relevant results of the multiple regression analysis is presented in Table 41.[27] This table shows the t-statistics and related p-values for tests of the null hypothesis that the regression coefficient of the risk (debt/equity) variable is zero. In no case can the null hypothesis be rejected at an alpha level of .10 or better. The implication is that in no case is debt/equity ratio a significant explanatory variable for abnormal returns, when controlling for firm size and industry.

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[27] Statistics are presented only for the one-day CARs. Those for the ten- and four-day CARs are not materially different.

TABLE 41

TESTS FOR SIGNIFICANCE OF DEBT/EQUITY VARIABLE

	SAR		MAR	
Event	t	p	t	p
1	.581	.5619	.720	.4720
2	-1.640	.1023	-1.477	.1409
3	.938	.3493	.804	.4226
4	-1.479	.1404	-1.347	.1794
5	-.286	.7752	.897	.3709
6	-1.350	.1783	-1.151	.2507
7	-.348	.7282	-.345	.7301

The outcome of both forms of analysis agree in finding no systematic relationship between risk (as measured by debt/equity ratio) and abnormal returns. It may therefore be concluded that failure to control for risk would not be expected to significantly bias the results of the analyses performed in this study. This conclusion is subject to the assumption that debt/equity ratio is an adequate proxy for risk, as discussed in Chapter Four. If this assumption is not warranted, it may be that the failure to find a relationship between debt/equity ratio and abnormal returns is due to the ambiguity of the relationship between the ratio and risk.

CHAPTER SIX

CONCLUSION, LIMITATIONS, AND IMPLICATIONS FOR FUTURE RESEARCH

Conclusion

Based upon the evidence presented in Chapter Five, several conclusions may be drawn about the economic consequences of the Tax Reform Act of 1986. First, the market reacted negatively with regard to the steel and machine tool industries. From this, one may infer a loss of wealth by individuals who directly or indirectly owned stock in firms in these industries. The magnitude of this loss of wealth is discussed in Appendix D.

These reactions are also evidence of allocation effects. Following the model in equation (1), a negative market reaction is caused by expectations of decreased future after-tax cash flows from investments by a firm.

Such a change in after-tax return would lower the marginal efficiency of investment, thus reducing the firm's demand for new investment (Koutsoyiannis, 1982). When the reaction is industry-wide, a reallocation of economic resources away from investments in the industry is implied. Therefore, one may conclude that the TRA caused the United States economy to devote a smaller proportion of its resources to steel and machine tool production than would have been the case under the prior tax regime.

At the same time that Congress was producing the TRA, it was also deliberating upon "sweeping" foreign trade legislation that was a "major legislative priority" of the Democratic Party (Pressman, 1986a, pp. 991, 994). The primary announced purpose of this legislation was to enhance the international competitiveness of, and prevent disinvestment in, certain identified industries, among them steel and machine tools.[28] A major piece of protectionist legislation passed the House in May of 1986 by a large

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[28] Pressman (1986a and 1986b) gives information about the legislative history of the bill. A representative example of the reasoning and interests underlying the bill is Hatsopoulos (1986). The need of the steel and machine tool industries, among others, to modernize in the face of international competition has been widely discussed (see, e.g., Nasar (1988), Vernon (1986), and Pine (1986)).

margin, with by-partisan support, and was defeated in the Senate only by the strong opposition of President Reagan (Pressman, 1986b). While the President opposed formal protectionist legislation, he at the same time asked four foreign countries to restrict their exports of machine tools to the United States, because domestic industry was unable to compete (Pine, 1986).

It is ironic that these Congressional and Presidential actions were contemporaneous with the TRA, which had a foreseeable contrary effect with regard to major industries.[29] The simultaneous pursuit of directly conflicting goals provides an interesting commentary on the caliber of federal economic policy-making.

Second, there is some evidence of the hypothesized positive market reaction with regard to the retailing industry. This occurred at the final passage of the TRA. However, the adverse impact of UNICAP and the installment sale changes on this industry had an offsetting effect. The overall pattern of reaction provides no evidence of a net windfall to shareholders of retail industry stocks.

Third, there is no evidence of the predicted negative reactions with regard to the real estate/construction and

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[29] The foreseeability of the effects is demonstrated by the market reaction.

auto industries. The failure to support the hypotheses must cast some doubt upon the widely-expressed predictions that these industries would be adversely affected by the TRA.[30] However, little weight should be placed on this analysis because of the difficulty in creating portfolios representative of these industries. The samples have considerable diversity in lines of business, both across and within firms. The consequent noise in the portfolio returns may well account for the failure to find significant reactions.

