

Dynamic Corporate Finance is Useful: A Comment on Welch (2013)*

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ABSTRACT

Welch (2013) critiques recent work in dynamic corporate finance. We offer the contrasting view that there is no logical reason to dismiss entire research methodologies, and that many methods can be useful. We explain why dynamic models and structural estimation are useful research tools, as well as why the criticisms of this research paradigm in Welch (2013) are incorrect.

Welch (2013) criticizes recent advances in dynamic corporate finance and emphasizes the importance of natural experiments for empirical work. Our primary goal in this paper is to respond to these claims and to demonstrate that they are irrelevant or based on an inaccurate interpretation of the literature.¹ Welch (2013) raises four main issues. First, he claims that dynamic models in corporate finance ignore many important variables that are relevant to the questions that they address. Second, he states that there is a large gap between the models and the tests of the models. Third, he

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¹ For a more complete review of dynamic corporate finance theory and empirical methods, see Strebulaev and Whited (2012).

asserts that tests of the models largely ignore basic econometric concerns. Fourth, he suggests that dynamic corporate finance models have not been held to high test standards. Welch (2013) also evaluates both Hennessy and Whited (2005) and Strebulaev (2007), using empirical tests of what he believes to be the empirical implications of these papers. Throughout, he points out that these models are difficult to understand.

Welch (2013) concludes that this general line of research has not helped the profession understand the important issues in corporate finance and that it is unlikely to be useful in the future. Instead, he claims that natural experiments abound in corporate finance, and that more insights are likely to arise from this approach. Even though Welch (2013) chooses capital structure as its primary target, the scope of its claims is much larger as it applies to the entire field of dynamic corporate finance, including such long-standing and central fields in finance as real options (Dixit and Pindyck, 1994), dynamic contracting (Bolton and Dewatripont 2005), and dynamic investment (Stokey, 2009).

Unfortunately, as we explain below, all of these criticisms fall wide of their intended mark. The first and third points above apply, in various degrees to most models and empirical methods in economics. The second and fourth points are based on an inaccurate representation of the literature. In addition, the fourth point is based on a straw man argument. Finally, the specific tests of the models in Hennessy and Whited (2005) and Strebulaev (2007) either are ill-conceived, are irrelevant to the theory, or actually support the theory.

Instead, we offer a different message. There is no logical reason to simply dismiss broad classes of research methodologies. Dynamic models, static models, structural estimations, natural experiments, and descriptive studies can all lead to new insights. Moreover, different methodologies can be complementary to one another. The point is to glean what we can from a broad range of approaches and weave them together to obtain a better understanding of corporate finance. A complete description of the advantages of reduced-form, quasi-experimental approaches can be found in Angrist and Pischke (2010). Here, we briefly explain why dynamic models and structural estimation are also useful, with examples from corporate finance.

Dynamic models are useful because they can provide insights that cannot be obtained in other ways. For example, the optimal *timing* of investment projects, equity issuance, and debt refinancing are inherently dynamic, as

is the *speed* of adjustment of a leverage ratio to its target (see, for example, McDonald and Siegel, 1986; Fischer *et al.*, 1989; DeAngelo *et al.*, 2011). Taking corporate finance dynamics seriously also helps to shed new light on questions that can be examined in static frameworks. Important examples include the detection of financial constraints, the corporate diversification discount, the low-leverage puzzle, the effects of agency on leverage and investment, and the anomalous negative sensitivity of leverage to income, which stands in the face of static capital structure theories that predict higher leverage for high-income firms (see, for example, Hennessy and Whited, 2005, 2007; Gomes and Livdan, 2004; Morellec *et al.*, 2012). More generally, dynamic models can be useful for specifying precise null hypotheses for reduced-form tests — hypotheses that can be poorly conceived when derived from loose verbal arguments (see, for example, Strebulaev, 2007; Riddick and Whited, 2009).

