Taxes and Corporate Policies: Evidence From a Quasi Natural Experiment

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ABSTRACT

We document important interactions between tax incentives and corporate policies using a "quasi natural experiment" provided by a surprise announcement that imposed corporate taxes on a group of Canadian publicly traded firms. The announcement caused a dramatic decrease in value although prospective tax shields partially offset the losses, adding 4.6% to firm value. In response to changing tax incentives, firms subsequently adjusted their corporate policies. They increased leverage to gain interest tax shields and reversed changes in other policies made to capitalize on tax benefits. The evidence supports the view that taxes are important for corporate decision-making.

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How do corporate policies interact with tax incentives, and do managers make significant changes to corporate policies in response to a change in taxes? While a large literature examines taxes and corporate policies and much progress has been made, Fama (2011) argues that the big open challenge in corporate finance remains to produce evidence on how taxes affect market values and thus optimal financing decisions. Stewart Myers injects further skepticism, stating that taxes are of "third order" importance for corporate decision-making (Myers et al. (1998)).

One reason the empirical challenge remains is that tax effects are difficult to document. Cross-sectional tests face identification challenges while tests based on tax policy changes are limited because such changes are rarely significant or separable from other changes and are often widely anticipated. Consequently, results may be not economically significant or may be open to multiple interpretations. Without a sizable and unambiguous change in tax policy, it is also difficult to examine the impact on a range of interrelated corporate policies, which as Graham (2003) suggests, offers significant potential to separate tax from nontax interpretations.

In this paper, we exploit a "quasi natural experiment" provided by a change in tax policy to identify a statistically and economically significant impact of taxes on corporate policies. Unlike many tax policy changes analyzed in prior literature (e.g., the Tax Reform Act of 1986), the tax change we exploit was largely unanticipated, dramatic (the corporate tax rate increased from 0% to 31.5%), and was not contaminated by other information or policy changes. We provide new market-based evidence on the value of tax shields and the extent to which taxes reduce firm value, and we show that there are significant changes in corporate policies in response to the tax change. Because we document changes in valuations and in several intertwined corporate policies, our evidence is supportive of the view that taxes are important for corporate finance.

The tax policy change we exploit is officially known as the Tax Fairness Plan (TFP) but is unofficially referred to as the "Halloween Massacre" because it took place after markets closed on October 31, 2006. The TFP affected a large number of publicly traded Canadian firms called income trusts. Prior to the TFP, firms that adopted the income trust structure could avoid paying almost all corporate taxes while retaining the advantages of the corporate structure. The trust structure, which arguably had no nontax rationale (Edgar (2004)), allowed trusts to pass all income through to investors

who were then taxed at the personal level. In the early 2000s, the Canadian equity market was transformed as trusts became an increasingly important part of the market. Established publicly traded corporations from a broad cross-section of industries converted to the trust structure and new firms went public as trusts, accounting for almost 70% of IPO proceeds raised from 2001 until the announcement of the TFP in 2006. Including publicly traded corporations that had announced plans to convert to the trust structure but had not yet completed the conversion, at its peak the trust market was worth $253 billion (Canadian dollars), almost 13% of the total value of the Toronto Stock Exchange.

The TFP eliminated the tax advantage of the income trust structure. It imposed a tax rate equivalent to that levied on corporations on all new trusts while existing trusts were allowed a four-year transition period. Importantly, the TFP did not contain any other major reforms or information about trusts. The plan to tax trusts broke a key election promise of the newly elected Conservative government and the announcement was a surprise to the market (see Figure 1). *Reuters News* reported that "Canada stuns market with pledge to tax income trusts" while Canada's leading business newspaper, *The Globe and Mail*, proclaimed that "Ottawa's tax surprise sends investment bankers scrambling, investors fleeing, stock prices plunging."[1]

The price reactions from this quasi natural experiment have been documented by others, including Elayan et al. (2009) and Edwards and Shevlin (2011). Elayan et al. focus on the role of tax clienteles to explain the variation in price reactions and to test theories about dividend policy. Edwards and Shevlin also explain the cross-sectional variation in price reactions, focusing on proxies for low tax clienteles (size, trading activity, and taxable income) as well as growth opportunities. Like these papers, we also document the price response to the TFP and show that the variation in price responses is influenced by tax clienteles. On average, we find that equity value falls by 14% over the shortest meaningful event window (Days 0 to 2) and by 19% over a longer window (Days 0 to 10) that allows for the initial uncertainty of the details of the TFP to be resolved and for market prices to more accurately reflect the full impact of the tax change. Firm value falls by 12% and 15% over these same windows.

Our paper provides new evidence along two main dimensions. First, we use the TFP event to examine corporate policies and their value. Trusts could offset the effects of the TFP by accessing tax shields. We use changes in market values around the TFP and exploit variation in trusts' access to prospective tax

shields to produce new market-based estimates of the value of tax shields. Importantly, these estimates do not require an accurate model of the full set of factors that might influence the costs and benefits of debt. As expected, we find that trusts with more prospective tax shields, measured as the median tax shield of firms in the same industry structured as corporations, were less affected by the TFP. Our estimates imply that prospective tax shields (debt plus nondebt) increase firm value by 4.6%. Further, we find that the value of tax shields depends on the tax status of the marginal investor. Using the ex-dividend drop ratio as a proxy for the marginal investor's tax status (see Elton and Gruber (1970)), we find that trusts with low tax marginal investors were more affected by the TFP. More interestingly, and new to the literature, we find that prospective tax shields are worth more for trusts with low tax clienteles. Finally, because the TFP was a change in the corporate tax rate (from 0% to 31.5%), we can directly measure the net impact of this change on firm value. Our estimates, which control for a variety of factors including the transition period and tax shields, imply that firm value fell by 17.5%.

Second, we examine how corporate policies adjust to changing tax incentives, starting with leverage. Prior to the TFP when debt had no value as a tax shield, trusts had lower leverage than when they were organized as corporates. With the loss of the trust tax shield due to the TFP, trusts could increase leverage to create an alternate source of tax shields. To identify a change in leverage in response to the TFP, we estimate regressions that compare year-to-year changes in leverage for trusts relative to corporates from 2007 to 2010. After controlling for observable and unobservable firm characteristics, plus industry and year effects, we find that trusts increased their leverage by six percentage points relative to corporates, an economically significant change given that the average debt-to-value ratio was about 20% in 2006.

In addition to leverage, tax incentives can affect other corporate policies. To fully exploit a tax benefit, firms may have to compromise or make accommodative changes to one or more corporate policies, with the specific changes driven by the details of the tax rules. Research on multinational firms, for example, shows such wide reaching impacts. Multinational firms can lower their tax obligations by locating operations in low tax countries or by shifting income to them. U.S. multinationals, however, face home country taxation of repatriated profits and therefore have incentives to delay repatriation, especially from affiliates in countries with low foreign tax rates (Desai, Foley, and Hines (2001)). These incentives

affect not only multinationals' tax rates, but also policies such as cash holdings, payout, and investment (Foley et al. (2007) and Faulklender and Petersen (2012)).

To fully capture the tax benefits of the trust structure, trusts had to pay out all earnings. This made it costly to build up cash holdings and to use these holdings to finance investment. Therefore, trusts had a tax incentive to increase payout and decrease cash holdings, and to the extent that external finance is costly, they had a disincentive to invest. The evidence is consistent with these predictions. Prior to the TFP, trusts had higher payout and lower cash holdings than when they were organized as corporates. After the TFP, they reversed these changes by reducing payout and increasing cash holdings.

More interesting are the results for investment. In frictionless markets, Modigliani and Miller (1958) show that investment policy determines value. Our results suggest that trusts altered their investment policy in response to tax incentives. In the pre-TFP period, trusts' investment in fixed assets was significantly lower than when they were corporates. In the post-TFP period, trusts significantly increased their investment relative to corporates. The fact that trusts altered their investment decisions to access a tax benefit signals the power of tax incentives to affect corporate policies.

As a final test of the interactions between tax incentives and corporate policy choices, we examine acquisitions. Given the tax advantage of the trust structure prior to the TFP, we predict that trusts were more likely to be acquirers and less likely to be targets compared to corporates. Similarly, with the loss of the tax advantage due to the TFP, we predict that trusts were more likely to be acquired after the TFP as takeovers are one channel that can change organizational form and potentially offer more tax shields. The results are consistent with these predictions.

In summary, this paper offers new market-based evidence on the value of tax shields as well as new evidence of strong interactions between tax incentives and corporate policy choices. The combination of event study and time-series evidence that we provide is difficult to reconcile with nontax explanations. The increase in leverage after the TFP is consistent with the importance of debt tax shields while the results for investment and acquisitions highlight the potential for tax incentives to alter value-relevant policy choices. Though the results for payout and cash holdings apply less broadly because they depend on specific rules related to income trusts, they do highlight a broader point, namely, that firms are willing to alter corporate policies in response to tax incentives.

This paper is related to the large literature that examines whether taxes impact leverage choices, for example, Bradley, Jarrell, and Kim (1984), Graham, Lemmon, and Schallheim (1998), Graham (1996, 1999), and Mackie-Mason (1990). Our time-series results on leverage changes are quite novel as little prior evidence documents the extent to which a change in taxes affects leverage (recent exceptions include van Binsbergen, Graham, and Yang (2010) and An (2012)). Our paper is also related to the literature that estimates the value of tax shields. This literature has produced a wide range of estimates, some of which are subject to nontax explanations or identification challenges (see the reviews by Graham (2003) and Hanlon and Heitzman (2010) for details). Our estimates of the value of tax shields are consistent with recent estimates provided by van Binsbergen, Graham, and Yang (2010) and Korteweg (2010), who use different samples and methodologies. Finally, we provide evidence on the overall impact of taxes on valuation, an issue that is central to our teaching.

The paper proceeds as follows. In Section I, we provide background information on trusts and the tax policy change. In Section II, we provide a simple framework to evaluate the implications of a change in taxes for corporate policies. Section III describes our data set and documents the impact of the TFP on valuations. Section IV examines the extent to which corporate policies mitigate the impact of the TFP. In Section V we examine changes in corporate policies when firms become trusts and then following the TFP announcement. We discuss robustness checks in Section VI and conclude in Section VII.

I. The Canadian Income Trust Market and the Tax Fairness Plan

A. The Income Trust Structure

The income trust structure allowed the owners of a firm to retain many of the nontax advantages of the corporate form, namely, limited liability and access to public capital markets, while avoiding the negative tax consequences.[2] By combining the tax benefits of a limited partnership with the benefits of being a publicly traded company, the trust structure was similar to Real Estate Investment Trusts (REITs) and to the master limited partnership structure used in the U.S. (see, for example, Gentry, Kemsley, and Mayer (2003) and Shaw (1991)). However, it is different in that the trust structure was available for a wide range of industries. The key feature of trusts was that income earned was generally not subject to corporate income tax because trusts were treated as flow-through entities for tax purposes. Income earned

by the trust flowed to investors and was taxed as ordinary income at the personal level. In contrast, income earned by public corporations is taxed twice, once at the corporate level and again at the shareholder level when income is distributed as dividends or repurchases. Although a dividend tax credit allows taxable Canadian shareholders to recover some of the corporate taxes, corporate and personal taxes are not fully integrated – if they were fully integrated, there would be no double taxation, and no tax rationale for the trust structure (Mintz and Richardson (2006)). The income trust structure eliminated the unintegrated portion of the corporate tax, which reduced the total amount of tax paid by investors.

In a typical trust structure, the trust sold units to investors and the proceeds were used to acquire all of the debt and equity of an operating corporation. The trust capitalized the operating corporation with non-arm's length private market debt ("internal" debt), which was long-term unsecured high-yield debt that was subordinated to debt issued to third parties ("external" debt). The internal debt was in effect a tax-advantaged form of equity. Its purpose was to generate tax-deductible interest payments sufficient to eliminate the operating corporation's income taxes so that income flowed to the trust tax-free. If the trust distributed all of its taxable income to unit holders, no corporate tax was paid at the trust level (undistributed income was subject to tax at the highest personal income tax rate, which at the time was 46% in Ontario). To fund growth, trusts could issue debt to third parties or sell more units.

Income generated by trusts faced fewer and lower taxes than income generated by corporations, with the tax gain from holding a trust depending on the investor's personal tax status. Tax exempt investors (e.g., investments held in individual investors' retirement accounts and pension funds) faced a 0% tax on distributed income and foreign investors paid only a withholding tax, which for U.S. investors amounted to 15%. Taxable investors paid taxes on this income at their marginal rate, which for the highest income Canadian investors was 46%. These rates compared favorably with the implied tax rate on income distributed as dividends from regular corporations, where the 35% corporate tax rate plus personal and withholding taxes produced effective individual tax rates for tax exempt, foreign, and taxable individual investors of 35%, 45% and 49%, respectively.[3]

B. The Growth of the Income Trust Market

Consistent with lower statutory taxes impacting firms' choices, Table I shows that the number of firms organized as trusts grew steadily until 2006 as publicly traded firms converted to trusts, spun off

part of their operations as trusts, or became trusts through an IPO. For example, from 2001 through 2006 trusts accounted for 70% of IPOs (by value) in the Canadian market place and 38% of seasoned equity offerings (by value), and corporations worth $33.7 billion converted to the trust structure.[4] At its peak just prior to October 31, 2006, the trust market included 216 trusts worth $165 billion and four corporations worth another $88 billion had announced plans to convert but had not completed the conversion.

Table I here

Trusts were initially owned primarily by retail investors, but by 2006 institutional investors held substantial stakes and built a powerful constituency to defend trusts' advantageous tax status.[5] This history helped create a diverse set of trust owners. In 2005, the Department of Finance estimated that in aggregate, 39% of trusts were owned by taxable Canadian investors, 39% by tax exempt investors, and 22% by foreign investors.

