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Ricardian Equivalence: An Evaluation of Theory and Evidence

1. Introduction

In recent years, skyrocketing federal deficits have generated widespread concern and fevered debate. Economists' analyses of the deficit problem have focused primarily upon two intellectual benchmarks. One school of thought, typically associated with Keynes, holds that deficit-financed tax cuts raise disposable income, thereby stimulating aggregate demand. As a result, deficits lead to high real interest rates, and crowd out private capital formation. If disequilibrium prevails, unemployment may also fall. The second school of thought holds that taxpayers see through the intertemporal veil, and realize that the present discounted value of taxes depends only upon real government spending—not on the timing of taxes. This foresight gives rise to a "Say's law" for deficits: the demand for bonds always rises to match government borrowing. As a result, deficits fail to stimulate aggregate demand, and in fact have no real effects. This second view is typically associated with Ricardo, no doubt much to his posthumous dismay (see O'Driscoll 1977).

The notion of "Ricardian equivalence" has come to play an important role in modern economic thought, due in large part to the work of Barro (1974). In evaluating the existing theory and evidence on Ricardian equivalence, it is essential to distinguish between the short-run effects of government borrowing (primarily the potential for stimulating aggregate demand, and its implications for macroeconomic stabilization policy) and the long-run effects (primarily the potential for depressing capital accumulation). I argue in section 2 that the theoretical case for long-run neutrality is extremely weak, in that it depends upon improbable as-

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sumptions that are either directly or indirectly falsified through empirical observation. In contrast, the approximate validity of short-run neutrality depends primarily upon assumptions that have at least an aura of plausibility. Nevertheless, even in this case behavioral evidence weighs heavily against the Ricardian view.

Efforts to measure the economic effects of deficits directly through aggregate data confront a number of problems which, taken together, may well be insuperable (section 3). It is therefore not at all surprising that this evidence has proven inconclusive. Studies using aggregate time-series data almost uniformly support the view that deficits significantly stimulate aggregate demand in the short run, yet they often fail to identify systematic short-run relationships between deficits and either interest rates, prices, or other nominal variables. Studies based upon international comparisons (including some new results) find a significant relationship between deficits and aggregate demand (section 4). Few if any studies have attempted to measure long-run effects directly.

Taken together, the existing body of theory and evidence does not justify claims that government borrowing has little or no effect on the economy. Rather, I conclude that there is a significant likelihood that deficits have large effects on current consumption, and there is good reason to believe that this would drive up interest rates. In addition, I find a complete lack of either evidence or coherent theoretical argument to dispute the view that sustained deficits significantly depress capital accumulation in the long run.

2. *Theoretical Considerations*

The central Ricardian observation is that deficits merely postpone taxes. A rational individual should be indifferent between paying $1 in taxes today, and paying $1 plus interest in taxes tomorrow. Since the timing of taxes does not affect an individual's lifetime budget constraint, it cannot alter his consumption decisions.

The relevance of this observation depends upon the length of consumers' planning horizons. If fiscal policy postpones tax collections until after current taxpayers have died, then it may well alter real economic decisions (see Diamond 1965 and Blanchard 1985). Barro's (1974) central insight was that intergenerational altruism may act to extend planning horizons, thereby reinstating strong versions of Ricardian equivalence.

Recent theoretical work has clarified the set of implicit and explicit assumptions upon which the Ricardian proposition depends. To establish the equivalence of taxes and deficits, one must assume that (1) successive generations are linked by altruistically motivated transfers; (2)

capital markets either are perfect, or fail in specific ways; (3) the postponement of taxes does not redistribute resources within generations; (4) taxes are nondistortionary; (5) the use of deficits cannot create value (that is, through bubbles); (6) consumers are rational and farsighted; and (7) the availability of deficit financing as a fiscal instrument does not alter the political process.[1] I will elaborate on several of these assumptions.

2.1. LINKAGES BETWEEN GENERATIONS

Under the Ricardian world view, intrafamily transfers between members of successive generations are ubiquitous, and motivated solely by altruism. Both components of this hypothesis are theoretically suspect. I will take them in order.

With regard to the first component, one line of criticism argues that, under plausible assumptions about preferences, productivity growth, and income distribution, many parents will bequeath nothing to their children (Andreoni 1986a, Feldstein 1986a, Laitner 1979). When this occurs, children may well help support their parents, in which case Ricardian equivalence still prevails for policies that do not alter the pattern of linkages (see Carmichael 1982, or Bernheim and Bagwell 1986 for a more general treatment). The relevant issue therefore concerns the likelihood that transfers flow neither from parents to children, nor from children to parents.

A number of authors have studied models that allow for both gifts (from child to parent) and bequests (from parent to child). Typically, there is a range of parametrizations for which transfers flow in neither direction (Buiter 1979, Carmichael 1982, Weil 1984, Abel 1985a, and Kimball 1986).[2] In general, I find this subliterature unenlightening. For one thing, I question the wisdom of rejecting certain specifications of preferences, as is the practice of several authors mentioned above, on the basis of criteria such as dynamic inconsistency (between members of successive generations), or divergence of utility streams. While consistency and convergence are analytically convenient properties, they are unjustifiable as primitive choice axioms. Dynamic inconsistency is simply a manifestation of conflict within families, and I see no reason to rule it out. Similarly, divergence of utility streams simply limits the usefulness

1. Despite some suggestions to the contrary, it is now clear that Ricardian equivalence does not depend upon the absence of uncertainty, except insofar as this uncertainty concerns the factors listed above (see Sheshinski and Weiss 1981, Chan 1983, Stiglitz 1983, and Bernheim and Bagwell 1986).
2. In certain special cases, transfers necessarily flow in one direction or the other (Burbidge 1983). This observation has sparked a debate over the proper specification of intergenerational altruism (Buiter and Carmichael 1984, Burbidge 1984).

of utility functions as representations of primitive preference orderings, and cannot logically invalidate the possibility that altruism is strong.

Furthermore, this entire subliterature considers a very special subset of preferences, in that direct altruism is generally limited to one's immediate successor and predecessor. If, for example, individuals care directly both about their children and grandchildren ($u_t = u(c_t, u_{t+1}, u_{t+2})$), then positive bequests may prevail in equilibrium even when capital accumulation is inefficiently high (that is, on the wrong side of the golden rule—see Ray 1987). One simply cannot rule out the possibility that generations are linked by applying this sort of a priori reasoning.

A second line of criticism spins off of Bernheim and Bagwell's (1987) observation that representative agent models abstract from interconnections between families. They argue that ubiquitous parent-child linkages would embed all (or nearly all) individuals in a single interconnected network. The consumption of each individual would then depend only upon total wealth, and an increment to total wealth would be divided among the entire population. Consequently, in a large dynamic economy, any incremental bequest would be divided between the recipient's contemporaries, and the resulting increment to the recipient's consumption would be negligible. Accordingly, the donor would prefer to make no bequest at all. In equilibrium, many donors would be driven to corners, so that no large interconnected network would remain.[3]

A third line of criticism argues that optimal government policy will generally entail driving successive generations to corners (Bernheim 1986). The reason is simple: when transfers are positive, each donor is indifferent on the margin between his own consumption and that of the corresponding recipient. If the planner cares directly about the donor and the recipient, he must in general prefer larger transfers—the initial configuration could not have been socially optimal. I have noted (1986) that one must qualify this conclusion when the planner can precommit himself to particular policies. In addition, note that Ricardian equivalence may still hold in economies where the government fails to act optimally.

I turn next to the second component of the linkage hypothesis—that intergenerational transfers are altruistically motivated. It is important to

3. While it is possible that the resulting pattern of binding corner constraints might still be consistent with Ricardian behavior (for example, suppose that each couple has a male and female child, only the male receives transfers, and all children marry), this outcome seems extremely unlikely. Some preliminary investigations into the nature of these endogenous patterns (Bergstrom, Blume, and Varian 1985, and Barro 1985) provide little if any reason to believe that neutrality propositions would survive in explicitly disaggregated settings.

emphasize that Ricardian equivalence holds under all specifications of altruism in which the utility of each individual is determined as a function of consumption profiles: one need not require that an altruist value only the utility of others, as in Barro (1974) (see Bernheim and Bagwell 1986). Various authors have suggested alternative motivations, including uncertainty concerning length of life (Davies 1981), intrafamily exchange (Sussman, Cates, and Smith 1970, Becker 1974, 1981, Ben-Porath 1978, Adams 1980, Tomes 1981, Kotlikoff and Spivak 1981, and Bernheim, Shleifer, and Summers 1985), and tastes for generosity (Blinder 1974, Andreoni 1986b). In general, these alternatives do not give rise to neutrality results (see Bernheim, Schleifer, and Summers). Unfortunately, it is very difficult to distinguish between different formulations of preferences on the basis of theoretical reasoning alone. Nevertheless, Bernheim, Schleifer, and Summers make an a priori case for the presence of exchange motives. They argue that Barro's dynastic specification, which portrays families as perfectly harmonious units, is extremely restrictive, and that more generally the preferences of distinct family members will conflict.[4]

Bernheim and Bagwell (1986) offer an alternative criticism of the Ricardian linkage hypothesis. Building on the observation that ubiquitous parent-child linkages would embed nearly everyone in a single, interconnected network, they show that this hypothesis implies the irrelevance of all redistributional policies, distortionary taxes, and even prices. Indeed, this remains true even when some fraction of the population makes no transfers, or is motivated by considerations other than altruism. Since these results are untenable, they conclude that, in some critical sense, the linkage hypothesis cannot even be approximately valid, and that all policy prescriptions based upon the dynastic framework are therefore suspect.

In defense of Ricardian equivalence, one might argue that the linkage hypothesis is a more appropriate approximation in some circumstances than in others. Specifically, the Bernheim-Bagwell results may depend on much longer chains of linkages than does the neutrality of government borrowing, particularly if the debt will be paid off within a few generations. If some sort of friction exists in each link, this might dissipate the Bernheim-Bagwell effects, without substantially altering the approximate neutrality of certain deficit policies. Yet formal analysis suggests

4. While there are some special cases in which the presence of an altruist will automatically serve to harmonize objectives (see Becker's (1974) "Rotten Kid Theorem"), this outcome is rather unlikely (Bergstrom 1986).

that this line of defense is flawed, and at best leads one to a different set of untenable conclusions (Abel and Bernheim 1986).[5]

Recently, several authors have questioned the importance of intergenerational issues in connection with the Ricardian debate (Poterba and Summers 1986, Hubbard and Judd 1986b).[6] Poterba and Summers argue that, under a variety of plausible deficit scenarios (including some historical ones), a substantial fraction of the deferred tax burden is not shifted to future generations.[7] On the other hand, a substantial fraction is shifted forward—is the glass half full or half empty? Furthermore, the current deficit experience is very atypical, and rational consumers might well expect to escape liability for paying off the lion's share of our current outstanding debt.

Both Poterba-Summers and Hubbard-Judd also point out that, because consumers have relatively long horizons, the marginal propensity to consume out of increments to wealth is small, perhaps on the order of .05. They conclude that factors such as liquidity constraints and myopia have a much larger bearing on the Ricardian debate.

The validity of this conclusion depends upon the policy issue that one has in mind. If one is concerned with short-run issues, such as stimulation of aggregate demand within a standard Keynesian setting, then Poterba-Summers and Hubbard-Judd are undoubtedly correct (although note that 5 percent of a $200 billion deficit is $10 billion, which constitutes a nontrivial rise in consumption). If, on the other hand, one is concerned with capital accumulation in the medium and long runs, intergenerational issues play the dominant role.[8] Indeed, simulations

5. By simultaneously taking the level of friction (tastes for generosity, or incomplete information) sufficiently small, and the population sufficiently large, one can obtain approximate Ricardian equivalence without rendering everything else neutral. However, by passing to both limits, one drives each individual's marginal propensity to consume lifetime resources to zero.
6. Kotlikoff (1986a) has also questioned the importance of intergenerational issues, but for different reasons. He considers a particular model of the political process, and shows that Ricardian equivalence holds even in the absence of intergenerational altruism. His result depends very heavily upon the assumption that each generation has coercive power over its own members, but no coercive power over the members of any other generation. This strikes me as highly unrealistic.
7. Note that arguments of this type are of little relevance when one is concerned with the effects of an unfunded social security system. Such a system necessarily entails substantial intergenerational transfers.
8. Note that a completely selfish individual exhausts his resources over his lifetime, regardless of whether he faces liquidity constraints, so that such constraints merely serve to alter the timing of accumulation over his life cycle. On the other hand, increments to his consumption arising from an indefinite postponement of taxes cumulate over his life cycle. Indeed, his marginal propensity to consume out of wealth, measured on a lifetime basis, is unity.

show that, under standard life-cycle assumptions, plausible deficit policies have enormous effects on medium- and long-run capital stocks, even though the short-run effects may be trivial or even perverse (Auerbach and Kotlikoff 1986).

