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The Impact of Inflation and Unemployment on Subjective Personal and Country Evaluations

Néstor Gandelman and Rubén Hernández-Murillo

The authors use data from the Gallup World Poll to analyze what determines individual assessments of past, present, and future personal and country well-being. These measures allow the analysis of two dimensions of happiness data not previously examined in the literature: the *better-than-average* effect and *optimism*. The authors find that individuals tend to evaluate their personal well-being as being better than their country's and tend to expect that their future well-being will improve. The authors also analyze the impact of inflation and unemployment on these subjective measures and find that both variables have a negative effect on individuals' assessments of past and present well-being for themselves and their country; in contrast with other studies, however, they do not find that the effect of unemployment is significantly different from that of inflation. (JEL D60, I30, E31, E24, Z13)

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Measuring the impact of economic policies on personal well-being is at the heart of most applied research. Traditionally, economists have been reluctant to use self-reports of well-being—or happiness—for policy evaluation because of their subjective nature. Instead, economists prefer to infer individual preferences from observed choices and evaluate the impact of policies with these choices and derived preferences. As Di Tella and MacCulloch (2006, p. 25) state, "economists typically watch what people do, rather than listening to what people say."

Easterlin's (1974) seminal paper is the first to seriously make use of self-reported happiness data. In this study, he documented that although happiness responses are positively correlated with individual income at a given point in time, self-reports of happiness in the United States had remained stagnant while average personal income increased over time. This pattern, often called the "Easterlin paradox," has also been observed in other countries (Veenhoven, 1993; Blanchflower and Oswald, 2004). One of the most favored explanations for this apparent puzzle in the literature is that individuals' happiness is determined by their income relative to other people's income; that is, they derive happiness not only from the levels of consumption attained with income, but also from their position in the income (and consumption) distribution relative to other members of their communities (Easterlin 1974, 1995, and more recently Di Tella and MacCulloch, 2006).

Although Easterlin's study was noted by some scholars when it was published, it took time for academics to engage in subjective data research to a substantial degree. Since the late 1990s the amount of research making use of happiness and satisfaction databases has increased considerably (see Frey and Stutzer, 2002, for a recent review of

Néstor Gandelman is a professor of economics at Universidad ORT Uruguay, and Rubén Hernández-Murillo is a senior economist at the Federal Reserve Bank of St. Louis. The authors thank the Inter-American Development Bank (IADB) and the Gallup Organization for facilitating access to the Gallup World Poll. This paper was written as background material for the Inter-American Development Bank 2009 Economic and Social Progress Report in Latin America. Christopher Martinek provided research assistance.

the literature). To measure the different concepts of *satisfaction*, *well-being*, and *happiness*, social scientists use nationally representative household surveys. For instance, past research has used the British Household Panel Survey (BHPS), the American General Social Survey (GSS), the German Socio-Economic Panel (GSOEP), Eurobarometer, Latinobarometro, the European Community Household Panel (ECHP), the Russia Longitudinal Monitoring Survey (RLMS), the International Social Survey Programme (ISSP), and the World Values Survey (WVS).

This paper uses the 2006 Gallup World Poll dataset. This newly designed survey contains information about an individual's assessment of her or his current, past, and future personal well-being and assessments of her or his country's current, past, and future well-being. With this novel dataset we do three things: (i) We examine the determinants and the effects of inflation and unemployment on past, present, and future individual assessments of personal and country well-being. (Previous studies have analyzed only personal assessments of current well-being.) (ii) We examine two aspects of happiness data that have not been previously addressed in the literature: First, comparing personal and country evaluations, we test for the *better-than-average* effect discussed in the cognitive psychology literature; then we construct measures of personal and country *optimism* comparing future and present evaluations and examine their determinants. Finally, (iii) we report some contrasting findings on the indirect preferences for inflation and unemployment implied in happiness reports.

The *better-than-average* effect refers to the tendency to overestimate one's personal traits or abilities (e.g., overrating one's own looks or the ability to drive; Caliendo and Huang, 2007). The better-than-average effect has been linked in the finance and economics literature to apparently irrational behavior because individuals are thought to exhibit an unrealistic, or overconfident, image of themselves. Camerer and Lovallo (1999), for example, study whether overconfidence leads to excess entry by firms. Caliendo and Huang (2007) argue that overconfidence about the average return on savings can have large effects in the work-life consumption profile in a life cycle model. In the finance literature, Chuang and Lee (2006), among others, have studied overconfidence linking investors' behavior to apparently anomalous phenomena. They argue that overconfident investors underestimate risk, trade in riskier securities, overreact to private information, underreact to public information, and trade more aggressively in subsequent periods after observing market gains. Benoît, Dubra, and Moore (2009) and Benoît and Dubra (2009), on the other hand, dispute the traditional interpretation that the better-than-average effect is a sign of irrational (overconfident) behavior. We do not concern ourselves in this paper with the relation between the better-than-average effect and the possibility of overconfidence. Rather, we consider that if individuals tend to describe their own well-being as better than the average—that is, better than their country's—the determinants of subjective well-being reports on personal and country evaluations do not need to be the same.

Most research using data from the Gallup World Poll (which began in 2005) has been published in the *Gallup Management Journal*. Because of copyright issues, the use of this database has been very restricted. To the best of our knowledge, only two papers in the economics literature have used these data: Deaton (2008), in a study of the effect of national income, age, and life expectancy on assessments of health satisfaction and general satisfaction with life; and Stevenson and Wolfers (2008), in a study on reassessing the Easterlin paradox.[1] Using data for many countries and over many years, Stevenson and Wolfers established, in contrast with Easterlin (1974), a positive link between average levels of subjective well-being and gross domestic product (GDP) per capita across countries; they also found evidence that, within countries, economic growth is associated with increasing happiness.

In addition to the connection with personal income, data on self-reports of well-being have

[1] Recently the Inter-American Development Bank published the book *Beyond Facts: Understanding Quality of Life* (Lora, 2008), which uses this database extensively to specifically describe Latin American and Caribbean countries.

also been used to analyze other implications of public policy. Clark and Oswald (1994), for example, used data from the BHPS to assess the utility levels of the unemployed. They find that unemployed people in Great Britain in 1991 had much lower levels of well-being than employed individuals. Recent studies on the economics of happiness include a paper by van Praag, Frijters, and Ferrer-i-Carbonell (2003) that analyzes the determination of an individual's self-reports of satisfaction with several aspects of life and how these combine into self-reports of general satisfaction with life. The authors used the GSOEP in their analysis. Scoppa and Ponzo (2008) analyze the determinants of individual subjective well-being in Italy. These authors used data from the Survey of Household Income and Wealth conducted by the Bank of Italy. Among other findings, they also report that individuals care about relative income. Several other applications of happiness data are discussed in the survey by Di Tella and MacCulloch (2006).

The literature has also analyzed the effect of macroeconomic variables on self-reports of life satisfaction and the implied preferences over inflation and unemployment. Di Tella, MacCulloch, and Oswald (2001), for example, analyzed whether the one-to-one marginal rate of substitution implied by the so-called misery index (the sum of the unemployment rate and the inflation rate) is validated in self-reports of happiness data.[2] In a different study, Di Tella, MacCulloch, and Oswald (2003) analyzed the impact of macroeconomic variables (including GDP per capita levels and growth in addition to inflation and unemployment) on happiness reports; they also examined the psychological cost of recessions (in excess of the fall of GDP and the rise in unemployment) implied by the happiness reports. The authors used data from Eurobarometer for 12 European countries between 1975 and 1995 and from the American GSS for the period 1972-94. They find that—in contrast to the common assumption—at the margin, unemployment seems to cause more unhappiness than inflation and conclude that the misery index underestimates the welfare cost of unemployment. Blanchflower (2007), using data from the World Database of Happiness for 25 Organisation for Economic Co-operation and Development (OECD) countries for 1973-2006, finds results consistent with those of Di Tella, MacCulloch, and Oswald (2001). In a related study, Wolfers (2003) finds evidence that inflation and unemployment lower perceived well-being and that macroeconomic volatility, especially unemployment volatility, also undermines well-being. Jayadev (2008), using data from the 1996 ISSP for 27 countries, studied the preferences of different socioeconomic classes over inflation and unemployment. The author found that the "working class," defined as those with lower occupational skills and economic status, is more likely to rank minimizing unemployment as a higher priority than maintaining low inflation. Easterly and Fischer (2001), using a 1995 survey of 38 countries (19 developed and 19 developing and transition countries), portray a different picture. They report that the poor are more likely than the rich to mention inflation as a top national priority. Lastly, Peiró (2006) also explores different microeconomic and macroeconomic determinants of happiness.