In October 2006, the widely held view was that the tax-advantaged status of income trusts was here to stay, particularly with a newly elected Conservative government. In early 2006, the government changed from the Liberals to the Conservatives. The Conservative party's election platform included a commitment to maintain the privileged tax status of the trust sector: "A Conservative government will… Stop the Liberal attack on retirement savings and preserve income trusts by not imposing any new taxes on them" (January 13, 2006). Following the election, the trust market grew significantly, with almost $70 billion worth of new trust conversion announcements.

C. The Surprise Announcement to Eliminate the Preferential Tax Status of Income Trusts

The event that sets the stage for this paper is the announcement of the TFP on October 31, 2006. The TFP eliminated trusts' privileged tax status. The government explained that the TFP was a response to "the growing trend in tax avoidance" with the goal of "leveling the playing field between trusts and corporations" (see www.fin.gc.ca/n06/06-061_1-eng.asp). The key feature of the TFP was a new tax on distributions made by certain publicly traded flow-through entities, although REITs were excluded. The tax rate on trust distributions was equivalent to the rate paid by corporations and would begin in 2007 for trusts that started trading after October 2006 and on January 1, 2011 for existing trusts, thus allowing them a four-year transition period. The distribution tax ended the tax advantage for trusts over corporations as trusts would face a tax rate of 31.5% in four years, the same rate faced by corporations.[6]

The TFP completely surprised investors, with no prior policy discussion, and government officials were prepared to pull the announcement if there were any signs of unusual trading activity. The dramatic price drop for income trusts shown in Figure 1 (cumulative abnormal returns on a value-weighted portfolio of trusts from September 26 through December 31, 2006) is consistent with the TFP being a surprise.

Figure 1 here

D. Organizational Change Options During the Transition Period

Funds stopped flowing into the trust sector following the TFP announcement. Most trusts made new organizational choices, likely driven by the change in tax status, the timing of that change, and the limited window allowed by the government for low cost conversion to corporate status. Existing trusts could retain their preferential tax status through January 1, 2011. At that point, they could remain as trusts although there would no longer be a tax benefit. The conversion from trust to corporation status would not create material adverse tax consequences if the conversion was completed by the end of 2012. At the end of 2010, only 32 trusts remained and that number decreased to 19 by the end of 2011 (see Table I). Thus, from 2006 to 2011, the number of trusts declined by 91% and the value of the trust sector fell by 96% (most remaining trusts are small and in press releases stated they did not want to incur the transaction costs associated with conversion).

Figure 2, Panel A plots organizational changes from 2007 to 2011. Consistent with Glew and Johnson (2010), it shows that many trusts were acquired while others converted to corporate status. Acquisition activity, which in one step impacted tax liabilities, was clustered in the first two years following the TFP and conversions were clustered at the end of the transition period so that most trusts retained their tax privileged status as long as possible. Panel B shows that by value (as of the end of 2006), most trusts waited until the end of 2010 to convert. These data suggest a powerful impact of tax policy.

Figure 2 here

II. The Impact of Taxes On Valuations and Corporate Policies

If corporate taxes significantly reduce value, firms should be willing to change their corporate policies or organizational structure to seek advantageous tax treatment. If the benefits from advantageous

tax treatment are large enough, firms should be willing to incur costs on other margins to gain preferential tax treatment, as long as there is a net benefit. This view suggests an interconnected nature across a range of corporate policies. The same predictions go in reverse if the preferential tax treatment is eliminated. Costly actions to take advantage of the now-lost tax benefits should be reversed and firms should undertake other actions to gain access to new tax shields.

A. Taxes, Valuation, and Tax Shields

One well-studied corporate policy is the decision to build tax shields through leverage and nondebt mechanisms. Well-established theoretical models predict how leverage choices influence firm value. In Modigliani and Miller (1963), corporate taxes reduce value by the full amount of the tax rate if there are no tax shields. With tax deductible interest payments, debt tax shields mitigate the impact of the corporate tax and are worth $T_c \times D$, where T_c is the corporate tax rate and D is permanent debt. For a firm with a D/V ratio of 30% that is fully taxed at a corporate tax rate of 31.5%, the Modigliani and Miller model predicts that tax shields are worth 9.5% of firm value.

Of course, the Modigliani and Miller benchmark is naïve and likely sets an upper bound. For example, the tax benefit of debt can be less than $T_c \times D$ if personal taxes are introduced. Miller (1977) identifies a specific condition where the personal tax disadvantage of debt relative to equity eliminates the corporate tax advantage of debt. More subtly, with personal taxes and investor clienteles, the benchmark would be lower for firms that pay out substantial amounts of cash to taxable investors compared to firms that do not, as investors in these latter firms can defer personal taxes. The benchmark is also too high if firms do not have sufficient profits to take full advantage of debt tax shields, if firms have access to tax shields other than debt, or if the corporate tax rate is expected to decrease in the future (see Kraus and Litzenberger (1973), DeAngelo and Masulis (1983), Green and Hollifield (2003), and Graham (2000)). More generally, under the tradeoff theory of capital structure, managers choose a capital structure so that the costs of additional leverage are balanced by the gains of leverage.

Prior to the TFP, debt provided no incremental tax benefits for trusts and trusts had no reason to choose it for tax reasons. Therefore, we expect firms to decrease leverage after becoming a trust. With the loss of the trust tax shield, trusts could increase leverage as an alternate source of tax shields as they transitioned out of trust status.

B. Tax Incentives and Other Corporate Policies

To take advantage of preferential tax treatment, firms often have to make compromises or adjust some of their corporate policies. Earlier, we noted that the choices multinational firms make to minimize corporate taxes can have consequences for other policies such as cash holdings, payout, and investment. Another example is organizational form choices. Most alternatives to the corporate form in the U.S. that allow flow-through tax status do not simultaneously provide limited liability and a public market for trading shares, thus reducing access to equity financing (Mackie-Mason and Gordon (1997)).

Though the specific changes depend on the details of the tax rules, the idea that tax incentives cause firms to compromise or make accommodative changes to various corporate policies to access a tax benefit holds more generally. Most interesting are changes to policies central to value creation such as investment and acquisitions. In the case of income trusts, the specific tax rules suggest that payout, cash holdings, and possibly investment will be affected. To take full advantage of the trust structure and eliminate taxes, trusts had to pay out all earnings. This requirement not only affected payout, but it also raised the cost of holding cash because earnings not paid out were taxed. These institutional features suggest that prior to the TFP firms increased payout and decreased cash holdings after becoming a trust. Following the TFP, we expect them to reverse these policies by reducing payout and increasing cash holdings.

Predictions about investment are less clear. If the only difference between trusts and corporates was that corporates paid additional taxes, firms should increase investment after becoming a trust in the pre-TFP period and trusts should decrease investment after the TFP. This follows from standard theories of investment that predict that higher corporate taxes reduce investment. But specific institutional features can lead to different predictions. As noted, taxable earnings not distributed by trusts were taxed at the highest personal tax rate so that financing investment with internal funds was costly. If trusts faced costs in raising external finance, for example, because the fixed costs of raising funds were large relative to the amounts raised, the tax incentives could discourage investment.[7] After the TFP, trusts no longer had a disincentive to fund investment internally and if they were underinvesting, might increase investment.

To summarize, we first test for an impact of tax incentives by examining leverage. The dramatic change in tax policy allows us to provide new evidence on the value of prospective leverage choices. By tracking leverage changes following the TFP we can also test whether tax policy has real effects on

financial policies. We next examine the impact of tax incentives on payout, cash holdings, and investment. Although the changes in payout, cash holdings, and investment that we document depend on specific income trust tax rules, this analysis allows us to address a broader question, namely, whether managers are willing to make substantial, and potentially costly, accommodations to a number of corporate policies to access preferential tax treatment.

III. Tax Policy and Valuation

A. The Sample of Income Trusts and Data Sources

To examine the impact of the TFP announcement on the corporate policies and valuations of income trusts, we assemble a complete list of trusts that traded on the Toronto Stock Exchange (TSX). We then identify those trusts with data available in *Datastream*. We exclude 10 U.S.-based trusts that were listed on the TSX via Income Participating Securities (IPSs) or Income Depositary Securities (IDSs) (see Halpern and Norli (2006)) and in all tables but one, we exclude REITs. REITs were exempt from the TFP but we show that they were temporarily affected by the announcement.

In October 2006, there were 216 trusts (see Table I). Of this total, 163 were from a wide variety of industries labeled as business trusts and 53 were energy trusts / pipelines. In addition, four firms had announced plans to convert to the trust structure but had not completed the conversion by October 2006 (pending trusts).

In our analysis, we require data on stock returns, market capitalization, trading volume (from *Datastream*), firm characteristics (from *Worldscope*), and securities issuance and mergers and acquisitions (from *SDC*). Data on firm characteristics from *Worldscope* are winsorized at the 1st and 99th percentiles to mitigate the impact of outliers. We use the Fama-French 49 industry classification scheme to define industries. In the sections that follow, the tests use different samples and time periods. In each section we provide details. All variables are defined in Table A.I and robustness checks are discussed in Section VI.

B. Initial Uncertainty Surrounding the Tax Fairness Plan

The TFP was announced after markets closed on October 31, 2006. In addition to completely surprising the market (see Section I and Figure 1), the TFP introduced significant uncertainty that took

several days to resolve. The actual TFP press release is brief and does not provide many details other than to state that a tax would be imposed on trust distributions, with a four-year transition period for existing trusts (see www.fin.gc.ca/n06/06-061_1-eng.asp). As one analyst stated, "Markets can handle good news, bad news, but they can't handle not knowing what the news is, and the government's paper was relatively short and lacking a number of details."[8] Consistent with this concern, the initial sell-off in the trust sector was to some extent indiscriminate. For example, REITs fell by almost 4% on November 1 and 2 even though they were exempt from the TFP. These losses were reversed on November 3 and 6. Much of the sell-off on November 2 was concentrated in the energy trust sector, where U.S. investors had substantial holdings and did not react immediately to the news on November 1.[9]

The uncertainty extended for several more days. The announcement of the TFP was a proposal, but had not been introduced or passed by parliament, where the Conservative party had a minority government. Aggressive lobbying by interest groups, including many who were large contributors to the Conservative party, to dilute the proposal added to the uncertainty. The government made statements on November 3 and 5 that began to reduce the uncertainty. The Finance Minister confirmed that pending trusts would not be granted any exemptions and that there would not be any changes or exceptions to the new rules. On the evening of November 7, Parliament passed a motion that would allow the government to tax income trusts. The government also stated that the new rules would be changed if any new structures emerged that were clearly designed to frustrate its policy objectives.

These facts suggest that to analyze price reactions around the TFP, we need an event window that allows the impact of the tax change to be fully priced by the market. At a minimum, we need to allow the initial uncertainty about the details of the TFP to be resolved, but ideally, we need a longer window that captures the policy uncertainty and volatility in the market. In the next section we discuss the abnormal trading volume model we use to identify the relevant event window.

C. Abnormal Trading Volume Around the TFP Announcement

Given the initial uncertainty surrounding the details of the TFP, we use a model of abnormal trading volume to identify an event window to focus on in our analysis (see Meulbroek (1992)). Specifically, we estimate the following regression over the period from July 1, 2005 to December 31, 2006:

$$ln(1 + v_{it}) = \alpha_i + \lambda_i \times ln(1 + v_{it-1}) + \beta_i \times ln(1 + v_{mt}) + \eta_i' \times Weekday + \delta_i' \times Event + \varepsilon_{it}\,, \quad (1)$$

where v_i is trust i's daily trading volume, v_m is the total daily volume of all corporates, *Weekday* is a vector that includes day of the week dummies, and *Event* is a vector that includes dummy variables for each event day from -5 to +15. The estimates of δ_i measure each trust's abnormal volume on a given event day. Because all firms have the same event date, cross-correlations can bias the *t*-statistics upwards. Therefore, we estimate the regression for each trust as a system of equations using a seemingly unrelated regression (SUR) (see Schipper and Thompson (1983)). The SUR framework requires complete data for each trust over the sample period. Therefore, we can only use trusts that existed prior to July 1, 2005. We lose 16 trusts that do not have data prior to this date. We drop another six trusts because we require that trusts trade on at least 90% of the trading days during the period. Finally, we exclude 20 trusts that do not have complete data on all variables used in the cross-sectional regressions in Section IV. This leaves a final sample of 149 trusts, including three pending trusts. We also estimate the regression separately for a sample of 22 REITs. The results are reported in the Internet Appendix, available in the online version of the article on The Journal of Finance website.

Consistent with the TFP being a complete surprise, there was no abnormal volume in the trust sector or among REITs prior to Day 0 (the TFP was announced after markets closed on October 31 so that November 1 is Day 0). Beginning on Day 0, the average value of the estimated δ_i's is positive and significant for each event date through Day 2 for REITs and through Day 10 for trusts (except for Day 4). After Day 10, it is no longer significant. Therefore, we focus on the (0,10) window (November 1 to 15).

D. The Value Drop Following the TFP Announcement

We estimate abnormal returns around the TFP announcement for each trust using the following regression model over the period from July 1, 2005 to December 31, 2006:

$$R_{it} = \alpha_i + \beta_i \times R_{bt} + \gamma_i \times Event + \varepsilon_{it}\,, \quad (2)$$

where R_i is the daily return for trust i, R_b is the value-weighted return on the benchmark portfolio, and *Event* is a dummy variable that equals one on the event days following the TFP. The benchmark portfolio includes all corporates (excludes all trusts and pending trusts) that have total assets of at least $10 million and trade on at least 40% of the trading days in a given year. The regression is estimated for each trust as a system of equations using SUR to account for cross-correlations in stock returns. The estimate of γ_i

measures each trust's average abnormal return over the event period (see Binder (1985)). To compute the cumulative abnormal return (CAR), each γ_i is multiplied by the number of days in the event period.

