# Cyclicality of U.S. Discretionary Fiscal Policy

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**Abstract**

The literature on fiscal policy conduct has failed to yield a consensus on even the most basic aspects of policy, such as whether and how it responds to the business cycle. Conflicting results may stem in part from model uncertainty, particularly uncertainty about which covariates belong in the underlying model of fiscal policy. I estimate the response of U.S. federal discretionary policy to different business cycle measures using a Bayesian framework that explicitly accounts for model uncertainty. I find that policy is countercyclical, responding primarily to the change in the unemployment rate, and that taxes make up a larger portion of the response than spending. Distinguishing between expansions and recessions makes it clear that countercyclical policy is limited to recessions; during expansions, in contrast, policymakers are unlikely to respond to economic conditions. Finally, I find no evidence of a structural break in business cycle responses, nor of substantive differences between intended policy and actual policy outcomes.

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# 1. Introduction

Despite some well-known instances of countercyclical discretionary fiscal policy by the United States government, it is far from clear whether such policy is systematic. For instance, the onset of the Great Recession in late 2007 motivated the passage of two stimulus packages, the Economic Stimulus Act and the American Recovery and Reinvestment Act, which were clearly designed to counteract the large decline in output. However, even as the economy experienced a tepid recovery and unemployment remained well above the natural rate, political focus shifted towards debt stabilization and a perceived need for fiscal austerity. In January 2013, with unemployment close to 8%, Congress passed the American Taxpayer Relief Act (ATRA) as a partial resolution to the U.S. fiscal cliff. ATRA led to a large increase in tax revenues, projected by the CBO to total 1.5 trillion dollars over the next five years.[1] Two months later, automatic cuts to discretionary spending went into effect as a result of the Budget Control Act of 2011.

Empirical work on fiscal policy has largely failed to yield a consensus on whether and how discretionary policy responds to the business cycle. Different studies have concluded that policy is procyclical, countercyclical, and acyclical, even across similar sets of countries and time periods.[2] Several papers have investigated the source of these discrepancies, emphasizing the importance of data vintages and accounting for potentially autocorrelated errors.[3] Far less attention has been paid to covariate selection. Unlike the monetary policy literature, in which policy is typically modeled as following a Taylor rule, there appears to be little agreement about how to model the conduct of discretionary fiscal policy. As a result, models can differ substantially from paper to paper. All include some measure of output, but even then there are differences in the specific measure used as well as the time period in which it enters the model.

[1] See "Estimates of the Budgetary Effects of H.R. 8, the American Taxpayer Relief Act, as passed by the Senate on January 1, 2013" at https://www.cbo.gov/sites/default/files/112th-congress-2011-2012/costestimate/american-taxpayer-relief-act0.pdf

[2] Table 1 in Golinelli and Momigliano (2009) provides a good summary of conflicting results.

[3] See, again, Golinelli and Momigliano (2009) as well as Plodt and Reicher (2015).

Table 1 provides a summary of covariates used in some well-known papers in the literature. It suggests that there is considerable uncertainty about the underlying model of fiscal policy conduct. It should be noted that, with the exception of Auerbach (2002 and 2003) and Cohen and Follette (2003), the literature has focused primarily on European fiscal policy. As a result, drawing conclusions about U.S. policy from these papers can be problematic. However, it seems likely that uncertainty about the correct model of fiscal policy extends to U.S. policy as well, particularly since it is less well-studied. Indeed, I find a number of instances in which failing to account for model uncertainty leads to flawed inferences about policy conduct.

Although counterintuitive, acyclical or procyclical policy may occur for a number of reasons. First, policymakers may think it unnecessary to respond to output if they believe the combined responses of monetary policy and non-discretionary fiscal policy can stabilize output without additional aid. Divisions between political parties may also make it difficult to pass legislation unless the situation is urgent, such as during a recession. In either case, we might expect discretionary responses to be limited to particularly severe economic events or times in which other types of policy are ineffective (for example, at the zero lower bound). Another possibility is that policymakers have other goals they see as being more important than and incompatible with countercyclical policy. For example, policymakers may care more about reducing government debt than stabilizing output. As the U.S. experience in 2013 demonstrates, concerns about fiscal responsibility can result in contractionary policy even during times of weak economic growth.

Finally, acyclical or procyclical policy could be unintentional. Legislation takes time to implement, which may cause changes in policy to occur later in the business cycle than intended. For example, spending increases in the American Recovery and Reinvestment Act, passed in early 2009, peaked in the first quarter of 2010, six months after the official end of the Great Recession. Similarly, policy decisions may be based on faulty information. Revisions to output and employment variables can be substantial, which may cause large

Table 1: Variables Included in Previous Cyclicality Studies

| Studies | Business Cycle Measure | Cyclical Period | CAD Lag | Debt | Election Dummy | Inflation | Other |
|---|---|---|---|---|---|---|---|
| Hallerberg & Strauch (2002) | Output Gap Change | $y_t$ | | | | | Time/Country FE |
| Auerbach (2003) | Output Gap | $y_{t-1}$ | | | | | Deficit Lag (Unadjusted)<br>Divided Gov. Dummy<br>President Party Dummy |
| Cohen & Follette (2003) | Output Gap | $y_{t-1}$ | x | | | | |
| Gali & Perotti (2003) | Output Gap | $y_t$<br>$y_{t-1}$ | x | x | x | | |
| Lane (2003) | Output Growth | $y_t$ | | | | | |
| Forni & Momigliano (2004) | Output Gap | $E_{t-1}y_t$<br>$y_{t-1}$ | x | x | | | |
| Mink & de Haan (2006) | Output Gap Change | $y_t$ | | | x | | Pre-Election Dummy<br>GDP Growth Forecast Error<br>Inflation Forecast Error |
| Garcia et al. (2009) | Output Gap | $y_t$ | x | x | | | |
| Egert (2010) | GDP Growth<br>Output Gap | $y_t$ | | x | x | x | Housing Price Growth<br>Stock Price Growth<br>Gov. Consumption<br>Population Growth<br>Interest Payments<br>Imports/Exports |
| Darby & Melitz (2011) | GDP Level | $y_t$<br>$y_{t-1}$ | | x | x | x | |
| Fatas & Mihov (2012) | Output Growth<br>Output Gap | $y_t$ | x | x | | | |
| Benetrix & Lane (2013) | Deviations in GDP from Quadratic Trend | $y_t$ | x | x | | | Current Account Balance<br>Domestic Credit Growth |
| Plodt & Reicher (2015) | Output Gap<br>Trend GDP | $y_t$ | | x | x | | Output Gap Revision<br>Output Gap Forecast<br>Chief Executive<br>Political Party Dummy |
| Bernoth et al. (2015) | Output Gap | $y_t$ | x | x | x | | |

differences between intended policy and actual policy outcomes.[4] This issue has been studied in the literature, with many studies concluding that intended policy tends to be more countercyclical than actual policy.[5]

Clarifying how fiscal policy responds to the business cycle is important for understanding and explaining historical episodes. It can also help inform the policymaking process. For example, if policymakers think that discretionary policy should be countercyclical, it is useful to understand whether and to what extent it has been in the past. Understanding how federal discretionary fiscal policy in particular responds to economic conditions is important for a number of reasons. First, discretionary policy allows for greater flexibility in the types of policy that can be pursued and may be easier to change than automatic policy, which requires changes to existing legislation on highly politicized programs like Medicaid and TANF (commonly referred to as welfare). In other words, changes to discretionary policy may be less prone to political gridlock and can be used to enact temporary policy tailored to specific economic conditions. Federal fiscal policy also has advantages over subnational fiscal policy. Since the federal government can accumulate debt, it is not constrained in the same way that states with balanced budget requirements are. It has the additional benefit of being able to coordinate policy across states.

The purpose of this paper is to determine whether and how federal discretionary fiscal policy responds to the business cycle in the United States. Motivated by discrepancies in the existing literature, I approach the issue of model uncertainty from a Bayesian perspective, treating the set of covariates that belong in the underlying model as a parameter to be estimated. Using Bayesian techniques I calculate posterior probabilities for each of a large set of models, where each model is defined by the included covariates. These posterior probabilities indicate the probability that a particular model is the underlying model that generated the data. I then average coefficient posteriors across the entire set of models using posterior model probabilities as weights. This procedure, known as Bayesian model averaging

[4]See Orphanides and van Norden (2002) and Aruoba (2008) for more about the magnitude of data revisions.
[5]See, for example, Forni and Momigliano (2004), Golinelli and Momigliano (2009), and Egert (2010).