This paper is organized as follows. In the next two sections we present the data and describe the estimation strategy. We then present our study results, followed by our conclusions.

DATA

The source for the personal and country evaluation is the Gallup World Poll. The Gallup World Poll is probably the world's most comprehensive database of behavioral economic measures. It surveys citizens in more than 140 countries, representing about 95 percent of the world's adult population. Our dataset contains responses from about 70,000 individuals in 75 countries for the year 2006.

In the research on the economics of happiness, the happiness measure, the key variable of analysis, is often constructed with the answer to a question; and the question is typically worded

[2] The misery index was introduced by Arthur Okun in the 1960s as a straightforward indicator of the social costs of inflation and unemployment (Lovell and Tien, 2000).

in one of two ways: "*Taking all things together, would you say you are: very happy, quite happy, not very happy, or not at all happy?*" or "*All things considered, how satisfied are you with your life as a whole?*" The possible answers for the latter range from "very dissatisfied" to "very satisfied," also with four or five possible responses.

The question on *personal assessment* in the Gallup World Poll is an example of the second form, in which the responses use a ladder analogy. The question is "*Please imagine a ladder/mountain with steps numbered from zero at the bottom to ten at the top. Suppose we say that the top of the ladder/mountain represents the best possible life for you and the bottom of the ladder/mountain represents the worst possible life for you. If the top step is ten and the bottom step is zero, on which step of the ladder/mountain do you feel you personally stand at the present time?*"

The Gallup World Poll includes additional questions on the individual's status 5 years ago: "*On which step of the ladder/mountain would you say you stood 5 years ago?*" And it includes expectations for the future: "*Just your best guess, on which step do you think you will stand on in the future, say 5 years from now?*"

The questions on *country assessment* in the Gallup World Poll are almost identical to the questions on one's personal situation: "*Once again, imagine a ladder with steps numbered from zero at the bottom to ten at the top. Suppose the top of the ladder represents the best possible situation for (name of country) and the bottom represents the worst possible situation. Please tell me the number of the step on which you think (name of country) stands at the present time.*" The survey also includes an assessment of the country's past situation: "*What is the number of the step on which you think (name of country) stood about 5 years ago?*" and the expected future: "*And just your best guess, if things go pretty much as you now expect, what is the number of the step on which you think (name of country) will stand about 5 years from now?*"

The measures of personal and country well-being in our dataset therefore range from zero to ten. We also constructed two additional variables to capture "optimism" regarding personal and country assessments. The optimism variables are defined as the difference between the answer to the "future" and "present" questions. These variables, of course, range from –10 to 10.

The Gallup World Poll has many individual-level variables that can be used as controls in the estimations, including the sex, age, marital status, employment status, location of residence (urban versus rural characteristics) of the respondent, and a categorical proxy for personal income.[3] We complemented the survey data with country measures from the World Development Indicators database on inflation and unemployment for the period 2002-05 (World Bank, 2007).

Table A1 of the appendix lists the countries in our sample, along with the averages for the dependent variables used in the analysis and their macroeconomic indicators for 2005.

ECONOMIC STRATEGY

Our estimation strategy is a variation of the methodology used by Di Tella, MacCulloch, and Oswald (2001). Our study differs from theirs in that they study a small cross section of European countries over several years, whereas we examine a larger set of countries from different regions of the world for only one year. More importantly, Di Tella, MacCulloch, and Oswald examine only life satisfaction as the dependent variable, and we analyze eight measures of happiness as dependent variables: current life satisfaction, past life satisfaction (5 years ago), expected future life satisfaction (in 5 years), life satisfaction optimism (defined as future minus present satisfaction), current country situation, past country situation (5 years ago), expected future situation (in 5 years), and country satisfaction optimism (defined as future minus present country situation).

In our estimations, we follow two basic approaches. The first approach is similar to the two-step procedure used by Di Tella, MacCulloch, and Oswald (2001). In the first step, we run an ordinary least squares (OLS) regression of each

[3] Additional variables not used in the current study include importance of religion, number of children, characteristics of current housing, and others.

variable of interest against a set of individual characteristics identified by the literature as affecting happiness and satisfaction levels.[4] We include country fixed effects and cluster the standard errors by country. The average residuals of this first regression for each country (including the estimated fixed effect) can then be interpreted as the average assessment of personal or country satisfaction that is not explained by individual characteristics. The second step of this approach then runs an OLS regression of the country-level averages of the residuals on inflation and unemployment. The estimated coefficients in the second stage are then interpreted as in Di Tella, MacCulloch, and Oswald (2001) as the impact on well-being from a 1-percentage-point change in either inflation or unemployment. (This interpretation clearly depends on the cardinal scale of the dependent variable.)

The first-step regression is then

$$Y_{ij} = \beta_0 + \beta_j + \beta_1 Male_{ij} + \beta_2 Age_{ij} + \beta_3 Age^2_{ij} + u_{ij}, \tag{1}$$

where i indexes individual respondents and j indexes countries. We control for country fixed effects, β_j.[5]

We define the unexplained part of dependent variable Y for each country j in regression (1) as the estimated fixed effect $\hat{\beta}_j$.[6] We then run the following regression at the country level in the second step:

$$\hat{\beta}_j = \alpha_0 + \alpha_1 Inflation_j + \alpha_2 Unemployment_j + v_j. \tag{2}$$

The second approach, which we undertake for comparison, consists of running a unique regression with variables measured both at the individual and at the country level. The basic estimation model is as follows:

$$Y_{ij} = \beta_0 + \beta_1 Male_{ij} + \beta_2 Age_{ij} + \beta_3 Age^2_{ij} + \alpha_1 Inflation_j + \alpha_2 Unemployment_j + u_{ij}. \tag{3}$$

Because this last regression already includes inflation and unemployment (which do not vary for individuals within a given country), we cannot include country fixed effects. In this approach, we also cluster the standard errors by country. For a robustness check, the appendix presents the results for these two approaches using a larger number of individual controls.

RESULTS

Summary Statistics

Table 1 presents the summary statistics of the measures of personal and country well-being used in our analysis. A one-way analysis of variance illustrates that although most of the variation is *within* countries (ranging between 70 and 90 percent of the entire variation, as represented by the total sum of squares), there is a sizable variation *between* countries. The averages of the well-being measures are better illustrated in Figure 1. The solid line represents the averages for past, present, and future personal assessments of life satisfaction. The dotted line represents the assessments for the country's situation. Three patterns are worth noting.

First, the line corresponding to personal evaluations is always above the line corresponding to the assessments of the country's situation. In other words, individuals tend to assess their personal situation as better than that of their country. We interpret the systematic differences between personal and country evaluations as a manifestation of the better-than-average effect discussed previously. The size of this effect is not small: The differences between individual life satisfaction and country situation range between 8 and 13 percent. As shown in Table 2, the differences are statistically significant, as evidenced by a standard t-test of differences in means.

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4 Di Tella, MacCulloch, and Oswald (2001) also used an OLS regression in the first stage. They report that an ordered probit model in the first stage yields similar conclusions, but it would require redefining the unexplained dependent variable used in the second stage. Our estimated second-stage coefficients are, therefore, comparable with theirs.

5 Introducing country fixed effects is necessary to account for any country-level bias—for example, bias from *focusing illusion*, which results when a respondent's relative position with respect to respondents from other countries affects his or her assessment of life satisfaction (Krueger, 2008).

6 In our data, this turns out to be indistinguishable from averaging the combined residual, $\hat{\beta}_j + \hat{u}_{ij}$, for each country j.