The CAR for each trust measures the abnormal change in the market value of equity. We focus on the change in firm value rather than equity value to facilitate comparison of our results with the theoretical and empirical literatures on debt tax shields (e.g., Modigliani and Miller (1963), Miller (1977), Graham (2000)). To compute the change in firm value (value drop), we assume, as is commonly done, that the value of debt is unaffected by the TFP (e.g., debt betas are zero). We compute the abnormal change in equity value (CAR) and express it as a percentage of firm value. This is equivalent to multiplying the CAR by the equity-to-value ratio (E/V).[10] Though none of the results that follow are sensitive to this normalization, the interpretation of the results differs (e.g., tax shields are worth x% of firm value versus y% of equity value). To compute the E/V ratio, we use the market value of equity at the close of trading on October 31, 2006 and the book value of debt at the end of 2005 (the most recent fiscal year-end prior to the TFP).

Table II reports the average CAR, value drop, and *t*-statistics for income trusts and REITs for Day 0 as well as the (0,1), (0,2), and (0,10) event windows. The price reactions for trusts, including business trusts and energy trusts / pipelines, are significant at the 1% level over all windows. By Day 10, prices stabilized and the trust sector had lost 15.2% in value (18.6% by equity value). Instead of valuing trusts as tax-free entities, the market now valued them as corporations with a four-year tax holiday. The market response for REITs is significant through the (0,2) window but is insignificant in subsequent windows. Consistent with the abnormal volume estimates, the price reactions for REITs, which were not affected by the TFP, suggest that the (0,2) window is the shortest reasonable window to examine. Over that window, the trust sector lost 11.6% in value (14.1% by equity value).

Table II here

IV. Taxes and the Value of Corporate Policies: Event Study Evidence

In this section, we examine whether corporate policies mitigate the extent to which trusts were affected by the TFP and whether personal taxes affect the value of corporate policies.

A. The Value of Tax Shields

To examine the relevance of taxes for corporate policies, we start with an event study. We use changes in market values around the TFP to test whether tax shields (debt and nondebt) are priced. Specifically, we estimate regressions where the value drop over the (0,10) window is the dependent variable. To ease interpretation of the results, we multiply the value drop by negative one, for example, the average value drop is 0.152. In the regressions, we require complete data on all firm characteristics for 2005, leaving a sample of 146 trusts and three pending trusts.[11] As discussed earlier, REITs are excluded. Because the observations are not independent for trusts in the same industry, we cluster the standard errors at the industry level.

The trust structure provided a tax shield until it expired at the end of 2010. After that, trusts could use debt or nondebt tax shields to lower their taxes. To capture prospective tax shields, we use the sum of debt and nondebt tax shields as a percentage of firm value used by corporations in the same industry.[12] For each industry we calculate the median debt and nondebt tax shields for all Canadian corporations in the *Worldscope* database for 2005. Although this measure captures industry-level differences in trusts' access to tax shields after the TFP, it does not assume that trusts necessarily migrate to the industry median. In fact, it is possible, and not a problem, if trusts have a different steady-state level of tax shields than corporates in the same industry, arising, for example, from some unobserved characteristic. The average (median) value is 23.1% (20.8%).

We do not include a variable that directly measures a trust's current third-party debt or nondebt tax shields in the regressions. The reason is that the value drop is calculated by multiplying the CAR by the E/V ratio. Therefore, the debt component of a trust's tax shield variable would be mechanically related to the dependent variable. In contrast, there is no possibility of a mechanical relation with the prospective tax shields variable because firms organized as a trust at any point in time are excluded from the sample when we compute it. To investigate the potential importance of firm-specific differences in tax shields we create a dummy variable that equals one for trusts with tax shields below the industry median in 2005 (*Low tax shield dummy*). Ex ante, it is unclear how much this variable should matter, even though it is firm-specific information and could capture some within-industry variation. The reason is that tax incentives would not matter to a trust when choosing its third-party debt level or nondebt tax shields, as

long as it expected to remain a trust and the government continued to grant preferential tax treatment to trusts.

The TFP delayed the introduction of the new trust tax for four years. Extending the tax-free period mitigated the impact of the TFP and should be worth more for trusts that were expected to realize a greater proportion of their value during this period. In the extreme, a trust that had all of its value associated with cash flows expected in the next four years would be unaffected by the TFP; similarly, a trust with all its value associated with cash flows more than four years out would be fully affected. We introduce a control variable that captures the percentage of firm value associated with cash flows in the next four years (*% value in first 4 years*). We construct this variable using a simple valuation model, with details provided in Table A.I. The average (median) trust had 29.7% (28.1%) of its value coming from the first four years following the TFP. Because this value differs across trusts, we prefer to use it as a control variable in the regressions instead of grossing up estimates from a regression that does not include it.

Table III presents the results. Model (1) includes *Prospective tax shields* along with control variables, *% value in first 4 years*, *Pending dummy*, and *Log(Total assets)* at the end of 2005 to capture any effects due to differences in firm size. The coefficient on *Prospective tax shields* is -0.198 with a *t*-statistic of -2.82, which indicates that prospective tax shields mitigate the impact of the tax. This estimate implies that a trust that is fully affected by the TFP (all value is due to cash flows generated in 2011 or later) and has mean prospective tax shields is worth 4.6% more compared to a trust with no prospective tax shields (-0.198 × 0.231 = -0.046). This estimate is consistent with estimates from other papers that use different samples and methodologies, for example, Graham (2000) finds that the gross tax benefit of debt is worth 9.7% of firm value, while van Binsbergen, Graham, and Yang (2010) and Korteweg (2010) estimate a net benefit to debt of 3.5% and 5.5%, respectively.

Model (2) adds *Low tax shield dummy*. Although trusts that do not have immediate access to as many tax shields have a larger drop in value, the coefficient on *Prospective tax shields* remains negative and significant. In these regressions, the main control variables have the expected signs, though they are not significant.

Table III here

B. Personal Tax Clienteles and the Value of Tax Shields

Miller (1977) argues that the personal tax disadvantage of debt relative to equity offsets the corporate tax advantage of debt. Consistent with this argument, Graham (2000) finds that personal taxes reduce the value of debt tax shields. For income trusts, Elayan et al. (2009), Edwards and Shevlin (2011), and Klassen and Mescall (2012) find evidence of personal tax clienteles within the trust sector and between the trust and corporate sectors. In this section we consider whether differences in trusts' tax clienteles influenced the market's response to the TFP. The main contribution of this analysis is to show that the value of tax shields depends on the tax status of the marginal investor.

At the outset, we note that as with any tests of personal tax status, we make relatively strong assumptions, for example, there is one marginal investor for both equity and debt. Therefore, our point estimates are probably noisier than those in the prior section that ignore personal tax effects. With this caveat in mind, we identify the tax status of the marginal investor using Elton and Gruber's (1970) ex-dividend day drop ratio, the ex-day price drop scaled by the amount of the dividend. Elton and Gruber argue that differences in the magnitude of a stock's price drop around the ex-day reveal differences in the tax status of the marginal investor in equity.[13] According to this view, firms with marginal investors with higher tax rates have lower ex-day drop ratios, for example, the drop ratio equals one for tax-exempt investors whereas it is less than one for investors who face a higher tax rate on dividends than capital gains because personal taxes reduce the value of the dividend.

Trusts that have marginal investors with lower personal tax rates (e.g., tax-free pension plans) should be most affected by the TFP and therefore have a greater value drop. It is important to note, however, that the tax interpretation of the ex-day drop ratio is debated in the literature.[14] If the ex-day drop ratio does not capture personal tax effects, there is no reason to expect it will be significant in our regressions. If it captures personal tax effects, the coefficient should be positive and significant.

For each trust, we compute the median ex-day drop ratio from January 2005 to October 2006, a period sufficiently long to produce multiple observations for each trust in the pre-TFP period. Therefore, we assume that the marginal investor remains the same before and after the TFP (there is no obvious reason for an immediate change in investor clienteles after the TFP given the four-year transition period). We focus on the median response because the monthly distributions are relatively small and price

responses can be swamped by noise due to ordinary volatility. We also winsorize the drop ratio at the 5th and 95th percentiles given that there are some extreme values in the raw measure. The mean (median) value is 0.67 (0.68) with substantial variation. To signal the potential imprecision of this proxy, we focus more on models that use a discretized version. The variable *Low tax investor dummy* equals one for trusts with drop ratios above the 75th percentile. Additional details are provided in Table A.I.

In model (3) we add to model (1) *Ex-day drop ratio* as a proxy for personal tax status. As expected, the coefficient is positive and significant, indicating that personal taxes are priced. That is, trusts with a lower tax marginal investor have greater value drops. The coefficient on *Prospective tax shields* remains negative and significant at the 1% level, but the value of prospective tax shields is lower than when we ignore personal taxes (e.g., 3.8% in model (3) compared to 4.6% in model (1)). When we use *Low tax investor dummy* instead in model (4), the results are similar.

More novel are the results in model (5) where we test whether the value of prospective tax shields differs depending on the tax status of the marginal investor. To model (4), we add an interaction variable comprised of our proxies for personal tax status and prospective tax shields (*Low tax investor dummy* × *prospective tax shields*). The negative and significant coefficient on the interaction indicates that prospective tax shields are worth more if the marginal investor has a lower personal tax rate, that is, trusts for which the marginal investor is hurt more by the tax change. This result is new to the literature.

V. Taxes and Changes in Corporate Policies

In Sections III and IV, we show that taxes have a significant impact on valuations and that prospective tax shields mitigate the impact. In this section we investigate further the interaction between taxes and corporate policies by examining changes in corporate policies around changes in tax status. We start with leverage, an important corporate policy with clear tax incentives. We then examine other policies more closely tied to the specific trust tax rules, namely, payout, cash holdings, and to the extent that external finance is costly, investment. Finally, we look at acquisitions.

If the tax advantage of trust status provided incentives to alter a range of corporate policies, the effects should be observable before the TFP as firms converted to trust status and after the TFP when trusts lost their tax advantage. We look at changes in corporate policies in both periods although we put

more weight on the evidence from the post-TFP period. These tests start with the largest possible sample, the tax policy shock was unexpected, the government committed to an implementation date of January 1, 2011, and all trusts were responding to the same shock. The tests from the pre-TFP period are informative, but face several limitations. First, the sample of trusts changed rapidly during that period and about 50% of trusts were formed between 2004 and October 2006 (see Table I). Second, many trusts were not publicly traded prior to assuming trust status, which limits the sample we can use in our tests. Finally, concerns about the endogeneity of the choice to assume trust status are more pronounced.

Throughout this section, we require that firms have assets of at least $10 million and data on various firm characteristics. The *t*-statistics in all regressions are clustered at the firm level.

A. Changes in Corporate Policies Prior to the TFP

In Table IV, we test for changes in corporate policies as firms switch from corporate to trust status in the pre-TFP period. We pool data from all publicly traded Canadian firms from 2001 to 2006, including both corporates and trusts. Although our focus is on firms that switched from corporate to trust status, we include firms that were never organized as trusts to provide a benchmark and to control for time effects. The dependent variables are the key corporate policy variables that we examine, namely, leverage, payout, cash holdings, and investment. The main variable of interest is a dummy that equals one if a firm was organized as a trust in given year (*Trust dummy*). At the end of 2006, we have 182 trusts and 774 corporates that have leverage data and complete data for the control variables.

In all regressions, we control for firm size (*Log(Total assets*)), year fixed effects, and firm fixed effects. In some regressions we include additional time-varying firm and industry-level controls as suggested by the literature. The year fixed effects capture time-varying effects on corporate policy choices that influence all firms, while the firm fixed effects capture unobservable time-invariant firm characteristics. The latter are particularly important, for instance, Lemmon, Roberts, and Zender (2008) show that unobserved firm characteristics explain the majority of variation in firms' leverage choices. Because the regressions include firm fixed effects, *Trust dummy* measures the change in a given policy after firms switch to trust status. This dummy is identified from the 43 firms that have complete data and converted from corporate to trust status during the sample period.

While trusts used debt for a variety of reasons, it had no value as a tax shield for them. Therefore, we expect firms to decrease their leverage after becoming a trust. The results for leverage (total debt / total assets) are in models (1) and (2). The parsimonious specification in model (1) controls for size as well as firm and year fixed effects while model (2) adds time-varying controls for growth opportunities, asset tangibility, profitability, industry cash flow volatility, and the median leverage of corporates in the same industry, all lagged by one year (see Fama and French (2002) and Lemmon, Roberts, and Zender (2008)). Consistent with firms decreasing leverage after becoming a trust, in model (1) the coefficient on *Trust dummy* is -0.033 and significant at the 5% level, while in the more demanding test in model (2) the coefficient is -0.026 and significant at the 10% level. To put this result in context, the average leverage of the 43 switching firms prior to converting was 0.235, so they decreased leverage by about 11% after converting to trust status.

Table IV here

To fully capture the tax benefits, trusts had to pay out all earnings. This tax incentive also increased the cost of holding cash. Therefore, we expect firms to increase payout and decrease cash holdings after becoming a trust. Models (3) and (5) show the parsimonious specifications for payout (distributions / total assets) and cash holdings (cash and short-term investments / total assets). The variable *Trust dummy* is positive and significant in the payout regression and negative but insignificant in the cash regression. When we add controls for lagged after-tax earnings, investment, and industry payout to the payout regression (see Fama and French (2002)) in model (4), the results are virtually the same as those in model (3).[15] When we add controls for lagged investment, growth opportunities, cash flow, leverage, net working capital (excluding cash), payout, industry cash holdings, and industry cash flow volatility to the cash regression (see Bates, Kahle, and Stulz (2009)), *Trust dummy* in model (6) becomes significant. These results suggest that firms increased payout and decreased cash holdings to maximize tax savings after becoming trusts.