(BMA), produces results that are not conditioned on any particular model and that, because they are weighted by posterior model probabilities, reflect uncertainty about the underlying model. It also produces inclusion probabilities that, in the current context, enable me to determine which variables matter to policymakers. In other words, BMA provides a formal statistical framework that explicitly accounts for the type of model uncertainty that appears to be widespread in this literature.

The set of covariates that I consider contains six measures of the business cycle as well as other control variables common in the literature. My inclusion of employment-based measures of the business cycle, the unemployment gap and the change in the unemployment rate, is novel. The number of models that I estimate is substantial, $2^{16} = 65{,}536$ for my initial results and $2^{33} = 8{,}589{,}934{,}592$ when I consider asymmetric responses to expansions and recessions. The computational requirements of estimating such a large number of models necessitates the use of Markov Chain Monte Carlo techniques developed by Madigan and York (1995).

My results suggest that federal discretionary policy in the United States is countercyclical: the portion of the deficit determined by discretionary policy actions increases in response to poor economic conditions. This response appears to be driven primarily by changes in taxes, although I find some evidence that spending exhibits a similar, albeit smaller, response. Distinguishing between expansions and recessions makes it clear that countercyclical responses are limited to recessions. Indeed, during expansions policy has a very low estimated probability of responding to business cycle measures in either direction. Posterior probabilities indicate that policy is much more likely to respond to employment-based measures of the business cycle than output-based measures, particularly the change in the unemployment rate. This is a striking result given the ubiquity of output-based measures elsewhere in the literature. Also notable is my finding that policy is unlikely to respond to the level of publicly-held debt. A comparison of my results with those obtained from more traditional fiscal policy models suggests that the importance of debt may be overstated in models that do not account for model uncertainty.

In contrast with Auerbach (2002 and 2003) and Cohen and Follette (2003), I find little evidence of a shift in the responsiveness of policy to the business cycle during the course of my fifty-year sample. The posterior probability that a structural break occurred in my model coefficients is just .48%. Finally, and again in contrast with Cohen and Follette (2003), replacing ex post data with real-time data reveals that intended responses to the business cycle are very similar to the responses that actually occur. In the latter instance BMA proves to be useful in identifying the source of these different findings.

The rest of the paper proceeds as follows: section 2 discusses traditional models of fiscal policy conduct and the implementation of Bayesian model averaging. Section 3 discusses data and section 4 presents my results. Section 5 concludes.

# 2. Methodology

*2.1 Traditional Models of Discretionary Policy Conduct*

Models of discretionary fiscal policy conduct usually take the form

$$CAD_t = \alpha + \beta_1 CAD_{t-1} + \beta_2 output_t + \beta_3 debt_{t-1} + \beta_4 Z_t + e_t \tag{1}$$

where $CAD_t$ is the so-called cyclically-adjusted deficit, *output* is usually the output gap or GDP growth, and $Z_t$ represents other possible control variables.

The cyclically-adjusted deficit measures what the deficit would be if the economy were at full employment and is constructed to eliminate the effects of automatic stabilizers on the deficit.[6] As a result, it should reflect discretionary fiscal policy actions. I use cyclically-adjusted net federal government savings as my measure of the cyclically-adjusted deficit.[7]

[6] Automatic stabilizers are defined as automatic changes in government revenues and expenditures that occur in response to the business cycle.

[7] For ease of interpretation, I reversed the sign of this variable so that a positive value indicates a deficit and a negative value indicates a surplus.

This measure, published quarterly by the Congressional Budget Office (CBO), is a translation of the U.S. federal budget into National Income and Product Accounts (NIPA) terms and therefore differs slightly from federal deficits and surpluses.[8] It has been used previously to study discretionary policy in the United States and is similar to measures constructed by the OECD and IMF to study European policy.[9] For simplicity I will refer to cyclically-adjusted net federal government savings as the cyclically-adjusted deficit for the remainder of the paper.

Equation (1) implies that discretionary fiscal policy responds to output within a period. Since most of the cyclicality literature uses annual data, this is a plausible assumption. However, since I use quarterly data the possibility of policy lags is much more likely. It is well-known that fiscal policy is prone to a number of lags that may prevent it from responding immediately to economic conditions. For example, noisy data may prevent policymakers from recognizing that policy is needed, and the sometimes contentious nature of the policymaking process may delay responses even once the need to respond is established. Indeed, the assumption that discretionary policy does not respond to output within a quarter is used by much of the fiscal multiplier literature to identify fiscal shocks.[10] As a result, I make the same assumption in this paper, using the first lag of each business cycle measure in each model I estimate.

I use three different measures of the output gap, real GDP growth, the unemployment gap, and the change in the unemployment rate as business cycle measures. Control variables are similar to those found elsewhere in the literature. They include publicly-held debt, the federal funds rate, inflation, political dummy variables, three lags of the dependent variable,

[8]Differences result from coverage adjustments (certain transactions are included in one framework but not the other), timing differences (some transactions are recorded on an accrual basis in the NIPAs but a cash basis in the federal budget), and differences in the categorization of transactions (some transactions count as negative taxes in one framework and positive spending in another). For more information see “NIPA Translation of the Fiscal Year 2017 Federal Budget” at https://www.bea.gov/scb/pdf/2016/04%20April/0416_nipa_translation_of_the_2017_federal_budget.pdf.

[9]See Auerbach (2002,2003) and Cohen and Follette (2003) for its use to study U.S. policy and Golinelli and Momigliano (2009) for a list of papers that use it to study European policy.

[10]See Blanchard and Perotti (2002) and Auerbach and Gorodnichenko (2012), among others.

and a time trend. These variables are discussed in further detail in section 3.

*2.2 Bayesian Model Averaging*

I consider j=1, ..., J linear regression models in which the cyclically-adjusted deficit is regressed on an intercept and a subset $k_j$ of $K$ possible explanatory variables. Formally, I estimate

$$CAD = \alpha\iota_T + X_j\beta_j + \epsilon \tag{2}$$

where $CAD$ is a $T \times 1$ vector holding observations of the cyclically-adjusted deficit, $\iota_T$ is a $T \times 1$ vector of ones, $X_j \in X$ is a $T \times k_j$ matrix containing the regressors in model $j$, and $\epsilon$ is assumed to be N($0_T$,$h^{-1}$ $\mathrm{I}_T$). I assume that uncertainty about the underlying model of fiscal policy conduct extends only to which covariates belong in the model. As a result, each model is defined by the included regressors, $X_j$. As suggested in Fernandez, Ley and Steel (2001b), each of the variables in X is de-meaned to ensure that the intercept, $\alpha$, has the same interpretation in each model.

The Bayesian approach to model comparison involves the estimation of posterior model probabilities, which indicate the probability that a given model is the true model. The posterior model probability for model $M_j$ is calculated as

$$\Pr(M_j|Y) = \frac{p(Y|M_j)\Pr(M_j)}{\sum_{i=1}^{J} p(Y|M_i)\Pr(M_i)} \tag{3}$$

where $\Pr(M_j)$ is the prior for model j and $p(Y|M_j)$ is the marginal likelihood. The marginal likelihood is the expected value of the likelihood function where the expectation is taken with respect to the prior for the model's parameters. It can be interpreted as the average fit of a particular model over the prior parameter values.

Once posterior model probabilities are estimated, posterior distributions for objects of interest (e.g. slope coefficients) can be obtained. One option is to focus on results from the model with the highest posterior probability. However, this approach is problematic if there are multiple plausible models that produce different or even conflicting results. Another option is to average results across all possible models using posterior model probabilities as weights. This procedure is known as Bayesian model averaging (BMA). Using BMA, the posterior distribution for some object of interest, $\lambda$, is calculated as

$$p(\lambda|Y) = \sum_{j=1}^{J} p(\lambda|Y, M_j) \Pr(M_j|Y) \tag{4}$$

BMA is beneficial for a number of reasons. First, estimates incorporate results from many possible models which, as mentioned above, reduces the likelihood that inferences are driven by model choice. In fact, as equation (4) makes clear, estimates are not conditioned on any particular model. Additionally, BMA estimates reflect uncertainty about the underlying model since results from each model are weighted by the associated posterior model probability. Estimates based on a single model, in contrast, are calculated under the potentially implausible assumption that the model they come from has a 100% posterior probability. Finally, BMA generates inclusion probabilities that indicate the likelihood that a particular variable belongs in the underlying model. In the current context, inclusion probabilities are useful in determining which variables matter to policymakers.