Table 1

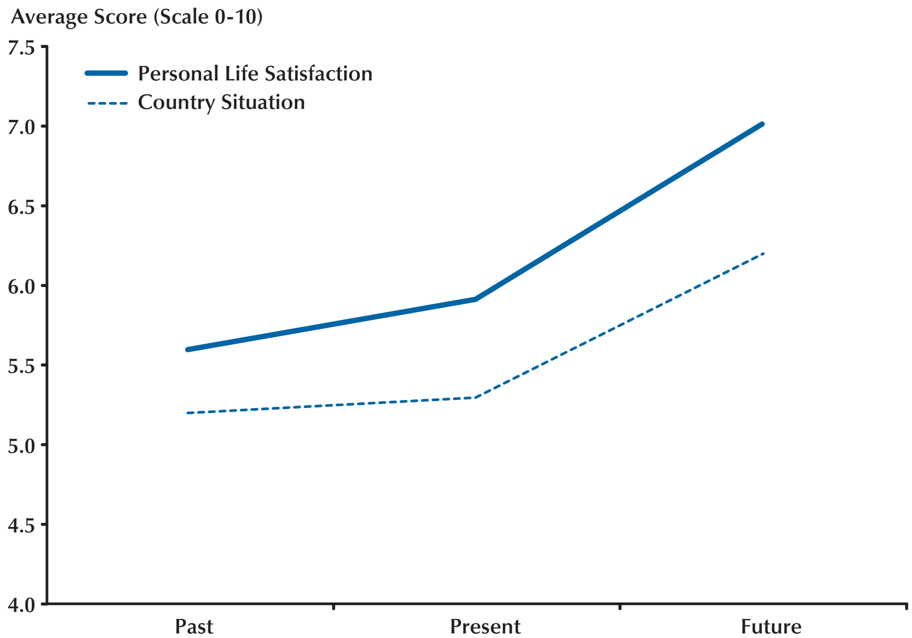
Summary Statistics Dependent Variables

	Number of countries	Number of observations	Average	Overall SD	Overall SS	Between country SS (%)	Within country SS (%)	Between country DF	Within country DF
Past life satisfaction	75	77,948	5.6	2.4	446,377.9	16.0	84.0	74	77,873
Present life satisfaction	75	78,482	5.9	2.2	394,160.2	21.2	78.8	74	78,407
Future life satisfaction	75	69,713	7.0	2.5	430,156.2	13.0	87.0	74	69,638
Past country situation	73	71,277	5.2	2.2	349,572.5	28.3	71.7	72	71,204
Present country situation	73	72,461	5.3	2.1	317,762.8	23.2	76.8	72	72,388
Future country situation	73	65,461	6.2	2.5	403,945.9	14.0	86.0	72	65,388
Personal optimism	75	69,574	1.0	2.0	273,393.8	9.5	90.5	74	69,499
Country optimism	73	65,193	0.9	1.9	244,974.1	13.1	86.9	72	65,120

NOTE: SD, standard deviation; SS, sum of squares; DF, degrees of freedom.

Figure 1

Happiness Reports



Second, there is a temporal tendency of improvements in both the assessments of personal and country well-being, as illustrated by the upward slope of both reports in Figure 1. Individual assessments of the future are better than assessments of the present, and assessments of the present are better than assessments of the past. We interpret these patterns as suggesting optimism in the well-being reports. As shown in Table 2, both personal and country measures of optimism that compare future and present assessments are also statistically significant.

Third, the rate of change between future and present and between present and past evaluations is larger in the personal assessments than in the country assessments. This indicates that people expect their personal well-being, on average, to improve more than the country's well-being will, again suggesting the presence of the better-than-average effect. Table 2 indicates that the differences between personal and country optimism (defined as comparing future and present assessments) are also statistically significant.

Table 3 reports summary statistics for the two macroeconomic variables studied in this paper. Data availability for these two variables determined the 75 countries that could be included in our estimations.[7] To mitigate year-to-year variation, inflation and unemployment measures were computed as the average between 2004 and 2005. We also computed the lags of inflation and unemployment as the average between 2002 and 2003 to check the robustness of our estimations.

Table 4 portrays the mean and standard deviation for the individual control variables. "Male" is a dummy variable taking the value 1 for males and 0 for females. In our database, 43.9 percent of respondents are males. "Age" is measured in years; the average age is 42.5 years. "Married" is a dummy variable taking the value 1 for married people and 0 otherwise. "Employed" is a dummy

[7] The World Development Indicators database contained missing values of the unemployment variable for some countries for some isolated years. These values were imputed using data for 1990-2005 with a *switching regression* technique.

Table 2

t-Tests for Equality of Means of Personal and Country Assessments

	t-test	Two-sided *p*-value	Upper one-sided *p*-value	Sample size
Differences between personal and country				
Past	50.69	0.0000	0.0000	70,471
Present	78.43	0.0000	0.0000	71,935
Future	70.68	0.0000	0.0000	60,287
Differences between future and present				
Personal optimism	132.83	0.0000	0.0000	69,574
Country optimsim	112.26	0.0000	0.0000	65,193
Differences in optimism				
Personal minus country	10.12	0.0000	0.0000	60,007

NOTE: The above *t*-tests correspond to paired tests assuming equal variances. However, similar conclusions are reached if we allow for unequal variances.

Table 3

Summary Statistics Inflation and Unemployment

	Average (%)	SD (%)	Minimum (%)	Maximum (%)	Number of observations
Inflation	5.0	4.6	–0.1	27.8	75
Unemployment	9.1	5.5	1.4	37.3	75
Lag inflation	5.3	6.3	–2.8	35.1	75
Lag unemployment	9.6	5.9	1.7	34.3	75

NOTE: Inflation and unemployment are computed as the average for the period 2004-05. Lagged inflation and unemployment correspond to the averages for 2002-03. SD is standard deviation.

variable taking the value 1 when the individual has a job (whether paid or unpaid) and 0 otherwise. "Urban" is a dummy variable taking the value 1 for individuals living in cities and 0 if they live in a rural area. Finally, we include a dummy variable (labeled "Poor") that takes the value 1 if the individual has an income of *at most* two U.S. dollars per day and 0 otherwise. The analysis of variance reveals, as in Table 1, that most of the variation corresponds to *within*-country variation.

Regression Analysis

Table 5 reports the results of the two-stage approach. The top panel presents the first-stage regression of the personal and country evaluations on only individual controls, accounting for country fixed effects. The middle and bottom panels present the second-stage regression illustrating the impact of inflation and unemployment (current and lagged values, respectively) on the country averages of personal and country evaluations that are not explained by individual controls. Tables 6 and 7 report the results of the second approach of a single regression of the well-being measures on individual controls and the macro variables.

Both estimation strategies yield similar results. At the individual level, males tend to be

Table 4

Summary Statistics Independent Variables

	Number of countries	Number of observations	Average (%)	Overall SD (%)	Overall SS	Between country SS (%)	Within country SS (%)	Between country DF	Within country DF
Male	75	79,458	43.9	49.6	19,566.1	1.4	98.6	74	79,383
Age*	75	79,430	42.5	17.7	24,795,363.8	6.9	93.1	74	79,355
Married	74	73,077	53.3	49.9	18,191.6	4.0	96.0	73	73,003
Employed	73	74,139	48.8	50.0	18,524.7	5.2	94.8	72	74,066
Urban	74	78,202	49.0	50.0	19,542.0	12.6	87.4	73	78,128
Poor	68	61,028	1.3	11.4	791.5	6.8	93.2	67	60,960

NOTE: SD, standard deviation; SS, sum of squares; DF, degrees of freedom. *Age is measured in years.

Table 5

Results of the Two-Stage Approach

	Life satisfaction				Country situation			
	Present	Past	Future	Future-Present	Present	Past	Future	Future-Present
First stage								
Male	–0.09041***	–0.01379	–0.11673***	–0.01955	–0.01411	–0.10445***	–0.05914*	–0.03783*
	(–4.01)	(–0.56)	(–4.68)	(–1.28)	(–0.57)	(–5.53)	(–1.80)	(–1.99)
Age	–0.03095***	–0.02204***	–0.03609***	–0.00911**	–0.01642***	–0.00308	–0.03344***	–0.01815***
	(–7.28)	(–6.09)	(–5.16)	(–2.08)	(–5.62)	(–0.94)	(–8.93)	(–8.14)
Age^2	0.00019***	0.00028***	–0.00003	–0.00018***	0.00015***	0.00007**	0.00029***	0.00014***
	(4.16)	(6.5)	(–0.43)	(–3.78)	(5.27)	(2.06)	(7.46)	(5.58)
Constant	6.88610***	5.94741***	8.60801***	1.76106***	5.71184***	5.23426***	7.06410***	1.33646***
	(71.2)	(74.59)	(55.83)	(19.9)	(80.59)	(71.13)	(82.6)	(27.11)
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	78,449	77,914	69,679	69,541	72,429	71,245	65,429	65,161
R^2	0.016	0.002	0.079	0.053	0.002	0.002	0.005	0.003
$R^{2(*)}$	0.22417	0.16168	0.19857	0.14271	0.23345	0.28479	0.14394	0.13403
Second stage: current variables								
Inflation	–0.06536**	–0.06496**	–0.01729	0.04615***	–0.05685*	–0.09574***	0.00034	0.05619***
	(–2.09)	(–2.47)	(–0.58)	(4.01)	(–1.90)	(–3.07)	(0.01)	(4.72)
Unemployment	–0.05961***	–0.04201***	–0.04075***	0.01761**	–0.05420***	–0.06799***	–0.01501	0.03839***
	(–5.15)	(–3.55)	(–3.44)	(2.19)	(–2.90)	(–3.80)	(–0.70)	(3.86)
No. of countries	75	75	75	75	73	73	73	73
R^2	0.218	0.208	0.077	0.21	0.186	0.298	0.008	0.28
F-test for inflation = unemployment	0.03668	0.68952	0.62259	3.33055	0.00611	0.64291	0.17494	1.07965
p-Value	0.84866	0.40907	0.43268	0.07215	0.93793	0.42537	0.67704	0.30235
Second stage: lag variables								
Lag inflation	–0.02683	–0.02243	–0.00944	0.01660*	–0.02508	–0.05527***	0.00815	0.03298***
	(–1.36)	(–1.59)	(–0.45)	(1.68)	(–1.54)	(–3.80)	(0.47)	(3.55)
Lag unemployment	–0.06220***	–0.04651***	–0.03883***	0.02215**	–0.05398**	–0.07139***	–0.01211	0.04104***
	(–4.59)	(–3.62)	(–2.80)	(2.45)	(–2.61)	(–3.28)	(–0.59)	(3.91)
No. of countries	75	75	75	75	73	73	73	73
R^2	0.173	0.135	0.074	0.113	0.145	0.267	0.007	0.257
F-test for inflation = unemployment	2.2978	1.52646	1.21838	0.14166	1.069	0.34143	0.46129	0.27637
p-Value	0.13394	0.22066	0.27335	0.70774	0.30473	0.56088	0.49926	0.60075