Structural estimation can also lead to useful insights. For example, because the number of high-quality natural experiments in corporate finance is likely to be much smaller than the number of interesting questions, structural estimation often offers a more feasible alternative for understanding these interesting questions. Next, dynamic models often provide quantitative rather than simply qualitative implications. This feature is particularly useful in capital-structure research, in which the relative magnitudes of the costs and benefits of leverage have been the center of much of the research agenda. Furthermore, parameter estimates that are obtained from structural estimations are useful for counterfactual (“what-if”) analysis. Counterfactuals are particularly useful for understanding how firms respond to changes in their environment, and they are sometimes even useful for evaluating policies. Finally, as pointed out in Taylor (2010) and Breza (2012), although natural experiments are useful for making causal inference, they are less useful for understanding the specific economic forces behind these inferences. In this case, structural estimation of models can fill in this gap.

The natural-experiment approach is obviously useful, but the enthusiasm for it in Welch (2013) ignores several difficulties in implementation. First, we believe that it is difficult to find credible natural experiments in corporate finance. For example, tax changes rarely constitute natural experiments, notwithstanding the many statements in Welch (2013) to the contrary. If lobbying ever affects legislation, it affects tax legislation.

Therefore, political-economy considerations make nearly all tax changes invalid natural experiments. Even from a macroeconomic perspective, large tax reforms, such as the one in 1986, do not occur in a vacuum, so that the same forces that induce governments to change taxes also likely affect firm behavior. In fact, exactly because of this endogeneity, we know precious little about the impact of taxation on corporate decisions, as can be demonstrated by a recent quote: “In my view, the big open challenge in corporate finance is to produce evidence on how taxes affect market values and thus optimal financing decisions.” (Fama, 2011). Although some recent studies have tried to deal with this endogeneity issue (for example, Pérez-González *et al.*, 2012; Heider and Ljungqvist, 2012), the small number of credible attempts speaks to the difficulty of finding natural experiments.

Second, the applicability of natural experiments can be limited because it is often difficult to transfer the results of these experiments to broader samples and issues. For example, insights from data on small, developing countries may or may not have implications for large, developed capital markets. Third, and more generally, it has long been argued, at least since Heckman (1997), that even natural experiments with truly random data variation require further assumptions in order for one to believe that one can estimate a causal effect (see also Keane, 2010a).

Although the quasi-experimental approach can be difficult to implement, the corporate finance literature does contain clever, insightful studies that are based on natural experiments. For example, Bennedsen *et al.* (2007) examine the effects of in-family CEO succession on firm performance using a biological instrument, and Becker (2007) examines the effects of credit supply on economic outcomes using senior citizen populations as exogenous variation in the supply of funds. The number of insightful natural experiment studies in corporate finance continues to grow. Therefore, we view natural experiments as one of many useful research tools because this approach can, if well-conceived and executed, generate new insights.

We organize the rest of this article as follows. First, we address the four main points in Welch (2013). Second, we comment on the alternatives to the dynamic paradigm that are put forth in Welch (2013). Third, we address several further broad themes in Welch (2013). Fourth, we address commonly echoed critiques of structural work that are, ironically, not in Welch (2013). Fifth, we address the specific tests of Hennessy and Whited (2005)

and Strebulaev (2007). Finally, we conclude with a comparison between this debate and an analogous one in economics.

1 Specific Criticisms of the Dynamic Paradigm

This section examines the four main claims in Welch (2013). The models omit important forces; there is a gap between the theory and its tests; the literature contains many econometric flaws; and the models have not been subjected to stringent tests.

1.1 Omitted Forces

First, Welch (2013) claims that dynamic corporate finance models ignore important forces. For example, he states: “Although built on plausible forces, they have ignored many other plausible forces not based on the data but based on their authors’ priors.” This statement is hard to view as a critique because it is tautologically true. Models, in all fields of scientific endeavor, by nature omit important forces, and the corporate finance literature readily admits this issue. For example, Hennessy and Whited (2007) state: “It is also worth noting theories that we exclude from the model.” More generally, any model in corporate finance omits important forces. An obvious example is the path-breaking work by Modigliani and Miller (1958).