Perhaps most interesting are the investment (capital expenditures / lagged total assets) results in models (7) and (8). Given the tax rules for trusts, financing investment with internal funds was costly. Therefore, to the extent that external finance is costly, the tax incentives could discourage investment for trusts. In both the parsimonious specification and the one that controls for lagged growth opportunities,

cash flow, leverage, and industry investment, we find that firms invest significantly less after they convert to trust status. If firms invested at optimal levels prior to converting to trust status, the results suggest that they made a considerable compromise to a fundamental corporate policy to access the tax benefit.

Overall, the results suggest that tax incentives have a potentially significant effect on corporate policies. While these results are informative, as noted earlier, they are subject to a number of potential limitations and we are cautious about making strong inferences from them. In the next two sections, we draw stronger conclusions from tests that examine changes in corporate policies after the TFP.

B. Changes in Leverage After the TFP

The TFP removed the tax shield provided by the trust structure. In this section we test whether trusts replaced some of this tax shield by increasing leverage after the TFP. Trusts could start to alter their leverage after the TFP announcement or wait until January 1, 2011 and change their leverage all at once. If there are frictions or perceptions of inefficiency in the market for external finance (the post-TFP period coincided with the financial crisis period) or if there are adjustment costs in increasing leverage, managers could choose to increase leverage throughout the period.

Figure 3, Panel A gives a preview of the results. To construct this figure we compute "excess leverage", defined as trust leverage minus the median leverage of corporates in the same industry. We are interested in the trend in this variable rather than the initial level (this simple approach does not control for observable and unobservable firm characteristics that might drive differences in the initial level). The figure plots the average excess leverage each year from 2006 through 2010. For ease of comparison, we focus on a constant sample and exclude trusts that were acquired over the sample period, although the figure is similar if we include all trusts. The figure shows a sustained increase in excess leverage over the period.

Figure 3 here

To provide more rigorous tests and better identification of the impact of the TFP, in Table V we estimate the following regression over the period from 2007 to 2010:

$$\frac{D_{it}}{A_{it}} - \frac{D_{it-1}}{A_{it-1}} = \alpha + \delta\, Trust\ dummy_i + \gamma' \Delta X_{it} + \lambda' Z_{it-1} + \eta_i + \nu_t + e_{it}. \quad (3)$$

The dependent variable in each regression is the change in book leverage from year *t-1* to year *t*, *Trust dummy* equals one for firms organized as trusts at the end of 2006 (regardless of whether they converted to corporate status prior to 2011), ΔX and Z are a set of time-varying firm and industry-level control variables, η is an industry fixed effect, and v is a year fixed effect. Trusts that were acquired drop out of the sample in the year of the acquisition.

We take as given the level of trusts' and corporates' leverage in 2006 and focus on how it changes after the TFP. Because we use changes, unobservable time-invariant firm characteristics drop out and cannot influence our results. All regressions control for size, as well as industry and year fixed effects. In models (2) to (5), we include additional controls as discussed in Section V.A except that we now define the controls in changes rather than levels. We also include lagged leverage (see Fama and French (2002)).

The results in Table V show that the change in tax status due to the TFP was associated with a change in leverage in subsequent years. The positive and significant coefficient on *Trust dummy* in models (1) and (2) indicates that trusts increased their leverage relative to corporates. The results are statistically significant and economically meaningful. Focusing on model (2), the coefficient on *Trust dummy* is 0.015, which implies that during the four-year period from 2007 to 2010, trusts cumulatively increased leverage six percentage points more than corporates, a substantial increase compared to the mean leverage of 20% for the full sample in 2006. This increase is not due to a decrease in trusts' total assets, which increased over the period. Because *Trust dummy* captures the year-to-year difference in changes in leverage between trusts and corporates, after controlling for firm, industry, and year effects, the simplest explanation is that trusts increased their leverage in response to the TFP.

The variable *Trust dummy* does not distinguish between trusts that converted to corporate status prior to 2011 and those that remained as trusts until then. Although the majority of trusts converted near the end of the 2010, some trusts converted earlier (see Figure 2). To determine if the increase in leverage is concentrated in trusts that converted early, we modify the regression. In model (3) we redefine *Trust dummy* so that it equals one for years when the firm is organized as a trust and is zero for the year of conversion and subsequent years. We then add a dummy that equals one during these years (*Trust converted dummy*). The coefficient on this dummy is positive, but not significant. Moreover, it is not significantly different from the coefficient on *Trust dummy*. These results indicate that the increase in

leverage occurred broadly and was not driven by trusts that converted prior to the end of the transition period.

In models (4) and (5) we examine changes in leverage within the sample of trusts. If tax factors play an important role, trusts that had more tax shields in 2006 should have a smaller increase in leverage following the TFP. To test this conjecture, we drop *Trust dummy* from the regression and add the level of each trust's tax shields in 2006. In model (4) we use debt and nondebt tax shields and in model (5) we use debt tax shields only. Because lagged trust leverage is highly correlated with the trust tax shield variables, we replace lagged leverage at the trust level with the median value for corporates in the same industry. The negative and significant coefficients on the tax shield variables show that trusts with more tax shields in 2006 increased leverage less. Because these regressions do not include lagged leverage at the trust level, in the Internet Appendix we report regressions that exclude trusts with tax shields in 2006 that are above the 90th or below the 10th percentile. These regressions help address the concern that the results are driven by mean reversion for trusts with unusually high or low tax shields in 2006. Overall, the results in models (4) and (5) provide additional evidence to support the conclusion that tax effects drive the changes in leverage following the TFP.

Table V here

C. Changes in Payout, Cash Holdings, and Investment After the TFP

In Section V.A we show that in the pre-TFP period firms altered payout, cash holdings, and investment after becoming trusts, likely in response to tax incentives that followed from the trust tax rules. In this section, we test whether firms reversed these changes after the TFP. As with leverage, we start with a figure. Figure 3, Panel B shows the evolution of excess payout from 2006 to 2010. It shows a decrease, with limited declines in the first two years. Panels C and D show that both excess cash holdings and excess investment increased.

In Table VI we provide more convincing tests. We construct the variables and tests in the same manner as in Table V. Our focus is on *Trust dummy*, which indicates trust status in 2006. We start with payout. Consistent with Figure 3, models (1) and (2) show that trusts significantly reduced payout compared to corporates following the TFP. To determine whether the decrease in payout is concentrated in former trusts that converted to corporate status early, as in Table V we introduce *Trust converted*

dummy in model (3). We expect that trusts that converted early reduce payout the most as they no longer had a tax incentive to maintain a high payout. The coefficient on *Trust dummy* remains negative and significant, but is smaller in magnitude compared to model (2). The coefficient on *Trust converted dummy* is also negative and significant and is larger than that on *Trust dummy* (-0.007 versus -0.021). An *F*-test shows that the coefficients are significantly different. Therefore, trusts decreased in payout in general but the decrease was greatest among trusts that converted before 2011. We also find evidence of a significant increase in cash holdings. The coefficient on *Trust dummy* is positive and significant in models (4) and (5). In model (6), *Trust converted dummy* is positive but not significant.

Finally, we test whether the change in tax incentives affected investment. Earlier we showed that firms decreased investment after becoming a trust, suggesting that they altered their investment policy to access the tax benefit of trust status. Now we examine changes in investment after the TFP. In models (7) and (8), the coefficient on *Trust dummy* is positive and significant. In model (8), which includes a full set of control variables, the coefficient is 0.015, indicating that trusts increased investment by six percentage points relative to corporates after the TFP. This is an economically significant change, given that in 2006 the mean level of investment was 15%. In model (9), the coefficient on *Trust converted dummy* is positive but not significant.

Table VI here

The evidence in this section shows that there were significant changes in payout, cash holdings, and investment after the TFP. Combined with the evidence in Section V.A, the simplest interpretation is that firms were willing to make substantial accommodative changes to these policies to access the tax benefit but reversed them when the tax benefit was lost. Most interesting are the investment results as investment policy is central to value creation.

D. Tax Incentives and Acquisition Policy

As a final test of whether tax incentives influence corporate policies, we examine acquisitions. Prior to the TFP, trusts' tax-advantaged status increased their value relative to corporates. Access to this tax benefit gave trusts an advantage in making acquisitions relative to corporates and we expect that trusts were more active acquirers than corporates prior to the TFP. Predictions for the post-TFP period are less clear. The four-year transition period gave trusts a continued, albeit reduced, advantage relative to

corporate acquirers. There is also anecdotal evidence that trusts sought to acquire firms with tax losses and other nondebt tax shields.[16]

Conversely, by increasing the value of trusts, the tax advantage of the trust structure made it more expensive for buyers outside the trust sector to acquire trusts. In this sense there was an opportunity cost to the trust structure as it built a barrier to takeovers.[17] After the TFP, with the loss of the trust tax shield, trusts immediately declined in value. Strategic and financial buyers that had been on the sidelines now potentially had a superior value proposition, depending in part on the importance of the transition period. Strategic buyers, such as taxable corporates, could now create value if they had a more productive use for the trusts' assets. After the acquisition, strategic buyers could also benefit from the ability to share already-established tax shields across the combined entity. Financial buyers could create more value if they had, or could quickly build up, superior tax shields. Trusts thus immediately became more attractive targets.

To test for an impact of tax status on acquisition activity, we estimate two regressions over the period from 2003 through 2011. The sample includes corporates and trusts with data on firm characteristics. In the first regression, the dependent variable equals one if a firm was acquired in a given year and zero otherwise (target regression):

$$Target_{it} = \alpha + \beta Post\text{-}TFP_t + \gamma\, Trust\ dummy_{it} + \delta Post\text{-}TFP_t \times Trust\ dummy_{it} + \lambda' X_{it-1} + \eta_i + e_{it}, \quad (4)$$

where *Post-TFP* is a dummy variable that equals zero from 2003 to 2006 and one from 2007 to 2011, *Trust dummy* equals one for firms organized as trusts in a given year, *Post-TFP* × *Trust dummy* is an interaction variable created from these two dummies, X is a set of control variables, and η is an industry fixed effect.[18] The second regression is similar except that the dependent variable equals one if a firm acquired another firm and zero otherwise (acquirer regression).

Table VII presents the results, starting with the target regression in model (1), which controls for firm size and industry fixed effects. In model (2) we expand the set of controls to include lagged measures of growth opportunities, profitability, leverage, and share turnover, as suggested by the literature (see Palepu (1986) and Cornett, Tanyeri, and Tehranian (2011)). We also include the contemporaneous stock price runup over the prior two years. The coefficient estimates on these additional control variables are consistent with results found in prior work. In both models, the coefficient on *Post-TFP dummy* is

insignificant, the coefficient on *Trust dummy* is negative and significant, and the coefficient on the interaction term is positive and significant. Therefore, acquisition activity was not different in the pre- versus post-TFP periods, trusts were less likely to be acquired than corporates before the TFP, and they were more likely to be acquired after the TFP. The findings are consistent with the trust structure acting as a barrier to takeovers before the TFP and that takeovers were one channel that resulted in a change in organizational form and potentially more tax shields after the TFP. Most takeovers occurred within the first two years after the TFP, following the initial dramatic decline in trust valuations (see Figure 2). In these two years acquisitions were more important by number and value than corporate conversions.

Models (3) and (4) present the results for the acquirer regressions. Again, *TFP dummy* is not significant. The variable *Trust dummy* is positive and significant, while the interaction is positive but insignificant. Trusts were more likely to be acquirers in the pre-TFP period but not in the post-TFP period.

Table VII here

VI. Robustness Checks

The conclusions in Sections IV and V are based on the specifications reported in Tables III to VII. In this section we present a number of checks to assess the robustness of the results. We present the key checks in Table VIII and discuss a number of other checks that are reported in the Internet Appendix. All regressions in Table VIII include a full set of control variables, but we do not report the coefficients and *t*-statistics to save space.

A. The Value of Corporate Policies

In Section V, we show that to capture the tax benefits, trusts made accommodative changes to several corporate policies prior to the TFP and reversed them afterwards. To the extent that these changes were common across all trusts, not accounting for them in the event study tests in Table III would affect the magnitude of the value drop, but not the cross-sectional results. However, accommodative changes to corporate policies might not have been common across all trusts and differences in them could influence our results. We therefore reestimate the regressions to allow for potential differences in trusts' policy responses to access the tax benefits. We use model (5) of Table III as the benchmark.

In models (1) and (2) of Table VIII, Panel A, we introduce controls for two important potential mechanisms that may have emerged simultaneously with the TFP and produced a price impact unrelated to taxes: altered investment and changes in the likelihood of takeovers. To capture cross-sectional differences in the potential pricing of altered investment, we include *Underinvestment* (industry investment minus trust investment prior to the TFP). To capture cross-sectional differences in the potential pricing of takeover likelihood we introduce a measure of the potential overhang of takeover activity based on U.S. data (the value of acquisitions of U.S. public firms in the same industry, scaled by the value of all U.S. listed firms and averaged over 2003 to 2006).[19] The coefficients on *Underinvestment* and *Takeover activity* are consistent with expectations, but neither is significant, and none of the main results are affected.