When the total number of models is small, BMA can be implemented using the following steps:

1. Calculate posterior statistics (e.g. coefficient posterior means) for all models.

2. Calculate posterior model probabilities, as in equation (3). When models are given equal prior odds, $\Pr(M_j)$ and $\Pr(M_i)$ drop out of the equation.

3. Average posterior statistics using posterior model probabilities as weights, as in equation (4).

In practice, implementing steps 1-3 above is computationally burdensome and potentially infeasible when the number of possible models is large. In that case, one can sample from the model space using the Markov Chain Monte Carlo Model Composition ($MC^3$) algorithm of Madigan and York (1995). $MC^3$ produces random draws from a Markov chain whose stationary distribution is the distribution defined by the posterior model probabilities.

Starting with an initial model, $M^{s-1}$, $MC^3$ proceeds in the following steps:

1. Propose a new model, $M^*$. This can be done using a symmetric proposal distribution that adds or deletes a single variable from the previous model. The use of a symmetric proposal distribution simplifies the acceptance probability in step 2.

2. Calculate the acceptance probability as

$$\alpha(M^{s-1}, M^*) = min\left[\frac{p(Y|M^*)\Pr(M^*)}{p(Y|M^{s-1})\Pr(M^{s-1})}, 1\right] \tag{5}$$

   With equal prior model odds, (5) simplifies to the ratio of marginal likelihoods for each model.

3. Accept $M^*$ as the new draw, $M^s$, with probability $\alpha$ and reject it with probability $(1-\alpha)$. If $M^*$ is rejected, the algorithm remains at $M^{s-1}$, in which case $M^s = M^{s-1}$

4. Calculate posterior statistics of interest for $M^s$.

5. Return to step 1 and repeat until convergence.

Once a sufficient number of draws are obtained, $\Pr(M_j|Y)$ can be calculated as the fraction of total draws for which model $M_j$ is selected.

Intuitively, $MC^3$ identifies a subset of models with relatively high posterior probability, which reduces the number of models under consideration. At each iteration a model is proposed and accepted based on how well it fits the data (as measured by its marginal likelihood). Consequently models with good explanatory power are drawn more frequently than those with poor explanatory power. In fact, models with very low posterior probability may not be drawn at all, in which case their estimated posterior probability will be zero. Since these models have such low posterior probability this approximation should have a negligible effect on estimated posterior probabilities for drawn models.

I implement BMA using $MC^3$ since doing so analytically would take a great deal of time at current computing speeds. For example, suppose a single model can be estimated in one-hundredth of a second. At that rate, estimating my largest specification, for which the total number of models is $2^{33}$ = 8,589,934,592, would take nearly 500 days to complete. However, as a robustness check I also calculated analytical results for my non-asymmetry model, for which the total number of models is a more manageable $2^{16}$ = 65,536. The analytical results are virtually identical to those obtained using $MC^3$, suggesting that my use of $MC^3$ is appropriate.

When I use $MC^3$, I assume that 250,000 draws are sufficient for the Markov chain to converge and use an additional 1,000,000 draws for inference. I also checked for convergence in a number of ways. First, I increased the number of burn-in draws to 1,000,000, which had no discernible effect on my results. Next, I initialized $MC^3$ using two very different models, one with no covariates and one with every possible covariate. Again, my results were unaffected by the initial model. Finally, I calculated posterior model probabilities analytically for the subset of models visited by the algorithm (posterior probabilities were set to 0 for models that were not drawn) and compared them with posterior probabilities obtained using $MC^3$ for the same set of models. The correlation between analytical and numerical results for visited models is greater than 0.99, as recommended in Fernandez, Ley, and Steel (2001b).

*Priors*

To implement BMA, prior density functions are required for all models and their parameters. I assume that each possible covariate enters the true model independently of all other covariates with probability $\theta$, suggesting a model prior of the form

$$\Pr(M_j) = \theta^{k_j}(1-\theta)^{K-k_j} \tag{6}$$

Setting $\theta = 0.5$, a common choice in the BMA literature, implies equal prior probability across all possible models, so that

$$\Pr(M_j) = \frac{1}{J}, \;\; j = 1, \cdots, J \tag{7}$$

Although (7) places equal prior weight on all models, the same is not true of model *size.* Instead, models with few or many regressors receive lower prior probability than models of moderate size. This is evident by noting that equation (6) implies a prior distribution for model size, $W$, of the form

$$W \sim \text{Binomial}(K, \theta) \tag{8}$$

This distribution is centered on $K\theta$, so the use of a uniform model prior ($\theta = 0.5$) means that models with $\frac{K}{2}$ variables receive the most prior probability while those with 1 or K variables receive the least.

As an alternative to (7), Ley and Steel (2009) suggest using a hierarchical prior, making $\theta$ random instead of fixing it at a particular value. Specifically,

$$\theta \sim \text{Beta}(a, b) \tag{9}$$

Ley and Steel recommend setting hyperparameter $a = 1$ and using a prior mean model size, $m = \frac{a}{a+b}K$, to elicit hyperparameter $b$. Setting $b = 1$ results in a uniform prior for model size,

$$\Pr(W = w) = \frac{1}{K+1} \quad \text{for } w = 0, \cdots, K \tag{10}$$

while setting $b > 1$ places greater prior probability on smaller models than larger models. Once the prior for model size is obtained, the model prior is calculated as

$$\Pr(M_j) = \frac{\Gamma(a+b)}{\Gamma(a)\Gamma(b)} \frac{\Gamma(a+k_j)\Gamma(b+K-k_j)}{\Gamma(a+b+K)} \tag{11}$$

For my main results I use (11) and set $b = 1$, although I demonstrate that these results are robust to the use of alternative model priors.

Turning to model parameters, I require a prior density function, $p(\alpha, \beta_j, h|M_j)$, for each set of parameters. I use the "benchmark" prior recommended in Fernandez, Ley, and Steel (2001) for use when, as in the current paper, there is uncertainty about the covariates in a normal linear regression model. The prior involves the use of improper non-informative priors for parameters that appear in all models ($\alpha$ and $h$) and informative priors for those that do not ($\beta_j$). Specifically,

$$p(h) \propto h^{-1} \tag{12}$$

$$p(\alpha) \propto 1 \tag{13}$$

$$\beta_j|h \sim N(\underline{\beta}_j, h^{-1}(\underline{g}X_j'X_j)^{-1}) \tag{14}$$

where $(\underline{g}X_j'X_j)^{-1}$ is the g-prior of Zellner (1986). I set $\underline{\beta}_j = 0_{k_j}$ and $\underline{g} = 1/\max\{T, K^2\}$, again as recommended in Fernandez, Ley, and Steel (2001). This prior is useful because it limits the choice of hyperparameters to one, $\underline{g}$, which is chosen in an automatic fashion. In addition to requiring little subjective information from the researcher, the authors find that it has little influence on posterior inference. Finally, it reduces the computational burden of implementing $MC^3$ since analytical results are available for $p(\beta_j|Y)$ and $\Pr(M_j|Y)$.

Table 2: Data Sources

| Variable | Source |
|---|---|
| Cyclically-adjusted net federal government savings | BEA (via CBO) |
| Real GDP | FRED |
| Real Output Gap | CBO (via FRED) |
| Nominal Potential GDP | CBO (via FRED) |
| Unemployment Rate | FRED |
| Natural Rate of Unemployment | FRED |
| Publicly-Held Federal Debt | FRED |
| Federal Funds Rate | FRED |
| CPI | FRED |
| GDP Deflator | FRED |
| Presidential Election Dummy | Wikipedia |
| United Government Dummy | Wikipedia |
| Recession Dummy | NBER |

Notes: BEA is the Bureau of Economic Analysis, CBO is the Congressional Budget Office, FRED is the Federal Reserve Economic Database, and NBER is the National Bureau of Economic Research.