NOTE: *t*-Statistics are in parentheses. R^2 corresponds to the between model. $R^{2(*)}$ accounts for the variation explained by the fixed effects. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table 6

Results of the Single-Stage Approach: Current Inflation and Unemployment

	Life satisfaction				Country situation			
	Present	Past	Future	Future-Present	Present	Past	Future	Future-Present
Male	–0.11486***	–0.05063	–0.12522***	–0.00558	–0.00118	–0.09736***	–0.06548	–0.05595**
	(–3.08)	(–1.41)	(–3.45)	(–0.24)	(–0.03)	(–3.16)	(–1.63)	(–2.36)
Age	–0.03312***	–0.02706***	–0.03543***	–0.00741	–0.01610***	0.00096	–0.03444***	–0.02036***
	(–4.55)	(–3.27)	(–4.61)	(–1.36)	(–3.85)	(0.2)	(–7.29)	(–6.38)
Age^2	0.00025***	0.00037***	–0.00003	–0.00024***	0.00014***	0.00004	0.00027***	0.00013***
	(3.13)	(4.09)	(–0.41)	(–4.07)	(3.52)	(0.91)	(5.55)	(3.82)
Inflation	–0.06799**	–0.06381**	–0.02603	0.04057***	–0.06020*	–0.10306***	–0.00112	0.06011***
	(–2.15)	(–2.41)	(–0.83)	(3.32)	(–1.99)	(–3.09)	(–0.04)	(4.92)
Unemployment	–0.05045***	–0.03048*	–0.03896***	0.01094	–0.05465***	–0.06762***	–0.01971	0.03641***
	(–3.36)	(–1.72)	(–3.25)	(1.12)	(–3.02)	(–3.89)	(–0.98)	(3.9)
Constant	7.63352***	6.56237***	9.05575***	1.49585***	6.49061***	6.23799***	7.33682***	0.83549***
	(36.41)	(32.81)	(42.1)	(10.3)	(27.05)	(26.39)	(31.41)	(5.83)
N	78,449	77,914	69,679	69,541	72,429	71,245	65,429	65,161
R^2	0.046	0.028	0.08	0.077	0.046	0.091	0.009	0.044
No. of countries	75	75	75	75	73	73	73	73
F-test for inf. = unemp.	0.32963	1.36935	0.17454	3.77308	0.02741	0.97235	0.28113	2.06848
p-Value	0.56762	0.24568	0.67732	0.05589	0.86898	0.3274	0.59759	0.1547

NOTE: *t*-Statistics are in parentheses. R^2 corresponds to the between model. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table 7

Results of the Single-Stage Approach: Lag Inflation and Unemployment

	Life satisfaction				Country situation			
	Present	Past	Future	Future-Present	Present	Past	Future	Future-Present
Male	–0.12150***	–0.05635	–0.12843***	–0.00255	–0.00536	–0.10516***	–0.06588	–0.05203**
	(–3.30)	(–1.57)	(–3.58)	(–0.11)	(–0.16)	(–3.35)	(–1.62)	(–2.14)
Age	–0.03184***	–0.02552***	–0.03500***	–0.00863	–0.01528***	0.00202	–0.03422***	–0.02089***
	(–4.33)	(–3.12)	(–4.53)	(–1.59)	(–3.58)	(0.41)	(–7.11)	(–6.32)
Age^2	0.00024***	0.00036***	–0.00003	–0.00023***	0.00014***	0.00004	0.00027***	0.00013***
	–3.07	(4.05)	(–0.44)	(–3.92)	(3.39)	(0.78)	(5.44)	(3.74)
Lag inflation	–0.0242	–0.01732	–0.00984	0.01173	–0.02665	–0.05935***	0.00713	0.03367***
	(–1.21)	(–1.06)	(–0.47)	(1.19)	(–1.65)	(–3.67)	(0.42)	(3.64)
Lag unemployment	–0.05350***	–0.03549**	–0.03848***	0.01503	–0.05385***	–0.07053***	–0.01601	0.03948***
	(–3.47)	(–2.13)	(–2.84)	(1.51)	(–2.70)	(–3.33)	(–0.81)	(3.87)
Constant	7.43711***	6.35424***	8.98092***	1.62238***	6.33002***	6.07736***	7.26082***	0.91666***
	(38.82)	(34.1)	(51.12)	(10.71)	(29.05)	(27.09)	(32.67)	(6.14)
N	78,449	77,914	69,679	69,541	72,429	71,245	65,429	65,161
R^2	0.034	0.017	0.078	0.071	0.035	0.081	0.008	0.041
No. of countries	75	75	75	75	73	73	73	73
F-test for inf. = unemp.	1.68022	0.88205	1.20986	0.06604	1.00789	0.15564	0.65196	0.15121
p-Value	0.19892	0.3507	0.27492	0.79791	0.31877	0.69437	0.42207	0.69853

NOTE: *t*-Statistics are in parentheses. R^2 corresponds to the between model. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

more critical of their current situation and their expectations about the future (as indicated by a negative and statistically significant coefficient of the "Male" variable on the respective regressions). Males also tend to be more critical of the past situation of the country, but we find no significant gender-based appreciation differences for the current assessment of country well-being. Males also tend to be *less optimistic* with respect to the country's future well-being; this can be seen in the significant negative coefficient in the future country well-being regression in Table 5 and in the negative significant coefficient for the expected country improvements (future-present) in Tables 5, 6, and 7.

In general we find that both age (measured in years) and its square are significant determinants of personal and country subjective evaluations, but there are quantitative differences of their effects on the different well-being measures, especially between the assessment for present and past well-being. The nonlinear nature of the estimated relationship with age implies that personal and country (past, present, and future) evaluations tend to decrease with age until a turning point in which the marginal effect of an additional year yields an improvement in the subjective evaluation. Di Tella, MacCulloch, and Oswald (2001) also find a negative sign on age and a small positive sign on age squared in their analysis of personal life satisfaction.

According to Table 5, the present life satisfaction is a decreasing function of age up to 81 years. The turning point for past life satisfaction is at 40 years. This may reflect a tendency of older people to better evaluate the past. Tables 6 and 7 present similar results. The age turning point in the life satisfaction regression is about 66 years, but in the regression for the past personal situation the turning point is about 37 years.

We have defined the measure of *optimism* as the change between the future and present situation (both with respect to the individual and country situation), and we interpret this variable as illustrating expectations about future improvements in one's own life satisfaction or future improvements in one's perceptions of the country's situation. The results indicate that the impact of age on future evaluation is more negative than in the evaluation of the past and present. According to Table 5, the turning point on future personal evaluation is above normal age spans (133 years), but the coefficient on age squared is not statistically significant. However, the regression of the personal optimism regarding life satisfaction (i.e., improvements in personal evaluation of life satisfaction) exhibits a significant negative age-squared term. This means that not only an additional year of life makes people believe that things will be worse for them in the future but also that the marginal effect of this additional year grows with age in absolute value, as life satisfaction optimism declines at increasing rates with age.