It is possible that the governance of trusts was different from that of corporates, in which case the TFP announcement might have triggered an expectation of a change in governance that was priced differently across trusts. We address this issue in two ways. First, the market might have anticipated that trusts would increase their cash holdings, and as a result face higher agency costs, especially those in industries where the norm is higher cash holdings. To test this hypothesis we use a proxy for future cash holdings, *Prospective cash holdings*, based on the corporate industry median. The results are in model (3). This variable is insignificant and our main results are unaffected. Second, in model (4), we examine potential differences in pricing a change in governance by using a comprehensive governance metric (based on an evaluation of shareholder rights, board composition, disclosure, and shareholding and compensation) compiled by *The Globe and Mail* and the Clarkson Centre for Business Ethics and Board Effectiveness at the University of Toronto. The coefficient on *Governance* is not significant and although the sample of firms with governance scores is smaller, our main results on *Prospective tax shields* are unaffected.

In model (5), we address the possibility that our control variables do not adequately capture nontax differences across trusts. We look beyond the event window and collect data on whether a trust was acquired in the four years after the TFP (*Acquired dummy*) and the number of months until the trust abandoned trust status (*Months survived*). The idea is that if there were inefficiencies associated with accessing the tax benefits and if they differed across trusts, trusts with the largest inefficiencies would

move more quickly to abandon the trust structure following the TFP. These variables are not significant and have no impact on the results.

In all of our tests, we use the change in firm value to facilitate comparisons with existing literature. To compute the change in firm value, we normalize each trust's CAR around the TFP announcement by its E/V ratio. In model (6), we use the CAR as the dependent variable. The results indicate that our inferences are not dependent on this normalization.

We conduct additional robustness checks that are reported in the Internet Appendix. First, we estimate regressions using the value drop computed over different event window lengths and find similar results. Second, we introduce as an alternative dependent variable the value drop unadjusted for market movements. Third, we include each trust's tax rate. Fourth, we include a business trust dummy to capture the possibility that effects were concentrated in this group, rather than more broadly. Finally, we use different variants to capture the value of the four-year transition period, replacing betas of corporates in the same industry with the trusts' own betas and I/B/E/S growth forecasts with trusts' two-year historical sales growth, and estimate an errors-in-variables regression to address the potential issue that our estimates of the value of the transition period are measured with error. We also exclude this variable from the regression and gross up the resulting estimates to reflect the fact that on average, about one-third of firm value is realized during the transition period. In all cases, the signs and significance of the coefficients on the key tax variables are similar to those reported in Table III.

Finally, these results are based on changes in market prices around the TFP announcement. Though the timing of the announcement was a complete surprise to the market, the market might have anticipated that the favorable tax status of trusts could be removed at some point (see footnote 1). While it is difficult to quantify this effect, if the market anticipated a change in tax policy, trusts would have lower valuations, the TFP announcement would have a smaller impact on prices, and our estimates of the value of prospective tax shields would be lower. Similarly, it is also possible that the market anticipated future corporate tax rate decreases beyond those already announced. If so, the tax shield associated with the trust structure would be worth less and the TFP announcement would have a smaller impact.

B. Changes in Corporate Policies

We next examine robustness checks for the key regressions from Section V for changes in leverage, payout, cash holdings, and investment (Tables V and VI). For each policy, we present three regressions. The first two regressions for each policy are basic modifications of those in Tables V and VI, for example, we exclude financial firms and we focus on a constant sample, where firms are required to have data in each year for 2007 to 2010. For trusts, this restriction amounts to excluding those that were acquired during this period. In both cases, we find similar results with the exception of the second cash holding regression, where the *t*-statistic is 1.60.

Thus far, we focus on year-to-year changes in the post-TFP period. Though we take as given trusts' and corporates' corporate policies in 2006 and focus on how they change after an exogenous tax policy change, it is possible that the regressions do not adequately control for the endogenous choice to be a trust in the first place. To address this concern, we now take a different approach. We define the dependent variable in levels and use a difference-in-differences regression estimated over the period from 2003 to 2010 so that we have four years of data before and after the TFP. For leverage, the regression is

$$\frac{D_{it}}{A_{it}} = \alpha + \delta Post\text{-}TFP_t \times Trust\ dummy_{it} + \lambda' X_{it-1} + f_i + v_t + e_{it}. \quad (5)$$

The coefficient δ is the difference-in-differences estimate. That is, the difference between the change in a trust's leverage relative to corporates before and after the TFP, *X* is a set of controls, and *f* and *v* are firm and year fixed effects. While this approach has the advantage that it explicitly benchmarks changes in the post-TFP period to changes in the pre-TFP period and better controls for the endogenous choice to be a trust, we caution that many trusts had limited or no data in the pre-TFP period. As a result, the group of trusts is potentially quite different in the pre- versus post-TFP periods. With this caveat in mind, we note that the results in models (3), (6), (9), and (12) show significant changes in response to the TFP, consistent with the results reported in Tables V and VI.

We also conduct two robustness checks that are reported in the Internet Appendix. First, we use changes in market leverage instead of changes in book leverage, and second, we define cash holdings as cash to net assets instead of cash to total assets. In both cases, we find similar results.

Finally, we conduct two robustness checks for the acquisition results in Table VII that are also reported in the Internet Appendix. First, *Trust dummy* in Table VII equals one if a firm is organized as a

trust in a given year. Alternatively, we define it so that if a firm was a trust in 2006, the dummy is set to one in all subsequent years. Second, to simplify interpretation, we estimate the models by OLS, despite the fact the dependent variable is binary. To check if this choice affects inferences, we also estimate logit models. In both cases, the results are similar to those reported in the table.

VII. Conclusions

In this paper we examine the interplay between taxes and corporate policies using a "quasi natural experiment" provided by an unanticipated and dramatic change in tax policy that affected a large number of Canadian publicly traded firms. This setting provides an opportunity to estimate the value of corporate policies and to analyze changes in corporate policies around the tax policy change. A key contribution of our analysis is that we can cleanly identify the impact of tax incentives on corporate policies and address some significant identification challenges found in previous empirical work. The combination of event study and time-series evidence that we document demonstrates important interactions between tax incentives and corporate policies.

The Tax Fairness Plan eliminated a mechanism that allowed trusts to substantially avoid paying all corporate taxes. We show that prospective tax shields mitigate the impact of the new tax and on average contribute 4.6% to firm value. Miller (1977) argues that the value of tax shields depends on personal taxes and that investors will form tax clienteles. We find that personal taxes affect the value of tax shields and that tax shields are worth more for firms that have a low tax investor clientele which is most hard hit by the tax. While these results are based on Canadian data, they likely have broader significance as the Canadian tax system has similarities to that of the U.S., for instance, a degree of double taxation of corporate income and higher personal taxes on interest compared to equity income.

Going beyond the value of corporate policies, we examine changes in corporate policies around tax changes. Consistent with the tax interpretation of the event study, we find that firms decreased their leverage after becoming a trust and increased it after the TFP, providing them with additional tax shields when the trust tax shield expired in 2011. To access the tax benefit, firms made accommodative changes to other corporate policies such as payout, cash holdings, and investment after becoming a trust. These changes were reversed after the TFP. We also show that tax incentives influenced acquisition policy.

Finally, our paper reinforces a key finding from the capital structure literature, namely, the importance of distinguishing between the gross benefit of a corporate policy choice that reduces corporate taxes and the net benefit. We find that this principle also applies when the tax savings opportunity is an organizational form choice. By organizing themselves as trusts, firms could avoid paying corporate taxes. In the absence of frictions and other costs associated with being a trust, the tax benefit could increase value by a maximum of 31.5%. However, trusts had to make accommodative changes to a number of corporate policies to access the tax benefit. For example, our evidence suggests that trusts made potentially inefficient and costly changes to their investment policies. As a result, the net tax benefit of being a trust was likely worth less than the potential gross benefit of 31.5%. While we cannot quantify the exact magnitude of this difference, the idea that firms are willing to make costly changes to important policies to gain preferential tax treatment is consistent with all our other evidence that suggests taxes have an important impact on corporate decision-making.

[1] See *Reuters News*, October 31, 2006 and *The Globe and Mail*, November 1, 2006, page B1. A Factiva search reveals that from the election of the Conservative government in January 2006 through September 2006, there was no discussion in the press that indicated market participants expected a tax policy change. In September and October 2006, two major telecommunications firms, Telus and BCE, announced plans to convert to the trust structure. Following the BCE announcement on October 11, some market commentators wondered whether the government would be forced to review the trust structure. However, none expected any action in the immediate future, noting the high political costs (see, for example, "Expert sees no change in income trust law: Despite costing $1B a year, Tories unlikely to act," *The Ottawa Citizen*, October 27, 2006). From the BCE announcement through October 31, returns in the trust market were 6.2% compared to 6.6% for the overall market.

[2] For further information on the history and institutional details of the income trust market, see Hayward (2002), Aggarwal and Mintz (2004), Edgar (2004), and Halpern and Norli (2006).

[3] Tax rates, from PriceWaterhouseCoopers (2006), include federal and provincial taxes (for Ontario). The 2006 federal budget announced that the corporate tax rate would fall from 21% to 19% by 2010. Adding provincial taxes (for Ontario), the corporate tax rate was 35% in 2006 and would fall to 32% by 2010. The TFP proposed to reduce it to 31.5% in 2011.

[4] Not all firms adopted this structure. Within an industry, more predictable earnings made some firms better candidates. Also playing a role were factors unrelated to fundamentals such as conservatism of boards to change their firms' organizational form. This was gradually being overcome, particularly as other firms successfully converted or announced plans to convert to a trust structure.

[5] Using data from a private vendor (www.targeted.ca), we estimate that the average institutional ownership of income trusts in October 2006 was 34%.

[6] The government's announcement on October 31 did not specifically mention the tax rate that would be applied to trusts but it did state that the federal corporate tax rate would decrease by one-half percentage point. Some press releases on November 1 noted that trusts would face a tax rate of 31.5% starting in 2011. However, in other press releases that day, several analysts and fund managers stated that they were uncertain about the extent to which trusts would be taxed. To implement the tax, trusts would not be allowed to deduct distributions of "nonportfolio earnings" to unit holders (nonportfolio earnings include income attributable to a business carried on in Canada and income from, or gains from the disposition of, nonportfolio properties). Consequently, trusts would pay tax on non-

deductible distributions at a rate of 31.5%. From an investor's perspective, a distribution paid by a trust would be treated in the same way as a taxable dividend from a Canadian corporation.

[7] This was a real concern among policy makers. David Dodge, the governor of the Bank of Canada, stated that "…the tax system was actually creating inefficiencies in capital markets, inefficiencies that, over time, would lead to lower levels of investment, output and productivity" (see www.bankofcanada.ca/2007/02/speeches/opening-statement-01-february-2007/).

[8] See "Income trust shock waves sink Toronto stocks," *Reuters News*, November 1, 2006.

[9] Another analyst stated, "There are too many uncertainties right now. I see continued volatility until we get more clarity. Flaherty's 'tax fairness' plan is still a proposal, not legislation, which is one point of uncertainty.... We believe the full extent of the changes has not been well-disseminated to US retail investors, and trusts with high US ownership are likely to continue to come under pressure in the coming days." See "Market plunges following new trust tax proposal," *Daily Oil Bulletin*, November 2, 2006.

[10] Suppose that on October 31, E_0 = \$100, D_0 = \$50, and V_0 = \$150, and after the TFP, E_1 = \$85, D_1 = \$50, and V_1 = \$135. The percentage change in equity value, $(E_1 - E_0)/E_0$, is -15% and the percentage change in firm value, $(V_1 - V_0)/V_0$, is -10%. Note that $(V_1 - V_0)/V_0 = (E_1 - E_0 + D_1 - D_0)/V_0$. Given that $D_0 = D_1$, it equals $(E_1 - E_0)/V_0 = ((E_1 - E_0)/E_0) \times E_0/V_0$. Therefore, the change in firm value equals the equity return multiplied by E_0/V_0, for example, -15% × 100/150 = -10%. We use the CAR rather than the actual (unadjusted) equity return. We prefer the CAR because it accounts for market movements unrelated to the TFP that could affect trust returns over the event window.

[11] We provide key summary statistics in the text of the paper. We provide more complete summary statistics for the variables used in this section in the Internet Appendix.

[12] We calculate nondebt tax shields as is commonly done in the literature as depreciation and amortization scaled either by firm value (Table III) or total assets (Table V); see, for example, Bradley, Jarrell, and Kim (1984), Graham (1996), and Fama and French (2002). We use the combined value of prospective debt and nondebt tax shields because they are substitutes and jointly determined.

[13] An alternative way to identify the tax status of the marginal investor is to use the percentage of shareholdings by shareholder type, for example, institutional versus retail. Desai and Jin (2011) argue that one reason many studies fail to find evidence of tax-related clientele effects is that the broad classification of investors as institutional or retail is too coarse. They show that there is substantial heterogeneity in the tax preferences of institutional investors. A similar issue arises with retail investors. We do not have data that allow us to identify shares held by institutional and retail investors in taxable versus nontaxable accounts. Therefore, we do not use this approach.

[14] Elton and Gruber (1970), Green and Rydqvist (1999), Graham, Michaely, and Roberts (2003), and Elton, Gruber, and Blake (2005), among others, find evidence consistent with tax effects. Arguments counter to the tax interpretation focus on short-term arbitrage trading around the ex-day (Kalay (1982)) and microstructure arguments such as price discreteness and the bid-ask bounce (Bali and Hite (1998) and Frank and Jagannathan (1998)). Graham, Michaely, and Roberts (2003) exploit changes in tick sizes in U.S. markets and do not find evidence consistent with the latter argument. Because income trusts pay out relatively small monthly payments, short-term trading is unlikely to be profitable after transaction costs.