## 3. Data

I use quarterly data covering the period 1966q1-2016q3. Table 2 lists the source of each of the data series used to construct my final variables. I use three different measures of the output gap. One measure comes from the CBO and I estimate the other two myself. The first (which I call the trend break output gap) is estimated using a linear time trend and a break in the trend after 1973 to allow for a slowdown in GDP growth.[11] The other is estimated using the filter proposed in Hamilton (2017).[12]

The cyclically-adjusted deficit and debt are expressed as percentages of potential GDP, as measured by the CBO.[13] An augmented Dickey-Fuller test indicates that the cyclically-adjusted deficit is stationary so I include it and its lags in levels.[14] Both inflation variables

[11] See Orphanides and van Norden (2002) for further explanation.
[12] The Hamilton filter is an alternative to the HP filter discussed in Hamilton (2017).
[13] Results are unchanged when these variables are expressed as percentages of the other two potential output measures that I estimate.
[14] First-differencing the cyclically-adjusted deficit changes the estimated coefficients on the policy lags but otherwise has no effect on results.

are calculated as quarterly growth in the corresponding price indices and annualized. Real GDP growth is also calculated as quarterly growth and annualized. Finally, the "presidential election" dummy variable takes on a value of one in all quarters during an election year while the "united government" dummy variable takes on a value of one for quarters in which the White House, Senate, and House of Representatives were all controlled by the same political party.

# 4. Results

*4.1 Baseline Results*

Inclusion probabilities, presented in Table 3, make it clear that discretionary policy responds to the business cycle: the posterior probability that at least one of the business cycle measures belongs in the underlying model of fiscal policy is 99.8%. Among the business cycle measures, the change in the unemployment rate receives far greater posterior probability, 99.1%, than any of the others. This result is striking since, to my knowledge, I am the first in this literature to consider employment-based measures of the business cycle. In contrast, GDP growth and the output gap, the measures most commonly employed in the literature, together receive just 22.7% posterior probability. Lastly, the unemployment gap receives 6.5% posterior probability, suggesting that policymakers care more about the direction of the unemployment rate than its level. This means, for example, that discretionary policy is less likely to respond to a high and stable unemployment rate than to a low unemployment rate that is increasing. The contraction in policy that occurred in 2013, when the unemployment rate remained elevated even after three years of steady decreases, is consistent with this finding.

Among the other variables, only the first two policy lags receive posterior probabilities greater than 50%. The high posterior probabilities received by the first and second lag, 100% and 94.8%, respectively, indicate that discretionary policy is persistent. Considering that most fiscal policy actions are determined as part of the budget negotiation process, this

Table 3: BMA Inclusion Probabilities

| Variable | Probability |
|---|---|
| CA Deficit First Lag | **100%** |
| CA Deficit Second Lag | **94.8%** |
| CA Deficit Third Lag | 3.5% |
| Time Trend | 3.3% |
| Output Gap (CBO) | 10.4% |
| Output Gap (TB) | 4.9% |
| Output Gap (Hamilton) | 5.7% |
| GDP Growth | 2.8% |
| Unemployment Gap | 6.5% |
| Unemployment Change | **99.1%** |
| Debt | 4.2% |
| Federal Funds Rate | 4.8% |
| CPI Inflation | 27.0% |
| GDP Deflator Inflation | 6.6% |
| Presidential Election | 3.6% |
| United Government | 3.7% |
| Total Output Gap | 19.9% |
| Total Inflation | 29.3% |

Notes: results for variables with inclusion probabilities greater than 50% are bolded.

finding is intuitive. It seems likely that the starting point for each year's budget is the budget from the previous year rather than a blank slate.

Finally, it is worth noting that the inclusion probability for the level of debt is just 4.2%. This is surprising given that Bohn (1998), Auerbach (2002 and 2003), and Cohen and Follette (2003) all find that U.S. fiscal policy responds to debt. Differences in findings may be driven in part by the use of a different dependent variable, in the case of Bohn (1998), or the use of a different debt measure, in the case of Auerbach (2002 and 2003) and Cohen and Follette (2003). However, they may also be due to the fact that these authors consider a single model instead of averaging results across many possible models as I do. As I demonstrate below, the use of a single model can result in significant estimated responses to this variable.

Table 4 and Figure 1 present information about coefficient posterior distributions, which measure short-run policy responses. Table 4 lists posterior means averaged across all possible

Table 4: BMA Posterior Means

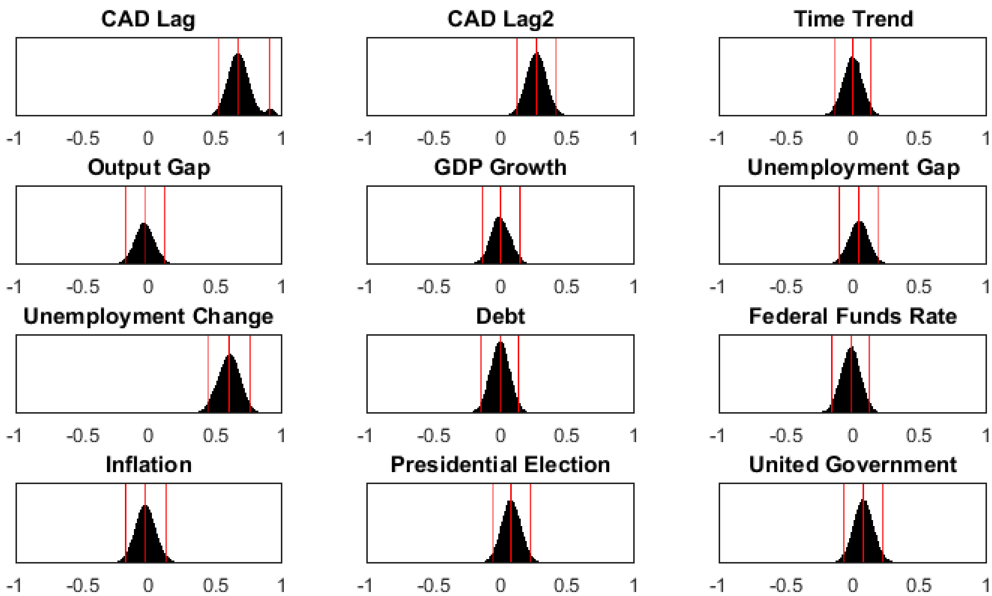
| **Variable** | **Posterior Mean (% potential)** | **Posterior Mean (dollars)** | **Posterior Mean (% deficit)** |
|---|---|---|---|
| Intercept | **2.85** | **537** | **91.3** |
| CA Deficit First Lag | **0.68** | **128** | **21.8** |
| CA Deficit Second Lag | **0.25** | **48** | **8.1** |
| CA Deficit Third Lag | 0.00 | 0 | 0.1 |
| Time Trend | 0.00 | 0 | 0.0 |
| Total Output Gap | -0.01 | -1 | -0.2 |
| GDP Growth | 0.00 | 0 | 0.0 |
| Unemployment Gap | 0.00 | 1 | 0.1 |
| Unemployment Change | **0.60** | **112** | **19.1** |
| Debt | 0.00 | 0 | 0.0 |
| Federal Funds Rate | 0.00 | 0 | 0.0 |
| Total Inflation | -0.01 | -1 | -0.2 |
| Presidential Election | 0.00 | 1 | 0.1 |
| United Government | 0.00 | 1 | 0.1 |

Notes: column 1 includes posterior means measured as a percentage of potential output. Column 2 converts the numbers in column 1 to billions of 2016q3 dollars. Column 3 converts the numbers in column 2 to a percentage of the 2016q3 deficit. Results for variables with inclusion probabilities greater than 50% are bolded.

models using posterior model probabilities as weights. For these results, I include values of zero that are assigned to coefficients whose corresponding variables are excluded from some models. In contrast, Figure 1 presents histograms for coefficient posterior distributions conditional on inclusion in the model. For these results I exclude values of zero for variables that do not appear in some models. In Table 4, posterior means are expressed in a number of different ways to ease interpretation. The first column lists posterior means as I estimate them, as a percentage of potential output. The second column converts the numbers in column one to billions of 2016q3 dollars to better convey their magnitude. Finally, the third column expresses the numbers in column two as a percentage of the 2016q3 cyclically-adjusted deficit to put these magnitudes in context.

Together, Table 4 and Figure 1 indicate that discretionary policy is countercyclical: the coefficient posterior mean for the change in the unemployment rate is positive, conditional

Figure 1: Coefficient Posterior Distributions, Conditional on Inclusion




Notes: for variables with inclusion probabilities less than 100% the large point mass at zero is ignored. Red lines indicate 5%, 50%, and 95% percentiles. Results for the third policy lag are omitted.

or unconditional on inclusion in the model. This means that an acceleration in the unemployment rate leads to larger deficits. The magnitude of this response is large. For example, assuming the unemployment rate has been stable, a one percentage point increase in this variable is predicted to increase the cyclically-adjusted deficit by about \$111 billion dollars (using 2016q3 prices), which was nearly 19% of the total deficit in 2016q3.