In Bernheim (1987), I survey the existing empirical literature on intergenerational transfers. My conclusions are as follows. Overall, bequests seem to be significant for a large segment of the population. In addition, much microeconomic evidence is inconsistent with traditional life-cycle models. One can explain some but not all of this evidence by appealing to risk aversion concerning length of life and unplanned expenses, and indeed some studies point directly to a bequest motive. However, much evidence indicates that bequests are not motivated solely by altruism. In contrast, no existing evidence successfully isolates the altruistic motive. I conclude that a substantial minority of individuals probably make few or no intentional transfers, and that most other individuals are motivated by a variety of factors.

2.2. CAPITAL MARKET IMPERFECTIONS

When inefficiencies in private capital markets make it difficult or impossible for households to obtain loans, government borrowing may have real effects (Buiter and Tobin 1981). Hubbard and Judd (1986a,b) emphasize the quantitative importance of liquidity constraints for short-run issues. Under the assumption that 20 percent of the population is liquidity constrained, they show that a $1 deficit-for-taxes swap could well increase current consumption by about 25¢. Clearly, this is far larger than the roughly 5¢ rise in consumption that would follow from a pure wealth effect. In a somewhat different vein, studies by Drazen (1978) and Pogue and Sgontz (1977) establish that, when liquidity constraints bind early in life, policies involving intergenerational transfers alter human capital accumulation, even in the presence of intergenerational altruism.[9]

Unfortunately, these studies are all somewhat unsatisfactory, in that they take liquidity constraints to be given exogenously. Certainly, the effect of government policy will in general depend upon the nature of the capital market failures that give rise to inefficiencies.

Recent work explains credit rationing as the consequence of asymmetric information (Stiglitz and Weiss 1981, Jaffee and Russell 1976). These approaches generate liquidity constraints as an endogenous as-

9. For example, social security entails a transfer from middle-aged children to elderly parents. As benefit levels increase, middle-aged parents will become more willing to "loan" their children money for education, since they know that the government will, effectively, enforce repayment.

pect of equilibrium. Using such models, Hayashi (1985) and Yotsuzuka (1986) argue that, in many situations, liquidity constraints adjust in response to government policies, and that the nature of this adjustment restores Ricardian equivalence.[10]

It is, however, apparent that the Hayashi-Yotsuzuka results are highly sensitive to assumptions about the distribution of the future tax burden. While I discuss distributional issues at greater length in the next subsection, it is important to emphasize the particular synergy between liquidity constraints and distribution. In Bernheim (1987), I show that if one modifies Hayashi's model to allow for the fact that taxes rise with income, one obtains the strong, and much more traditional, result that deficits increase the consumption of *every* consumer.

There is by now a large literature that tests various implications of the joint hypotheses that consumers maximize intertemporal utility, and capital markets function efficiently. In the interests of conserving space, I refer the reader to two excellent surveys of this literature by King (1983) and Hayashi (1985). Despite numerous problems with estimation and interpretation, the evidence on balance supports the view that a sizable minority (roughly 20 percent) of individuals fail to behave in a way that is consistent with unconstrained intertemporal optimization. However, one is well advised to heed Hayashi's caveats concerning the relevance of these findings to the Ricardian debate.

2.3. REDISTRIBUTION

Even if individuals are infinitely long-lived and capital markets are perfect, the deferral of taxes through deficits may alter the pattern of incidence. Simply put, different people may bear a larger share of the tax burden at different points in time. In effect, all the real effects of debt described so far arise from redistributions of this sort, either between successive generations, or between separate incarnations of the same individual in distinct periods.

A redistributional policy can significantly alter current aggregate consumption only when two conditions are satisfied. First, the relevant parties must not be linked through chains of altruistically motivated transfers (I do not give to or receive from my children; I do not save for or borrow against my future). Second, these parties must have different propen-

10. This result is contrary to the conclusion of King (1984), who also models the credit market failure explicitly. Yotsuzuka (1986) attributes this apparent inconsistency to differences between assumptions about information transmission (Hayashi (1985) assumes that all contracts are observable, while King assumes they are not). He then establishes an extremely strong neutrality result for environments in which the exchange of information is determined endogenously.

sities to consume resources in the current period (I consume today, but my children do not, and neither do my future incarnations). For redistributions other than those considered in subsections 2.1 and 2.2 it is very easy to argue that the first condition holds, but very difficult to establish the second in a convincing way. As a result, there is a common presumption that, ignoring intergenerational issues and liquidity constraints, the distributional consequences of postponing taxes are of second-order importance.

Certain considerations suggest that debt (Abel 1986) and social security (Abel 1985b) may stimulate aggregate consumption by redistributing resources toward individuals with higher marginal propensities to consume. These policies also tend to favor families with few children, and this may affect fertility, thereby altering consumption indirectly (Becker and Barro 1985, Wildasin 1986). It is, however, difficult to gauge the quantitative importance of these effects.

A separate set of issues arises when the distribution of future taxes is not known with certainty (Buiter and Tobin 1981). To the extent consumers save more as a precaution against uncertainty, deficits may actually "crowd-in" investment (Chan 1983), although once again it is difficult to assess the quantitative significance of this factor. I find this argument implausible. If the world was otherwise Ricardian, then one would never expect to observe deficits since the electorate would universally oppose the gratuitous introduction of uncertainty. Rather, huge current surpluses would result from the public's desire to resolve uncertainty as quickly as possible.

Future taxes may also be uncertain because they are related to income, which is itself random. In this case, postponement of taxes may result in a reduction of future net income risk, thereby inducing a rise in consumption (Barsky, Mankiw, and Zeldes 1986). I find this observation unsatisfactory, in that the absence of relevant insurance markets is never explained. Hayashi's analysis of liquidity constraints certainly drives home the point that it is dangerous to discuss the effects of government policy in the presence of market failures without modeling the failures explicitly. Indeed, in a Barsky-Mankiw-Zeldes world, the government should intervene in insurance markets, and help to pool risks associated with future income. Risk pooling should in no way depend directly upon the timing of taxes. Note that a similar objection also applies to the "crowding-in" view mentioned above.

2.4. DISTORTIONARY TAXATION

The timing of taxes may be quite important if taxes distort behavior, and if marginal future taxes depend upon different decisions than marginal

current taxes.[11] This observation is especially relevant to the current discussion when deficits are used to postpone capital income taxes, since a reduction in marginal capital income tax rates may cause individuals to save more and consume less. Auerbach and Kotlikoff (1986) argue that this effect could dominate the net wealth effect in the short run, even in a life-cycle world. The plausibility of this outcome depends in large part on intertemporal substitution elasticities. Measurement of these elasticities is a dicey problem, and well beyond the scope of this article. However, in evaluating the marcoeconomic evidence discussed in sections 3 and 4, the reader should bear in mind that deficits may substantially depress the long-run capital stock even when the short-run effect is to crowd-in investment.

A more general treatment of deficit financing in a world with distortionary taxes gives rise to the theory of tax smoothing. This theory suggests that tax rates should be kept relatively constant even when output fluctuates. Intuitively, since the deadweight loss of taxation rises (approximately) with the square of the tax rate, a constant rate minimizes the average distortion (Barro 1979, Prescott 1977, Lucas and Stokey 1983, Kingston 1984).[12] While tax smoothing is an important normative concept, this theory does not appear to generate any clear positive implications concerning the link between deficits and aggregate consumption.

2.5. PERMANENT POSTPONEMENT

Feldstein (1976) criticized Barro's analysis of fiscal policy by arguing that, when the growth rate of the economy exceeds the interest rate, the government can roll over deficits indefinitely. As a result, no generation need ever actually pay any portion of the postponed taxes. Barro (1976) responded that intergenerational linkages might actually prevent the economy from accumulating an inefficiently large amount of capital. This exchange spawned a separate subliterature that focused on the relationships between gifts, bequests, and capital overaccumulation. I have already referred to many of these papers in subsection 2.1. While I do not think one can draw any conclusions about the form of intergenerational altruism or the pervasiveness of intergenerational linkages from this literature, I am convinced that two points are now well established.

11. Technically, future taxes could be designed to depend on the same behavior as present taxes. For example, it is possible to defer taxes by requiring payment of only 90 percent of calculated tax liabilities in the current year, with the other 10 percent due in some future year. The considerations raised in section 2.4 are not applicable to policies of this sort.

12. Feldstein (1985) has disputed the superiority of temporary debt finance over temporary tax finance, but his analysis is evidently in error (Kingston (1986)).

First, contrary to Barro's conjecture, intergenerational altruism does not rule out dynamically inefficient outcomes. Indeed, for certain specifications of preferences it is possible to obtain inefficient equilibria with either positive gifts (Weil 1984, Abel 1985a) or positive bequests (Ray 1987). Second, even when the economy overaccumulates capital, Ricardian equivalence still holds as long as successive generations are operatively linked (Carmichael 1982, Bernheim and Bagwell 1986). The key to this second result is that the government cannot, in the absence of other market failures, alter the aggregate opportunity set of the private sector through borrowing. If voluntary transfers are operative, then individual families could mimic a policy of rolling over deficits indefinitely by cutting the bequests (or increasing the gifts) of every generation.[13]

Despite the fact that cases of dynamic inefficiency have received a large amount of attention, it is not obvious that this work is of any practical relevance. Few if any economists today believe that the U.S. capital stock is inefficiently large. Permanent deferral of taxes may simply be infeasible.

3. *Direct Evidence: Aggregate Time Series*

I now turn to studies that attempt to measure the effects of deficits directly from aggregate time-series data. In subsection 3.1 I discuss some general problems which, taken together, may well be insuperable. Subsections 3.2 and 3.3 concern studies of real variables (especially consumption) and nominal variables (especially interest rates), respectively. Subsection 3.4 summarizes.

3.1. GENERAL PROBLEMS

It is useful to begin by summarizing eight common problems shared by virtually all studies that use macro time-series data to measure the effects of deficits.

13. O'Connell and Zeldes (1986) have argued that, on the contrary, the permanent deferral of taxes may stimulate consumption when the stock of capital is inefficiently high. Their analysis builds on that of Gale (1983), who pointed out that Barro's dynastic model determines a game played by a sequence of generations, and that this game admits a vast multiplicity of legitimate equilibria. For an appropriately constructed refinement of this equilibrium set, O'Connell and Zeldes demonstrate that dynasties behave as though they value "terminal wealth," which depends on government debt. Thus, the permanent postponement of taxes is not irrelevant. However, one could also select refinements that yield neutrality, and such refinements may in some senses be more natural (Bernheim and Bagwell 1986, Bernheim 1986). The O'Connell and Zeldes results effectively depend upon the use of debt as a signal to shift between nonequivalent members of an equilibrium set. Since the choice of a signal is inherently arbitrary, one could in this way establish that any given event has any desired effect on consumption.

First, measurement of debts and deficits is problematic (see Eisner and Pieper 1984, 1986, Eisner 1986, and Boskin 1982, 1986). Measurement issues include: inflation adjustments, adjustments from par to market values, properly accounting for government assets and investments as well as for contingent liabilities, and valuing liabilities from social insurance programs. Many of these issues pose thorny conceptual problems, so that the "right" measure is not obvious (Kotlikoff 1986b). Furthermore, econometric estimates appear highly sensitive to the set of corrections that one actually makes (Boskin 1986).

Second, completely aside from issues about the deficit, it is not at all clear that economists have yet devised satisfactory models for aggregate variables, including consumption and interest rates (see for example Hayashi's (1985) discussion of aggregate consumption relationships). To the extent one misspecifies the relationship of interest, estimates of fiscal effects may be highly unreliable, being contaminated by biases of unknown direction and magnitude. Evidence that appears to reject some hypothesis about deficits may in fact simply reject the underlying model.