[15] In the standard dividend model, lagged dividend status is a strong predictor of whether a firm pays dividends in the current year. Almost all trusts had high payouts, while the median corporate had zero payout. Therefore, lagged payout at the firm level is highly correlated with *Trust dummy*. We use lagged industry payout instead.

[16] See "Ottawa faces tax loss in trusts," *The National Post*, January 27, 2010. It is also important to note that trusts that exceeded "normal growth" guidelines during the transition period would lose their preferred tax status. Based on market capitalizations on October 31, 2006, trusts were allowed to grow via equity issues and acquisitions by a total of 100% through January 1, 2011. However, acquisitions of other trusts were typically not considered as growth (see www.fin.gc.ca/n06/06-082-eng.asp).

[17] See "Takeover proof trusts have a cost," *The Globe and Mail*, October 19, 2006.

[18] Although the model has a binary dependent variable, we report results based on OLS estimations. The reason is that the interpretation of β, γ, and δ is more complicated in nonlinear models like logit or probit, for example, β is not a time effect constant across groups and γ is not a group difference constant over time. Both vary by group and by *X*, though they do implicitly define time and group effects (see Puhani (2012)). Finally, we are interested in differences between trusts and corporates in both the pre- and the post-TFP periods. Therefore, we prefer this specification to one that excludes *Post-TFP* and *Trust dummy* and includes firm and year fixed effects instead.

[19] We use U.S. data for two reasons. First, these data capture the fact that many potential acquirers were American firms. Second, the Canadian market is much smaller and in many industries there are a small number of observations, some of which include acquisitions of trusts that would skew the measure.

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Table I
The Income Trust Market

This table shows the number of income trusts at the end of each year that are in *Datastream*. U.S.-based trusts listed on the Toronto Stock Exchange via Income Participating Securities or Income Depositary Securities and REITs are excluded. "Pending" refers to firms that announced plans to convert to the trust structure, but had not completed the conversion before the TFP announcement on October 31, 2006. Market value ($, millions) is based on year-end prices. The last column shows the percentage of total market value in Canada accounted for by the trust sector.

Year	Business trusts	Energy trusts / pipelines	All trusts	Market value of trusts	% of total market value
1996	2	15	17	$2,104	0.37%
1997	7	19	26	$2,991	0.42%
1998	10	22	32	$4,116	0.55%
1999	11	23	34	$8,009	0.79%
2000	13	22	35	$12,001	1.00%
2001	18	26	44	$18,719	1.89%
2002	51	30	81	$32,278	3.62%
2003	69	41	110	$64,388	5.88%
2004	95	47	142	$98,061	7.41%
2005	139	58	197	$156,480	9.62%
2006	163	53	216	$165,410	8.77%
Pending in 2006	3	1	4	$88,287	
Total 2006	166	54	220	$253,698	12.85%
2007	136	44	180	$158,968	8.06%
2008	115	38	154	$87,008	6.65%
2009	94	31	126	$101,251	6.38%
2010	30	2	32	$9,254	0.48%
2011	18	1	19	$10,844	0.61%

Table II
The Value Drop Following the Tax Fairness Plan Announcement

The regression $R_{i,t} = \alpha_i + \beta_i \times R_{b,t} + \gamma_i \times Event + \varepsilon_{it}$ is estimated for each trust. R_i is the daily return for trust i and R_b is the value-weighted return on the benchmark portfolio that includes corporates only. *Event* is a dummy variable that equals one on days included in the event window. The (0,1) window includes November 1 and 2; the (0,2) window includes November 1, 2, and 3, etc. γ_i is the average abnormal return estimated over the event window for each trust. The CAR reported in the table equals the average value of the γ_i's over each event window, multiplied by the number of days in the event window. The value drop (change in firm value) equals the CAR × (E/V). The sample includes 149 income trusts (104 business trusts and 45 energy trusts / pipelines) and 22 REITs. U.S.-based trusts listed on the TSX via IPSs or IDSs are excluded. The model is estimated as a system of equations using SUR from July 1, 2005 to December 31, 2006 and is estimated separately for income trusts and REITs. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

		Day 0		(0,1) window		(0,2) window		(0,10) window	
	N	CAR	Value drop	CAR	Value drop	CAR	Value drop	CAR	Value drop
Income trusts									
All trusts	149	-0.1247***	-0.1021	-0.1747***	-0.1439	-0.1410***	-0.1158	-0.1857***	-0.1523
		(-18.11)		(-17.76)		(-9.62)		(-6.15)	
Business trusts	104	-0.1256***	-0.1028	-0.1700***	-0.1402	-0.1365***	-0.1125	-0.1719***	-0.1409
		(-18.92)		(-17.39)		(-9.42)		(-6.69)	
Energy trusts / pipelines	45	-0.1225***	-0.0996	-0.1854***	-0.1507	-0.1512***	-0.1223	-0.2175***	-0.1785
		(-13.46)		(-14.91)		(-8.59)		(-7.69)	
REITs	22	-0.0178***	-0.0094	-0.0369***	-0.0194	-0.0230**	-0.0121	0.0110	0.0076
		(-2.81)		(-4.16)		(-2.09)		(0.51)	

Table III
The Value of Corporate Policies

This table presents cross-sectional regressions that estimate the value of tax shields. The sample includes 146 trusts and three pending trusts with complete data on characteristics, measured at the end of 2005. U.S.-based trusts listed on the TSX via IPSs or IDSs and REITs are excluded. The dependent variable is the value drop following the TFP announcement on October 31, 2006. It is computed over the (0,10) window and is multiplied by -1. Day 0 is November 1. All variables are defined in Table A.I. *t*-statistics are computed with standard errors clustered by industry. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Predicted sign	(1)	(2)	(3)	(4)	(5)
Constant		0.342***	0.263**	0.400***	0.403***	0.372***
		(2.88)	(2.46)	(3.32)	(3.48)	(3.15)
Prospective tax shields	–	-0.198***	-0.272***	-0.165**	-0.181***	-0.119*
		(-2.82)	(-3.56)	(-2.57)	(-2.76)	(-1.84)
Low tax shield dummy	+		0.049***			
			(2.76)			
Ex-day drop ratio	+			0.049***		
				(2.94)		
Low tax investor dummy	+				0.033***	0.097***
					(2.91)	(6.05)
Low tax investor dummy × prospective tax shields	–					-0.341***
						(-3.96)
% value in 1st four years	–	-0.074	-0.062	-0.094	-0.092	-0.096
		(-1.04)	(-0.92)	(-1.42)	(-1.41)	(-1.53)
Pending dummy	–	-0.037	-0.042*	-0.037	-0.026	-0.022
		(-1.59)	(-1.82)	(-1.52)	(-1.11)	(-0.97)
Log(Total assets)	?	-0.009	-0.004	-0.017*	-0.015*	-0.013
		(-1.09)	(-0.50)	(-1.86)	(-1.76)	(-1.54)
Number of observations		149	149	149	149	149
Adjusted R^2		0.0753	0.1309	0.1062	0.0921	0.1235

Table IV
Changes in Corporate Policies After Firms Become Income Trusts

This table presents regression estimates of changes in firms' corporate policies after they become an income trust from 2001 to 2006. The sample includes corporates and trusts with data on firm characteristics. U.S.-based trusts listed on the TSX via IPSs or IDSs and REITs are excluded. The dependent variable is leverage, payout, cash holdings, or investment. *Trust dummy* equals one for firms organized as trusts in a given year. *Industry median* is industry median leverage in models (1) and (2), payout in (3) and (4), cash holdings in (5) and (6), and investment in (7) and (8). Control variables are lagged by one year. All variables are defined in Table A.I. *t*-statistics are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Leverage		Payout		Cash holdings		Investment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	0.006	-0.101	0.024	0.027	0.477***	0.440***	0.932***	0.926***
	(0.07)	(-1.21)	(1.56)	(1.55)	(6.60)	(5.86)	(9.44)	(9.54)
Trust dummy	-0.033**	-0.026*	0.074***	0.073***	-0.010	-0.016**	-0.045**	-0.062***
	(-2.30)	(-1.87)	(10.24)	(10.21)	(-1.32)	(-2.01)	(-2.36)	(-3.15)
Growth opportunities$_{t-1}$		-0.005*				0.003		0.018***
		(-1.80)				(0.95)		(5.93)
Tangibility$_{t-1}$		0.122***						
		(4.57)						
Profitability$_{t-1}$		-0.054**						
		(-2.12)						
After-tax earnings$_{t-1}$				0.009***				
				(3.44)				
Investment$_{t-1}$				-0.005		-0.045***		
				(-1.30)		(-2.71)		
Cash flow$_{t-1}$						-0.004		0.038**
						(-0.21)		(2.24)

Table IV, continued

	Leverage		Payout		Cash holdings		Investment	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Leverage_{t-1}$						-0.060***		-0.121***
						(-2.65)		(-4.55)
$NWC\ (excluding\ cash)_{t-1}$						-0.023		
						(-0.85)		
$Industry\ CF\ volatility_{t-1}$		0.260***				0.024		
		(2.59)				(0.22)		
$Industry\ median_{t-1}$		0.124***		0.048		0.112**		0.103
		(2.92)		(0.14)		(2.05)		(0.85)
$Log\ assets_{t-1}$	0.019***	0.021***	-0.001	-0.002	-0.030***	-0.027***	-0.066***	-0.067***
	(2.78)	(3.19)	(-1.05)	(-1.04)	(-5.00)	(-4.24)	(-8.10)	(-8.26)
Firm fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Number of observations	4888	4888	5088	5088	4297	4297	4729	4729
Adjusted R^2	0.7789	0.7848	0.7352	0.7360	0.7673	0.7693	0.7549	0.7651

Table V
Changes in Leverage After the Tax Fairness Plan

This table examines changes in leverage for income trusts from 2007 to 2010. The sample includes corporates and trusts with data on firm characteristics. U.S.-based trusts that were listed on the TSX via IPSs or IDSs and REITs are excluded. The dependent variable in each regression is the change in book leverage from year t-1 to year t. In models (1) and (2), *Trust dummy* equals one for firms organized as trusts at the end of 2006. In (3), it equals one for firms organized as trusts at the end of 2006 but is set to zero in the year of conversion and in subsequent years for trusts that converted to a corporation. *Trust converted dummy* is set to one for these observations. In (4) and (5), the sample includes firms that were organized as trusts at the end of 2006. In (4), *Trust tax shields* in 2006 equals total tax shields at the end of 2006. In (5) it equals debt tax shields. All variables are defined in Table A.I. *t*-statistics are clustered by firm. The *F*-test tests whether the coefficients on *Trust dummy* and *Trust converted dummy* are equal. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Full sample			Income trusts only	
	(1)	(2)	(3)	(4)	(5)
Constant	0.029**	0.059***	0.060***	0.052	0.050
	(2.00)	(3.62)	(3.69)	(1.45)	(1.39)
Trust dummy	0.010***	0.015***	0.016***		
	(2.76)	(3.54)	(3.62)		
Trust converted dummy			0.006		
			(0.64)		
Trust tax shields in 2006				-0.084***	-0.089***
				(-4.27)	(-4.20)
Change growth opportunities		0.011***	0.011***	-0.003	-0.003
		(3.16)	(3.14)	(-0.17)	(-0.17)
Change tangibility		0.163***	0.163***	0.193*	0.197*
		(5.39)	(5.38)	(1.92)	(1.96)
Change profitability		-0.063**	-0.063**	-0.094	-0.088
		(-2.47)	(-2.48)	(-1.37)	(-1.29)
Change industry CF volatility		0.058	0.058	-0.119	-0.120
		(0.69)	(0.69)	(-0.84)	(-0.85)
Change industry leverage		0.509***	0.507***	-0.112	-0.112
		(3.04)	(3.03)	(-1.39)	(-1.38)
$Leverage_{t-1}$		-0.140***	-0.139***		
		(-10.46)	(-10.34)		
$Industry\ leverage_{t-1}$				-0.030	-0.028
				(-0.38)	(-0.36)
$Log(Total\ assets)_{t-1}$	-0.002**	-0.001	-0.001	0.002	0.002
	(-2.54)	(-0.67)	(-0.65)	(0.69)	(0.71)
Industry fixed effects	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes
F-test (*p*-value)			0.29		
Number of observations	3716	3716	3716	619	619
Adjusted R^2	0.0094	0.0956	0.0956	0.1046	0.1045

Table VI
Changes in Payout, Cash Holdings, and Investment After the Tax Fairness Plan

This table examines changes in payout, cash holdings, and investment for income trusts from 2007 to 2010. The sample includes corporates and trusts with data on firm characteristics. U.S.-based trusts that were listed on the TSX via IPSs or IDSs and REITs are excluded. The dependent variable is the change in payout, change in cash holdings, or change in investment from year *t-1* to year *t*. In models (1), (2), (4), (5), (7), and (8), *Trust dummy* equals one for firms organized as trusts at the end of 2006. In (3), (6), and (9), it equals one for firms organized as trusts at the end of 2006 but is set to zero in the year of conversion and in subsequent years for trusts that converted to a corporation. *Trust converted dummy* is set to one for these observations. *Change industry median* is the change in median industry payout in (1) to (3), cash holdings in (4) to (6), and investment in (7) to (9). All variables are defined in Table A.I. *t*-statistics are clustered by firm. The *F*-test tests whether the coefficients on *Trust dummy* and *Trust converted dummy* are equal. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Change in payout			Change in cash holdings			Change in investment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant	-0.000	-0.000	0.000	-0.027**	0.000	0.001	0.017	0.023	0.024
	(-0.11)	(-0.11)	(0.05)	(-2.06)	(0.02)	(0.06)	(1.19)	(1.54)	(1.61)
Trust dummy	-0.010***	-0.010***	-0.007***	0.010***	0.008***	0.009***	0.016***	0.015***	0.017***
	(-8.00)	(-8.09)	(-5.63)	(3.91)	(3.22)	(3.45)	(3.32)	(3.19)	(3.58)
Trust converted dummy			-0.021***			0.004			0.003
			(-7.79)			(0.70)			(0.20)
Change after-tax earnings		0.029***	0.029***						
		(5.07)	(5.00)						
Change investment		-0.005**	-0.005**		-0.055***	-0.055***			
		(-2.25)	(-2.39)		(-3.76)	(-3.76)			
Change growth opportunities					0.039***	0.039***		0.007	0.007
					(8.43)	(8.43)		(1.33)	(1.32)
Change CF					0.021	0.021		0.015	0.015
					(1.60)	(1.59)		(1.10)	(1.09)
Change leverage					-0.179***	-0.180***		-0.012	-0.013
					(-6.55)	(-6.54)		(-0.35)	(-0.37)