In the same vein of criticism, Welch (2013) provides a list of topics in capital structure that have received more attention from reduced-form empirical papers than from dynamic corporate finance studies. The claim is that the dynamic corporate finance literature has ignored these important topics. Of course, most of the studies on the list have (understandably) ignored other issues on the list. Such is the nature of research. It is hard to tackle more than one or two issues at a time. In addition, many of these questions have in fact been studied using structural estimation of dynamic models (see, for example, Dimopoulos and Sacchetto, 2011; Warusawitharana and Whited, 2012).

Finally, most structural-estimation papers do consider other plausible forces. In structural estimation, as in reduced-form work, it is important to check whether conclusions are robust to assumptions. Structural-estimation papers typically include a robustness section that addresses potential bias from forces that are omitted from the model (see, for

example, Taylor, 2010, 2013; Nikolov and Whited, 2012). It is common for the authors to extend the core model to include various omitted forces and check whether the main conclusions still go through. These robustness exercises are akin to including additional control variables in a regression, but they are disciplined by a formal, economic model.

1.2 The Gap Between Theory and Tests

The second broad criticism in Welch (2013) is the hardest to address because it uses a straw man argument. Welch (2013) states: “The gap between the theory and the evidence is so unusually large because the model tests lean heavily on inversions of structural first-order conditions, often linking entirely different kinds of variables together. My paper argues that, in the corporate finance context, direct empirical proxies for structural variables can often be found, and direct variables are likely to yield better tests.” Put differently, the claim in Welch (2013) is that the point of dynamic corporate finance is to find causal relationships between unobservable variables and observable variables, and that this task is hopeless because it is likely impossible to find credible proxies for the unobservable variables in dynamic models.

Welch (2013) attempts to establish this point via a simple linear model that combines elements from econometric (not economic) omitted-variables models and errors-in-variables models. Specifically, suppose that a dependent variable, y , depends in reality on a “right” variable, r , but that the researcher believes it depends on a “wrong” (and unobservable) variable, w . Suppose also that there is an observable proxy for w , $M \equiv w + r$. If a researcher runs the regression,

$$y = \hat{a} + \hat{b}M,$$

the individual will find a positive estimate of \hat{b} and will conclude incorrectly that w influences y .

Although there is nothing wrong with this specific argument at face value, it has nothing to do with the dynamic corporate finance literature. The purpose of Hennessy and Whited (2005) or Strebulaev (2007) is not to ascertain the effect of an unobservable variable, r , on an observable variable, y . Welch (2013) is correct in asserting that this task is virtually hopeless at worst and difficult at best. Fortunately, this task is not the point of dynamic corporate finance models. Although the models do in fact contain

unobservable variables, such as shocks to productivity and demand, the role of these variables is to represent specific sources of uncertainty. More importantly, the questions that are addressed by the models are not associated with how these unobservable shocks causally affect variables such as leverage and investment. Instead, the intent of both Hennessy and Whited (2005) and Strebulaev (2007) is to further both the qualitative and quantitative understanding of the effects of taxes on capital structure, of the interaction between investment and capital structure, and, most importantly, of relationships between endogenous variables that one *observes* in the data. In this sense, the link between theory and the data is quite close. All of the predictions from these models concern explicitly observable variables. Thus, the comment in Welch (2013) about a gap between theory and tests reflects a mischaracterization of the literature, and, more importantly, an attempt to tear down a straw man.