Fourth, the market reaction reflects the differential treatment of large and small banks by the TRA. While the overall impact of the legislation on banks is obscured by contradictory findings, the differential impact among banks is clearly and strongly supported. The TRA served to increase after-tax cash flows of small banks relative to those of large banks. It is plausible to infer that the competitive position of small banks would be enhanced as a result. For example, small banks could increase their deposit bases by offering higher interest rates to

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[30] As pointed out in Chapter Five, the nature of the firms traded OTC resulted in a sample that was not representative of primary automobile manufacturing. The results of this study should not be extended to inferences about auto manufacturers.

depositors and still earn the same after-tax rate of return as large banks.

The inconclusive nature of the findings makes it difficult to assess the overall impact of the TRA on banks. The widely-predicted negative impact is not clearly and unambiguously exhibited by the data analysis. This is surprising, considering the vehemence with which dire contemporaneous predictions were made by such eminent authorities as the chairmen of the Federal Reserve Board and the FDIC. It is not clear that the market shared these apprehensions.

Finally, this study was able to isolate the independent effects of two material provisions of the TRA, apart from the overall effect of the law. This was possible because the Senate bill altered the terms of the House bill substantially in respect to these provisions, and a differential reaction to the two versions of the bill within a single industry may be considered a reaction to the altered provision. From this analysis, it is possible to conclude that inclusion of the loan loss reserve disallowance of §585 in the TRA had a materially adverse effect on large banks (i.e., those affected by the rule). Without this provision, the TRA would have been favorable (or at least neutral) to the banking industry as a whole.

It can also be concluded that extension of the uniform capitalization rules of §263A to retailers had a materially adverse impact on the retail industry.[31]

Additional evidence as to the impact of §263A is provided by indications that book publishers (affected by UNICAP) fared worse than other publishers and broadcasters. However, this evidence is offset by unexplained contradictory findings, and its reliability is therefore reduced.

It should be noted that both §263A and §585 are accounting provisions that affect only the timing of deductions, and therefore the timing of firms' income tax payments. Consistent with the model of equation (1), it is clearly established that the timing effect of these provisions was, by itself, sufficient to cause an observable decline in the stock prices of affected firms. This finding demonstrates the substantive importance of tax accounting rules.

In addition to the above findings with regard to the economic consequences of the TRA, this study makes two contributions to the event study literature.

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[31] A portion of this reaction may have been due to the installment sale changes. It is not possible to distinguish the effects of the two concurrent changes.

First, it is found that infrequent trading in many over-the-counter stocks creates a problem when using OTC firms in event studies with daily returns. Relatively frequent zero-trade, zero-return days make it infeasible to use the standard market model to estimate abnormal returns. The researcher must either use a sample of considerably reduced size, subject to a size bias in selection, or adopt a different and less widely used metric for abnormal returns. The method suggested by Dimson (1979) for dealing with the non-trading problem is not effective for non-trading of the magnitude encountered with small firms on the OTC market.

The second contribution relates to identification of relevant events, which Manegold and Karlinsky (1988) identify as an important problem inherent in studies involving political events. As noted in Chapter Three, there is currently little guidance in theory or the empirical literature for determining relevant event dates for tax legislation.

The pattern of response encountered in this study suggests that it is the formal votes of the houses of Congress, as opposed to committee votes, that are regarded as most meaningful by the market. This pattern is shown in Table 42, which lists the event dates for which significant

reactions were found for each hypothesis.[32] The table is dominated by Event Six, which is the final passage of the bill by both houses of Congress. This indicates that the market to some extent reserved acting on the provisions of the TRA until passage had actually occurred. At this point, enaction of the legislation was assured, because the bill had the support of the President. Further, it was not until final passage that the ultimate terms of the measure could be known with certainty.

The only other event with more than a single significant reaction is Event Two, passage of the initial bill by the House. This event was a signal to the market that major tax legislation was a distinct possibility, and provided an indication of the direction the legislation would take. In contrast to these reactions, there is little response to the committee votes. This is an indication that the market does not regard these votes as conveying substantial information about the probability or content of a tax bill.

Also noteworthy is the frequency with which significant reactions are detected in the pooled CARs. This finding supports the expectations-revision model of equation (2),

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[32] "Significance" is defined by a familywise Type I error rate of .05.