Third, it is important to distinguish between expected and unexpected movements of explanatory variables. Indeed, under the stochastic permanent income hypothesis, only unexpected changes matter. Accordingly, if consumers anticipated historical movements in the deficit perfectly, then we might well find no empirical relationship between deficits and concurrent economic activity, even though real activity would have been much different had the government followed some alternative deficit policy.[14]

One method of distinguishing the effects of unexpected changes in the deficit is to model the evolution of deficits statistically, and relate deficit innovations to consumption innovations (that is, run VARs, and employ cross-equation restrictions implied by rational expectations). This procedure is valid only if consumers form expectations by the same statistical model used by the econometrician. In practice, VAR models are very

14. To illustrate, suppose that some generation lives for three periods. This generation correctly anticipates that during the second period the government will run a large deficit in order to cut taxes, and will retire the deficit by imposing higher taxes on some subsequent generation. Suppose further that consumers are completely selfish, utility is time separable, future utility is discounted at the rate p, and $p = 1/(1 + r)$, where r is the interest rate. Then consumption will be the same in every period. Yet the deficit moves from zero in period 1, to a positive number in period 2, back to zero in period 3. Expected future deficits (during the life of this generation) move from a positive number in period 1, to zero in periods 2 and 3. Debt moves from 0 in period 1 to a positive number in periods 2 and 3. Accordingly, consumption will not move in response to any of these variables, and a regression of consumption on any subset of them will fail to reveal a link. Yet the deficit policy increases the consumption of this generation in every period.

parsimonious, and omit a tremendous amount of information, so that a portion of the deficit "innovation" may be expected. Indeed, evidence presented in Bernheim (1987) suggests that VAR forecasts may differ substantially from expectations based upon the kind of well-publicized institutional information that, in all likelihood, individuals actually use. This creates serious biases in favor of Ricardian equivalence.[15]

Fourth, the levels of explanatory variables, as well as the innovations in these variables, convey an amalgam of information about future events. Current deficits (or innovations in the deficit) may be correlated with future income or government spending. Realizing this, consumers may change spending today in response to current deficit policy, but not because of the deficit.[16]

Fifth, endogeneity is a severe problem. Deficits, government spending, consumption, income, and interest rates may all be determined simultaneously. While some authors have employed instrumental variables, the exogeneity of their instruments is highly questionable.

Sixth, it is very difficult to distinguish between the effects of different fiscal policy variables. Most obviously, until very recently government spending and deficits moved together very closely. This aside, there is a more subtle problem concerning identification. We are generally concerned with five fiscal policy variables: government spending, transfers, taxes, deficits, and debt. Each has an independent effect on economic activity. Yet the first four variables plus interest on the debt sum to zero. Typically, one deals with this problem by assuming that, since taxes decrease disposable income, the tax coefficient equals the negative of the income coefficient. But this assumption is valid only when taxes are nondistortionary. Recall that a rise in deficits with spending constant may

15. In his analysis of deficits and interest rates (see secton 3.3), Plosser (1982) has suggested a method for dealing with this problem. He argues that if one uses lagged interest rates to help predict current values of policy variables, then these predictions will reflect all past information relevant to interest rates (one could apply a similar technique in studies of consumption). I find this argument unpersuasive, since it implicitly assumes that a scalar summary statistic suffices to capture the full richness of all past information affecting current interest rates. Certainly, this property can only be valid under extremely restrictive circumstances. Furthermore, if it is valid, why bother with a VAR model? Instead, we could simply predict policy decisions with lagged values of the dependent variable.

16. Feldstein (1982) argues that, when one discusses the effects of the deficit, one should include these sorts of expectational considerations. I disagree. Since our empirical macro models use only a few variables to explain aggregate economic behavior, we cannot distinguish between the hypotheses that deficits are correlated with other information, and that deficits actually *carry* the relevant information. If the first of these hypotheses is correct but not the second, the estimated relationships are of no value as guides to policy, since the relationship between deficits and other information need not, and in general will not, remain stable when the policy regime changes.

well imply a fall in current capital income taxes, so saving may rise even though deficits themselves actually stimulate consumption. To identify these models properly, we therefore need data on effective marginal tax rates, especially for capital income. Unfortunately, movements in marginal tax rates may follow movements in revenue rather closely, making identification difficult. Furthermore, measurement of effective tax rates on capital income is both complex and controversial (see for example King and Fullerton 1984). It would be extremely difficult to obtain a reliable series.

Seventh, no existing study has attempted to measure the relationship between the effects of deficits and the length of the associated payback period. Poterba and Summers (1986) argue that deficits have actually been paid off rather quickly during the twentieth century. If so, evidence from this century only bears on deficit policies where rapid payback is envisioned. Since the current fiscal experience is extremely atypical, it is not at all clear that the historical pattern will persist, and it is even less clear that the average taxpayer expects it to persist. It is therefore extremely dangerous to draw inferences from U.S. time series about the effects of current deficit policy, or any other policy that involves the extended deferral of taxes.

Eighth, it may be extremely difficult, if not impossible, to distinguish between interesting hypotheses given the level of macroeconomic noise. Recall the discussion of section 2: when the intergenerational linkage hypothesis is violated, the marginal propensity to consume out of temporary deficits may be only five cents on the dollar. Since this short-run effect is so small, we may have difficulty picking it up in the data, despite the existence of enormous long-run effects. Indeed, it may be hard to distinguish between zero, five cents, and twenty-five cents on the dollar (the latter being the hypothesized effect in a world where some consumers are liquidity constrained).

3.2. STUDIES OF REAL VARIABLES

3.2.1. Consumption Function Studies Table 1 summarizes the methodologies and results of studies that estimate aggregate consumption functions in order to assess the real impact of deficits. A cursory reading of these papers suggests that various authors have reached markedly different conclusions through essentially similar analyses of U.S. time-series data. I begin this subsection by arguing that these differences are largely illusory. Interpreted properly, virtually all studies indicate that every dollar of deficits stimulates between $0.20 and $0.50 of current consumer spending.

Apparent differences in results can in most cases be traced to different

formulations of the null hypothesis. A number of authors set out to estimate an equation such as the following:

$$C_t = \alpha_0 + \alpha_1(Y_t - T_t) + \alpha_2(T_t - G_t - r_tD_t) + \alpha_3G_t + \alpha_4D_t + \alpha_5W_t + X_t\underline{\alpha} + \varepsilon_t, \quad (1)$$

where C is consumption, Y is national income, T is tax revenues, G is government spending, D is debt, W is private wealth, r is the interest rate, X is a vector of other exogenous variables, and ε is a stochastic error term (so that $Y - T$ is disposable income, and $T - G - rD$ is government surplus). The natural null hypothesis for this specification is $\alpha_2 = 0$—the alternative is the so-called "tax discounting hypothesis," which holds that consumers at least partially anticipate future taxes. A second group of authors estimate

$$C_t = \beta_0 + \beta_1Y_t + \beta_2(T_t - G_t - r_tD_t) + \beta_3G_t + \beta_4D_t + \beta_5W_t + X_t\underline{\beta} + \eta_t \quad (2)$$

(that is, they use gross income rather than net income). For equation (2), the natural null hypothesis is $\beta_2 = 0$, which corresponds to pure Ricardian equivalence.

The hypothesis that $\alpha_2 = 0$ is certainly a straw man—few people believe that all consumers are completely myopic. Rather than test polar views, one should be concerned with quantifying the effects of deficits on current consumption. In this spirit, it is useful to note that equations (1) and (2) are "almost" the same. Specifically, if the interest rate is time-invariant, then one obtains (2) from (1) through a linear transformation of variables. The coefficients and error terms are then related as follows: $\beta_0 = \alpha_0$, $\beta_1 = \alpha_1$, $\beta_2 = \alpha_2 - \alpha_1$, $\beta_3 = \alpha_3 - \alpha_1$, $\beta_4 = \alpha_4 - r\alpha_1$, $\beta_5 = \alpha_5$, $\underline{\beta} = \underline{\alpha}$, and $\eta_t = \varepsilon_t$; $\alpha_2 = 0$ ($\beta_2 = -\beta_1$) represents the pure Keynesian view, $\alpha_2 = \alpha_1$ ($\beta_2 = 0$) represents perfect Ricardian equivalence, and $\alpha_1 - \alpha_2$ (β_2) measures the effect on current consumption of a \$1 tax-for-deficit swap.

Ricardian advocates have objected to this interpretation of $\alpha_1 - \alpha_2$ on two grounds. First, they claim that since government surplus is more variable than disposable income, one would expect to find $\alpha_2 < \alpha_1$ (see for example Tanner 1978). Yet this is plainly false. Under the Ricardian view, variability of the surplus has no effect on consumption, so in specification (2) one must estimate $\beta_2 = 0$ (equivalently, $\alpha_2 = \alpha_1$). In terms of equation (1), $\alpha_2 = \alpha_1$ is required to purge disposable income of the spurious tax component (α_3 and α_4 will adjust accordingly). Indeed, under the Keynesian view, high volatility biases β_2 toward zero, so if

Table 1 SUMMARY OF CONSUMPTION FUNCTION STUDIES

Study	*Data*	*Key omitted variables*	*Technique*	*Instruments*	*Deficit effect* α_2	*Deficit effect* $-\beta_2$	*Debt effect*
Tanner (1970)	Canadian, Q, 1951–67	DEF, GS, W, MTR	OLS, L	NA	NA	NA	0.034 to 0.040 (0.018) (0.017)
Kochin (1974)	U.S., A, 1952–71	GS, W, DEBT, MTR	OLS, L, D	NA	0.096 to 0.276 (0.048) (0.120)	NA	NA
Yawitz and Meyer (1976)	U.S., A, 1953–67	DEF, GS, MTR	OLS, L	NA	NA	NA	0.05 (0.013)
Tanner (1978)	U.S., A, 1929–40, 1947–71	DEBT, MTR	OLS, L	NA	0.220 to 0.279 (0.076) (0.067)	NA	NA
Tanner (1979)	U.S., A, 1947–74	MTR	OLS, L	NA	0.291 (0.088)	NA	−0.014 (0.38)
Barro (1978)	U.S., A, 1928–74	DEBT, MTR	OLS, L	NA	0.05 to 0.22 (0.08) (0.06)	NA	NA
Feldstein (1982a)	U.S., A, 1930–76	MTR	2SLS, L	T(−1), Y(−1)	−0.083 to −1.73 (0.131) (2.27)	0.222 to 0.276 (0.247) (0.086)	0.023[a] to 0.175 (0.179)
Seater (1982)	U.S., A, 1929–76	MTR	OLS, L	NA	−0.15 to 0.29 (0.10) (0.10)	NA	−0.002 to 0.11 (0.08) (0.03)
Kormendi (1983)	U.S., A, 1929–76	MTR	OLS, D	NA	NA	−0.01 to −0.07 (0.10) (0.08)	−0.055 to −0.032 (0.018) (0.020)

Seater and Mariano (1985)	U.S., A, 1929–75	Corporate MTR	2SLS, L	MG, STR, WCR	NA	−0.146 to 0.176 (0.465) (0.575)	−0.012 to 0.026 (0.101) (0.125)
Evans (1985)	U.S., A, 1901–29	W, MTR	2SLS, L	[b]	NA	−0.307 to 0.273 (0.763) (0.132)	−0.041 to −0.028 (0.062) (0.061)
Reid (1985)	U.S., MA, 1890–1981	DEBT, MTR	OLS, L	NA	NA	0.140 to 0.441 (0.080) (0.180)	NA
Boskin (1985)	U.S., A[c]	MTR	OLS, L	NA	−0.014 to 0.219 (0.102) (0.072)	0.327 to 0.403 (0.089) (0.089)	0.03[d]
Modigliani and Sterling (1986)	U.S., A, 1949–84	MTR	OLS, L	NA	−0.100 to 0.187 (0.132) (0.069)	NA	0.073 to 0.106 (0.024) (0.033)

[a] Since this effect is calculated as the difference between two coefficients, I could not recover a standard error.
[b] I was unable to determine Evans's instruments from the text of his paper.
[c] I was unable to determine Boskin's sample period from the text of his paper.
[d] Boskin reports that this coefficient is statistically significant, but does not give its standard error.

Key

Q: quarterly
A: annual
MA: multiple year average
DEF: government deficits
GS: government spending
DEBT: government debt
W. private wealth
MTR: marginal tax rate
T(−1): lagged taxes
Y(−1): lagged income
MG: money growth
STR: statutory tax rates
WCR: wartime casualty rates
NA: not applicable

anything this consideration suggests that β_2 ($\alpha_2 - \alpha_1$) understates the effect of deficits. Second, they argue that if one measures Y_t as current income rather than permanent income, one may well find $\beta_2 \neq 0$ simply because the government surplus helps to predict permanent income. Indeed, if consumption does depend upon permanent income and if current income and taxes move procyclically, then one would expect to find $\beta_2 < 0$ ($\alpha_2 < \alpha_1$). However, many authors include cyclical variables in X_t. It is then not at all clear that the partial correlation between permanent income and taxes is negative. Other authors use cyclically adjusted measures of deficits. In these cases, the direction and significance of the resulting bias is not at all obvious.