It is informative to compare the results from my model with those that would be obtained from a more traditional cyclicality model like equation (1). To that end, Table 5 presents OLS estimates for individual models alongside my BMA estimates. For the OLS estimates, each model includes a single cylical variable as well as my non-cyclical covariates.[15] Bolded coefficients indicate a p-value of less than 0.1 (for columns 1-6) or an inclusion probability greater than 90% (for the BMA column).

[15]I exclude GDP deflator inflation because I do not want to include two inflation variables in an OLS regression and CPI inflation appears to explain the data better than GDP deflator inflation.

Table 5: Results from BMA and OLS

| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **BMA** |
|---|---|---|---|---|---|---|---|
| CA Deficit First Lag | **0.69** | **0.69** | **0.67** | **0.74** | **0.70** | **0.64** | **0.68** |
| CA Deficit Second Lag | **0.20** | **0.21** | **0.21** | **0.22** | **0.20** | **0.27** | **0.25** |
| CA Deficit Third Lag | 0.00 | -0.01 | 0.03 | -0.01 | -0.01 | 0.05 | 0.00 |
| Time Trend | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| Output Gap (CBO) | **-0.08** | | | | | | -0.01 |
| Output Gap (TB) | | **-0.04** | | | | | 0.00 |
| Output Gap (Hamilton) | | | **-0.05** | | | | 0.00 |
| GDP Growth | | | | **-0.05** | | | 0.00 |
| Unemployment Gap | | | | | **0.09** | | 0.00 |
| Unemployment Change | | | | | | **0.65** | **0.60** |
| Debt | 0.00 | **-0.02** | -0.01 | **-0.01** | -0.01 | -0.01 | 0.00 |
| Federal Funds Rate | -0.01 | -0.02 | 0.00 | -0.02 | -0.01 | -0.01 | 0.00 |
| CPI Inflation | -0.03 | **-0.04** | **-0.04** | **-0.04** | -0.03 | -0.04 | -0.01 |
| Presidential Election | 0.07 | 0.06 | 0.06 | 0.07 | 0.06 | 0.09 | 0.00 |
| United Government | 0.11 | 0.13 | 0.10 | 0.03 | 0.06 | 0.05 | 0.00 |

Notes: each of columns (1)-(6) lists OLS results from a single model, where each model differs only by which business cycle measure it uses. The last column lists my BMA results. Coefficients with p-values less than 0.1 or inclusion probabilities greater than 90% are bolded.

Table 5 demonstrates that the importance of debt and inflation may be overstated in a single model that does not account for model uncertainty. This is particularly evident in columns (2) and (4), where coefficients for these variables have p-values of less than 0.1. I find, in contrast, that policymakers are unlikely to respond to either variable: the inclusion probability for CPI inflation is 27% while the inclusion probability for debt is just 4.2%. Similarly, the estimated responses to these variables are often larger in magnitude when a single model is used since BMA shrinks coefficients on variables with low estimated probability of being in the model towards zero.

In sum, my results indicate that discretionary policy in the United States is countercyclical. I find little evidence to support the idea that policymakers respond to output-based measures of the business cycle, an assumption made, to my knowledge, by all of the existing literature. Instead, policy is far more likely to respond to the change in the unemployment rate. Posterior probabilities also indicate that policy is persistent and unlikely to respond to the other covariates that I consider. Finally, accounting for model uncertainty greatly reduces the estimated influence of debt and inflation on policy outcomes.

*4.2 Model Prior Robustness*

The model prior employed in the previous section assigns equal prior probability to different model sizes but unequal prior probability to individual models. It is possible, then, that the results presented earlier are driven by the choice of model prior. To address this possibility, I examine the sensitivity of my results to the use of different model priors. Specifically, I consider a uniform prior across all models, achieved by setting $\theta = 0.5$ in equation (6), as well as a prior that places greater probability on smaller models than larger models, achieved by setting $b > 1$ in equation (11). In fact, when $b > 1$ the prior mode for model size is equal to zero. I considered a range of values for $b$ but only present results for $b = 15$.[16]

Table 6 lists inclusion probabilities and averaged coefficient posterior means using three different priors: $b = 1$ (the original model prior), $\theta = 0.5$, and $b = 15$. Unsurprisingly, the average model size increases when greater prior probability is placed on larger models, and decreases when greater prior probability is placed on smaller models. This is due primarily to differences in inclusion probabilities for variables that are considered unlikely under the initial model prior. Inclusion probabilities for these variables are roughly twice as large when $\theta = 0.5$ and about half as large when $b = 15$. Since inclusion probabilities for these variables are so small under the initial model prior, even large proportional changes have little effect on overall conclusions. In contrast, inclusion probabilities are mostly unchanged for variables with high inclusion probabilities under the initial model prior. As a result, slope coefficient posterior means are also similar across model priors. It appears, then, that the basic conclusions from the previous section hold regardless of which model prior is used.

[16]Results for other values of $b > 1$ yield similar conclusions to $b = 15$, which I chose because it implies a prior mean model size of one.

Table 6: Inclusion Probabilities and Posterior Means for Alternate Model Priors

| Variable | Probability ($b = 1$) | Probability ($\theta = 0.5$) | Probability ($b = 15$) |
|---|---|---|---|
| CA Deficit First Lag | **0.68** | **0.66** | **0.70** |
| | **(100%)** | **(100%)** | **(100%)** |
| CA Deficit Second Lag | **0.25** | **0.26** | **0.24** |
| | **(94.8%)** | **(98.0%)** | **(88.6%)** |
| Time Trend | 0.00 | 0.00 | 0.00 |
| | (3.3%) | (9.9%) | (1.2%) |
| Total Output Gap | -0.01 | -0.01 | 0.00 |
| | (19.9%) | (38.2%) | (10.7%) |
| GDP Growth | 0.00 | 0.00 | 0.00 |
| | (2.8%) | (6.3%) | (1.5%) |
| Unemployment Gap | 0.00 | 0.01 | 0.00 |
| | (6.55%) | (13.0%) | (3.3%) |
| Unemployment Change | **0.60** | **0.60** | **0.58** |
| | **(99.1%)** | **(99.85%)** | **(97.7%)** |
| Debt | 0.00 | 0.00 | 0.00 |
| | (4.2%) | (12.0%) | (1.4%) |
| Federal Funds Rate | 0.00 | 0.00 | 0.00 |
| | (4.8%) | (11.2%) | (2.2%) |
| Total Inflation | -0.01 | **-0.01** | 0.00 |
| | (29.3%) | **(55.8%)** | (13.8%) |
| Presidential Election | 0.00 | 0.01 | 0.00 |
| | (3.6%) | (8.8%) | (1.4%) |
| United Government | 0.00 | 0.01 | 0.00 |
| | (3.7%) | (9.55%) | (1.4%) |
| **Average Model Size** | 3.8 | 4.9 | 3.3 |

Notes: average model size excludes the constant that appears in every model. Coefficient estimates are on the same line as the corresponding variable name while inclusion probabilities are in parentheses. Variables with inclusion probabilities greater than 50% are bolded.

*4.3 Business Cycle Asymmetry*

It is possible that policy responds differently to economic conditions in expansions and recessions, for reasons outlined in the introduction. Cohen and Follette (2003) consider asymmetric responses to the business cycle in the United States by estimating responses to positive and negative output gaps, which are included as two separate variables in their model. They find that responses to negative output gaps are significant and countercyclical while responses to positive output gaps are not significant, suggesting that discretionary policy is more likely to respond to poor economic conditions than to good economic conditions.[17]

I approach business cycle asymmetry in a slightly different manner. Instead of using positive and negative values of my business cycle measures to measure the strength of the economy, I use a lagged recession indicator that is equal to 1 if the economy was in a recession the previous period.[18] In contrast with Cohen and Follette (2003), I also allow responses to all covariates to differ between expansions and recessions instead of just the business cycle measures. To construct my asymmetry specification I interact the constant and each covariate with the lagged recession indicator, include the 17 interaction terms (which I call recessionary variables) alongside the uninteracted variables (which I call expansionary variables), and estimate the specification using BMA.