TABLE 42

SIGNIFICANT EVENT DATES FOR THE TAX

REFORM ACT OF 1986

	Event							
Hypothesis	1	2	3	4	5	6	7	Pool
1						St		Mn
2								Mtn
4						Mn		StnMtn
6b		StMt				Mt		StMt
7	Sn	SnMn			Mn	Mn		SnMtn
8		StnMt				StMt		StnMtn
10				StMt		StnMt		SnMtn

Events:

1 House Ways and Means report
2 Initial House passage of bill
3 Senate Finance Committee report
4 Initial Senate passage of bill
5 Conference Committee report
6 Final passage of bill by both houses
7 Signing of bill by President

Legend:

Metrics:

S Market-adjusted
M Mean-adjusted

Tests:

t t-test
n non-parametric

in which each event in the legislative process represents a further step toward passage of the legislation, and therefore can cause revision of subjective probabilities of passage. Even though the relevant content of the bill may not change from event to event, there is potential information content in each event because of this revision of probabilities, and the consequent revision of expected values from the legislative change.

Finally, it should be noted that this study finds evidence (via stock price changes) of economic effects of the Tax Reform Act of 1986, in contrast to Cutler (1988), the only similar study to date. There are several possible explanations for the differing outcomes. First, sample firms for this study are drawn from the over-the-counter market rather than from the major stock exchanges. Statistically significant reactions may be more readily found in the OTC market, as discussed below. Second, this study may use more carefully constructed industry portfolios, resulting in a clearer focus and less noise within portfolios.[33] Third, as discussed above, this study

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[33] Cutler does not make clear his method of assigning firms to industry portfolios. His industries seem to follow SIC classifications (p. 1112), which have been criticized as inaccurate. His footnote 7 refers to a requirement that firms have at least 50% of their sales from "manufacturing", but this does not deal with the problem of diversification

was able to some extent to overcome the "noise" inherent in such a far-reaching bill by isolating the effects of particular provisions of the TRA. Fourth, this study was more comprehensive, examining six legislative events in contrast to two examined by Cutler. This difference is especially important in light of the apparent significance of Event Six, the final passage of the bill, which was not examined by Cutler.

One of the motivations for Cutler's study was to contribute to the debate over the incidence of the corporate income tax. A possible inference from his finding of no stock market reaction to the TRA is that firms are able to shift the tax burden (e.g., to employees or customers). In contrast, this study finds stock price reactions in several industries, which implies that, at least to some material extent, the corporate income tax is not shifted, but is borne by the shareholders.

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across lines of manufacturing.

Limitations

Analysis and inference in this study is made at the industry level. Thus, adequate operationalization of the "industry" construct is essential. If the industries used are not meaningful, or if firms are not properly assigned to portfolios, the validity of the study is adversely affected. This consideration is more important in light of the frequent unreliability of published industry classifications, such as SIC codes or Moody's. While particular care has been taken in the creation of the portfolios, lack of available information and clear industry definitions leave room for error.

It is instructive to note that the three research hypotheses not adequately supported involved the three industries (automotive, real estate/construction, and publishing/broadcasting) that were most heterogeneous, and posed the greatest problems in operationalizing the industry construct. The inability to clearly define these industries in operational terms and to select an appropriate sample is a likely explanation for the failure to support these hypotheses.

Another limitation on this study is inherent in all event studies. The methodology can detect only those

reactions that occur with considerable force over a relatively short window of time. When there is a relatively long lead time before an event, the market can impound the implications of the event gradually over this time period. The market reaction, though real, is thereby so diluted as to be undetectable. There is reason to believe that tax legislation, because of the protracted and public nature of the process, is especially subject to this sort of dilution (Cutler, 1988; Plummer and Robinson, 1990). Thus, market reactions to the TRA may have occurred which this study was unable to detect.

It should be noted that the TRA poses particular problems for an event study. It was the broadest and most comprehensive piece of tax legislation in history, with a multitude of important provisions, affecting virtually all firms in some way. Thus, it is possible that many or even most stock returns over the event windows were influenced by the TRA, and that therefore returns to the market as a whole were also influenced by it. This seems quite likely from the pattern of returns to the market shown in Table 6. It is generally assumed in event studies that return to the market is unaffected by the event being studied, and that firm-specific market-adjusted returns (such as market model residuals or the SAR used in this study) are a more or less

pure distillation of the reaction to the event. This assumption may not be warranted in the case of the TRA or any other event of pervasive impact. An indeterminate bias may therefore exist in the SAR metric, limiting the validity of the conclusions to be drawn from its use.