With this observation in mind, I now turn to specific studies. The column labelled "α_2" under the heading "Deficit Effect" contains results based upon specifications that resemble equation (1). Although we are ultimately interested in the magnitude of $\alpha_2 - \alpha_1$, I report estimates of α_2 for two reasons. First, it is often difficult to infer α_1 from these studies.[17] Second, even if one had an estimate of α_1, it would be impossible to calculate a standard error for the difference in the absence of information about correlations between parameter estimates. I will simply note that in most cases, the estimated coefficient of current disposable income is at least 0.6 or 0.7, even when one includes a lagged income variable. Since the long-run marginal propensity to consume is probably more on the order of 0.8 or 0.9, one should regard 0.6 or 0.7 as lower bounds for α_1.

Inspection of the α_2 column reveals that most estimates tend to cluster around 0.25. The highest is 0.29, and a number of estimates are substantially lower. In addition, most of these coefficients are estimated fairly precisely—standard errors tend to be in the neighborhood of 0.1. Accordingly, the evidence uniformly supports the view that a $1 deficit-for-taxes swap raises consumption by at least $0.40 to $0.50, and one can be extremely confident that the estimated effect exceeds $0.10 to $0.20.[18]

Inspection of the $-\beta_2$ column reveals a somewhat more conservative

17. Authors typically include both a current and lagged income variable, but only a current deficit variable. Magnitudes are therefore not strictly comparable. One exception to this is Modigliani and Sterling (1986). They include distributed lags on both income and deficits, and report the sum of coefficients on each. It is therefore proper to compare their deficit coefficients with their estimates of the long-run marginal propensity to consume, which generally range between 0.8 and 0.9. In some studies, the inclusion of interactions between income and the rate of unemployment further confounds efforts to recover a marginal propensity to consume.

18. Kochin's (1974) study merits special comment. By including the lagged value of consumption, he essentially adopted (presumably unintentionally) an Euler equation specification. Accordingly, under the Ricardian hypothesis, only new information should matter. This makes it very difficult to interpret the coefficients on any of the explanatory variables in his equation.

picture. The median estimate is around 0.2, indicating that a $1 deficit-for-taxes swap raises consumption by about $0.20. The range of estimates is, however, quite large. The low estimates come exclusively from three studies: Kormendi (1983), Seater and Mariano (1985), and Evans (1985). In the last two cases, the corresponding standard errors are so large that any reasonable confidence interval subsumes every hypothesis ranging from pure Keynesianism to pure Ricardianism.[19] Kormendi's (1983) estimates are far more precise, but even in this case reasonable confidence intervals do not rule out the kind of small positive wealth effects that would be associated with failure of the intergenerational linkage hypothesis. Note that the other coefficients in this column consistently indicate that deficits have significant real effects, and in many cases the estimate is quite precise. Two studies merit further comment. Since Reid (1985) employs multiyear full-cycle averages, the transitory components of his variables are presumably smaller than in other studies. It is therefore noteworthy that he obtains relatively large deficit coefficients. Boskin's (1985) estimates, which are based upon improved measures of the deficit (including cyclical corrections), also imply large real effects.

Table 1 also contains a column labeled "Debt Effect." To evaluate these coefficients, one should compare them to marginal propensities to consume out of other forms of private wealth.[20] Various studies place this propensity around 0.03 to 0.05. Note that Tanner (1970), Yawitz and Meyer (1976), Feldstein (1982a), Seater (1982), Seater and Mariano (1985), Boskin (1985), and Modigliani and Sterling (1986) all produce estimates in this range, many of which are quite precise. This supports the view that government bonds are net wealth.[21] Several studies, including Tanner

19. A close reading of Evans's analysis reveals the source of imprecision in both studies. His lower estimate comes from a specification in which he includes a measure of temporary government spending. Since his sample period spans World War I, this variable is highly colinear with deficits. The corresponding coefficients simply bifurcate, and the data tell us nothing. When Evans excludes the transitory spending variable, he obtains a precisely estimated coefficient, the magnitude of which is more in line with other studies (0.273). Likewise, Seater and Mariano (1985) include a measure of transitory government spending, and use a sample period that spans World War II.
20. I report estimates of α_4 and β_4. Since $\alpha_4 = \beta_4 + r\alpha_1$, we would expect estimates of α_4 to be slightly higher (by perhaps 0.01 or 0.02).
21. Tanner (1970) draws the opposite conclusion. His regression includes a measure of currency. Since the estimated coefficient for currency is 50–100 times larger than the estimated coefficient of debt, Tanner concludes that 98–99 percent of future taxes are foreseen, or equivalently, only 1–2 percent of the value of government bonds is net wealth. Yet this interpretation is strained. Private wealth (other than currency) is omitted from the equation, and may be more highly correlated with currency than with government debt. Furthermore, while many securities are probably highly substitutable for debt, the same is not true of currency, so changes in currency may well have

(1979), Seater (1982), and Evans (1985) produce very small or even negative coefficients, but in each case the estimated standard error is very large, and any reasonable confidence interval subsumes every conceivable hypothesis ranging from pure Ricardianism to pure Keynesianism. Only Kormendi's (1983) results allow one to reject the hypothesis that a substantial fraction of government bonds is net wealth.

Since Kormendi stands alone in finding support for the Ricardian view, his work deserves further comment. It is noteworthy that other aspects of his results are extremely peculiar (for example, he finds that the long-run marginal propensity to consume out of income is around 0.3), and indicative of potentially severe misspecification. Furthermore, his findings do not appear to be very robust either with respect to the sample period, or with respect to plausible alternative specifications (see Barth, Iden, and Russek 1984–1985, 1986, Modigliani and Sterling 1986, and Kormendi and Meguire 1986 for a response).

Note that the preponderance of studies in table 1 employ OLS (see the column labeled "Technique"). Failure to treat potential endogeneity is a serious omission. Shocks to consumption may be correlated with shocks to income, which in turn raise tax revenues (lower deficits). Thus, there is a natural bias in favor of Ricardian equivalence, even in a Keynesian world. Although Feldstein (1982a) and Seater and Mariano (1985) employ instrumental variables, the validity of their instruments is highly questionable.[22]

Table 1 also contains a partial listing of "key omitted variables."[23] A number of studies fail to include either a measure of government deficits or government debt. Such studies are naturally less informative, and tend to confound the effects of debts and deficits, which are correlated. Others omit a measure of government spending. Since deficits are highly correlated with spending, and since government consumption appears to be a substitute for private consumption (Kormendi 1983, Aschauer 1985, Kormendi, LaHaye, and Meguire 1986), this creates a bias in favor of Ricardian equivalence. Several other studies (Evans 1985, Boskin 1985, Modigliani and Sterling 1986) include government spending, but impose

large effects on consumption. It is more sensible to compare the debt coefficient to plausible propensities to consume from private wealth. This leads one to the conclusion that virtually the entire value of government bonds is net wealth.

22. Indeed, Seater and Mariano (1985) report that their instruments fail exogeneity tests.
23. This list of omitted variables is by no means exhaustive. Complete specifications would include measures of government transfers, retained earnings, unemployment (or other cyclical variables), social security, and perhaps even interest rates and monetary variables.

the potentially spurious restriction that government spending is neither a substitute nor a complement for private consumption. In some cases, private wealth is omitted. Yawitz and Meyer (1976) show that this may bias estimates of the deficit effect downward. In addition, private wealth is also almost certainly correlated with government debt. Finally, all studies (with the exception of Seater and Mariano 1985) exclude measures of marginal tax rates. I emphasized the importance of controlling for tax rates in subsection 3.1. Indeed, Seater and Mariano's finding that personal tax rates enter significantly corroborates this view. Unfortunately, they do not include corporate tax rates, which are perhaps the most critical determinants of effective levies on capital income (see King and Fullerton 1984).

Several papers have documented the sensitivity of certain results to the redefinition of certain variables (see, for example, Yawitz and Meyer 1976, Buiter and Tobin 1979, Seater and Mariano 1985, Boskin 1985, Barth, Iden, and Russek 1984–1985, 1986, and Modigliani and Sterling 1986), as well as to the choice of sample period (see, for example, Barth, Iden, and Russek 1984–1985, 1986 or Tanner 1978). The atypical nature of war years is particularly evident, and calls into serious question studies that focus on wartime periods (for example, Evans 1985).

In addition to the studies listed, there is also a literature that estimates the relationship between aggregate social security wealth and consumption (see, for example, Feldstein 1974, Munnell 1974, Barro 1978, and Darby 1979). These studies are subject to the general criticisms noted in section 3.1. In addition, they are of limited interest because the critical variable, aggregate social security wealth, is nearly impossible to measure. Extreme sensitivity of estimates to the method of constructing this variable is evident in papers by Barro (1978), Leimer and Lesnoy (1982), and Feldstein (1982b). Auerbach and Kotlikoff (1983) have also conducted simulations that suggest that macro estimates of social security's impact on consumption are likely to be highly unstable. Little weight should be attached to these studies.

3.2.2. Consumption Euler Equation Studies There is a close relationship between the Ricardian equivalence proposition and the stochastic permanent income hypothesis. This observation suggests that it may be possible to design more powerful tests of the Ricardian view by employing a consumption Euler equation approach. Yet this approach has important limitations.

Tests of the Euler equation specification are generally unfavorable to the stochastic permanent income model of aggregate consumption (see

Hayashi 1985). Yet this finding may reflect factors that have nothing to do with Ricardian equivalence (such as failure of conditions for valid aggregation). Furthermore, even if these factors were related to Ricardian assumptions, one would learn very little. As long as the Euler equation is misspecified, one cannot use it to measure the effects of deficits (a different specification, based upon some alternative behavioral model, would be called for).

Failure to reject the Euler equation restrictions is, in the current context, also uninformative. Ricardian equivalence may fail for reasons that do not invalidate this specification (for example, consumers plan consumption and saving rationally, but are myopic about the link between current deficits and future taxes). In addition, it is difficult to gauge the power of such tests against interesting alternative (failure to reject may result from imprecision). Nevertheless, in this case one could in principle measure the effects of government borrowing by including a measure of the current deficit innovation.

Relatively few studies of deficits have adopted this approach. Aschauer (1985) estimates a consumption Euler equation using quarterly U.S. data (1948–1981), and finds that lagged values of deficits are correlated with the consumption innovation. Rather than reject either Ricardian equivalence or the behavioral specification, he attributes this to two factors: first, deficits are correlated with government spending, and second, government spending is substitutable for private consumption. He then tests (and does not reject) the hypothesis that lagged deficits matter only insofar as they are used to form expectations about government spending. Aschauer claims that his evidence jointly supports the stochastic permanent income hypothesis, rational expectations, and Ricardian equivalence. I take this support to be extremely weak at best, for three reasons. First, if consumers are rational about consumption but myopic about the link between taxes and deficits, or if the intergenerational linkage hypothesis is invalid, one would find exactly this pattern, despite the failure of Ricardian equivalence. Past deficits would be uncorrelated with Euler equation errors except insofar as they helped to predict government consumption (a component of total consumption). Nevertheless, unanticipated changes in the deficit would still have an independent effect on current consumption. Aschauer does not test for this. Second, the Euler equation specification might still fail other tests for reasons bearing on the validity of Ricardian equivalence. Third, Aschauer does not discuss the power of his test against particular alternatives. It is possible that one would also be unable to reject the hypothesis that lagged deficits have very large effects on the current con-

sumption innovation, independent of their role in predicting government spending.

Poterba and Summers (1986) also estimate a consumption Euler equation using quarterly U.S. data (1970–1986). They include a tax abatement variable, which reflects the current-year impact of tax cuts enacted into law during previous years. The coefficient of this variable is very large, but estimated imprecisely. Furthermore, since the abatement variable belongs to each consumer's lagged information set, Poterba and Summers effectively reject their Euler equation specification on the basis of an orthogonality test. This obscures the interpretation of the tax abatement coefficient.