Table 7 presents inclusion probabilities and posterior means for the asymmetry specification alongside my initial results. Together, they make it clear that the discretionary response to business cycle measures estimated in the previous section is driven by large responses to the change in the unemployment rate during recessions. Inclusion probabilities for expansionary business cycle measures are all less than 5%, even for the change in the unemployment rate. Correspondingly, coefficient posterior means for these variables are small in magnitude although conditional on inclusion all but GDP growth are indicative of countercyclical policy. Thus it appears that during expansions policymakers feel little need to respond to economic conditions. The only variables to receive posterior probabilities greater than 5% are the first and second lags of the cyclically-adjusted deficit. The posterior means for these variables are relatively large at 0.76 and 0.17, indicating a great deal of persistence

[17] Because they define the output gap in the opposite way that I do, subtracting actual output from potential output, they actually estimate a significant and countercyclical response to *positive* output gaps, which they define as potential output exceeding actual output.

[18] Using a lagged recession indicator is consistent with my assumption that discretionary policy does not respond to economic conditions within a quarter.

Table 7: Asymmetry Specification Inclusion Probabilities and Posterior Means

| **Variable** | **Baseline** | **Expansion** | **Recession** |
|---|---|---|---|
| CA Deficit First Lag | **0.68** | **0.76** | 0.73 |
| | **(100%)** | **(100%)** | (15.1%) |
| CA Deficit Second Lag | **0.25** | **0.17** | 0.15 |
| | **94.8%** | **(72.3%)** | (11.2%) |
| Total Output Gap | -0.01 | 0.00 | 0.00 |
| | (19.9%) | (1.9%) | (2.3%) |
| GDP Growth | 0.00 | 0.00 | 0.00 |
| | (2.8%) | (0.5%) | (0.6%) |
| Unemployment Gap | 0.00 | 0.00 | 0.00 |
| | (6.5%) | (0.6%) | (0.7%) |
| Unemployment Change | **0.60** | 0.00 | **1.16** |
| | **(99.1%)** | (0.7%) | **(99.8%)** |
| Debt | 0.00 | 0.00 | 0.00 |
| | (4.2%) | (0.5%) | (1.1%) |
| Federal Funds Rate | 0.00 | 0.00 | 0.00 |
| | (4.8%) | (1.1%) | (6.8%) |
| Total Inflation | -0.01 | 0.00 | -0.01 |
| | (29.3%) | (5.3%) | (11.9%) |
| Presidential Election | 0.00 | 0.00 | 0.00 |
| | (3.6%) | (0.7%) | (0.5%) |
| United Government | 0.00 | 0.00 | 0.00 |
| | (3.7%) | (0.6%) | (0.5%) |

Notes: coefficient estimates are on the same line as the corresponding variable name while inclusion probabilities are in parentheses. Results are bolded for variables with inclusion probabilities greater than 50%. Results for the third deficit lag and time trend are omitted.

in policy.

During recessions, in contrast, policymakers have a very high probability of responding to the change in the unemployment rate. The inclusion probability for this variable increases from 0.7% during an expansion to 99.8% during a recession and its posterior mean, 1.16, is nearly twice as large as what I estimate for the non-asymmetry specification. It implies that, assuming a previously constant unemployment rate, a one percentage point increase in the unemployment rate during a recession causes a countercyclical response equal to about 220 billion dollars (in 2016q3 prices) or slightly less than 40% of the 2016q3 cyclically-adjusted deficit. This estimate appears to be plausible. For example, the change in the unemployment rate increased from 0.3% to 0.7% between 2008q3 and 2008q4 which, using my estimate, implies a 87.8 billion dollar increase in the cyclically-adjusted deficit (in 2016q3 dollars). The following quarter, the cyclically-adjusted deficit increased by 282 billion dollars.

### *4.4 Changes in Policy Over Time*

A number of papers in the monetary policy literature have estimated changes in policy conduct over time, concluding that the Federal Reserve has responded more aggressively to changes in inflation since the mid-1980s.[19] Similarly, changes in political ideologies and priorities may have altered the conduct of fiscal policy over the past fifty years. Auerbach (2002 and 2003) and Cohen and Follette (2003) find evidence to support this, concluding that discretionary policy has responded more strongly to the output gap since 1993, a date chosen to coincide with the beginning of the Clinton administration. However, these authors do not test whether the change in policy responsiveness is significant, instead basing their conclusions on differences in coefficients estimated using different sample periods.

I consider changes in policy by searching for evidence of a structural break in the coefficients of my non-asymmetry specification, allowing for uncertainty across a number of dimensions.[20] Bayesian model comparison techniques outlined in section 2 can be used to compare "no break" models of the form

$$CAD_t = \alpha + X_t\beta + \epsilon_t \tag{15}$$

with structural break models of the form

$$CAD_t = \alpha + X_t\beta_1 + \gamma D_t + (X_tD_t)\beta_2 + \epsilon_t \tag{16}$$

[19] See, for example, Taylor (1999b) and Stock and Watson (2002).

[20] I do not consider the asymmetry model since the number of recession observations is greatly reduced when the sample is split in two. This means, for example, that a break date during the last decade of the sample would cause post-break policy to be identified solely off of the fiscal policy response to the Great Recession, an unusually severe and prolonged recession.

where $D_t$ takes on a value of zero before a particular break date and a value of one during and after the break date. In this setup there is uncertainty about the covariates in the model ($X_t \in X$), the existence of a structural break (whether equation (15) or (16) is the appropriate model type), and the date in which such a break may have occurred ($D_t$). I assume that if a variable appears in a particular structural break model, its coefficient is allowed to break. This reduces the model space, enabling me to compute analytical results.

As in earlier sections, inclusion probabilities can be obtained for individual covariates. Posterior probabilities can also be calculated for the existence and location of a structural break. Mathematically, the posterior probability that a structural break exists is

$$\Pr(\text{“break”}|Y) \propto p(Y|\text{“break”}) \Pr(\text{“break”}) \tag{17}$$

where

$$p(Y|\text{“break”}) = \sum_{X_t \in X} \sum_{D_t} p(Y|X_t, D_t, \text{“break”}) \Pr(X_t|D_t, \text{“break”}) \Pr(D_t|\text{“break”}) \tag{18}$$

is the marginal likelihood averaged across the set of structural break models using priors $\Pr(X_t|D_t, \text{“break”})$ and $\Pr(D_t|\text{“break”})$ as weights, and where $\Pr(\text{“break”})$ is the prior probability that a structural break exists. Posterior probabilities for different break dates are calculated in a similar manner,

$$\Pr(D_t|\text{“break”}, Y) \propto p(Y|D_t, \text{“break”}) \Pr(D_t|\text{“break”}) \tag{19}$$

where $\Pr(D_t|\text{“break”})$ is the prior probability that a particular break date is the true break date.

Since I have no prior beliefs about the existence and location of a structural break, the date range I test for a break as well as the prior probabilities I use in (17)-(19) reflect that. Following the recommendation in Andrews (1993), I use a 15% trimming value for a date

Table 8: Prior and Posterior Probabilities for the Existence of a Structural Break

| Pr("break") | Pr("break"$\vert Y$) |
|---|---|
| 50% | 0.48% |
| 75% | 1.42% |
| 90% | 4.13% |
| 99% | 32.1% |
| 99.9% | 82.7% |

Notes: The first column displays different priors for the existence of a break date, Pr("break"), while the second column displays the corresponding posteriors, Pr("break"$|Y$).

range of (1973q2,2009q1). I set Pr("break"), the prior probability that a break exists, equal to 0.5 and Pr($D_t|$"break"), the prior probability that the break occurred at a particular date, equal to $\frac{1}{c}$ where $c = 144$ is the total number of dates under consideration. I use the same model prior, Pr($X_t|D_t$, "break"), that I use to obtain my baseline results.[21]

In contrast with previous studies, I find little evidence that a shift in policy conduct occurred sometime during the past fifty years. The posterior probability that a break occurred, Pr("break"$|Y$), is .48%. Similarly, the posterior odds ratio,

$$\frac{\Pr(\text{``no break''}|Y)}{\Pr(\text{``break''}|Y)} \tag{20}$$

indicates that the possibility that a break did not occur is 209 times more likely than the possibility that one did. This result is hard to overturn, requiring prior probabilities that overwhelmingly favor the existence of a break. As Table 8 demonstrates, Pr("break") must be set to more than 99% to produce a posterior probability greater than 50%.