1.3 Econometric Issues

The third point in Welch (2013) concerns econometric problems in dynamic corporate finance. He states: “Their tests have largely ignored other econometric issues of importance in the corporate finance context, such as selection biases, survivorship biases, controls, and so on.” Welch (2013) specifies this point in relation to both Hennessy and Whited (2005) and Strebulaev (2007). Regarding both papers, Welch (2013) claims that they offer “few controls or residual diagnostics. [They do] not consider the effects of selection and survivorship biases.” These statements are puzzling because a large majority of the papers in this literature are pure theory pieces that, like all pure theory papers, contain no reduced-form empirical tests (see Strebulaev and Whited, 2012). Of course, econometric issues do not arise in theory papers. In particular, neither Hennessy and Whited (2005) nor Strebulaev (2007) contains reduced-form tests on actual, real-world data. Both papers conduct reduced-form regressions on simulated data, but this kind of exercise is theoretical in nature — not empirical. Although Hennessy and Whited (2005) contains a structural estimation exercise using real data, the types of econometric issues that are mentioned in Welch (2013) are not relevant because they pertain specifically to reduced-form regressions. For the types of econometric problems that *do* come up in structural estimation in corporate finance, see Strebulaev and Whited (2012). Finally, the papers that contain reduced-form tests of

dynamic models typically contain careful reduced-form tests. For example, Leary and Roberts (2005) consider data censoring, while Hennessy (2004) and Riddick and Whited (2009) treat measurement error bias.

1.4 Stringent Tests

The fourth main point in Welch (2013) is that the models in dynamic corporate finance have not been subject to stringent tests. He states: “The models have not been held to appropriately high test standards (in-difference estimation, quasi-experimental identification, out-of-sample prediction). Few tests have allowed for strong specific alternative explanations.” Although we welcome careful reduced-form work that is aimed at testing dynamic models, most of this statement reflects an inaccurate characterization of the literature. First, the few papers that conduct serious reduced-form tests of the implications of dynamic models (see, for example, Whited, 2006; Leary and Roberts, 2005) do consider many alternative explanations. Second, all of the structural-estimation papers in this literature have fewer parameters than the moments used to estimate them and thus are subject to standard specification tests.

Finally, and most importantly, the models that are used in structural estimation studies have nearly always been shown to be able to reproduce features of the data that were not used to estimate them. These types of exercises constitute out-of-sample tests. For example, the sensitivity of leverage to lagged weighted Tobin’s q (Baker and Wurgler, 2002) is not used to estimate the model in Hennessy and Whited (2005), yet the model reproduces the negative sensitivity that is seen in the data. The model in DeAngelo *et al.* (2011) can reproduce estimates of the speed of adjustment of capital structure to target that is found in actual data, even though no estimates of speed of adjustment are used to identify and estimate the model’s parameters. Although it is possible to estimate models using data from different time periods, it is uninformative to use this kind of exercise as an out-of-sample test to see whether the parameter estimates from one half of the sample predict the outcomes in the other. Although this type of test can be highly informative in the context of a reduced-form time-series model, in the context of the estimation of an economic model with a panel of firms, this kind of test is simply a useful tool for uncovering cross-sectional or time-series heterogeneity, which occurs naturally in corporate finance panel data. Thus, the critique based on a lack of out-of-sample tests is unjustified.

2 Alternative Methodologies

Welch (2013) offers two suggestions for alternatives to studying and estimating dynamic models. The first proposal is that dynamic models should be tested in quasi-experimental settings. However, this suggestion is largely infeasible. Finding a good natural experiment that actually corresponds to a model is the intersection of two extremely hard tasks: finding a good natural experiment and building a model that is rich enough to illuminate the experiment. We know of no such work in finance.

Interestingly, the tiny handful of studies that combine experimental and structural approaches in economics take a converse approach. They both test models with experimental data and use models to understand the economic forces behind the elasticities estimated from experimental data. For example, Attanasio *et al.* (2012) use experimental data from a Mexican cash transfer experiment to estimate a structural model and also use the model to interpret the data. Kaboski and Townsend (2011) test a dynamic model of household decision making with data from a Thai micro finance experiment and also use the model to interpret these data. Breza (2012) uses a natural experiment to identify peer effects that are related to the repayment of microfinance loans, and then estimates a dynamic discrete choice model to understand the reasons for these effects. All of these studies exploit random governmental interventions in relatively low-stakes markets in developing countries. It is hard to imagine governments using randomized experiments in high-stakes financial markets. Thus, although this approach can produce highly insightful studies, its applicability in corporate finance may be limited, and, as explained in Strebulaev and Whited (2012), structural estimation can lead to new insights without the use of experimental data.