Two other limitations, discussed more fully above, are significant enough to bear repeating. First, as discussed in Chapter Two, any conclusions about distribution effects are subject to caveat. Such conclusions apply only to shareholders in specific industries, taken as a group, ceteris paribus. They do not take into account the effect of portfolio diversification at the level of the individual investor. Second, the usefulness of the MAR is subject to weaknesses inherent in that metric, as discussed in Chapter Four.

Implications for Future Research

Several of the findings of this study may be useful to scholars pursuing research on the economic consequences of tax legislation, or to others using the event-study methodology.

First, this study adds to the growing body of research demonstrating that tax legislation has economic effects that can be ascertained using market-based methodology. It re-emphasizes that this can be an important line of inquiry, providing evidence of potential use in public policy deliberations.

Second, a comparison of the results of this study with that of Cutler (1988) indicates that use of over-the-counter firms can prove fruitful in event studies. The rationale for using OTC firms, discussed more fully in Chapter Four, appears to be sound. While this study does not provide direct evidence as to the relative efficiency of the OTC and major stock exchange markets, it is suggestive that significant reactions were detected here, while none were found by Cutler. It may be that the gradual impounding of developing events over time, referred to above as restricting the ability of event studies to detect reactions, occurs less rapidly and consistently in the OTC market, thus making overt reactions to specific events such as legislation more likely.

Third, researchers wishing to make use of OTC firms in market-based studies must be aware of the non-trading problem, and the difficulties it poses in estimating abnormal returns. The feasibility of using the standard

market model is reduced, and alternative metrics have weaknesses. The method suggested by Dimson (1979) is not effective in dealing with the problem.

Fourth, the results summarized in Table 42 add to our knowledge of the pattern of reaction to tax legislation. Although the finding is by no means conclusive and requires replication, it appears that the market reacts most strongly to formal votes of houses of Congress, and especially to the final passage of the bill. In addition, the use of pooled CARs allows the researcher to detect a reaction occurring incrementally over the course of the legislative process. This knowledge may aid researchers in developing more efficient research designs in the future.

Finally, this study may serve as a basis for future research. Two methods of extending it bear mention here.

One line of extension would be to go beyond industry effects and look at other characteristics of firms that determined market reaction to the TRA. It has been shown that industry effects were important, but it is also clear that, within industries, there were often widely differing reactions among firms. Other possible factors for investigation include extent of foreign operations, extent

of institutional holdings, and existence of net operating loss carryovers.[34]

Another possible extension would be to attempt to determine the magnitude of the economic impacts found in this study. For example, how big were the aggregate wealth transfers resulting from the TRA? While there are sizable methodological problems in making such estimates, the answer would doubtless be of interest to policymakers and analysts, as well as to scholars. Aspects of extending this study in this manner are discussed in Appendix D.

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[34] The importance of foreign operations in reaction to the TRA is suggested by the research of McGowan (1988).

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APPENDIX A

JOURNALISTIC SOURCES

In formulating and validating the research hypotheses in this study, I relied heavily on contemporaneous journalistic analysis, as explained in Chapter Three. Greatest emphasis was given to the Wall Street Journal, in which I read more than fifty articles, and Tax Notes, in which I read more than one hundred articles. This appendix presents in tabular form a list of the articles which I found to be most useful in the hypothesis-generation phase of the study. The tables below list for each article the date of publication, the page on which the article begins, the primary subject of the article, and the primary source(s) of the opinion/analysis in the article. If no source is listed, none was given in the article. A source listed as "staff" refers to the staff of the periodical itself.

WALL STREET JOURNAL

Date	Page	Subject (source)
4/2/86	6	Banking (Seidman, chairman, FDIC)
4/3/86	6	Banking (Volcker, chairman, FRB)
5/9/86	3	Winners/losers
5/12/86	3	Capital investment (CEOs)
5/12/86	37	Banking (economists)
5/14/86	3	General (analysts, CPAs)
5/14/86	11	UNICAP (CPAs)
7/2/86	29	Winners/losers (CEOs)
8/18/86	8	General (staff)
8/18/86	9	Winners/losers
8/19/86	6	General (analysts, CPAs)
8/25/86	8	Machine tools (CEOs)
9/29/86	14	General (staff)