Tables A2, A3, and A4 (see appendix) reproduce the two estimation approaches with a wider set of individual explanatory variables. In line with previous research (see, for example, Di Tella, MacCulloch, and Oswald, 2001), we find that married people tend to report higher personal life satisfaction than nonmarried people. However, according to the results reported in the appendix, the impact of marriage status on the country evaluations is not robust to the estimation strategy. We also find that employed people tend to report higher assessments of personal and country well-being than unemployed people, but their assessment for the future both with respect to the individual and country situation is lower than for unemployed individuals. People living in urban centers tend to show higher individual assessment of life satisfaction than people living in rural areas, but we find no significant differential effect for the assessments of country well-being. Finally, as expected, poor people tend to assess their current, past, and future well-being as being worse than richer people, as is the case for their assessment of the country's current well-being. More interesting, poorer people expect their well-being to improve in the future more than richer people do, as implied by the positive coefficient in the optimism regressions.

EFFECTS OF INFLATION AND UNEMPLOYMENT ON LIFE SATISFACTION

Both estimation strategies (and using both sets of individual controls) report a very consistent set of results. We find that an individual's present and past assessments of personal well-being tend to be negatively affected by the country's inflation and unemployment levels. The expectations about future personal well-being are not affected by the level of inflation but are negatively affected by the level of unemployment. An individual's assessment of the country's present and past well-being is also negatively affected by current inflation and unemployment, but not the assessment of the country's future well-being.

With respect to the effects on optimism about personal satisfaction and the country's well-being, we find that individuals' optimism measures tend to respond positively to current inflation and unemployment. Rather than interpreting this result as inflation and unemployment having a boosting effect on optimism, we believe that these macroeconomic variables depress the evaluation of the present relative to the evaluation of the future. In other words, higher current inflation or unemployment creates the effect of improved optimism, not because the future is assessed as more favorable but because individuals believe the present looks grimmer.

The regression using lagged inflation and lagged unemployment values shows significant effects on the assessment of personal optimism only from unemployment in the two-stage approach (Table 5), but not in the single-stage approach (Table 7). Both inflation and unemployment seem to have an effect on the assessments of country optimism using either approach.

Finally, in contrast with previous studies, we did not find statistically significant differences in the coefficients for inflation and unemployment for most of the regression specifications in our analysis, as indicated by the insignificant Wald F-statistics in the tables in a test of equality of the coefficients for inflation and unemployment. This difference from other studies may be attributed to two important factors. First, we analyze a set of countries from several world regions that exhibit widely different patterns of inflation and unemployment, while previous studies analyze countries that often belong to more homogeneous regions or income groups. Second, although most other studies analyze a reduced number of countries, they also have data for several years, whereas we have data only for 2006.

CONCLUSION

In this study, we used Gallup World Poll data to analyze the determinants of individual assessments of personal and country well-being. With these data we extended the number of countries in the analysis beyond those of other studies in the literature.

Using individual assessments of past, present, and future measures of personal and country well-being, we examine two dimensions of happiness data that have not been previously analyzed in the literature. (i) By comparing the assessments for personal and country well-being we found evidence of the better-than-average effect identified in the overconfidence literature. (ii) By comparing future and present evaluations we also found evidence of optimism in the assessments of well-being.

We also analyzed the effects of inflation and unemployment on eight subjective measures of well-being (past, present, and future assessment of personal and country well-being, and personal and country optimism). We found that both inflation and unemployment have a negative effect on individuals' assessments of personal and country past and present well-being. We also found a positive impact of inflation and unemployment on the optimism measures because both inflation and unemployment worsen the evaluations of present well-being relative to the future. Our results suggest that policymakers designing measures targeted at reducing the perceived costs of inflation and unemployment may consider exploiting the differential effect of these macroeconomic variables on expectations of future well-being relative to current well-being.

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APPENDIX

Table A1

List of Countries Used in the Analysis

Country	Macroeconomic variables (2005): GDP per capita (2000 USD)	Inflation (percent)	Unemployment (percent)	Average personal life satisfaction: Past	Present	Future	Average country situation: Past	Present	Future	Average optimism: Personal	Country
1 Norway	40,597	1.5	4.6	6.8	7.5	8.1	7.3	7.6	7.7	0.6	0.0
2 Japan	38,962	–0.3	4.4	6.5	6.5	6.4	5.7	5.4	5.2	–0.1	–0.2
3 United States	37,084	3.4	5.1	6.5	7.3	8.1	6.9	5.9	6.1	0.8	0.2
4 Switzerland	34,903	1.2	4.4	7.0	7.5	7.9	7.3	7.0	6.7	0.4	–0.3
5 Denmark	31,597	1.8	4.8	7.3	8.0	8.5	7.4	7.1	7.3	0.4	0.1
6 Hong Kong	30,405	0.9	5.6	5.5	5.7	6.4	5.0	5.9	6.5	0.6	0.6
7 Sweden	30,124	0.5	7.7	6.7	7.4	7.9	6.8	6.3	6.3	0.5	0.0
8 Ireland	29,839	2.4	4.3	6.3	7.2	8.2	6.2	7.3	8.1	1.0	0.8
9 United Kingdom	27,034	2.8	4.6	6.1	7.0	7.7	6.3	5.6	5.7	0.7	0.1
10 Finland	26,329	0.9	8.4	7.2	7.6	7.6	7.2	7.3	7.2	0.0	–0.1
11 Singapore	25,968	0.4	4.2	6.0	6.6	7.4	6.3	6.9	7.8	0.7	0.8
12 Canada	25,452	2.2	6.8	6.6	7.4	8.2	7.1	6.9	7.0	0.7	0.0
13 Austria	25,301	2.3	5.2	6.5	7.1	7.8	6.8	6.6	6.5	0.7	–0.2
14 Netherlands	24,997	1.7	5.2	7.1	7.6	7.7	7.1	5.9	5.5	0.2	–0.4
15 Belgium	23,799	2.8	8.1	6.8	7.4	8.0	6.9	6.6	6.4	0.6	–0.2
16 Germany	23,788	2.0	11.1	6.4	6.6	6.8	6.3	4.7	4.9	0.2	0.3
17 France	23,650	1.8	9.8	6.5	7.0	7.6	6.2	5.4	5.3	0.5	0.0
18 Australia	23,031	2.7	5.1	6.5	7.4	8.1	7.3	6.7	6.4	0.7	–0.4
19 Italy	19,380	2.0	7.7	7.0	7.0	7.4	6.3	5.3	5.3	0.4	–0.1
20 Israel	19,259	1.3	9.0	6.9	7.2	8.1	5.6	5.5	6.4	0.9	0.9
21 Greece	16,054	3.6	9.6	6.2	6.4	7.0	5.1	5.1	5.6	0.5	0.5
22 Spain	15,688	3.4	9.2	6.8	7.1	7.3	5.8	5.9	6.1	0.3	0.3
23 New Zealand	15,098	3.0	3.7	6.6	7.4	8.2	6.8	6.9	7.0	0.7	0.1
24 Cyprus	14,408	2.6	5.3	6.3	6.2	6.7	5.8	5.7	5.9	0.4	0.1
25 South Korea	13,240	2.8	3.7	5.6	5.7	6.7	5.1	5.2	6.4	0.9	1.2
26 Slovenia	11,432	2.5	5.8	5.9	5.9	6.2	5.3	5.8	6.4	0.2	0.6
27 Portugal	11,093	2.3	7.6	5.9	5.4	5.8	5.1	4.3	5.1	0.2	0.7
28 Trinidad & Tobago	9,309	6.9	8.0	5.8	5.8	7.4	5.7	4.3	4.7	1.6	0.3
29 Argentina*	8,094	9.6	10.6	5.8	6.3	7.6	3.8	5.9	7.3	1.3	1.4
30 Czech Republic	6,676	1.8	7.9	6.1	6.4	6.7	4.7	5.0	6.4	0.2	1.4
31 Uruguay*	6,548	4.7	12.2	5.7	5.6	6.8	4.3	5.2	6.8	1.2	1.6
32 Estonia	6,211	4.1	7.9	4.7	5.4	6.4	4.1	5.3	6.6	1.0	1.3
33 Mexico	6,163	4.0	3.5	6.3	6.7	7.7	5.4	5.8	6.6	0.9	0.7
34 Hungary	5,870	3.6	7.2	5.5	5.2	5.5	4.8	4.3	5.2	0.2	0.9
35 Chile	5,719	3.1	6.9	5.9	6.2	7.4	5.2	6.1	7.3	1.2	1.2
36 Croatia	5,238	3.3	12.7	6.1	5.8	6.3	4.4	4.8	5.9	0.5	1.1
37 Poland	5,225	2.1	17.7	6.2	5.9	6.3	4.5	4.0	4.8	0.3	0.7
38 Latvia	5,047	6.8	8.7	4.3	4.7	5.8	3.7	4.3	5.6	1.1	1.2

NOTE: *Indicates country with a missing value for the unemployment rate over the 2002-05 period in the 2007 World Development Indicators database. Missing data were imputed.