It is also obvious from inspection of their tables that Poterba and Summers's results are driven by recent experience—during the 1980s deficits have been high and saving low. While this could be explained by rising expectations, the authors argue that forecasts of income and GNP growth during this period looked relatively bad. Yet econometric forecasts need not reflect consumer expectations. In particular, much evidence suggests that President Reagan has inspired public confidence.[24]

3.2.3. Studies of Aggregate Demand and GNP Eisner and Pieper (1984, 1986) and Boskin (1986) estimate reduced-form macro models to measure the effects of deficits on the size and composition of GNP. Unfortunately, there is no compelling a priori reason for excluding variables in any reduced-form equation, so it is hard to distinguish cause and effect. In my view, this evidence simply describes correlations without permitting a behavioral interpretation. The results of such studies are at best suggestive of the possibility that deficits might have real effects.

3.3. STUDIES OF NOMINAL VARIABLES

3.3.1. Interest Rates Common wisdom holds that deficits raise interest rates. Economic theory suggests that this might occur for two reasons. First, if deficits depress saving, then interest rates must rise to bring saving and investment back into balance. Second, if deficits stimulate aggregate demand, then the transactions demand for money may rise. With a fixed stock of money, higher interest rates are necessary to choke off the

24. This view is certainly consistent with the University of Michigan's Social Research Center's finding that consumer confidence reached an 18-year high in the first quarter of 1984 (*Wall Street Journal*, May 8, 1984, p. 7). One might try to test whether this explains saving behavior in the 1980s by including a consumer confidence series in the regressions. But any proxy for confidence is likely to be very imperfect, and spurious factors may still pick up the bulk of this effect.

excess desire for liquidity. Since both effects can occur only if individuals perceive government bonds to be net wealth, these observations suggest a test of the Ricardian hypothesis.

The value of this test depends to a very large extent upon one's view of international capital markets. If international capital flows equalize interest rates across countries, then U.S. deficits cannot sustain domestic interest rates in excess of world rates. Work by Feldstein and Horioka (1980) and Harberger (1978) has spawned a substantial literature that studies the issues of capital mobility and interest rate equalization. A detailed discussion of this work would carry me much too far afield. Instead, I simply note that one cannot distinguish between the Ricardian equivalence and perfect capital markets hypotheses on the basis of reduced-form relationships between government borrowing and interest rates.

There are nevertheless a very large number of studies that estimate such relationships. The Congressional Budget Office (1987) has recently summarized the methods and results of some two dozen studies. The evidence is extremely mixed. Rather than reiterate this summary, I will organize my discussion around the major problems encountered by analyses of interest rates, and I will focus primarily upon recent papers that make some attempt to overcome these problems (Dwyer 1982, Evans 1985, 1986a, 1986b, 1986c, 1987, Plosser 1982, 1986, and Feldstein 1986b).

It is important to emphasize that all of the issues raised in subsection 3.1 are directly relevant in the current context. Two of these merit further elaboration.

First, a number of papers employ very specific models of interest rate determination. Studies by Plosser (1982, 1986) and Evans (1986a, 1986b, 1986c, 1987) invoke the efficient markets hypothesis, and employ restrictive assumptions about the term structure, such as time and maturity invariant risk premia. One must always bear in mind that these studies test Ricardian equivalence jointly with these strong maintained hypotheses.

It is particularly interesting to note that most studies employing highly restrictive models of interest rate determination find a significant negative relationship between deficits and interest rates. One explanation is that, by introducing uncertainty, deficits crowd-in investment. For reasons mentioned in section 2, I regard this theory as far-fetched. This leaves two possibilities. First, reductions in marginal capital income tax rates may stimulate saving in the short run. While this explanation may account for historical experience, it renders the evidence uninformative for policy purposes—deficits resulting from cuts in labor income taxes or inframarginal income taxes might well significantly stimulate current

consumption. Second, restrictive interest rate models may simply yield spurious result.[25] In either case, the evidence is of little relevance.

Second, no study provides a fully satisfactory treatment of information and expectations. Feldstein (1986b) has emphasized that interest rates are probably much more responsive to expected deficits than to either current deficits or outstanding debt. Yet it is not at all clear that current deficits, or innovations in current deficits, are more highly correlated with future deficits than are other measures of current economic activity or fiscal policy. Likewise, current deficits may be highly correlated with the future values of other variables. Suppose, for example, that current deficits are generally followed by budget-balancing cuts in government spending. Since temporary spending tends to raise interest rates (see, for example, Barro 1986), current deficits might then be inversely correlated with long-term rates.[26]

Several authors have made some effort to cope with these difficulties. Feldstein (1986b) and Plosser (1986) both include measures of expected future budget deficits in various interest rate specifications. The measurement of these expectations is, of course, problematic, and undoubtedly subject to serious error (see section 3.1). In addition, these authors fail to include measures of expectations about other variables, including government spending, output, and prices; the preceding remarks suggest that omitted and included variables may be highly correlated.

Evans (1987) adopts a much different approach. He begins by specifying a reduced form equation for current interest rates as a function of lagged rates, current and lagged values of various other variables (including policy instruments), and expected future values of these other variables. His central assumption is that the coefficients on all deficit variables (including expectations) should be positive. He justifies this with reference to standard theory. He then supplements this equation with a model of the term structure and a stochastic VAR process describing the evolution of the other variables. Combining these equations, he shows that a standard VAR should satisfy the restriction that the deficit

25. It is particularly hard to account for Plosser's (1986) finding that neither government spending nor monetary policy affects interest rates without attributing his results to specification error.

26. The potential significance of these effects is evident in studies that explicitly describe VAR results. For instance, Plosser's (1986) figures suggest that future inflation tends to decline in response to a debt innovation. Since inflation presumably raises nominal interest rates, high current deficits may be spuriously associated with low long-term interest rates simply because deficits help to predict future inflation. Similarly, his estimates suggest that future real public debt growth rises in response to military spending innovations. This raises the possibility that current spending, rather than current deficits, may be the best proxy for future deficits.

coefficients sum to a positive number. He finds that this restriction is inconsistent with the data.

Unfortunately, Evans's derivation employs some unusually strong and objectionable maintained hypotheses.[27] More fundamentally, I question the validity of his central assumption. Evans's original reduced-form equation omits expected future interest rates. To the extent future rates affect current behavior, Evans's basic equation represents a quasi-reduced form, from which expected interest rates have already been eliminated. This dramatically alters the interpretation of all coefficients. Suppose, for example, that some current (period t) policy variable raises future (period $t + 1$) interest rates. Then individuals will typically tend to reduce consumption in the relative short run (periods t and $t + 1$), in order to save more for the long run. Current saving (period t) will therefore rise. To bring current saving and investment back into balance, current interest rates (period t) must then *fall*. Evans has simply neglected such intertemporal feedbacks.

I now turn to some problems that are specific to the analysis of interest rates. These problems have a common root, which is that interest rate equations have no direct behavioral interpretation. Rather, they are quasi-reduced forms, reflecting the interplay of forces that alter the supply and demand for funds. Interest rate effects depend upon the kind of behavioral relationships discussed in section 3.2—these are simply behind the scenes, shrouded by another layer of economic noise (such as term structure relations and international capital flows). As a result, when studies of behavior and interest rates conflict, I am inclined to be very skeptical about the conclusions of the latter. Along these lines, three specific points merit discussion.

First, reduced-form relationships may be highly unstable, and unreliable as policy guides (Lucas 1976). Ample evidence indicates that this is the case in the current context.[28]

27. Aside from the term structure assumptions, he also imposes restrictions on the VAR process that rule out the possibility that current changes (for example, in the deficit) help to predict future changes (for example, in government spending). I have already argued that in such cases there may be spurious negative correlations between deficits and interest rates.

28. Evans (1987) provides separate estimates for twelve distinct subperiods. While he does not report the results of Chow tests, the estimates appear to move dramatically between subperiods. Despite employing very similar methodologies in his two papers, Plosser's (1982, 1986) own estimates are highly contradictory. In his first study, he found that government spending and monetary policy affected interest rates as predicted, and that deficits were essentially irrelevant, while in his second study, he found that nothing mattered except deficits, which depressed interest rates. Barth, Iden, and Russek [1984–1985] also document the sensitivity of other estimates to sample periods, as well as to variable definitions. Undoubtedly, some of this instability arises from the

Second, it is virtually impossible to assess the power of tests based upon interest rate equations. When estimating consumption functions, one has both a pure Ricardian and pure Keynesian benchmark. But in the case of interest rate equations, we have only a Ricardian benchmark: deficits do not alter interest rates. Since the empirical model is intended to represent a reduced form rather than a behavioral relationship, one cannot, in the absence of extensive information about various elasticities, construct a natural Keynesian benchmark. Indeed, given the small magnitude of plausible wealth effects and the tendency for interest rates to equalize across countries, there is little reason to believe that deficits should significantly raise interest rates in the short run.

These observations lead me to conclude that interest rate studies are intrinsically uninformative as tests of the Ricardian hypothesis.[29] What can we deduce from the coefficient of deficits in an empirical relationship explaining interest rates? If it is significantly positive (as in Feldstein 1986b), one might conclude that consumers perceive some fraction of government bonds to be net wealth, but one cannot estimate this fraction. If the coefficient is not significantly different from zero (as in Dwyer 1982[30] or most of Evans 1986a estimates), one cannot reject Ricardian equivalence, but one also cannot determine whether the estimates are inconsistent with any other conceivable hypothesis of interest. If the coefficient is significantly negative, one has probably learned nothing at all.

Third, estimated models of interest rate determination are extremely parsimonious. It is difficult to believe that the lion's share of movements in interest rates is driven by perhaps three to five explanatory variables. This raises the possibility that the included variables simply proxy for a variety of omitted factors. In most studies, omissions of this sort undoubtedly bias the coefficients of interest.[31]

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inclusion of data from years in which the United States was at war (this is most relevant in the case of Evans's (1987) study). Studies that employ wartime data (especially Evans 1985 and Evans 1986c) are highly suspect.

29. This is not to say that such studies are unimportant. Clearly, they can (subject to the other qualifications noted here) quantify the effects of deficits on interest rates. This relationship is of independent interest (indeed, it is one of the reasons that we care about the Ricardian hypothesis). However, it may tell us more about elasticities than about the validity of the central Ricardian assumptions.

30. Dwyer's strategy is to estimate a VAR for deficits, inflation, interest rates, and the monetary base, and then to test the hypothesis that the coefficients of deficits in the equations for other variables equal zero. In principle, one can implement this test using the results of any study that includes VAR estimates. It is interesting to note that most results (such as Plosser 1986) appear inconsistent with Dwyer's conclusion that deficits are irrelevant.

31. Feldstein (1986b), for example, fails to include a measure of government spending. Since temporary spending tends to raise both deficits and interest rates, this may ac-

Not all of the evidence on interest rates is based upon econometric estimation. Barro (1986) has argued that two episodes in British history, during which the government ran deficits for apparently exogenous reasons, provide natural experiments by which to judge the Ricardian hypothesis. He points out that interest rates did not rise significantly in either case. Yet one episode concerned extremely short-term borrowing, while the other involved a complex set of transfers. In neither case would one have anticipated a significant rise in consumption and/or interest rates under the standard Keynesian view (see Bernheim 1987 for an elaboration). This evidence is therefore inconclusive.

3.3.2. Other Variables A small number of studies have examined the link between deficits and exchange rates (Hooper 1985, Hutchinson and Throop 1985, Evans 1986d, Feldstein 1986c). Conventional reasoning holds that, by raising domestic interest rates, deficits lead to inflows of foreign capital. International account balance requires offsetting inflows of goods. Accordingly, the value of the domestic currency must rise in order to stimulate imports and discourage exports.

Empirical evidence on this point is mixed, with Feldstein and Evans bracketing the available estimates. This is hardly surprising: since the link between interest rates and exchange rates has nothing to do with Ricardian equivalence, analysis of exchange rates merely adds a layer of noise to the interest rate relationship. Accordingly, when Feldstein uses the same methodology as in his interest rate study, he reaches the same conclusions (similarly for Evans). If anything, estimation of exchange-rate relationships is more problematic, in that one must work with relatively little data (the United States floated the dollar in 1973), and worry about a larger number of explanatory variables (those describing the economic environment of each trading partner).