Next, we comment on the statement in Welch (2013) that empirical studies in capital structure would be better off using linear regressions laden with proxy variables. This statement is troublesome in that it is well known that proxy variables contain measurement error, that measurement error can bias regression coefficients either upward or downward, and that even modest amounts of measurement error can flip regression coefficient signs when regressors are highly correlated with one another (see, for example, Klepper and Leamer, 1984; Krasker and Pratt, 1986; Erickson and Whited, 2012). Structural estimation, in contrast, is usually cast in terms of observable variables. Thus, the comment in Welch (2013) that

there is a large gap between theory and tests of the theory is all the more inaccurate.

3 Other Themes

We now address four further points that arise throughout Welch (2013). The first is the suggestion that dynamic capital structure is unnecessarily difficult and that this complexity is unwarranted given that we know very little about capital structure. We argue that dynamic capital structure considers simple economic mechanisms of first-order importance and that it is relatively straightforward. Dynamic models of capital structure are based on fundamental economic forces, such as taxes, distress costs, funding needs, asymmetric information, and agency. It is hard to argue that these notions are either esoteric or frivolous, given the current state of understanding of corporate capital structure.

Dynamic capital structure models themselves are also not complicated. First, the class of models that contains Hennessy and Whited (2005) is based on the simple idea that managers maximize the expected present discounted value of cash flows to equity holders. Indeed, if the backbone of a model can be written down in four equations, as done in Strebulaev and Whited (2012), it is hard to describe the model as complex. The class of models that contains Strebulaev (2007) is based on the simple idea that debt and equity are options on the underlying asset value of the firm. The key distinguishing feature of both of these classes of models is the basic idea that what you do today reflects the choices that you will or may make in the future.

Of course, both Hennessy and Whited (2005) and Strebulaev (2007) contain many minor details, so these two studies appear more complex than they actually are. We confess that the exposition can be dense. However, for an accessible tutorial on these methods, see Strebulaev and Whited (2012).

Second, Welch (2013) claims that because corporate managers are not subject to third-party arbitrage, dynamic corporate finance models are unlikely to be able to lend insights into firm behavior. This argument is incorrect because it implies that almost all dynamic models in economics are virtually useless inasmuch as few economic agents operate under the tight arbitrage conditions that one observes in derivatives markets. Dynamic

economics is generally widely accepted, and it is safe to say that dynamic economics has lent the profession many useful insights.

Third, Welch (2013) misrepresents the basic content of both Hennessy and Whited (2005) and Strebulaev (2007). In particular, he characterizes Hennessy and Whited (2005) as containing mostly qualitative insights and characterizes Strebulaev (2007) as a study that emphasizes quantitative predictions. These labels are unfortunate because they convey an inaccurate picture of the literature. It is true that Strebulaev (2007) derives actual quantitative predictions from a model, and these predictions are an advantage because they make the model and other similar models falsifiable. However, the most important results are qualitative in nature in that they develop and clarify the economic mechanisms that can drive the results in traditional empirical studies. In addition, Hennessy and Whited (2005) is also quantitative because it contains an explicit empirical section that analyzes actual data by using these data to estimate model parameters. The paper thus reaches explicit quantitative conclusions. In contrast, Strebulaev (2007) is not a structural-estimation paper, and it does not conduct regression analysis on actual, real-world data.