Table A1, cont'd

List of Countries Used in the Analysis

Country	Macroeconomic variables (2005) GDP per capita (2000 USD)	Inflation (percent)	Unemployment (percent)	Average personal life satisfaction Past	Present	Future	Average country situation Past	Present	Future	Average optimism Personal	Country
39 Venezuela*	5,001	16.0	16.8	6.2	7.2	8.5	4.9	5.8	7.6	1.2	1.7
40 Lithuania	4,873	2.7	8.3	5.6	5.9	6.7	4.5	4.7	6.1	0.7	1.3
41 Slovakia	4,733	2.7	16.2	5.2	5.2	5.6	4.1	4.2	5.2	0.4	0.9
42 Costa Rica	4,505	13.8	6.6	6.8	7.0	7.7	6.2	5.7	6.0	0.6	0.4
43 Panama	4,429	3.3	10.3	5.6	6.2	8.1	4.8	5.4	7.2	1.9	1.8
44 Botswana*	4,382	8.6	23.8	4.1	4.6	6.5	5.3	5.8	6.9	1.9	1.2
45 Malaysia*	4,360	3.0	3.0	5.2	6.1	7.6	5.5	6.4	7.6	1.5	1.3
46 Brazil*	3,951	6.9	9.7	5.7	6.5	8.6	5.2	4.9	7.0	2.1	2.0
47 South Africa	3,429	3.4	26.7	5.2	5.4	7.0	5.4	5.6	7.0	1.5	1.4
48 Turkey	3,425	10.1	10.3	4.5	4.7	5.8	3.7	4.6	5.9	1.1	1.3
49 Jamaica	3,291	15.3	10.9	5.2	6.2	8.3	4.5	4.9	5.7	2.0	0.9
50 Thailand	2,494	4.5	1.3	5.6	6.0	7.4	6.0	5.4	6.8	1.3	1.3
51 Dominican Rep.	2,471	4.2	17.9	4.9	5.1	7.7	3.9	5.0	7.3	2.5	2.2
52 Russia*	2,444	12.7	8.1	4.7	5.0	6.2	3.6	4.7	6.1	1.1	1.3
53 Peru	2,396	1.6	11.4	4.7	4.9	6.8	4.4	4.1	5.8	1.7	1.6
54 Romania	2,259	9.0	7.2	5.6	5.3	5.9	4.1	3.8	5.2	0.5	1.4
55 El Salvador*	2,202	4.7	7.4	5.8	5.6	5.5	5.3	4.7	4.1	–0.2	–0.4
56 Colombia	2,199	5.0	9.5	5.6	5.9	7.9	4.5	5.2	6.9	1.9	1.6
57 Bulgaria	2,107	5.0	10.1	4.0	3.8	5.0	3.1	3.3	5.0	1.1	1.7
58 Jordan*	2,104	3.5	12.4	6.1	6.3	7.1	5.9	6.4	7.3	0.7	0.9
59 Kazakhstan	1,978	7.6	8.1	4.5	5.5	7.3	3.9	5.9	7.8	1.8	1.9
60 Macedonia	1,892	0.0	37.3	4.6	4.5	5.5	3.6	3.5	4.8	1.0	1.2
61 Guatemala*	1,719	8.4	3.9	6.0	6.0	6.9	5.0	4.8	5.3	0.8	0.5
62 Egypt*	1,643	4.9	10.7	5.2	5.2	7.0	NA	NA	NA	1.7	NA
63 Ecuador	1,589	2.4	7.7	5.3	5.1	6.3	4.5	4.0	5.1	1.1	1.0
64 Morocco	1,562	1.0	11.0	4.0	4.6	7.1	3.8	4.8	7.1	2.5	2.3
65 China	1,451	1.8	4.2	3.8	4.8	6.7	NA	NA	NA	1.9	NA
66 Paraguay*	1,361	6.8	7.6	5.6	4.9	5.2	4.9	3.7	4.2	0.4	0.5
67 Philippines	1,117	7.6	7.4	4.8	4.7	5.7	5.1	4.0	5.0	1.0	1.0
68 Bolivia*	1,061	5.4	4.3	4.9	5.4	7.0	3.8	5.0	6.8	1.7	1.8
69 Honduras	1,039	8.8	4.2	4.9	5.3	7.1	4.3	4.1	4.6	1.6	0.5
70 Sri Lanka	1,007	11.6	7.6	3.6	4.3	6.2	3.8	4.8	6.7	1.8	1.8
71 Georgia	974	8.2	13.8	3.4	3.6	5.6	2.4	3.9	6.2	1.7	2.2
72 Ukraine	962	13.5	7.2	4.8	4.9	5.8	3.9	3.9	4.7	0.8	0.8
73 Indonesia	942	10.5	10.3	5.1	5.0	6.6	5.4	4.9	6.4	1.6	1.5
74 Pakistan	606	9.1	7.7	5.0	6.1	7.4	4.9	5.2	6.4	1.3	1.3
75 Moldova	468	13.1	7.3	5.0	4.9	5.6	4.4	4.5	5.6	0.7	1.0

NOTE: *Indicates country with a missing value for the unemployment rate over the 2002-05 period in the 2007 World Development Indicators database. Missing data were imputed.

Table A2

Results of the Two-Stage Approach

	Life satisfaction				Country situation			
	Present	Past	Future	Future-Present	Present	Past	Future	Future-Present
First stage								
Male	–0.12100***	0.0212	–0.14417***	–0.02933	0.01849	–0.08521***	–0.01069	–0.02822
	(–5.10)	(0.88)	(–4.82)	(–1.53)	(0.85)	(–4.20)	(–0.33)	(–1.33)
Age	–0.06079***	–0.02668***	–0.06108***	–0.00431	–0.02070***	–0.00329	–0.03789***	–0.01743***
	(–10.64)	(–6.54)	(–7.05)	(–0.74)	(–5.15)	(–0.93)	(–7.36)	(–5.04)
Age^2	0.00051***	0.00033***	0.00023***	–0.00024***	0.00019***	0.00007*	0.00034***	0.00013***
	(8.76)	(6.59)	(2.74)	(–3.84)	(4.7)	(1.92)	(6.21)	(3.66)
Married	0.36510***	0.23948***	0.15947***	–0.19388***	0.06233**	0.06136***	0.07331**	0.01374
	(12.66)	(7.05)	(6.01)	(–7.19)	(2.6)	(2.85)	(2.4)	(0.64)
Employed	0.35951***	–0.0308	0.34214***	–0.0035	0.05498*	–0.00891	0.02101	–0.03793
	–9.86	(–0.82)	–8.72	(–0.15)	(1.94)	(–0.35)	(0.58)	(–1.53)
Urban	0.24150***	0.27612***	0.27106***	0.04752	0.03328	0.0246	–0.00832	–0.0343
	(5.82)	(6.88)	(6.24)	(1.40)	(1.19)	(0.81)	(–0.21)	(–1.17)
Poor	–0.85726***	–0.56094***	–0.59997***	0.24786**	–0.26221**	–0.14593	–0.18102	0.05851
	(–6.61)	(–2.79)	(–2.83)	(2.07)	(–2.18)	(–0.87)	(–0.88)	(0.43)
Constant	7.12950***	5.91649***	8.80361***	1.68644***	5.72661***	5.22123***	7.07598***	1.31686***
	(57.96)	(66.5)	(47.62)	(15.07)	(67.22)	(61.02)	(67.31)	(18.02)
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	52,447	52,168	46,746	46,681	51,650	50,937	47,054	46,892
R^2	0.036	0.009	0.1	0.064	0.002	0.002	0.004	0.003
$R^{2(*)}$	0.24984	0.15034	0.23647	0.14662	0.24829	0.30501	0.15001	0.14344
Second stage: current variables								
Inflation	–0.05801*	–0.06304**	–0.01061	0.04553***	–0.04692	–0.09577***	0.01395	0.05894***
	(–1.79)	(–2.36)	(–0.36)	(4.22)	(–1.54)	(–3.05)	(0.52)	(4.82)
Unemployment	–0.06159***	–0.04749***	–0.03912***	0.02060**	–0.05708***	–0.06848***	–0.01504	0.04058***
	(–5.59)	(–4.41)	(–3.07)	(2.60)	(–3.05)	(–3.85)	(–0.69)	(4.20)
No. of countries	65	65	65	65	65	65	65	65
R^2	0.234	0.26	0.069	0.259	0.177	0.307	0.01	0.317
F-test for inf. = unemp.	0.01341	0.31444	0.85204	2.77443	0.08623	0.60882	0.61559	1.19122
p-Value	0.90818	0.57699	0.35955	0.10083	0.77001	0.4382	0.43568	0.27931
Second stage: lag variables								
Lag inflation	–0.02929	–0.02964	–0.00191	0.02651**	–0.02306	–0.06508***	0.02626	0.04819***
	(–0.98)	(–1.46)	(–0.07)	(2.00)	(–0.89)	(–2.84)	(1.04)	(4.51)
Lag unemployment	–0.06335***	–0.05048***	–0.03832**	0.02325***	–0.05713***	–0.07072***	–0.01514	0.04056***
	(–4.83)	(–4.25)	(–2.57)	(2.82)	(–2.69)	(–3.21)	(–0.72)	(4.28)
No. of countries	65	65	65	65	65	65	65	65
R^2	0.199	0.191	0.067	0.184	0.15	0.272	0.023	0.32
F-test for inf. = unemp.	1.01542	0.67955	1.15526	0.03438	0.8782	0.02737	1.24545	0.23442
p-Value	0.31753	0.4129	0.28662	0.85352	0.35233	0.86913	0.26873	0.62997