Papers by Dwyer (1982), King and Plosser (1985), and Protopapadakis and Siegel (1984) study the relationship between deficits and inflation. According to standard theory, deficits should accelerate inflation by stimulating aggregate demand. Before the 1980s there was indeed a high correlation between deficits and inflation in the United States. However, this correlation was largely spurious, since constant real deficits imply

count to some extent for Feldstein's findings (although Barth, Iden, and Russek (1984–1985) have noted in a separate study that the inclusion of a government spending variable did not substantially alter their estimates). Evans (1987) omits a measure of inflation. Unexpected inflation tends to reduce the real deficit, while increasing nominal interest rates. This could account in part for his finding that deficits depress interest rates.

higher nominal deficits during inflationary periods (see Dwyer). While studies tend to find little or no effect of real deficits on inflation, most of the criticisms raised in the context of interest rates are applicable.

Finally, Evans (1985) and Barth, Iden, and Russek (1984–1985) estimate money demand equations. Evans focuses on World War II, during which Treasury bill rates were pegged, and finds no evidence to support the traditional view; Barth, Iden, and Russek modify a money demand equation originally estimated by Hafer and Hein (1984) by including a measure of deficits, and find that government borrowing significantly stimulates money demand. In addition to most of the problems discussed above, these studies may also confound supply and demand effects. Furthermore, since the real stock of money depends upon the price level, estimates also reflect the link between deficits and inflation.

3.4. SUMMARY

While attempts to measure the effects of deficits directly may well face insuperable difficulties, they do supply one more piece of the overall picture. A succession of studies have established the existence of a robust short-run relationship between deficits and aggregate consumption. While there are many potential explanations for this pattern, it is at least consistent with the traditional Keynesian view. Results for interest rates are mixed, and considerably more difficult to interpret. Thus, while time-series evidence weighs against Ricardian equivalence, it does not by itself tip the scale. However, in the context of theoretical reasoning and behavioral analyses, a coherent picture emerges in which the Ricardian outcome appears relatively unlikely.

Recent experience confirms this evaluation: during the 1980s, deficits and interest rates rose dramatically, while savings rates plummeted (see Poterba and Summers 1986 or Feldstein 1986b for discussions). This period was relatively unique in U.S. history, in that large deficits resulted from declines in net revenue, rather than temporary increases in government spending, so that it presents us with a fairly clean experiment. This interpretation of the 1980s is, of course, controversial, and formal statistical analyses have produced conflicting results (compare Evans 1985, 1986b with Feldstein 1986b). One key issue in this controversy concerns timing: when exactly did taxpayers begin to anticipate large deficits? I tend to believe that changes in expectations were approximately coincident with movements in interest rates and savings (indeed, more recently real interest rates and deficit projections have fallen together, although the direction of causality is perhaps not clear). However, I am more struck by the comparison of the 1980s with earlier periods. There is

no question whatsoever that expected deficits, interest rates, and consumption have all on average been significantly higher since 1982 than in, for example, the 1970s. While this is conceivably attributable to spurious factors (see section 3.2.2), the comparison is highly suggestive.

4. *Direct Evidence: International Comparisons*

As an alternative to analyzing time-series data for individual countries, one could also measure the effects of government borrowing through international comparisons. This approach offers certain natural advantages. First, there is much more independent movement of deficits and government spending across countries than there is within countries. Second, by averaging over substantial periods of time, one can hope to measure the more permanent components of each variable, and thereby minimize problems arising from the informational and expectational issues dicussed in section 3.1. In addition, results based on multiperiod averages may provide some clue as to the long-run effects of sustained deficits. Finally, if one is willing to forego multiyear averages and pool time-series cross-section data, one can greatly expand the number of available observations.

On the other hand, cross-country comparisons hardly provide a panacea. Many of the issues described in section 3 apply equally well to international data. In addition, several problems become much more serious. First, the relevant variables may be measured differently (or mismeasured differently) in different countries.[32] Second, countries differ structurally in terms of institutions, and the population of each country may behave somewhat idiosyncratically. Third, deficits may move for reasons unrelated to the Ricardian hypothesis (for example, shocks to oil prices).[33] Finally, practical considerations may seriously limit the number of observations available for analysis.

It is difficult to weigh the relative importance of these advantages and disadvantages a priori. Evidence from cross-country comparisons is therefore neither definitive nor irrelevant—it simply adds one more piece to the overall picture.

32. There is reason to believe that patterns of mismeasurement may systematically bias estimates of the relationship between deficits and consumption (see in particular Boskin's (1986) discussion of Japan). Standardization of measurement for a large sample of countries would undoubtedly be extremely difficult, and is the proper object of a separate research program.
33. Note, however, that if consumption does not respond to these unrelated shocks, then this factor will tend to bias results in favor of Ricardian equivalence.

4.1. SOCIAL SECURITY

To date, relatively few studies have used data from international cross-sections to address issues raised in the Ricardian debate. Almost all of the existing work, including papers by Feldstein (1977, 1980), Barro and MacDonald (1979), Kòpitz and Gotur (1979), and Modigliani and Sterling (1983), analyzes the effect of social security on private saving or consumption. Generally, these studies describe regressions of saving or expenditure on age distribution variables, retirement variables, income growth, and a measure of social security benefits or entitlements.[34]

In attempting to reconcile the mixed findings that emerge from these studies, Modigliani and Sterling point out that social security has an indirect effect on saving through its impact on retirement. Furthermore, this works to offset the direct effect. Since Feldstein and Modigliani-Sterling control for retirement, they tend to find a positive relationship between social security and spending, while Barro-MacDonald and Kopitz-Gotur, who fail to control for retirement, find little or no relationship. Under this view, social security has little effect on aggregate capital accumulation, but for reasons entirely unrelated to Ricardian equivalence. However, this explanation provides only a partial reconciliation of the existing results, in that neither Barro-MacDonald's nor Modigliani-Sterling's findings are robust. Given the difficulty of measuring social security wealth, particularly in a way that is comparable across countries, and given the degree of uncertainty involved in selecting a specification that adequately allows for various socioeconomic and demographic differences between countries, sensitivity to variable definitions and changes in functional specification is extremely disturbing. The evidence simply does not justify any strong inferences.

4.2. DEFICITS

In this section, I present new evidence on the relationship between government borrowing and private consumption. The novel aspect of my analysis is that it is based upon international comparisons, rather than aggregate time series. Work in progress by Modigliani, Mason, and Sterling (1986) also tests the Ricardian hypothesis with cross-country data. Their preliminary results, based upon an alternative specification and a substantially different sample, corroborate my findings.

The specifications estimated below are based upon equation (2), which

34. Barro adds government purchases and unemployment, but omits retirement, as do Kopitz and Gotur.

I modify in the following ways. First, I drop the intercept term (that is, I assume that utility is homothetic), and divide both sides of the equation by Y to adjust for heteroskedasticity. This obviates the need to convert quantities to per capita figures, or to a common currency through exchange rates. Second, due to the lack of data, I omit W from the equation entirely.[35] Third, I add measures of real income growth (YG) and population growth (PG). These variables may capture at least some of the socioeconomic factors that create international variation in propensities to consume. My final empirical specification is therefore

$$C/Y = \beta_1 + \beta_2 DEF/Y + \beta_3 G/Y + \beta_4 D/Y + \beta_6 YG + \beta_7 PG + \varepsilon. \quad (3)$$

(where DEF is the deficit).

I estimate equation (3) using data obtained from the IMF's *International Financial Statistics*. I measure the variable Y as gross domestic product, C as private consumption expenditure, D as total outstanding government debt,[36] DEF as the current net deficit (surplus) of the consolidated central government, adjusted for inflationary erosion of the real value of outstanding debt,[37] and G as current government consumption, which is calculated in the manner prescribed by the United Nations' system of Standard National Accounts.[38] I measure YG as the log of the ratio of current to lagged real GDP; similarly, PG is the log of the ratio of current to lagged population.

Data quality is a severe problem. Many of the governmental variables for *International Financial Statistics* are evidently constructed internally by the IMF, and are considered highly unreliable. Accordingly, one should think of this exercise as a suggestive exploration. However, one should also bear in mind that poor measurement of the fiscal variables should bias my findings in favor of the Ricardian hypothesis.

For many countries, one or more critical variables are simply unavailable. In other cases, data are missing in particular years. The most trouble-

35. For every specification described below, I experimented with adding a measure of money and quasi-money as a proxy for private wealth. This variable was never significant, frequently entered with the wrong sign, and its addition had little or no effect on the remaining parameters. I therefore excluded it from the final regressions.
36. Unfortunately, a measure of domestically held debt is not available for a sufficiently large sample of countries.
37. The adjustment for inflation had only a very minor effect on the resulting coefficient estimates.
38. I also experimented with specifications that included measures of total government spending; the addition of this variable did not alter appreciably any of the results reported below.

some variable is outstanding government debt; the availability of this variable largely dictates sample selection. I found that by focusing on the twelve-year period 1972–1983, I could assemble a relatively complete data set based on a sample of 26 countries.[39]

In an effort to identify robust empirical relationships, I have analyzed these data in a number of different ways. The first approach is to take twelve-year averages for each country, and run simple cross-sectional regressions. The primary advantage of this approach is that it allows me to relate long-run levels of the explanatory variables to long-run private consumption. Transitory movements, which may carry spurious information or alter expectations in unknown ways, become significantly less important. As mentioned above, data on certain countries is not quite complete. In such cases, I base averages on the years for which all relevant variables are available. This may introduce a small amount of heteroskedasticity.

Estimates of the basic specification are given as equation 2.1 in table 2. Note that the coefficient of deficits is extremely large, and in fact exceeds the marginal propensity to consume. Although this coefficient is not estimated very precisely, it does differ from zero at the 10 percent level of confidence. Note also that the coefficient of debt suggests a plausible wealth effect, although it too is imprecise.

Since the data may not distinguish between interesting hypotheses on the basis of wealth effects, I reestimate the basic specification, omitting D (note that this does not bias the remaining coefficients under the Ricardian view). Results are given in equation 2.2. The deficit coefficient rises, and becomes highly statistically significant.

Results based upon simple cross-sectional estimates are clearly suspect. A parsimonious regression cannot possibly capture the richness of socioeconomic factors that create variation in saving rates across countries. Unobserved factors that are correlated with private extravagance (high levels of consumption) may also be associated with public extravagance (large deficits); this may in part explain the very large deficit coefficients in equations 2.1 and 2.2. Differences in accounting practices may also make international comparisons of deficit levels problematic.

To address these considerations, I divide the twelve-year sample into two distinct six-year subperiods (1972 to 1977 and 1978 to 1983), and analyze the relationship between *changes* in consumption and *changes* in deficits. Since this amounts to allowing for country-specific differences in

39. The sample consists of Argentina, Belgium, Canada, Costa Rica, El Salvador, Finland, France, Germany, Guatemala, Iceland, India, Italy, Korea, Luxembourg, Morocco, New Zealand, Norway, the Philippines, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Thailand, the United States, and Venezuela.

Table 2 CROSS-COUNTRY REGRESSION RESULTS

Equation number	*Technique*	*Intercept*	*DEF*	*G*	*YG*	*PG*	*D*	R^2
2.1	12 year averages	0.822 (0.064)	1.16 (0.64)	−1.41 (−.29)	−0.464 (0.750)	−0.112 (1.14)	0.080 (0.078)	0.609
2.2	12 year averages	0.847 (0.059)	1.50 (0.55)	−1.44 (0.29)	−0.553 (0.746)	−0.04 (1.14)		0.589
2.3	6 year changes	−0.015 (0.011)	0.329 (0.226)	−0.733 (0.443)	−0.367 (0.293)	−1.99 (1.45)	0.134 (0.104)	0.418
2.4	6 year changes	−0.010 (0.010)	0.442 (0.213)	−0.492 (0.409)	−0.444 (0.293)	−1.74 (1.46)		0.361
2.5	pooled panel	0.805 (0.017)	0.586 (0.109)	−1.28 (0.082)	−0.367 (0.092)	1.141 (0.320)	0.084 (0.020)	0.560
2.6	pooled panel	0.833 (0.016)	0.756 (0.104)	−1.30 (0.084)	−0.426 (0.093)	−0.230 (0.328)		0.532
2.7	pooled panel, fixed effects	NA	0.370 (0.056)	−0.395 (0.114)	−0.202 (0.047)	−0.427 (0.229)	0.059 (0.024)	0.924
2.8	pooled panel, fixed effects	NA	0.382 (0.055)	−0.317 (0.111)	−0.229 (0.048)	−0.566 (0.234)		0.919

the propensity to consume out of income (that is, fixed effects), it should minimize biases arising from unobserved socioeconomic differences. In addition, fixed effects estimation eliminates many problems arising from systematic differences between accounting techniques, since results depend upon changes in variables, rather than their absolute levels.