TAX NOTES

Date	Page	Subject (source)
10/14/85	127	Housing (lobby)
11/25/85	782	Machine tools (lobby)
12/2/85	881	Heavy industry (economists)
1/20/86	192	General (Congressional staff)
1/20/86	259	Depreciation (economists)
2/3/86	383,385	Capital investment (economists)
2/10/86	486	Banking (Congressional testimony)
2/17/86	580	General (Chamber of Commerce)
3/17/86	1096	General (staff)
3/31/86	1315	Retailing
4/7/86	8	Banking (Volcker, chairman of FRB)
5/19/86	647	Real estate (Congressional staff)
6/2/86	873	Winners/losers (Hulten, economist)
6/30/86	1337	General (staff)
8/18/86	681	Winners/losers (Price Waterhouse)
9/1/86	852	Consumer interest deduction
9/8/86	940	LTCG repeal (economists)
9/27/86	1124	Economic effects (economists)
10/20/86	233	Real estate (Coopers and Lybrand)

APPENDIX B

SAMPLE FIRMS

Retail

Elder Beerman
D. H. Holmes
Jacobson Stores
Mars Stores
Sage Allen
Stuarts Department Stores
Bayly
C & R Clothiers
Clothestime
Dressbarn
Evans
Judy's
Merry Go Round
Paul Harris
S & K Famous Brands
Z & Z Fashions
Begley
Big B
Bruno's
American Midland
Marsh
Medicine Shoppes
Aaron Rents
Cochrane Furniture
Cousins Home Furnishings
Designhouse
Haverty Furniture
Lane
J. Michaels

Rhodes
W. Bell & Co.
Casey´s General Stores
Dairy Mart
Danners
Dollar General Stores
Farm Fresh
Wag´s Stores
Cache
Charming Shoppes
Color Tile
Crown Books
Deb Shops
E & B Marine
Hechinger
Helen of Troy
Michael Stores
National Lumber
Oshman Sporting Goods
Parisian
Stereo Village
Tennis Lady
Toys Plus
Crazy Eddie
Highland Superstores
Sportecular

Steel

Amcast
Anadite
Bowline
UNR Industries
Apollo Industries
Wolverine Technologies
Laclede
Oglebay Norton
Precision Castparts
Worthington Industries

Auto (primary)

Collins Industries
Mack Truck
PACCAR
Douglas and Lomason

Harvard Industries
Trico Products
Filtertek
New York Testing Laboratories
Walbro

Auto (secondary)
Champion Parts
Chief Automotive Systems
Crown Auto
Mr. Gasket
Republic Automotive Parts
TBC
Trak Auto

Machine Tool

AEC
Advanced Monitoring Systems
Denning Mobil Robotics
Duriron
Energy Factors
Equipment Company of America
Helix Technology
Lindberg
Maxco
Morehouse Industries
Paul Mueller
Nordson
Robbins & Myers
Robotic Vision Systems
Tylan
Up Right
Valley Forge
Central Sprinkler
Amistar
Boston Digital
Cross & Trecker
Flow Systems
Kaydon
Kulicke & Soffa
Lawson Products
MHP Machines
Simpson Industries

Arts Way Manufacturing
Athey Products
Brenco
Britt Technology
Franklin Electric
Goulds Pumps
Transact International
JLG Indutries
Kalvar
Kevex
Mine Safety Appliances
Newport
Oilgear
Photo Control
Positech
Prab Robots
Taylor Devices
Tennant
Varlen
Washington Scientific Industries
Ross Industries
Dewey Electronics
Dickey-john
EIL Instruments
Engineering Measurement
Ensun
Finnigan
Geotel
GigaTronics
Hathaway
Humphrey
Laser Precision
Liebert
Mast/Keystone
Moore Products
Numerex
OI
Optical Coating Laboratories
Research Inc.
Superior Electric
TSI
Zygo
QMax Technology Group
Scientific Measurement Systems
Technology Research
Accuray
Andros Analyzers
Austron

Cade Industries
Cybermedic
Environmental Techtonics
Epsco
H & H Oil Tool
Haber
Kaman
Millipore
New Brunswick Scientific
Petroleum Equipment Tools

Construction (Primary)

ACMAT
American Aggregates
Bird, Inc.
Cascade
Welbilt
Cronus Industries
Deltak
Firecom
Genova
Industrial Acoustics
P & F Industries
Justin Industries
Manitowoc
Northwestern States Portland Cement
Patrick Industries
Super Sky
Supradur
Thermal Profiles
Thermal Systems
W. W. Williams
Williams Industries

Other Construction/Real Estate

Bohemia
Dynamic Homes
Equinox Solar
L. B. Foster
Groff Industries
Kasler
Palmer Lewis
Mor Flo
Ocilla