### *4.5 Spending and Tax Responses*

Thus far, policy has been measured as the portion of the deficit resulting from discretionary policy actions. While this measure has the advantage of summarizing overall policy, it does not distinguish between taxes and spending. Consequently it is useful to separately

[21] Results are unchanged when I use the alternate model priors discussed in section 4.2 instead.

Table 9: BMA Results for Cyclically-Adjusted Tax Revenues and Outlays

| **Variable** | **Revenues** | **Outlays** |
|---|---|---|
| Dep. Var. First Lag | **0.62** | **0.84** |
| | **(100%)** | **(100%)** |
| Dep. Var. Second Lag | **0.26** | 0.02 |
| | **(96.7%)** | (10.1%) |
| Time Trend | 0.00 | 0.00 |
| | (2.2%) | (1.8%) |
| Total Output Gap | 0.00 | **-0.01** |
| | (9.1%) | **(55.2%)** |
| GDP Growth | 0.00 | 0.00 |
| | (2.5%) | (4.9%) |
| Unemployment Gap | 0.00 | 0.00 |
| | (3.4%) | (9.3%) |
| Unemployment Change | **-0.44** | 0.01 |
| | **(99.5%)** | (4.6%) |
| Debt | 0.00 | 0.00 |
| | (11.7%) | (1.8%) |
| Federal Funds Rate | 0.00 | 0.00 |
| | (12.8%) | (1.7%) |
| Total Inflation | 0.00 | 0.00 |
| | (13.3%) | (4.6%) |
| Presidential Election | 0.00 | 0.00 |
| | (1.8%) | (3.2%) |
| United Government | 0.00 | 0.00 |
| | (1.9%) | (3.6%) |

Notes: coefficient estimates are on the same line as the corresponding variable name while inclusion probabilities are in parentheses. Results are bolded for variables with inclusion probabilities greater than 50%. Results for the third policy lag are omitted.

estimate tax and spending responses to determine whether Congress has relied more heavily on one fiscal lever than the other in responding to the business cycle. For the results below I replace the cyclically-adjusted deficit and its lags in equation (2) with the relevant fiscal measure.

My results, presented in Table 9, indicate that earlier results are driven primarily by taxes. Inclusion probabilities for the tax specification closely resemble those in section 4.1, suggesting that taxes have a high probability of responding to business cycle measures, particularly the change in the unemployment rate. The coefficient posterior mean for this variable is large and negative, indicating a countercyclical response. In contrast, the spending response to the business cycle appears to be much more modest. The probability that any one of the six business cycle measures belongs in the underlying spending model is 67.5% and, as Figure 2 makes clear, coefficient posterior distributions for these variables are centered on or near zero even conditional on being included in the model.

It is surprising that spending appears unlikely to respond to economic conditions given that unemployment benefit extensions and grants to states, both of which are included in cyclically-adjusted outlays, have been part of the federal response to many recent recessions.[22] It is therefore worth considering how results change when variations of the spending measure are used. As defined, cyclically-adjusted outlays include spending on defense and interest payments, neither of which are likely to be adjusted in response to the state of the economy. Instead, defense spending should depend primarily on U.S. military operations while interest payments are predetermined. Together, spending on defense and interest comprises nearly 40% of total cyclically-adjusted outlays over my fifty-year sample, which may make it harder to detect an empirical relationship between spending and business cycle measures even if one exists.

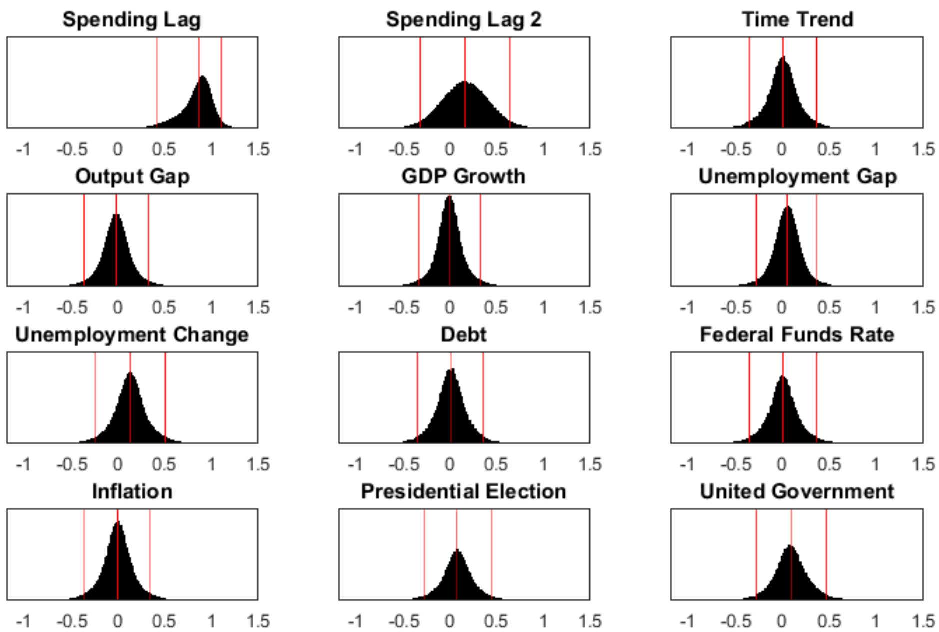
Cyclically-adjusted outlays also exclude two categories of spending commonly associated with fiscal stimulus legislation: gross government investment and capital transfer payments. The former includes direct federal spending on structures such as schools and highways, while the latter includes grants to state and local governments for additional transportation infrastructure.[23] It is worth noting that government investment and capital transfer payments, which include spending on "shovel-ready projects," together accounted for just over 14% of the total expenditures included in the American Recovery and Reinvestment Act.[24]

[22] Unemployment benefit extensions are not counted as automatic stabilizers because they require legislation to be enacted.

[23] See "Concepts and Methods of the U.S. National Income and Product Accounts" at https://www.bea.gov/national/pdf/allchapters.pdf for further explanation.

[24] See "Effect of the ARRA on Selected Federal Government Sector Transactions" at https://www.bea.gov/recovery/pdf/arra-table.pdf.

Figure 2: Coefficient Posterior Distributions (Spending Specification)




Notes: for variables with inclusion probabilities less than 100% the large point mass at zero is ignored. Red lines indicate 5%, 50%, and 95% percentiles. Results for the third policy lag are omitted.

However, it is possible that they have played a larger role in government responses to other economic events over the past fifty years.

Column 1 of Table 10 presents inclusion probabilities and averaged coefficient estimates for the unadjusted spending variable alongside results for two variations of this measure. The first, whose corresponding results are listed in column 2, excludes spending on defense and interest payments. The second, whose results are listed in column 3, adds gross government investment and capital transfer payments to the measure in column 2. Table 10 shows that alternate definitions of discretionary spending provide stronger evidence that policymakers adjust spending in response to the business cycle. The inclusion probability for the change in the unemployment rate increases from 4.6% to 66.4% when spending on defense and interest payments is excluded, and increases further to 85.1% when gross investment and capital transfers are added. Similarly, the probability that any of the business cycle measures

Table 10: BMA Results for Alternate Spending Variables

| **Variable** | **(1)** | **(2)** | **(3)** |
|---|---|---|---|
| Outlays First Lag | **0.84** | **0.70** | **0.56** |
| | **(100%)** | **(100%)** | **(100%)** |
| Outlays Second Lag | 0.02 | 0.04 | **0.40** |
| | (10.1%) | (19.2%) | **(100%)** |
| Total Output Gap | **-0.01** | -0.01 | 0.00 |
| | **(55.2%)** | (40.2%) | (13.5%) |
| GDP Growth | 0.00 | 0.00 | 0.00 |
| | (4.9%) | (3.7%) | (5.9%) |
| Unemployment Gap | 0.00 | 0.00 | 0.00 |
| | (9.3%) | (7.9%) | (2.6%) |
| Unemployment Change | 0.01 | **0.17** | **0.23** |
| | (4.6%) | **(66.4%)** | **(85.1%)** |

Notes: column (1) lists results for the unadjusted spending variable. Column (2) removes spending on defense and interest payments from (1) and column (3) adds gross investment and capital transfers to (2). Coefficient estimates are on the same line as the corresponding variable name while inclusion probabilities are in parentheses. Results are bolded for variables with inclusion probabilities greater than 50%.

belongs in the model is about 95% for both alternate spending definitions. Coefficient posteriors suggest that spending responses are countercyclical. The coefficient posterior mean for the change in the unemployment rate is much larger in magnitude than for the unadjusted spending variable, albeit smaller than the response estimated for the tax specification. In sum, it appears that policymakers respond to the business cycle with a combination of tax and spending changes, although taxes tend to make up a larger portion of the response.