To implement this second approach, it is necessary to apply a somewhat more demanding sample selection criterion, in that one needs enough data to calculate a reliable average for each six-year subperiod. Accordingly, I was forced to drop three countries. All of the remaining estimates are based upon a sample of 23 countries.

It is possible to get a feel for general patterns by looking at the data in a relatively unprocessed form (see table 3). For each country, I list the change in private consumption and the change in adjusted deficits between the two six-year subperiods. All values are expressed relative to concurrent GDP.

Note first that for roughly two-thirds of the sample (15 of 23 countries), deficits and consumption moved in the same direction between

Table 3 CHANGES BETWEEN SIX-YEAR SUBPERIODS

Country	ΔC	ΔDEF
Belgium	0.037	0.057
Canada	−0.002	0.016
Costa Rica	−0.058	−0.020
El Salvador	0.031	0.034
Finland	−0.005	0.020
France	0.025	0.008
Germany	0.014	0.006
Guatemala	0.025	0.025
Iceland	−0.024	−0.071
India	−0.026	0.021
Italy	−0.004	−0.030
Korea	−0.049	0.007
Morocco	−0.017	0.005
New Zealand	−0.015	0.006
Norway	−0.037	−0.025
South Africa	−0.031	−0.012
Spain	0.010	0.032
Sri Lanka	0.008	0.059
Sweden	−0.007	0.059
Switzerland	0.024	0.001
Thailand	−0.011	0.020
United States	0.009	0.008
Venezuela	0.093	0.010

the two six-year subperiods. This correlation is stronger in cases in which the change in deficits was large. Sixteen countries experienced a change in excess of 1 percent of GNP; in 11 cases, deficits and consumption moved in the same direction. Confining our attention to cases in which the change in consumption was also large (greater than 1 percent of GDP), we see that deficits and consumption moved together in 9 of 11 cases. If we focus on countries experiencing a change in average deficits exceeding 2 percent of GDP, the comparable numbers are 9 of 13, and 7 of 9 countries, respectively. By restricting our attention to those countries experiencing very large changes (more than 3 percent of GDP), we find that consumption and deficits moved together in 6 of 7 cases. Finally, deficits and consumption moved in the same direction for every country (4 of 4) experiencing both a very large change in deficits (greater than 3 percent) and a significant change in consumption (greater than 1 percent). Note that while the U.S. experience is consistent with this pattern, it is by no means egregious. In fact, given my choice of subperiods, the change in U.S. deficits was not even particularly large.

While the relationship between deficits and consumption is apparent in the raw data, one cannot measure it by inspection of the numbers, nor be certain that it does not reflect spurious correlations. Accordingly, I regress the change in private consumption on the changes in each of the dependent variables. Results are given as equations 2.3 and 2.4. Several patterns emerge. First, the deficit coefficients are, as expected, much smaller. However, the estimated effect is still quite sizable (a $1 deficit-for taxes swap would raise consumption by $0.33 to $0.44). Unfortunately, the coefficient is statistically significant at conventional levels only when debt is omitted. While the coefficient of debt is still positive, it remains imprecise.

I have also estimated 2.3 and 2.4 as Euler equations by moving consumption for the first six-year subperiod to the right-hand side of each equation. This yielded a slight increase in both the size and precision of the coefficients for the fiscal variables. Deficits entered significantly regardless of whether the debt variable was included. See Bernheim (1987) for details.

As a final step, I also estimated regressions based on pooled, single-year, time-series cross-section data. Equations 2.5 and 2.6 do not include country-specific intercepts. While these estimates bear a strong resemblance to those obtained from twelve-year averages, there are also some important differences. The coefficients of deficits are large, but unlike those in equations 2.1 and 2.2, not "too" large. In fact, the marginal propensities to consume from deficits and income are almost indistinguishable when debt is omitted. Standard errors are also much lower, owing

to the relative size of the data sample. In equation 2.5 the debt coefficient is positive, sensible, and statistically significant.

The inclusion of country-specific intercepts somewhat alters this picture. Not surprisingly, these estimates bear a strong resemblance to those based on changes between six-year subperiods: deficits appear to stimulate consumption by about $0.40 on the dollar. Once again, this effect is estimated very precisely. The debt coefficient is again positive, sensible, and statistically significant.

Overall, analysis of cross-country data supports the view that government deficits stimulate private consumption. The robustness of this conclusion with respect to alternative specifications, estimation techniques, and samples is quite striking.

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Comment

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In light of the many strong assumptions required to get Ricardian equivalence to hold, I certainly agree with the author's position that it is extremely unlikely that the world is characterized by exact Ricardian equivalence. This strong theoretical presumption against exact Ricardian equivalence is bolstered by data on actual bequests, which confirms that some non-negligible fraction of the population does not leave positive bequests. However, I doubt that the proponents of the Ricardian paradigm would choose to pitch the battle over the issue of whether the exact form of Ricardian equivalence does or does not hold. Instead, I think that proponents of the Ricardian model would argue that the Ricardian equivalence hypothesis should be viewed as a starting point or benchmark from which to characterize the nature and magnitude of the economy's departures from the idealized model. For this reason, I think the debate should center around the usefulness of Ricardian equivalence as an approxima-

tion; that is, we should focus the inquiry on the nature of the observed departures from exact Ricardian equivalence, their pervasiveness, and their quantitative importance. Since the central issue, in my view, is an empirical one, I will focus my comments on Bernheim's discussion of the empirical evidence.

My primary point has to do with the article's relative emphasis on the different lines of empirical research bearing on the validity of Ricardian equivalence. In particular, I would like to focus on the author's use of the empirical consumption literature. While the article mentions a few empirical studies based on the Euler equation line of consumption research opened up by Hall's 1978 paper, the author places considerably more emphasis on the older line of empirical research that estimates aggregate consumption functions in a nonrational expectations specification and then tests for the presence of effects of the deficit on consumption. In my mind, these two approaches—the nonrational expectations aggregate consumption function approach and the Euler equation approach—are mutually inconsistent. In a critical survey of the empirical evidence on a topic such as Ricardian equivalence, I think that it is reasonable to ask the author to evaluate whether the basic specification of a particular piece of empirical work, or of a whole line of empirical work, is consistent with the hypothesis being tested.

In order for Ricardian equivalence to hold, at the very least it has to be the case that agents maximize intertemporal utility and form expectations rationally. Since these two assumptions are the basis of the Euler equation approach, the Euler equation specification seems to be the natural framework to use in testing the Ricardian equivalence hypothesis.

The nonrational expectations aggregate consumption function approach, on the other hand, seems to me to be fundamentally inconsistent with the Ricardian equivalence hypothesis. In this approach aggregate consumption is regressed on either gross income or disposable income and, for example, a variable measuring the deficit. Ricardian equivalence is then accepted or rejected on the basis of the coefficient on the deficit variable. In order to show concretely why I think that this specification is inconsistent with the Ricardian equivalence hypothesis, let me assume that exact Ricardian equivalence holds; in other words, let me assume that the consumption data is generated by the Euler equation derived under the assumptions of intertemporal utility maximization and rational expectations, and see what sort of results one would obtain if one estimated a nonrational expectations aggregate consumption function of this type.

The Euler equation, of course, is based on the familiar first order condition

$$E_t\left(\frac{1 + r_t}{1 + \delta} U'(C_{t+1})\right) = U'(C_t).$$

Assuming that the utility function exhibits constant relative risk aversion and making standard distributional assumptions, the Euler equation takes the form

$$\log C_{t+1} - \log C_t = \text{intercept} + \alpha^{-1}E_t\log(1 + r_t) + (\log C_{t+1} - E_t\log C_{t+1})$$

where α is the coefficient of relative risk aversion.

Simplifying the notation by letting $\log C_t = c_t$ we have simply

$$\Delta c_{t+1} = \text{intercept} + \alpha^{-1}E_t\log(1 + r_t) + (c_{t+1} - E_t c_{t+1}).$$

Thus the Euler equation decomposes the growth rate of consumption into a forecastable component, which depends on the intercept and the conditional expectation of the interest rate, and an expectational error.

The nonrational expectations aggregate consumption function specification has the general form:

$$C_t = \text{constant} + \beta Y_t + (\beta - \gamma)\text{Deficit}_t + \omega_t$$

where $\beta - \gamma > 0$ is interpreted as a departure from Ricardian equivalence. Some studies estimate the aggregate consumption function in levels, others in differences. To facilitate comparison with the Euler equation framework, let us consider the specification in first differences:

$$\Delta C_t = \text{constant} + \beta\Delta Y_t + (\beta - \gamma)\Delta\text{Deficit}_t + \eta_t.$$

The aggregate consumption function is estimated in differences of levels of consumption, whereas the Euler equation is typically estimated in differences of the log of consumption, but the log transformation of the consumption data is unlikely to make an important difference with respect to my point, so let me gloss over the discrepancy between logs and levels.

Comparing the Euler equation specification for the change in consumption with the aggregate consumption function, one disparity is that the aggregate consumption function omits the conditional expectation of the real rate of return. However, my interpretation of the Euler equation literature is that the expected interest rate term has not really made a substantial contribution to the explanation of the change in con-

sumption, so the omission of this term is not my major concern. Instead, my major concern has to do with the interpretation of the error term in aggregate consumption. That is, if the world is really Ricardian, and hence the consumption data is generated according to the Euler equation, these studies are estimating the following:

$$\Delta C_t = \text{constant} + \beta \Delta Y_t + (\beta - \gamma)\Delta\text{Deficit}_t + (C_t - E_{t-1}C_t).$$

Thus under Ricardian equivalence, the error term in the equation is the expectational error in consumption in time t. Further, under Ricardian equivalence, the coefficients of both $\Delta\text{Deficit}_t$ *and* ΔY_t should be zero. In interpreting the error term it is clear that any variable that is useful in revising our forecast of future disposable income or future rates of return will help determine the expectational error in consumption. To capture this notion, let's think of the expectational error term as

$$(C_t - E_{t-1}C_t) = \phi_Y \varepsilon_Y + \phi_T \varepsilon_T + \phi\varepsilon,$$

where the epsilons, ε, are all innovations in a VAR; ε_Y is the innovation in gross income, ε_T is the innovation in tax revenues, and ε is a vector consisting of the innovations in all the other informational variables used by agents. Since I have no intention of actually estimating the VARs (instead I am simply using it as a conceptual construct to interpret the error term), we can think about the ε vector as having a very large dimension and actually incorporating all of the information used by agents. Thus the aggregate consumption function studies are estimating

$$\Delta C_t = \text{constant} + \beta \Delta Y_t + (\beta - \gamma)\Delta\text{Deficit}_t + (\phi_Y \varepsilon_Y + \phi_T \varepsilon_T + \phi\varepsilon).$$

Most of the aggregate consumption function studies estimate this equation by OLS, although a few use instrumental variables. In the studies using OLS there is obviously going to be simultaneity bias due to the endogeneity of gross income, taxes, and the deficit. However, even aside from the simultaneity problem, it is evident once one examines this specification in the context of the Euler equation framework that the expectational error term is going to be correlated with all of the explanatory variables.

The author's contention is that the aggregate consumption function econometric specifications will be unbiased if Ricardian equivalence holds and biased in favor of Ricardian equivalence if Ricardian equivalence does not hold. He then concludes that the empirical finding that

the estimate of $\beta - \gamma$ is significantly different from zero as fairly strong evidence against Ricardian equivalence. Viewing these regressions from the framework provided by the Euler equation approach, however, my position is that these regressions are biased, and that we can't predict the direction or magnitude of the bias a priori. I would like to add as a footnote that a few of the aggregate consumption function studies do estimate the equation with instrumental variables. Of the studies using instruments, almost all use contemporaneous values of exogenous variables. While these instruments would ameliorate the problem of simultaneity between consumption and income to some degree, the contemporaneous exogenous variables are still correlated with the expectational error under the null hypothesis.