NOTE: *t*-Statistics are in parentheses. R^2 corresponds to the between model. $R^{2(*)}$ accounts for the variation explained by the fixed effects. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A3

Results of the Single-Stage Approach: Current Inflation and Unemployment

	Life satisfaction				Country situation			
	Present	Past	Future	Future-Present	Present	Past	Future	Future-Present
Male	–0.14382***	–0.00865	–0.14486***	–0.00911	0.01299	–0.08618**	–0.01903	–0.03044
	(–3.80)	(–0.26)	(–3.49)	(–0.38)	(0.39)	(–2.25)	(–0.45)	(–1.12)
Age	–0.05555***	–0.02477***	–0.05550***	–0.00473	–0.01609***	0.00194	–0.03566***	–0.02059***
	(–7.86)	(–4.42)	(–6.12)	(–0.77)	(–3.06)	(0.33)	(–6.01)	(–4.58)
Age^2	0.00049***	0.00034***	0.00018**	-0.00027***	0.00016***	0.00004	0.00029***	0.00013***
	(7.15)	(5.4)	(2.05)	(–4.12)	(2.91)	(0.7)	(4.73)	(2.81)
Married	0.20341***	0.14978**	–0.0011	–0.19320***	–0.08472*	–0.09479*	0.01012	0.09213***
	(3.79)	(2.61)	(–0.02)	(–5.86)	(–1.92)	(–1.99)	(0.19)	(2.66)
Employed	0.58601***	0.13680*	0.48929***	–0.07918**	0.26497***	0.25389***	0.07319	–0.18720***
	(8.14)	(1.9)	(6.77)	(–2.04)	(4.49)	(3.15)	(1.11)	(–3.86)
Urban	0.27025***	0.28274***	0.36324***	0.11456**	0.09756	–0.0416	0.15528*	0.06906
	(4.32)	(4.48)	(5.42)	(2.58)	(1.23)	(-0.57)	(1.71)	(1.31)
Poor	–0.92360***	–0.74566**	–0.46159	0.51478**	–0.0924	–0.31355	0.08347	0.20168
	(–3.68)	(–2.35)	(–1.50)	(2.41)	(–0.31)	(–0.95)	(0.16)	(0.7)
Inflation	–0.07548**	–0.07551**	–0.0324	0.04144***	–0.05884*	–0.11252***	0.00916	0.06684***
	(–2.04)	(–2.53)	(–0.94)	(3.62)	(–1.87)	(–3.23)	(0.33)	(4.99)
Unemployment	–0.06019***	–0.04581***	–0.04100***	0.01835**	–0.05907***	–0.06764***	–0.02235	0.03667***
	(–5.71)	(–4.62)	(–3.43)	(2.61)	(–3.42)	(–4.12)	(–1.15)	(4.27)
Constant	7.82296***	6.55580***	9.16865***	1.39489***	6.37335***	6.20237***	7.16724***	0.78952***
	(30.59)	(29.87)	(34.04)	(9.77)	(24.33)	(23.5)	(25.9)	(4.69)
N	52,447	52,168	46,746	46,681	51,650	50,937	47,054	46,892
R^2	0.096	0.052	0.114	0.097	0.058	0.108	0.01	0.056
Adjusted R^2	0.095	0.052	0.114	0.096	0.058	0.107	0.01	0.056
No. of countries	65	65	65	65	65	65	65	65
F-test for inf. = unemp.	0.204	1.03141	0.06423	2.62362	0.00005	1.55137	0.80747	3.41143
p-Value	0.65304	0.31365	0.80075	0.1102	0.99446	0.21747	0.37224	0.06937

NOTE: *t*-Statistics are in parentheses. R^2 corresponds to the between model. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A4

Results of the Single-Stage Approach: Lag Inflation and Unemployment

	Life satisfaction				Country situation			
	Present	Past	Future	Future-Present	Present	Past	Future	Future-Present
Male	–0.15915***	–0.02274	–0.15165***	–0.00105	0.00034	–0.11051***	–0.01657	–0.0157
	(–4.40)	(–0.69)	(–3.78)	(–0.04)	(0.01)	(–2.77)	(–0.39)	(–0.58)
Age	–0.05481***	–0.02382***	–0.05505***	–0.00529	–0.01556***	0.00269	–0.03550***	–0.02078***
	(–7.58)	(–4.20)	(–6.02)	(–0.86)	(–2.84)	(0.44)	(–5.85)	(–4.41)
Age^2	0.00049***	0.00034***	0.00018**	–0.00027***	0.00016***	0.00004	0.00029***	0.00013***
	(7.04)	(5.32)	(2.04)	(–4.08)	(2.85)	(0.69)	(4.67)	(2.67)
Married	0.19901***	0.14613**	–0.00377	–0.19104***	–0.08913**	–0.10142**	0.01039	0.09729***
	(3.45)	(2.39)	(–0.08)	(–5.42)	(–2.00)	(–2.11)	(0.2)	–2.68
Employed	0.61000***	0.16328*	0.50346***	–0.08836**	0.28595***	0.27683***	0.08759	–0.19377***
	(7.66)	(1.98)	(6.9)	(–2.25)	(4.73)	(2.96)	(1.39)	(–3.74)
Urban	0.28732***	0.29353***	0.36927***	0.10597**	0.11322	–0.00907	0.14368	0.0416
	(4.28)	(4.23)	(5.23)	(2.46)	(1.31)	(–0.12)	(1.53)	(0.84)
Poor	–1.16219***	–0.97938***	–0.56267*	0.61521***	–0.2888	–0.63651**	0.07375	0.37104
	(–5.83)	(–3.66)	(–1.88)	(2.89)	(–0.97)	(–2.37)	(0.14)	(1.33)
Lag inflation	–0.03784	–0.03406	–0.01252	0.02312*	–0.02981	–0.06930***	0.01993	0.04868***
	(–1.25)	(–1.60)	(–0.43)	(1.99)	(–1.20)	(–2.84)	(0.83)	(4.82)
Lag unemployment	–0.06087***	–0.04783***	–0.04057***	0.02008***	–0.05737***	–0.06913***	–0.0206	0.03693***
	(–4.95)	(–4.28)	(–2.93)	(2.74)	(–2.89)	(–3.37)	(–1.05)	(4.24)
Constant	7.64618***	6.36076***	9.06714***	1.46946***	6.21891***	6.02331***	7.08293***	0.85063***
	(32.18)	(31.34)	(39.05)	(9.87)	(25.45)	(23.46)	(26.9)	(4.85)
N	52,447	52,168	46,746	46,681	51,650	50,937	47,054	46,892
R^2	0.084	0.04	0.112	0.093	0.049	0.092	0.01	0.054
Adjusted R^2	0.084	0.04	0.112	0.093	0.049	0.092	0.01	0.054
No. of countries	65	65	65	65	65	65	65	65
F-test for inf. = unemp.	0.48893	0.29531	0.66663	0.03895	0.68133	0.00003	1.38459	0.66487
p-Value	0.48694	0.58872	0.41726	0.84417	0.4122	0.99598	0.24368	0.41787

NOTE: *t*-Statistics are in parentheses. R^2 corresponds to the between model. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

A Journal Ranking for the Ambitious Economist

Kristie M. Engemann and Howard J. Wall

The authors devise an "ambition-adjusted" journal ranking based on citations from a short list of top general-interest journals in economics. Underlying this ranking is the notion that an ambitious economist wishes to be acknowledged not only in the highest reaches of the profession, but also outside his or her subfield. In addition to the conceptual advantages that they find in their ambition adjustment, they see two main practical advantages: greater transparency and a consistent treatment of subfields. They compare their 2008 ranking based on citations from 2001 to 2007 with a ranking for 2002 based on citations from 1995 to 2001. (JEL A11)

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Nearly every ranking of economics journals uses citations to measure and compare journals' research impact.[1] Raw citation data, however, include a number of factors that generally are thought to mismeasure impact. For example, under the view that a citation in a top journal represents greater impact than a citation elsewhere, it is usual to weight citations according to their sources. The most common means by which weights are derived is the recursive procedure of Liebowitz and Palmer (1984) (henceforth LP), which handles the simultaneous determination of rank-adjusted weights and the ranks themselves.