*4.6 Intended Policy*

Since Orphanides (2001), it has become common to assess monetary and fiscal policy conduct using both real-time and ex post data. ex post data is useful for estimating actual policy outcomes but it may do a poor job of explaining intended policy if the information available to policymakers in real time differs substantially from fully revised data. In that case, intended policy responses can be estimated by replacing ex post data with real-time data. Using real-time data, studies of fiscal policy in European and OECD countries have often concluded that intended policy is more countercyclical than actual policy. Cohen and Follette (2003), in contrast, find that in the United States intended policy is less countercyclical than actual policy. They estimate a countercyclical response to the output gap using both ex post and real-time data, but find that the magnitude of the response is about half as large when real-time data is used.

In line with the existing literature, I estimate intended responses to the business cycle by replacing ex post data for business cycle measures with real-time data and re-estimating equation (2). Real-time data for each variable is available from the Archival Federal Reserve Economic Data (ALFRED) database. Because of data availability, I am forced to make a number of alterations to my dataset. First, real-time data for the CBO output gap and unemployment gap are available only for recent years so I exclude both as possible covariates. This reduces the total number of business cycle measures to four: two measures of the output gap, GDP growth, and the change in the unemployment rate.[25] Second, my real-time dataset begins in 1968q4 instead of 1966q1, again because of data availability.[26] Fortunately, results using ex post data for the smaller set of covariates and shorter sample, listed in column 1 of Table 11, are very similar to those presented in earlier sections, suggesting that such alterations are inconsequential.

My results indicate that there is little difference between intended and actual policy: estimated responses to business cycle measures, shown in column 2 of Table 11, are largely unchanged when real-time data is used. The estimated response to the change in the unemployment rate falls from 0.58 to 0.56, a difference of 3.5%, while the response to GDP growth is close to zero using both ex post and real-time data. The estimated response to the output gap changes by a large amount proportionally, from -0.01 to 0.00, but the magnitude of this coefficient is small enough relative to the coefficient on the change in the unemployment rate that it has little bearing on overall conclusions.

[25]Real-time data is available for potential output at an annual frequency from 1991 and biannual frequency from 1999 and is available for the natural rate of unemployment from 2011.

[26]Real-time data for GDP is available from 1965 but the estimation of the Hamilton output gap, which involves regressing $y_{t+h}$ on $y_t, \cdots, y_{t-3}$ causes me to lose observations for the first three years.

Table 11: BMA Results for Real-Time Business Cycle Measures

| **Variable** | **Ex Post** | **Real Time** |
|---|---|---|
| CA Deficit First Lag | **0.68** | **0.69** |
| | **(100%)** | **(100%)** |
| CA Deficit Second Lag | **0.25** | **0.25** |
| | **(94.1%)** | **(93.7%)** |
| Time Trend | 0.00 | 0.00 |
| | (4.7%) | (4.8%) |
| Total Output Gap | -0.01 | 0.00 |
| | (24.6%) | (10.0%) |
| GDP Growth | 0.00 | 0.00 |
| | (3.9%) | (5.9%) |
| Unemployment Change | **0.58** | **0.56** |
| | **(96.0%)** | **(97.9%)** |
| Debt | 0.00 | 0.00 |
| | (6.4%) | (7.5%) |
| Federal Funds Rate | 0.00 | 0.00 |
| | (6.2%) | (5.8%) |
| Total Inflation | -0.01 | -0.01 |
| | (37.2%) | (44.1%) |
| Presidential Election | 0.01 | 0.01 |
| | (6.3%) | (6.3%) |
| United Government | 0.00 | 0.00 |
| | (5.1%) | (4.9%) |

Notes: coefficient estimates are on the same line as the corresponding variable name while inclusion probabilities are in parentheses. Results are bolded for variables with inclusion probabilities greater than 50%.

It is worth noting that, although small, the differences in results that I estimate using ex post and real-time data are in line with the finding by Cohen and Follette (2003) that intended policy is less countercyclical than actual policy. For both the change in the unemployment rate and the output gap, the sign of the coefficient posterior mean is unaffected when real-time data is used but the magnitude is smaller. It is possible that Cohen and Follette overestimate the magnitude of policy differences because they assume that policymakers respond to the output gap, a variable that I estimate to have a low probability of belonging in the underlying model. Re-estimating equation (2), first-differencing the dependent variable as in Cohen and Follette, confirms this. Conditional on inclusion in the model, the coefficient posterior mean for the output gap falls from -0.05 when ex post data is used to -0.02 when real-time data is used. This result is very similar to the values of -0.04 and -0.02 estimated by Cohen and Follette.

The output gap may produce larger differences in estimated policy responses because it is less accurate in real-time than the change in the unemployment rate. The information presented in Table 12 supports this idea. The first column, intended to convey average revision size for each business cycle measure, lists the average magnitude of revisions to each variable as a percentage of the average magnitude of the corresponding variable, measured using ex post data. The second column, intended to convey the frequency with which policymakers have had access to accurate real-time data, lists the percentage of observations for which the magnitude of a variable's revision is smaller than five percent of the variable's average magnitude, again measured using ex post data. Table 12 indicates that real-time data for the change in the unemployment rate has been more accurate than real-time data for the other business cycle measures. Policymakers have had access to accurate real-time data for this variable more often than for other business cycle measures, and when real-time data has been inaccurate, revisions have tended to be smaller in magnitude than revisions to other variables.

In sum, the use of the output gap may yield inaccurate conclusions about differences between actual and intended policy. Posterior probabilities indicate that policymakers are much less likely to respond to the output gap, a variable prone to relatively large and frequent inaccuracies in real time, than to the change in the unemployment rate, a variable that is more accurate in real time. Accounting for model uncertainty and in contrast with Cohen and Follette (2003), I find little difference between intended policy and actual policy outcomes in the United States.

Table 12: Business Cycle Measure Revision Summary

| **Variable** | **Average Revision Size** | **RT Accuracy Frequency** |
|---|---|---|
| Output Gap (TB) | 89.6% | 1.6% |
| Output Gap (Hamilton) | 43.5% | 6.3% |
| GDP Growth | 42.8% | 6.4% |
| Unemployment Change | 35.6% | 24.5% |

Notes: "Average Revision Size" is calculated as the average revision magnitude as a percentage of the average ex post variable magnitude while "RT Accuracy Frequency" is calculated as the percentage of observations for which the revision magnitude is less than five percent of the ex post variable magnitude.

# 5. Conclusion

The conduct of discretionary fiscal policy has been the subject of a large number of papers. Nevertheless, these papers have failed to yield a consensus on even the most basic aspects of policy, such as whether and how it responds to the business cycle. Conflicting results may stem in part from model uncertainty, particularly uncertainty about which covariates belong in the underlying model of fiscal policy. Motivated by discrepancies in the existing literature, I estimate the response of U.S. policy to different business cycle measures using a Bayesian approach that explicitly incorporates model uncertainty. My results indicate that policy responds to business cycle measures, particularly the change in the unemployment rate, in a countercyclical manner. These countercyclical responses are driven by large responses to recessions. During expansions, in contrast, policy shows little indication of responding to economic conditions. Policymakers appear to rely more heavily on tax cuts than spending increases during recessions, although I find evidence that both change in response to the change in the unemployment rate. Finally, I find no evidence of a structural break in my model coefficients, nor of substantive differences between intended policy and actual policy outcomes.

Although my focus in this paper is on responses to the business cycle, my finding that debt has little effect on discretionary fiscal policy relates to a large and growing literature on fiscal and monetary policy switching. In that literature there exist two fiscal policy regimes, active and passive, where the regimes are defined by whether or not fiscal policy stabilizes debt. My results suggest that if fiscal policy can be described as operating under two regimes, those regimes may be better defined by whether the economy is in a recession or an expansion, with the terms "active" and "passive" referring to whether or not fiscal policy responds to business cycle measure.

The conduct of federal discretionary fiscal policy is an important topic in and of itself. However, any analysis of fiscal policy is incomplete as long as it excludes non-discretionary and subnational policy. Both types of policy operate under very different conditions from federal discretionary policy and as such likely respond to the economy in very different ways. Applying the techniques employed in this paper to either type of policy is an obvious avenue for future research.

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