A related problem in Welch (2013) is the terminology that is used to categorize the literature. Specifically, the author describes Hennessy and Whited (2005) as a deep structure model with mostly qualitative insights and Strebulaev (2007) as a study that emphasizes quantitative predictions. We have already argued that both Hennessy and Whited (2005) and Strebulaev (2007) are quantitative. We now turn to the use of the term structural. According to Welch (2013), a deep structure model is one that contains an unobservable structural variable that influences observable variables such as investment and leverage. In Hennessy and Whited (2005) this variable is an unobserved shock to a profit function. However, Strebulaev (2007) also contains an unobservable structural variable — again a profitability shock. Thus, according to the definition in Welch (2013), both models are deep structure models. The definition of deep structure is thus not particularly useful. A theoretical model is structural if its parameters are arguably invariant to counterfactual exercises in which one changes one model feature to trace out the optimal reaction to this change. Thus, a model can be structural relative to some of its features, but not others, and in this context, structural is clearly a relative term. In terms of econometrics, structural

work derives equations, either analytically or numerically, to be estimated directly from a formal economic model.²

4 Unvoiced Critiques

In this section, we consider several further critiques of structural estimation that we have heard repeatedly, but that are not in Welch (2013). First, some researchers argue that because it is hard to make model assumptions that everyone can agree are correct, it is unclear whether the final results can be taken at face value. Of course, all empirical work uses assumptions. The difference between structural work and regression analysis is that all of the assumptions are laid out in the former but are implicit, and thus harder to find and critique, in the latter.

Next, some researchers argue that structural models are incorrect, so all estimation results are suspect. The answer here is that all models are simply approximations to reality and are therefore false. The interesting question is not whether the models that are used in structural estimation can be falsified, because all can. The interesting question is whether newer false models can do a better job in furthering the understanding of features of the data than older false models can.

A related question is whether one can interpret parameter estimates and other results if the model fails the test of overidentifying restrictions, which is fairly common. In other words, why isn't the game over as soon as the data reject the model? The answer is that *any* model, structural or reduced form, can be rejected if one confronts it with enough features of the data. However, with structural estimation, it is usually easier to understand precisely why the model breaks because all of the model assumptions are explicit. Furthermore, the careful delineation of assumptions is precisely what makes it possible to learn from models when they do fail. The classic example is Hansen and Singleton (1982), which contains a model that is strongly rejected by the data. It is safe to say that the profession has learned a great deal about asset pricing from this paper.

Finally, we have often been asked whether other models could fit the data equally well. Of course, this type of blanket skepticism is true of all empirical

² The term structural models has been confusingly used, for historical reasons, to describe a class of credit risk and corporate finance models that are based on option-pricing models. Strebulaev and Whited (2012) suggest replacing it with contingent claims models in all applications.

work. It is always easy to find ad hoc models with little economic content that fit data better than structural models. So again the right question is whether a model can provide further understanding of patterns in the data.

5 Tests

A portion of Welch (2013) is devoted to testing the models in Hennessy and Whited (2005) and Strebulaev (2007). We now explain why these tests are irrelevant.

5.1 Hennessy and Whited (2005)

Welch (2013) claims that one implication of the model in Hennessy and Whited (2005) is that leverage should be negatively correlated with lagged liquidity. He therefore regresses the *dca* variable from Welch (2004) on lagged cash balances.³ He obtains a negative coefficient and a tiny R^2 , and he concludes that the model fails the test. A closer read of Hennessy and Whited (2005) reveals that the word liquidity is intended to be the flow of liquidity (that is, operating income or cash flow), not the stock, and it is well-documented that leverage is negatively correlated with lagged operating income. Hennessy and Whited (2005) do not use this flow variable arbitrarily. Instead, they use it in an attempt to make sense of previous empirical work that has used this exact same variable, for example, Fama and French (2002). Ironically, if one were to run the regression in Welch (2013) on data simulated from the model, one would in fact find a zero coefficient because the firm cannot simultaneously have cash and debt in the model.