Reading
Brandywine
Hollywood Park
Universal Medical Buildings
Bristol Gaming
Abrams
American Recreation Centers
Stacy
Chicago Pacific
Compucon
ERB Lumber
Research Industries

Engineering Services

Algorex
Caci
Gilbert Associates
A. D. Little
Mechanical Technology
Norstan
Perceptronics
Possis
Questech
RCM Technology
Refac Technology Development
STV Engineers
General Devices
General Physics
Gulf Applied Technology
National Technical Systems
Oceaneering

Banks (Large)

PNC Financial
Bank of New England
Citizens and Southern
National City Corp.
Corestates Financial
Allied Bancshares (Texas)
Valley National
Comerica
Sovran Financial
Society Corp.
Ranier

Ameritrust
U.S. Bancorp.
Northern Trust
Michigan National
Boatmen´s Bancshares
Manufacturer´s National
Mercantile
Fidelcor
Shawmut
BayBanks
Huntington
Meridian
Florida National
Security Corp. (Delaware)
Centerre
Riggs National
Third National Corp.
United Banks of Colorado
Commerce Bancshares
Continental Bancorp.
State Street Boston
Dominion
First Maryland
Bancorp Hawaii
Indiana National
First Kentucky
South Carolina National
SouthTrust
American Fletcher
First Florida
Old Kent Financial
Marshall and Ilsley
Central Bancorp
United Missouri
Equitable Bancorp
First Commerce Corp.
First Alabama
Howard Savings
Cullen/Frost Bankers
First Jersey
First Hawaiian
First National (Cincinnati)
Pennbancorp
Zions
Central Fidelity
Central Bancshares South
Moore Financial Group
Colorado National

Deposit Guaranty
National Bancshares (Texas)
Fifth Third
Dauphin Deposit
Marine Corp.
Mercantile Bankshares
Affiliated Bankshares of Colorado
Fourth Financial
First Empire State
Conifer Group
City National
Society for Savings
Pacific First Financial
First Bancorp (Ohio)
Bank South
Sunwest Financial
United Bancorp (Arizona)
First Capital
Wilmington Trust
Midwest Financial
Multibank Financial
Citizens Banking (Michigan)
Security Bancorp (Michigan)
Summit
Banks of Iowa
CityTrust
Valley Bancorp (Wisconsin)
New Jersey National
Apple Bank for Savings
National Community Bank
Independence Bancorp
Southern National
Puget Sound
United Carolina
Valley National (New Jersey)
Victoria
Imperial
Indian Head
Citizens Financial Group
First Eastern
Keystone Financial
Statewide
Amoskeag
CB&T Bankshares
Central Banking
Ohio Bancorp
Bank of Delaware
Intrawest Financial

National Bancorp of Alaska
Mark Twain
Magna Group
Ameribanc
Provident
First American Bank and Trust (Palm Beach)
First Amarillo
First Interstate of Iowa
First Commercial
One Bancorp
Commercial Bancshares
Associated Banc-Corp
UST Corp.
Boston Bancorp
Brenton Banks
Central Jersey
First Valley Corp.
Fort Wayne National
Ultra
WestAmerica
Colonial Bancgroup
Lincoln Financial
Independent Bankshares
Susquehanna
Chittenden
Guaranty Bancshares
United Counties
First Illinois
Commonwealth
Alaska Mutual
First Source
Florida Commercial
United Bankers
USBancorp
Great American Corp.
First Ohio
First Financial
First National Bancorp (Georgia)
Seattle Trust and Savings
Pacwest
CNB Bancshares
Bankeast
Central Pacific
Unibancorp
Nashville City Bank and Trust
BankVermont
Bank of New Hampshire
Central Wisconsin Bankshares

FirstBank (Illinois)
CCNB
Bankers First
Jefferson Bankshares
First Colonial

Banks (Small)

United Oklahoma
Trustco Bank
St. Joseph
Santa Monica
Simmons First
National City (Minnesota)
Howard Bancorp
First Interstate (Alaska)
North Fork
Merchants Bank (New York)
BT Financial
Colonial American
Capitol Bancorp
Central Fidelity Banks
Putnam Trust
Piedmont Bank Group
Popular Bancshares
Seacoast Banking
Alaska National Bank of the North
National Penn
Westport Bancorp
Broadway Financial
Arrow Bank
First Bankshares (Texas)
Prudential Bancorp
Jefferson Bancorp
United Vermont
American Bank of Connecticut
River Forest Bank
CVB Financial
Bank of Granite
RNHB
Eldorado
Plaza Commerce
TransWorld Bancorp
Merchants Capital Corp
Founders Bank