Table VI, continued

	Change in payout			Change in cash holding			Change in investment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change NWC (excluding cash)					-0.110***	-0.110***			
					(-4.57)	(-4.57)			
Change payout					-0.029	-0.032			
					(-0.34)	(-0.38)			
Change industry CF volatility					0.105	0.105			
					(1.21)	(1.21)			
Change industry median		0.856	0.909		0.735***	0.734***		1.264***	1.267***
		(1.23)	(1.31)		(5.28)	(5.27)		(9.97)	(9.98)
Industry payout$_{t-1}$		-0.823*	-0.819*						
		(-1.77)	(-1.78)						
Log(Total assets)$_{t-1}$	0.000***	0.000***	0.000***	0.001*	0.001	0.001	-0.003***	-0.003***	-0.003***
	(2.77)	(2.75)	(3.02)	(1.75)	(0.65)	(0.65)	(-2.75)	(-2.93)	(-2.90)
Industry fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
Year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes	yes
F-test (*p*-value)			0.00			0.44			0.25
Number of observations	3622	3622	3622	3064	3064	3064	3631	3631	3631
Adjusted R^2	0.0374	0.0525	0.0613	0.0145	0.1184	0.1181	0.0472	0.1026	0.1026

Table VII
Acquisitions Before and After the Tax Fairness Plan

This table presents OLS regressions that compare acquisition activity by income trusts and corporates before and after the TFP from 2003 to 2011. The sample includes corporates and trusts with data on firm characteristics. U.S.-based trusts that were listed on the TSX via IPSs or IDSs and REITs are excluded. In the target regressions in models (1) and (2), the dependent variable equals one if a firm was acquired in a given year and zero otherwise. In the acquirer regressions in (3) and (4), the dependent variable equals one if a firm acquired another firm and is zero otherwise. *Post-TFP* is a dummy that equals zero for 2003 to 2006 and one for 2007 to 2011. *Trust dummy* equals one for firms that were organized as trusts in a given year. With the exception of *Runup*, all control variables are lagged by one year. All variables are defined in Table A.I. *t*-statistics are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

	Target regressions		Acquirer regressions	
	(1)	(2)	(3)	(4)
Constant	0.030	0.073***	-0.103	-0.064
	(1.23)	(2.66)	(-1.21)	(-0.76)
Post-TFP dummy	0.005	0.004	0.007	0.012
	(1.04)	(0.79)	(0.83)	(1.40)
Trust dummy	-0.030***	-0.036***	0.066***	0.053**
	(-3.86)	(-4.56)	(2.79)	(2.23)
Post-TFP × Trust dummy	0.057***	0.058***	-0.025	-0.016
	(4.33)	(4.47)	(-0.96)	(-0.60)
Runup		0.001		0.005*
		(1.16)		(1.94)
Growth opportunities$_{t-1}$		-0.011***		0.011**
		(-5.22)		(2.30)
Profitability$_{t-1}$		0.004		0.090***
		(0.19)		(2.87)
Leverage$_{t-1}$		0.024*		-0.068***
		(1.84)		(-3.19)
Turnover$_{t-1}$		0.031***		0.028***
		(4.69)		(2.62)
Log(Total assets)$_{t-1}$	-0.001	-0.005***	0.018***	0.015***
	(-0.98)	(-3.62)	(6.18)	(4.57)
Industry fixed effects	yes	yes	yes	yes
Number of observations	7323	7323	7323	7323
Adjusted R^2	0.0108	0.0164	0.0312	0.0378

Table VIII
Robustness Checks

Panel A presents robustness checks based on model (5) of Table III. Models (1) to (5) add variables to capture various nontax explanations of the value drop. Model (6) uses the change in equity value (CAR) instead of the value drop as the dependent variable. *t*-statistics are computed with standard errors clustered by industry. Panel B presents robustness checks based on model (2) of Table V and models (2), (5), and (8) of Table VI. In models (1), (2), (4), (5), (7), (8), (10), and (11), the dependent variable and control variables (not reported) are defined in changes. The models are estimated from 2007 to 2010. In models (3), (6), (9), and (12), the dependent variable and control variables (not reported) are defined in levels. The models are estimated over 2003 to 2010. *Trust dummy* equals one for firms organized as trusts at the end of 2006. *Post-TFP* is a dummy that equals zero for 2003 to 2006 and one for 2007 to 2010. All variables are defined in Table A.I. *t*-statistics are clustered by firm. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.384***	0.390***	0.423***	0.387**	0.360***	0.305***
	(3.09)	(3.19)	(3.82)	(2.83)	(3.46)	(2.99)
Prospective tax shields	-0.118*	-0.122*	-0.131**	-0.313**	-0.118	-0.091
	(-1.85)	(-1.89)	(-2.15)	(-2.73)	(-1.67)	(-1.46)
Low tax investor dummy	0.094***	0.097***	0.095***	0.035	0.096***	0.086***
	(6.31)	(6.25)	(5.68)	(1.02)	(5.86)	(5.03)
Low tax investor dummy × prospective tax shields	-0.330***	-0.360***	-0.327***	-0.067	-0.349***	-0.327***
	(-3.95)	(-3.96)	(-3.83)	(-0.50)	(-4.08)	(-3.37)
Underinvestment	0.064					
	(1.33)					
Takeover activity		-0.784***				
		(-3.53)				
Prospective cash holdings			-0.279			
			(-1.33)			
Governance				-0.001		
				(-0.77)		
Acquired dummy					-0.022	
					(-0.99)	
Months survived					-0.000	
					(-0.01)	
Controls included	yes	yes	yes	yes	yes	yes
Number of observations	149	149	149	52	149	149
Adjusted R^2	0.1210	0.1344	0.1270	0.2832	0.1229	0.0868

Table VIII, continued

Panel B

	Change leverage	Change leverage	Leverage	Change payout	Change payout	Payout	Change cash	Change cash	Cash holdings	Change investment	Change investment	Investment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Constant	0.063***	0.038**	-0.060	-0.001	0.008*	0.003	-0.000	0.007	0.528***	0.030*	0.013	1.112***
	(3.55)	(2.07)	(-0.90)	(-0.13)	(1.67)	(0.32)	(-0.02)	(0.68)	(9.40)	(1.75)	(0.89)	(11.18)
Trust dummy	0.014***	0.022***		-0.010***	-0.010***		0.008***	0.005		0.016***	0.013**	
	(3.11)	(4.91)		(-8.49)	(-8.47)		(2.97)	(1.60)		(3.18)	(2.52)	
Post-TFP × Trust dummy			0.079***			-0.008**			0.022***			0.030***
			(7.64)			(-2.21)			(3.93)			(3.52)
Controls included	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Firm FE	no	no	yes	no	no	yes	no	no	yes	no	no	yes
Industry FE	yes	yes	no	yes	yes	no	yes	yes	no	yes	yes	no
Year FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Number of observations	3290	2680	7133	3217	2640	7237	2972	2134	6040	3267	2604	6970
Adjusted R^2	0.1018	0.1327	0.7473	0.0625	0.0583	0.7105	0.1166	0.1013	0.7647	0.1057	0.1072	0.6715

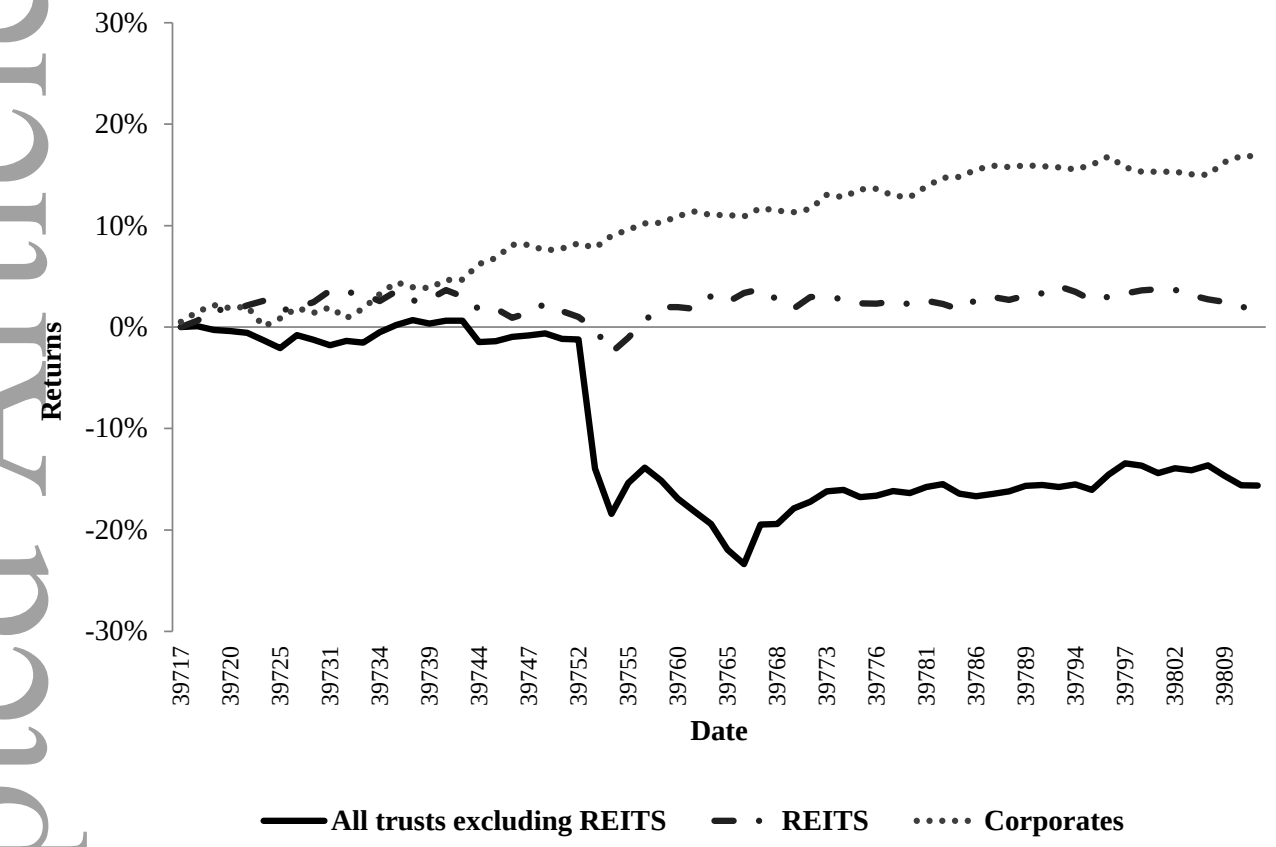

Figure 1. Returns around the Tax Fairness Plan announcement. This figure shows cumulative abnormal returns on portfolios of trusts and REITs from September 26 to December 29, 2006 (days -25 to +40 around the October 31 announcement). It also shows cumulative returns on a value-weighted portfolio of corporates. To compute abnormal returns, the regression $R_{pt} = \alpha + \beta_m \times R_{bt} + \beta'_i \times R_{indt} + \gamma' \times Event + \varepsilon_t$, is estimated from July 1, 2005 to December 31, 2006. *Event* is a vector that includes dummy variables for the event days, where November 1, 2006 is Day 0. R_p is the daily return on a value-weighted portfolio that includes all trusts (includes pending trusts but excludes REITs and U.S.-based trusts that were listed on the TSX via IPSs or IDSs) or on a value-weighted portfolio of REITs. R_b is the daily return on the value-weighted benchmark portfolio and R_{ind} is a vector that includes the daily returns on five value-weighted industry portfolios based on the Fama-French 5 industry classifications. The benchmark and industry portfolios include corporates that have total assets of at least $10 million and trade on at least 40% of the trading days in a given year.