Welch (2013) runs a separate test by regressing the change in leverage on the level of leverage. The intent of this exercise is to test the sentence from Hennessy and Whited (2005) that “firms with high lagged debt use more debt than otherwise identical firms.” He finds a negative coefficient and declares that the model fails the test. Of course, if one has a persistent but stationary time series, one naturally obtains a negative coefficient when one regresses changes on lagged levels. Thus, this test actually supports the model.

³ The *dca* variable is designed to measure active, rather than passive, changes in leverage.

Finally, Welch (2013) runs a test by regressing dca on operating profits. He interprets the finding of a negative coefficient and a tiny R^2 as a failure of the model. However, this test is another example of why it is perilous to try to ascertain the correct form of reduced-form tests from verbal arguments. The exact prediction from Table I in Hennessy and Whited (2005) is that a regression of debt to assets on Tobin's q and on the ratio of operating income to assets yields a negative coefficient on operating income. It has nothing to do with active changes in leverage. We welcome attempts to understand the quantitative and precise predictions of this type of model involving other measures of leverage such as the active leverage changes in Welch (2013). However, without such work, which is beyond the scope of this article, it is impossible to say whether the test in Welch (2013) is, or is not, a test of the model in Hennessy and Whited (2005).

In sum, the three tests of Hennessy and Whited (2005) in Welch (2013) are essentially tests of sentences extracted from the introduction. The tests are not of specific mathematical model predictions. Because language is by nature less precise than math, one of these tests is irrelevant to the model, another actually supports the model, and further research is necessary to see whether the third supports the model or not. It is worth noting that it is *always* possible to find a model implication that can be falsified in the data because models abstract from reality. However, falsifying models is useful because it provides avenues for new research. It is also worth noting that one of the main messages in Hennessy and Whited (2005) is that using models can produce tighter implications for the signs of regression coefficients than loose verbal arguments can. Ironically, the tests in Welch (2013) also confirm this message.

5.2 Strebulaev (2007)

We have already addressed a number of specific comments that Welch (2013) makes about Strebulaev (2007), several more are worth mentioning. First, Welch (2013) suggests that Strebulaev uses a 5% refinancing friction parameter, while the benchmark parameter that is used actually is 0.5%, which makes quite a bit of a difference. Second, Welch also states: "Its reduced-form inference was based on 75 years of simulated quarterly data, a marked contrast to the average 10-year life span of publicly traded firms." This statement makes little sense as well because, although the paper

does use long-term simulations, firms go bankrupt and disappear from the economy with a reasonable average life expectancy.

Although the laundry list of these inaccurate comments can be continued, we concentrate here on the most important point in Welch (2013). Specifically, he states: “Like all tax-distress-friction models, the Strebulaev model is fundamentally incapable of explaining high levels of non-adjusting managerial activity.” This criticism in Welch (2013) is therefore about the underpinning assumption of much of the dynamic literature in economics that there are fixed costs that prevent frequent readjustments.⁴

Welch aims to demonstrate the existence of frequent capital structure adjustments by calculating his *dca* measure. Unfortunately, his *dca* measure, although potentially useful in various contexts, is difficult to reinterpret in terms of the frequency and size of refinancing.

At the end of the day, whether firms refinance frequently or not, and to what extent they refinance, is an empirical question. If it turns out that firms do refinance frequently, then it is likely that the fixed costs of financing are not of first order. Such evidence would then only provide more impetus for building more dynamic models to find alternative explanations. However, the evidence suggests that many firms refinance infrequently (Leary and Roberts, 2005; Danis *et al.*, 2012). To explore this issue further, we use Compustat quarterly data for all public non-financial U.S. firms between 1984 and 2009.⁵ We concentrate on actual issuance of equity and debt every quarter, which are available either from the statement of cash flows or from balance sheet information.