Book Publishing

Addison Wesley
Commerce Clearing House
Educational Development
Goodheart Willcox
Thomas Nelson
Plenum
Sadlier
Scholastic
John Wiley & Sons
Dalton Communication

Publishing/Broadcasting

American Sports Advertising
Baker Communication
Gray Communication
National Lampoon
Park Communication
Value Line
ACS Enterprises
American Communication and Television
Cardiff Communication
Clear Channel
Jacor
Josephson
Lin Broadcasting
Scripps Howard
Sun Group
United Telecommunications
Westwood One
CS Television
Comcast
Sattelink
TCA Cable
Telecrafter
Rogers
Satellite Music Network
Barris Industries
Mizlou
Northwest Teleproductions
Reeves Communication
Telemation

APPENDIX C

GLOSSARY OF ACRONYMS

ACRS	Accelerated Cost Recovery System; depreciation system in effect 1981-1986
AMEX	American Stock Exchange
AMT	Alternative Minimum Tax; penalty tax on individuals and corporations deemed to be making "excessive" use of certain tax-preferred items
CAPM	Capital Asset Pricing Model
CAR	Cumulative Abnormal Return; measure of abnormal return over some period of time; defined in equations (3) and (4)
CFO	Cash Flow from Operations; defined in equation (1)
CRSP	Center for Research in Security Prices (University of Chicago); compiler of tapes containing stock price and other data on publicly-traded companies; source of data for this dissertation
DJNS	Dow Jones News Service
ERTA	Economic Recovery Tax Act of 1981
ITC	Investment Tax Credit; credit against federal income tax allowed for purchase of qualifying business assets; repealed by TRA

MAR	Mean-Adjusted Return; a measure of abnormal return used in this dissertation; defined in equation (20)
MBAR	Market-Based Accounting Research; body of research using behavior of security prices to investigate effects of accounting regulation and federal legislation
NYSE	New York Stock Exchange
OTC	Over-The-Counter; means of trading for publicly-held companies not traded on a stock exchange; firms are generally smaller and more thinly traded than exchange firms
PAL	Passive Activity Loss; loss from business activity in which taxpayer does not actively participate; generally must be deferred under provisions of TRA; reformed aimed at curbing tax shelters
SAR	Size-Adjusted Return; a measure of abnormal return used in this dissertation for all firms except banks; a means of adjusting firm returns for market returns while controlling for size effect; defined in equation (19)
SIC	Standard Industrial Classification; US Department of Commerce system for classifying firms by industry
TRA	Tax Reform Act of 1986
UNICAP	Uniform Capitalization; tax accounting rules introduced in TRA; designed to increase tax revenue by requiring capitalization (and therefore deferral) of costs previously allowed to be expensed

APPENDIX D

DOLLAR MAGNITUDE OF EFFECTS

The purpose of this appendix is to explore the possibility, raised at the end of Chapter Six, of estimating the dollar magnitude of economic effects. Consistent with prior literature, this study has focused on determining the existence of such effects, inferred from statistically-significant stock price changes. However, it is possible for price changes to be statistically significant ("large" relative to past behavior) and yet not monetarily significant ("large" in the view of affected parties). The well-known accounting concept of materiality emphasizes that the magnitude of an item is of relevance to decision-makers. Therefore, estimates of the magnitude or monetary significance of the economic effects of tax legislation potentially provide additional useful information.

Despite the potential usefulness of this information, there is little such analysis in the MBAR literature.[35] This appendix is therefore exploratory in nature, and is intended to suggest possibilities for future research. A single industry/event is chosen for examination, in order to illustrate a possible methodology and some of the operational and interpretational problems encountered.

The analysis is performed using data for the machine tool industry, sixth event, MAR metric, one-day window. This industry/event exhibits a negative reaction that is highly significant statistically (see Table 21), allowing a comparison of statistical and monetary significance. The one-day window is chosen because it provides the least computational difficulty, as discussed below.

Because the industry CARs are equal-weighted (see equation (4) in Chapter Four), the dollar calculation cannot be done at the aggregate level. Instead, it is necessary to calculate the change in value of each firm in the industry portfolio separately. These can then be

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[35] The only attempt of which I am aware to estimate the magnitude of distributive effects of tax legislation is by Manegold and Karlinsky (1988). The procedure used here improves upon their analysis in some respects.

aggregated to determine the monetary effect on the industry as a whole.