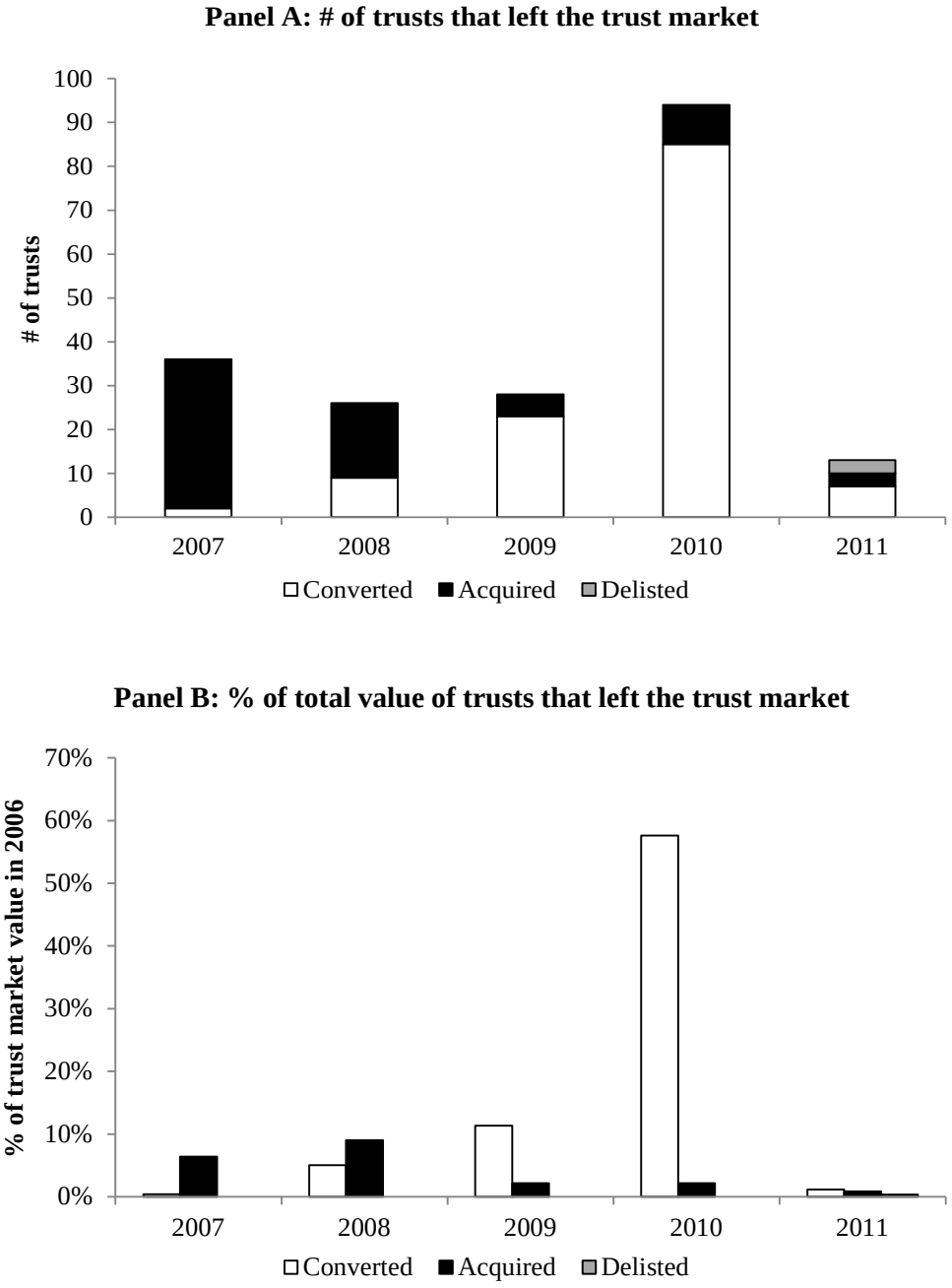

Figure 2. The decline of the income trust market after the Tax Fairness Plan. Panel A shows the number of trusts that were acquired, converted to a public corporation, or delisted in a given year from 2007 through 2011. Panel B shows the percentage of the total market value of trusts (at the end of 2006) that were acquired, converted, or delisted in a given year. For example, in 2007, 34 trusts worth $10.6 billion at the end of 2006 were acquired. These 34 trusts accounted for 6.4% of the total market value of trusts at the end of 2006 (the total value of the trust sector was $165 billion). U.S.-based trusts that were listed on the TSX via IPSs or IDSs and REITs are excluded.

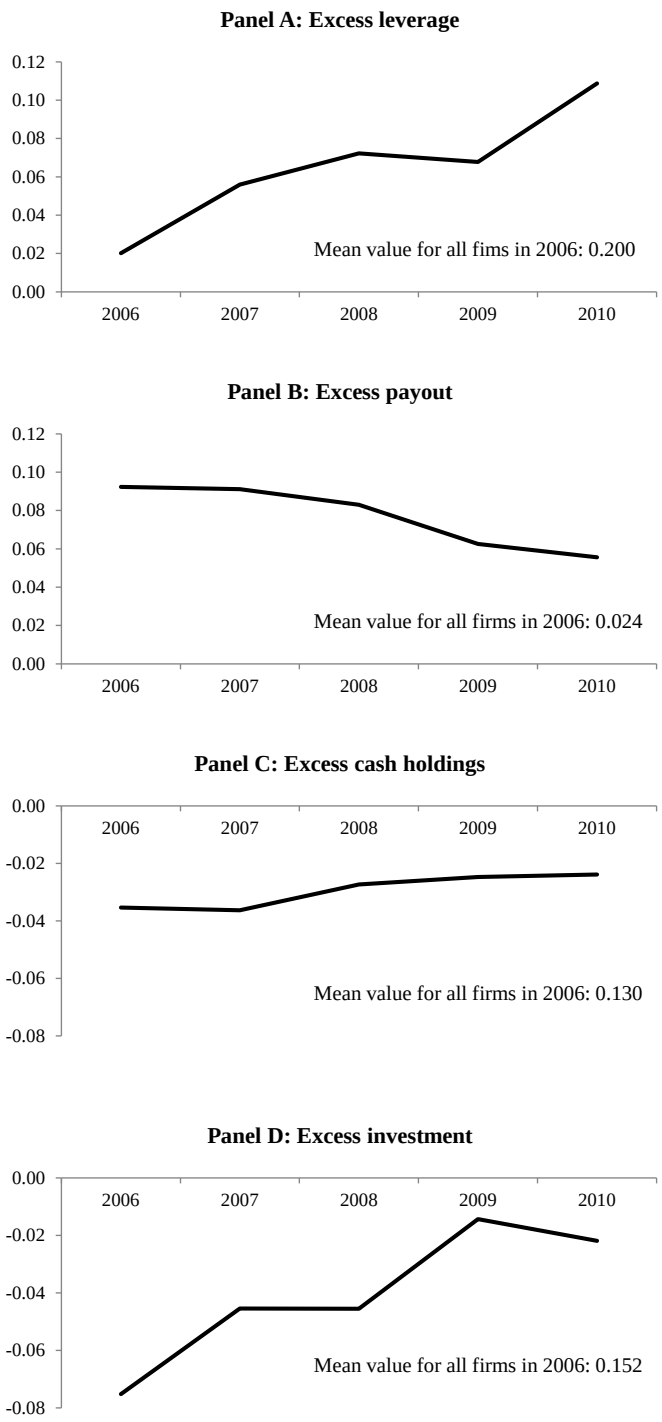

Figure 3. The evolution of corporate policies after the Tax Fairness Plan. Panel A shows trusts' leverage in excess of the median leverage of corporates in the same industry based on the Fama-French 49 industry classification scheme. Each year, excess leverage is computed for each trust and is averaged across trusts. The sample is restricted to trusts that are in the sample each year from 2006 to 2010. U.S.-based trusts that were listed on the TSX via IPSs or IDSs and REITs are excluded. Panels B, C, and D are similar but show the evolution of excess payout, cash holdings, and investment. All variables are defined in Table A.I.

Table A.I
Variable Definitions

All firm-level accounting data are from *Worldscope*. Stock return and volume data are from *Datastream*. M&A data are from *SDC*. With the exception of total assets and market capitalization, firm characteristics are winsorized at the 1% and 99% levels. Industries are defined using the Fama-French 49 industry classification scheme.

Variable	Definition
Acquirer	Equals zero if a firm did not make an acquisition in a given year between 2003 and 2011, and equals one if a firm acquired another firm.
Acquired dummy	Equals one for trusts that were acquired between November 1, 2006 and December 31, 2010.
After-tax earnings	Net income before extraordinary items / Total assets.
Cash flow	(Net income before extraordinary items + Depreciation and amortization) / Total assets.
Cash holdings	(Cash + Short-term investments) / (Total assets).
Excess leverage	Trust leverage – Industry leverage.
Excess payout	Trust payout – Industry payout.
Excess cash holdings	Trust cash holdings – Industry cash holdings.
Excess investment	Trust investment – Industry investment.
Ex-day drop ratio	$\frac{P^b \times (1+ER) - P^{ex}}{D}$, where P^b is the price the day before the trust goes ex-dividend, *ER* is the trust's expected return (market return adjusted by the trust's beta), P^{ex} is the price on the day the trust goes ex-dividend, and *D* is the amount of the distribution (trusts pay distributions rather than dividends). We calculate this ratio around all distribution payments from January 2005 to October 2006 for each trust and use its median drop ratio over this period. We use only dates when there was one distribution payment and we drop observations for which the payment was not "regular cash," "special cash," or "partnership distribution." In particular, we drop distributions that include a return of capital. We also exclude observations when the distribution is so small (<\$0.01) that the response will be swamped by noise and when the trading volume suggests other information is also released on that day (volume > mean plus three standard deviations). The drop ratio is winsorized at the 5th and 95th percentiles.
Firm value	Short-term debt + Long-term debt + Market capitalization.
Governance	Governance score complied by *The Globe and* Mail and the Clarkson Centre for Business Ethics and Board Effectiveness at the University of Toronto and measured at the end of September 2006. Governance scores range from 0 to 100 and are based on 1) board composition, 2) shareholding and compensation, 3) shareholder rights, and 4) disclosure.
Growth opportunities	Proxied by Q, where Q equals (Short-term debt + Long-term debt + Market capitalization) / Total assets.
Investment	Capital expenditures$_t$ / Total assets$_{t-1}$.
Industry CF volatility	For each firm, CF is computed annually as (Net income before extraordinary items + Depreciation and amortization) / Total assets. For a given firm in year *t-1*, the standard deviation of CF is computed over the previous (up to) nine years. Industry CF volatility in year *t* equals the average standard deviation of corporates in a given industry.

Table A.I, continued

Variable	Definition
Industry leverage	Median leverage of corporates in a given industry in a given year.
Industry payout	Median payout of corporates in a given industry in a given year.
Industry cash holdings	Median cash holdings of corporates in a given industry in given year.
Industry investment	Median investment of corporates in a given industry in a given year.
Leverage	(Short-term debt + Long-term debt) / Total assets.
Log(Total assets)	Log of total assets.
Low tax shield dummy	Equals one for trusts with tax shields below the industry median tax shield at the end of 2005.
Low tax investor dummy	Equals one for firms with ex-day drop ratios above the 75th percentile.
Months survived	The number of months a trust remains as a trust following the TFP announcement through December 2010. Trusts that do not survive were either acquired or converted to a corporation.
Nondebt tax shields	Depreciation and amortization.
NWC (excluding cash)	(Current assets – Cash – Current liabilities) / Total assets.
% value in 1st 4 years	The ratio of the present value of cash flows for the next four years, $\frac{D_0(1+g)^t}{(1+K_e)^t}$, to firm value on the day prior to the TFP announcement. Because trusts paid out substantially all of their earnings, the present value of distributions is computed assuming they grow at market estimates. The annualized distribution (excluding special distributions and any return of capital) is based on October 2006 distributions. Earnings growth estimates are from I/B/E/S. The forecasted distributions are discounted at industry discount rates, summed, and divided by the value of the trust on the day before the TFP announcement. The discount rate is based on the beta for corporates in the same industry, under the assumption that these will be the relevant rates for trusts in the future.
Payout	(Dividends or Distributions) / Total assets.
Pending trust dummy	Equals one for firms that announced plans to convert to a trust but had not completed the conversion by October 31, 2006.
Post-TFP dummy	Equals zero during 2003 to 2006 and one during 2007 to 2011.
Profitability	(Operating income + Depreciation and amortization) / Total assets.
Prospective cash holdings	The median cash holdings of corporates in the same industry, at the end of 2005 (trusts are excluded). Cash holdings equal (Cash + Short-term investments) / Firm value.
Prospective tax shields	The median tax shield of corporates in the same industry, at the end of 2005 (trusts are excluded). Tax shields equal (Short-term debt + Long-term debt + Nondebt tax shields) / Firm value.
Runup	Total stock return over the prior two years.

Table A.I, continued

Variable	Definition
Tangibility	Net PPE / Total assets, where Net PPE equals Gross property, plant, and equipment minus accumulated reserves for depreciation, depletion, and amortization.
Takeover activity	The value of acquisitions of U.S. public firms in the same industry scaled by the value of all U.S. public firms and averaged over 2003 to 2006.
Target	Equals zero if a firm was not acquired in a given year between 2003 and 2011. It equals one if a firm was acquired.
Trust dummy (Table V)	Equals one for firms organized as trusts in a given year between 2001 and 2006.
Trust dummy (Table VI)	In models (1), (2), (4), (5), (7), and (8), it equals one for firms organized as trusts at the end of 2006. In (3), (6), and (9), it equals one for firms organized as trusts at the end of 2006 but is set to zero in the year of conversion and in subsequent years for trusts that converted to a corporation.
Trust dummy (Table VII)	Equals one for firms that were organized as trusts in a given year during 2003 to 2011.
Trust converted dummy	Equals one in the year of conversion and in subsequent years for trusts that converted to a corporation between November 1, 2006 and December 31, 2010.
Trust tax shields in 2006	In Table V, model (4), it equals (Short-term debt + Long-term debt + Nondebt tax shields) / Firm value at the end of 2006 for trusts. In model (5) it equals (Short-term debt + Long-term debt) / Firm value.
Turnover	Annual number of shares traded / Number of shares outstanding.
Underinvestment	Industry investment – Trust investment, at the end of 2005. Investment equals Capital expenditures / Firm value.
Value drop	The abnormal change in firm value over the event window. It is computed as the CAR × (E/V), where the CAR equals the estimate of γ for each trust multiplied by the number days in the event window. γ is the average abnormal return for each trust and is estimated from the regression $R_{it} = \alpha_i + \beta_i \times R_{bt} + \gamma_i \times Event + \varepsilon_{it}$. The E/V ratio for each trust is measured on October 31, 2006.