To summarize, I doubt that many proponents of Ricardian equivalence would be dissuaded, on the basis of the aggregate consumption function studies, from thinking of the equivalence hypothesis as a reasonable approximation, since the basic specification of the aggregate consumption function in these studies has a traditional Keynesian form that is inconsistent with the equivalence hypothesis being tested. Nevertheless, I think that there is considerable evidence, based on the Euler equation approach, of quantitatively large departures from the consumption behavior required by the Ricardian equivalence hypothesis. For example, David Wilcox (1986) presents empirical evidence that the response of consumption to anticipated increases in Social Security benefits is statistically significant and large in magnitude. Wilcox's evidence, based on monthly data, of excess sensitivity of consumption to predictable changes in income is particularly striking since the adjustments in Social Security benefits are explicitly announced at least six weeks in advance. Thus his rejection of Ricardian equivalence does not depend on a strong form of the rational expectations assumption in which agents are assumed implicitly to make time-series predictions of future benefits. To briefly cite another example of evidence against the Ricardian equivalence hypothesis, Stephen Zeldes (1986) shows that the Euler equation for consumption can be statistically rejected for families with relatively low ratios of wealth to income, using data on food consumption of family units from the Panel Study of Income Dynamics.

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Comment

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When I first saw the title of this article, I thought that I was about to embark on an objective tour of the theory and evidence pertaining to "Ricardian Equivalence." Given what I knew to be the difficulties of the task I was anxious to see the outcome. Unfortunately, by page two I gave up my hope that this was going to be a dispassionate discussion of the issues and became curious as to how the author could justify his interpretation of the evidence, given the "insuperable" problems. My assessment is that Bernheim's conclusions indeed summarize his posterior distribution on "Ricardian Equivalence," but, unfortunately it seems indistinguishable from his prior distribution. Whether or not this comes about because the likelihood function is truly uninformative (which I regard as a distinct possibility), or because Bernheim chooses to ignore parts of it. I will leave the reader to decide.

The article is broadly divided into two parts. The first part focuses on the theoretical case for "Ricardian Equivalence" and the second discusses the empirical evidence.

Bernheim begins by discussing several of the assumptions underlying the irrelevance proposition and concludes that the "theoretical case . . . is extremely weak." I do not find criticizing assumptions a useful approach to evaluating an economic model. Nevertheless, there are important lessons to be learned from these theoretical investigations. I view the "Ricardian Equivalence" in much the same way that modern financial economists view the Modigliani-Miller theorems. While very few take the M-M propositions literally, they have served to focus the study of corporate financial policy by providing clues regarding where researchers might look to develop a positive theory of corporate finance. Viewed from this perspective, the first part of the paper could have usefully highlighted areas of investigation to be explored if we are interested in developing positive theories of government financial policy.

Bernheim, and most of the literature in this area, instead focuses on models that attempt to establish a wealth effect of deficits due to some

break in the intergenerational linkage. As might be expected, if one is free to close markets by assumption and choose different utility functions, one can generate endless scenarios in which neutrality either holds or does not. While these are interesting models in their own right, two arguments lead me to question the fruitfulness of this line of investigation for evaluating "Ricardian Equivalence." First, it is doubtful that we will ever be able to discriminate empirically among these various scenarios of intergenerational dependencies. Bernheim himself shows how difficult this can be in the empirical section. Second, Poterba and Summers (1986) have made a reasonable case that, in a pure life-cycle model with no bequests and employing plausible deficit scenarios, the wealth effect is small (3 to 5 cents on the dollar). Thus, to generate large wealth effects requires that agents (1) must be more myopic than even a life-cycle model or (2) be constrained in some other essential way. The empirical evidence (which I discuss later), on bequests can also be interpreted as indicating that wealth effects are not the likely channel by which deficits affect current real variables.

In my view, the most likely avenue for isolating the effects of deficits is distortionary taxes and the associated intertemporal substitution effects. Unfortunately, Bernheim only devotes one page to this topic. In cases where deficits are created by tax cuts, it is important which taxes were cut and/or how future tax revenues are likely to be generated. Thus, the standard tools of public finance may be very useful in explaining why some empirical results are not stable over time and thus why some deficit episodes are different from others. Barro (1979, 1986), for example, has exploited a positive theory of tax smoothing with some limited empirical success.

Bernheim argues that the evidence on intergenerational transfers suggests that bequests are significant but not motivated solely by altruism. Thus he concludes that the evidence is inconsistent with "Ricardian Equivalence." While the conclusion may be correct, it is difficult to justify on the basis of the evidence he presents. His conclusion in part relies on the distinction between exchange and altruistic bequests. But presumably both can exist side by side without overturning "Ricardian Equivalence." In particular, it is the marginal bequest decisions in response to deficits that are important. Unfortunately, the evidence is not informative in this dimension. One might just as well conclude that since so many people do leave bequests and since it is very difficult to distinguish altruistic transfers from other transfers at the margin, there is a reasonable case to be made that the lack of intergenerational bequests is *not* a likely candidate for overturning "Ricardian Equivalence."

The direct evidence based on aggregate time-series data is broadly

focused on results from estimated consumption functions and the behavior of interest rates.

A most troubling feature of the discussion of empirical work in this area is how willing Bernheim is to accept some results as evidence against the irrelevance hypothesis and to dismiss other results for reasons that are equally applicable to his supporting evidence. For example, Bernheim dismisses virtually the entire literature on interest rates due to endogeneity problems, omitted variables, and misspecification. At the same time, he is willing to accept the results from naive Keynesian consumption functions as evidence against "Ricardian Equivalence."

One major source of empirical evidence on the effects of deficits is found in attempts to link interest rate movements to deficits. Indeed, the effect of deficits on interest rates is one of the more frequently cited implications of deficits and motivated much of the empirical work cited by Bernheim.

Bernheim admits that the available evidence is consistent in that no one is able to find an important association between deficits and nominal interest rates. However, he dismisses these findings for a number of reasons.

For example, he argues that these results rely on specific models of interest rate determination. More precisely, they exploit a particular model of the term structure. If the term structure model is incorrect, it may account for what Bernheim regards as anomalous results. The suspect assumption of the models is that any term premium variation in longer maturity securities is unrelated to the innovations in the public debt. Unfortunately, I am not aware of any model that systematically relates deficit policy to expected premia. While this is an interesting avenue for research I doubt that such potential effects will alter the basic empirical findings for two reasons. First, in my 1982 paper (Plosser 1982), I focus primarily on the behavior of treasury bills of maturity less than one year. Although Fama (1984) shows that there is variation in the premia, it is probably less important for these very short maturities than for long-term bonds. Second, to offset the effects of debt shocks on interest rates in the context of these studies would require that deficits lower maturity premia. I anticipate that this will be a difficult result to obtain in a theory of fiscal policy.

Bernheim also argues that the nominal interest rate results are questionable because some researchers find negative effects of deficits and he finds this difficult to explain. This result however, follows in a straightforward way if deficits have no wealth effects and the deficit is created by an income tax cut. The tax cut generates increased supply through intertemporal substitution channels and interest rates fall.

Another concern repeated by Bernheim is the difficulties associated with isolating expected and unexpected components. Many argue that residuals from a VAR are very poor proxies for unexpected movements in any variable because agents undoubtedly use larger information sets than the VAR. This is an empirical claim that can be investigated. Unfortunately, Bernheim does not provide any supporting evidence. Many who have worked with the data have found that the nature of relation between deficits and interest rates does not appear particularly sensitive to the specification of the prediction equation. This does not mean that there isn't an improved model that significantly alters the results, but Bernheim's generalities offer no evidence to the contrary.

One final comment involves the distinction between real and nominal interest rates. Bernheim criticizes the interest rate evidence because of its focus on nominal rates. In Plosser (1986) I attempt to look at the effects on ex-ante real rates. The results are best described as mixed. Before 1978, deficits appear to have a significant negative association with real rates, while after 1978 the association appears to be zero or marginally positive.

There are problems associated with the results on interest rates. First, as Bernheim points out, it is reduced-form evidence; thus the results may be consistent with alternative structural specifications. Nevertheless there may be advantages to working with reduced forms. For example, they may be more robust to certain types of misspecification than, for example, consumption function equations or other behavioral equations.

Related to this observation is the current popular claim that interest rates did not move in the recent episode because the United States is a small open economy that faces a world interest rate. Thus the deficit manifested itself in the trade balance. I think this is interesting and bears further investigation. Evans (forthcoming, *Journal of Monetary Economics*) provides some preliminary evidence along these lines by investigating the effects of the "world" deficit. His results are similar to those that treat the United States as a closed economy. The difficult question becomes, why hasn't there been a link between the deficit and the current account in the past? To answer the question requires a more coherent positive theory of deficits.

In summary, I fail to see how Bernheim can conclude on the basis of the existing empirical work that the "evidence weighs heavily against the Ricardian view." My own view is more circumspect. While I think the evidence casts doubt on the importance of the wealth effect as the primary channel, considerable work remains to be done in arriving at a better understanding of government financial policy and its effects. The issues, however, will ultimately be decided on the basis of empirical re-

search and thus many of the econometric problems pointed out by Bernheim and others must be struggled with before any consensus is likely to emerge.

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Discussion

Rudiger Dornbusch suggested that Bernheim's international evidence omits several important factors. If a country has a large external debt, then an increase in the world interest rate increases the budget deficit, creating an automatic link between interest rates and the deficit—indeed a similar link exists with internal debt. In an oil-exporting country, a fall in the price of oil pushes the deficit up. These factors should be included in the regressions, which are otherwise misspecified. He also noted that *International Financial Statistics* fiscal data are unreliable.

Robert Barro questioned Bernheim's claim that the empirical evidence provides strong support for the Keynesian view. There are so many problems in the empirical studies that their results are uninformative. One problem is the endogeneity of the budget deficit. Because the deficit is countercyclical, and consumption smoother than GNP, consumption is high relative to GNP when the deficit is large. This means that the deficit will have a positive sign in the Keynesian-type consumption regression even when Ricardian equivalence holds. Another possibility leading to a positive coefficient on the deficit, discussed by Aschauer, is that taxes may serve as a proxy for future government expenditures. In this case, a tax cut leads to a high deficit and low expected future government expenditures. If government expenditure is a substitute for personal consumption, then current consumption will increase—and there is a positive correlation between the deficit and the consumption. Barro also commented that Bernheim dismissed studies on interest rates and

deficits too quickly. The negative effect of the government deficit on the interest rate, which puzzled Bernheim, is easy to explain when taxes are distortionary. Finally, Barro stressed the importance of studying episodes to test the Ricardian hypothesis. One such episode is U.S. experience since 1984, when the budget deficit increased exogenously.

Bennett McCallum questioned the use of current income in the regressions. Permanent income, not current income, is relevant for Ricardian equivalence.

Some additional considerations were suggested by Arnold Harberger. First, the traditional answer for the question of when the government should go into debt rather than tax to finance expenditure is when the benefit is expected in the future. The effect of debt financing in these circumstances is worth investigating. Another relevant line of research would be to examine how people react when they receive inheritances.

Robert Gordon argued that recent experience is unfavorable to Ricardian equivalence. It was not higher saving that absorbed the deficit, as Ricardian equivalence implies, but rather the foreign capital inflow.

Martin Feldstein interpreted a negative coefficient on the deficit in a consumption function regression in the following way. A current increase in government expenditure increases expected future government expenditure and future taxes, which leads to lower current consumption. He also pointed out that the presence of bequests, even if motivated only by altruism, might not imply Ricardian equivalence in the presence of uncertainty. Finally he argued that recent experience is unfavorable to Ricardian equivalence. The decline in the expected future deficit since 1984 has led to a lower real interest rate.

James Poterba commented that the notion of myopia is slippery. That people ignore the future taxes implied by current deficits is not necessarily enough to preclude Ricardian equivalence.

Responding to Marjorie Flavin's comments, Douglas Bernheim defended his rather strict definition of Ricardian equivalence on the grounds that many people take pure Ricardian equivalence seriously. He did not use Euler equation studies in the empirical evidence because the data may fail to satisfy the Euler equation for reasons unrelated to Ricardian equivalence—and because the Euler equation specification is often rejected. Further, there is no inherent reason why "structural" consumption functions are inconsistent with rational expectations: with the right income and wealth variables, and interest rates, (including expectations of future incomes and interest rates) "structural" consumption functions are completely consistent with the Euler equation approach under rational expectations.

He also clarified his claim on the empirical evidence being favorable to

the Keynesian view. It is difficult to learn anything from the aggregate studies, but if they are taken at face value, they are pro-Keynesian. He criticized the view that Ricardian equivalence serves as an intellectual benchmark. The assumptions used to produce Ricardian equivalence do not make sense, as pointed out by Bernheim and Bagwell. He commented that if the government deficit lowers interest rates through distortionary taxes, then those empirical studies using interest rates do not test Ricardian equivalence. Finally he pointed out that institutional variables, such as official economic forecasts, are not included in studies that use vector autoregressions. Such studies are uninteresting because they excessively restrict the information set available to investors.