The change in value (D) of firm i due to a TRA-related event (in this case, the final passage of the bill) is modeled as

$$D_i = MV_i * AR_i \qquad (1)$$

where MV_i is the market value of the firm (equal to price per share times outstanding shares) at the close of the trading day before the first day of the event window, and AR_i is the abnormal return to the firm over the event window. It is assumed that the entire abnormal return is due to the effect of the event.

For a one-day window, as used here, AR_i is simply the abnormal return to the firm for the event day. It should be noted that for a multi-day window, it is not appropriate to use CAR_i (equation (3) in Chapter Four), the sum of the daily abnormal returns. Instead, a "compounded abnormal return", the product of the daily returns, should be used. Alternatively, D_i can be estimated separately for each day in the window, and the D_i can then be aggregated.

The 75 firms in the machine tool portfolio for this event had a mean market value (MV_i) of over $55 million, and an aggregate market value of approximately $4.18 billion. The aggregate decline in market value on the

event day was approximately $25.2 million, a return to the portfolio as a whole of -.00603. The mean loss in value was $337,000 per firm.

In interpreting these findings, the tentative conclusion is that, in the aggregate, shareholders of machine tool/equipment manufacturers lost $25 million on the day of the passage of the TRA as a result of this legislation. This conclusion is subject to several caveats.

First, it is expressed in terms of a distribution effect (pecuniary loss to shareholders). There appears to be no model or theory which can be used to estimate the effect on the allocation of resources in a useful way. Second, when drawing a distributional conclusion, it should be noted that it is not possible to attribute the losses to individual shareholders. Thus, it is not possible to assess the materiality of the losses to affected individuals. Further, as discusssed in Chapter Two, the effect of portfolio diversification on the net wealth of individuals is not known. The conclusion can only relate to losses suffered by shareholders of these firms in that capacity, and not to a net loss of wealth as a result of the TRA. It should also be noted that not all firms in the industry had negative ARs, and the net amount therefore

understates the loss to shareholders of negatively affected firms, and ignores an apparent windfall to shareholders of positively affected firms.[36]

Third, the $25.2 million figure relates only to those firms included in the sample. It does not consider the OTC machinery firms (a majority of the total) for which data were not available, nor any NYSE or AMEX firms, which are generally larger than OTC firms. Therefore, it undoubtedly substantially understates the economy-wide monetary impact. Any method of extrapolating these findings to the economy as a whole would be ad hoc and arbitrary, and such extrapolation is not attempted.

Fourth, the method used in this appendix measures only monetary effects occurring within a single event window. In this study, statistical significance was more frequently attained using CARs pooled over six events, and it would therefore have been of interest to calculate pooled monetary effects. Such calculations would give a more complete picture of the overall impact of the legislation.

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[36] Of the 75 firms, 48 had negative changes aggregating $49.4 million (average, $1,030,000), and 24 had positive changes aggregating $24.2 million (average $1,008,000). Three had no change in value. Ex post analysis has not identified a criterion for distinguishing the positive from the negative reaction firms.

Operationally, however, it is not clear what method might validly be used to calculate a pooled monetary effect. The usefulness of the method is accordingly limited, unless this operational question can be solved.

As a final methodological point, it is interesting to note that the magnitude of the rate of return calculated in this appendix (-.00603) differs considerably from the CAR reported in Table 20 (-.00871). The former is weighted by firm size, while the latter weights all firms equally. The former probably provides a more accurate picture of "returns" to a portfolio than the latter. That they differ a good deal suggests that future researchers may wish to use a value-weighted CAR in addition to the equal-weighted.

In summary, this appendix has explored the possibility of calculating the dollar magnitude of economic effects of tax legislation. A simple model has been proposed and implemented. The method can potentially provide useful information to policy-makers and others, especially if certain operational problems can be solved. Interpretation of the results is not straight-forward, and must be done bearing in mind the assumptions and limitations involved.

DONALD SAMELSON

VITA

Donald Samelson was born in Minneapolis in 1951, and attended the public schools there. He attended Macalester College, majoring in history and economics, and was elected Phi Beta Kappa in 1972. He graduated summa cum laude with special honors in 1973. After receiving the Master of Accountancy degree from the University of Wisconsin in 1975, he passed the CPA Examination and practiced public accountancy in Minneapolis. Since 1978, he has been an accounting professor at Moorhead State University in northwestern Minnesota.