Table 1 reports the results of this exercise. We are interested in a simple question: how many firms issue less equity/debt than a certain cutoff. Because firms are of different sizes, we concentrate on ratios of net equity and debt to beginning-of-quarter book assets. Table 1 shows that 30% of firm-quarters exhibit less than 0.25% of debt issuance (both short-term and long-term) and 50% exhibit less than 1% change. Big changes are relatively infrequent. Roughly, firms change their leverage by more than 5% of book assets only about one quarter in 10.⁶ Moreover, we conjecture that what

⁴ For a broad treatment of this approach, see Stokey (2009), whose title, *The Economics of Inaction*, is telling.

⁵ See Korteweg and Strebulaev (2012) for details concerning the construction of the sample and descriptive statistics.

⁶ The 5% threshold approximates the refinancing mode in the benchmark implementation in Strebulaev (2007).

Cutoff (%)	Net debt-net			
	equity	Net equity	Net debt	Net debt (BS)
0	6.5	30.3	21.5	13.3
0.25	29.3	69.5	44.7	31.2
0.5	40.7	78.6	53.3	39.2
1	54.7	86.2	63.5	49.9
2	69.5	91.8	75.2	64.2
5	86.3	96.3	89.2	83.8
10	93.9	98.1	95.6	93.5

Note: Calculations are from Compustat quarterly data for all public non-financial U.S. firms between 1984 and 2009. This table reports the percentage of quarters in which firms issue or retire net equity or net debt below (or equal to) a given cutoff (in absolute value). The cutoff is relative to beginning-of-quarter book assets. Net debt and net equity issuance are from the statement of cash flows, where debt is only long-term debt. Net debt issuance (BS) is from the change in book debt from the balance sheet, using both short- and long-term debt.

Table 1. Frequency of refinancing.

this cross-sectional evidence hides is that while some firms may refinance frequently, most other firms rarely refinance.

We conclude that firms make big changes to capital structure infrequently, and this inaction is in line with the rationale behind the fixed cost assumption. Our conclusion confirms solid evidence from other fields, such as changing menus in response to changing prices, investment activity, and labor hiring and firing. At the same time, firms do tinker with their equity and debt. How many of these changes result from active decisions rather than from mechanical, pre-committed changes is unclear. For example, many small changes to outstanding long-term debt could result from amortization. This behavior does remind one of individual borrowers, who may change the balance on their credit card frequently. However, because of fixed costs, they change their mortgage very infrequently, even when interest rates move substantially. An important point is that there seems to be different mechanisms behind small and large changes to capital structure. It would therefore be interesting to do a careful empirical investigation of the motives behind capital structure adjustments of different sizes.

The main criticism of the class of models that includes Strebulaev (2007) is not to be found in Welch (2013). The model in Strebulaev (2007) assumes

exogenous investment and organizational processes. In choosing capital structure, firms do not consider investment and restructuring (for example, M&A) activities. We now know that firms often conduct major refinancings to coincide with major capital investment expenditures. We are aware of many ongoing efforts of studying the interactions between financing and investment within this paradigm, and this is the way to go forward.

6 Conclusion

The critique in Welch (2013) and our comments loosely resemble recent debates between economists about the relative worth of reduced-form and structural approaches (see, for example, Angrist and Pischke, 2010; Keane, 2010a, 2010b; Sims, 2010; Nevo and Whinston, 2010). We welcome this type of healthy debate that is based on careful analysis and deep understanding of the relevant issues at hand. Such dialog has an opportunity to improve academic discourse tremendously. In contrast to this debate, the issues in Welch (2013) are not based on an accurate characterization of the literature. As such, most of the points in Welch (2013) are not relevant to dynamic corporate finance. To close, we note that one of the main themes in Welch (2013) is the proposition that we learn less from formal dynamic models and more from reduced-form quasi-experimental regressions. As such, his depiction of the situation in empirical corporate finance seems similar to the situation in asset pricing when the first model, the CAPM, was developed. It replaced ad hoc regressions of prices on explanatory variables, summarized by Keenan (1970), by a seemingly oversimplified model for the discount rate. Even though asset pricing has moved far beyond this simple model, it cannot be doubted that the CAPM opened up new and exciting vistas.

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