We devise an alternative "ambition-adjusted" journal ranking for which the LP procedure is replaced by a simple rule that considers citations only from a short list of top general-interest journals in economics.[2] Underlying this rule is the notion that a truly ambitious economist wishes to be acknowledged not only in the highest reaches of the profession, but also outside of his or her subfield. Thus, an ambitious economist also would like to publish his or her research in the journals that are recognized by the top general-interest outlets. In addition to the conceptual advantages that we find in our ambition adjustment, we see two main practical advantages: greater transparency and a consistent treatment of subfields.

The virtues of transparency are that the ranking has clear criteria for measuring the citations and these criteria are consistent over time. The LP procedure, in contrast, is largely a black box: It is not possible to see how sensitive the weights (and therefore the rankings) are to a variety of factors. The obvious objection to our rule is its blatant subjectivity. Our counter to this objection is to point out that the LP procedure, despite its sheen of objectivity, contains technical features that make it implicitly subjective.

First, as pointed out in Amir (2002), rankings derived using the LP procedure are not independent of the set of journals being considered: If a journal is added or subtracted from the set, the

1 A recent exception is Axarloglou and Theoharakis (2003), who survey members of the American Economic Association.

2 *American Economic Review* (*AER*), *Econometrica*, *Economic Journal* (*EJ*), *Journal of Political Economy* (*JPE*), *Quarterly Journal of Economics* (*QJE*), *Review of Economic Studies* (*REStud*), and *Review of Economics and Statistics* (*REStat*).

Kristie M. Engemann is a senior research analyst and Howard J. Wall is a vice president and economist at the Federal Reserve Bank of St. Louis.

rankings of every other journal can be affected. It is for this reason that journals in subfields are treated differently. Significant numbers of citations come from journals that are outside the realm of pure economics (e.g., finance, law and economics, econometrics, and development), but the LP procedure does not measure all these citations in the same manner. For example, Amir attributes the extremely high rankings sometimes achieved by finance journals to data-handling steps within the LP procedure. On the other hand, for journals in subfields such as development, rankings are depressed by the exclusion of citations from sources other than purely economics journals.

Palacios-Huerta and Volij (2004) pointed out that a second source of implied subjectivity in the LP procedure is differences in reference intensity across journals. Specifically, they find a tendency for theory journals, which usually contain fewer citations than the average journal, to suffer from this reference-intensity bias. By convention, the typical theory paper provides fewer citations than the typical empirical paper, so journals publishing relatively more theory papers tend to see their rankings depressed.

An advantage of our blatantly subjective weighting rule is that it avoids the hidden subjectivity of the LP procedure by treating all subfields the same. First, the subfields are evaluated on equal footing as economics journals: i.e., journals in finance, law, and development are judged by their contributions to economics only. One might prefer a ranking that does otherwise, but this is the one we are interested in. Second, the cross-field reference-intensity bias is ameliorated by considering citations from general-interest journals only.

Before proceeding with our ranking of economics journals, we must point out that any ranking should be handled with a great deal of care when using it for decisionmaking. It would be a mistake, for example, to think that a journal ranking is anything like a definitive indicator of the relative quality of individual papers within the journals. First, any journal's citation distribution is heavily skewed by a small number of very successful papers, and even the highest-ranked journals have large numbers of papers that are cited rarely, if at all (Oswald, 2007; Wall, 2009). Put another way, citation distributions exhibit substantial overlap, meaning that (i) large shares of papers in the highest-ranked journals are cited less frequently than the typical paper in lower-ranked journals; and, conversely, (ii) large shares of articles in low-ranked journals are cited more frequently than the typical paper in the highest-ranked journals.

COMMON PRACTICES AND RECENT RANKINGS

There is no such thing as the correct ranking of economics journals. Instead, there is a universe of rankings, each the result of a set of subjective decisions by its constructor. With the constructors' choices and criteria laid out as clearly as possible, the users of journal rankings would be able to choose the ranking, or rankings, that are the best reflection of the users' own judgment and situation. As outlined by Amir (2002), subjective decisions about which journals to include can inject bias through the objective LP procedure. In addition, every ranking is sensitive to the number of years of citation data, the choice of which publication years are to be included, and whether or not to include self-citations. Choices such as these are unavoidable. And any journal ranking, no matter how complicated or theoretically rigorous, cannot avoid being largely subjective. That said, there is much to be gained from a journal ranking that is as objective as possible and for which the many subjective choices are laid out so that the users of the ranking clearly understand the criteria by which the journals are being judged.

In an ideal world, the user will have chosen rankings on the basis of the criteria by which the rankings were derived and not on how closely they fit his or her priors. However, in addition to the usual human resistance to information that opposes one's preconceptions, users are also often hindered by a lack of transparency about the choices (and their consequences) underlying the various rankings. The onus, therefore, is on the constructors of the rankings to be as transparent as possible, so that the users need not depend on their priors when evaluating the many available rankings.

With this in mind, we lay out the most common practices developed over the years for constructing journal rankings. We assess our ranking along with a handful of the most prominent rankings of economics journals on the basis of their adherence to these practices (summarized in Table 1). Three of these rankings—Kalaitzidakis, Mamuneas, and Stengos (2003); Palacios-Huerta and Volij (2004); and Kodrzycki and Yu (2006)—are from the economics literature and are accompanied by analyses of the effects of the various choices on the rankings. The other two—the Thompson Reuters *Journal Citation Reports* (*JCR*) Impact Factor and the Institute for Scientific Information (ISI) Web of Science *h*-index—are commercially produced and widely available rankings covering a variety of disciplines. There has been little analysis of the reasonableness of their methods for ranking economics journals, however.[3]

Control for Journal Size

Most rankings control for journal size by dividing the number of adjusted citations by the number of articles in the journal, the number of adjusted pages, or even the number of characters. Whichever of these size measures is chosen, the purpose of controlling for journal size is to assess the journal on the basis of its research quality rather than its total impact combining quantity and quality.[4] Of the five other rankings summarized in Table 1, all but one control for journal size. The ISI Web of Science produces a version of the *h*-index, which was proposed by Hirsch (2005) to measure the total impact of an individual researcher over the course of his or her career. Tracing a person's entire publication record from the most-cited to the least-cited, the *h*th paper is the one for which each paper has been cited at least *h* times. The intention of the *h*-index is to combine quality and quantity while reining in the effect that a small number of very successful papers would have on the average. In Wall (2009) the ranking according to the *h*-index was statistically indistinguishable from one according to total citations, indicating that *h*-indices are inappropriate for assessing journals' relative research quality. The other four rankings are, however, appropriate for this purpose.

The size control that we choose for our ranking is the number of articles. The primary reason for this choice is that the article is the unit of measurement by which the profession produces and summarizes research.[5] Economists list articles on their curriculum vitae, not pages or characters. Generally speaking, an article represents an idea, and citations to an article are an acknowledgment of the impact of that idea. It matters little whether that idea is expressed in 20 pages or 10. The reward for pages should not be imposed but should come through the effect that those pages have on an article's impact on the research of others. If a longer article means that an idea is more fully fleshed out, is somehow more important, or will have a greater impact, then this should be reflected in the number of citations it receives.

Control for the Age of Articles

Presumably, the most desirable journal ranking would reflect the most up-to-date measure of research quality that is feasible given the data constraints. As such, the information used to construct the ranking should restrict itself to papers published recently, although the definition of "recent"

[3] Note that we have not included the several rankings provided on the RePEc website. The methodology used in those rankings is similar to what is used in the rankings that we discuss here. They deviate from usual practice in that their data include working paper series and the small set of journals that provide citation data for free. Given the heavy use of so-called gray literature and the biased set of citing journals, the website warns that the rankings are "experimental."

[4] Our purpose is to rank journals on the basis of the quality of the research published within them, so a measure that controls for size is necessary to make the ranking useful for assessing the research quality of papers, people, or institutions. Others, however, might be interested in a ranking on the basis of total impact, whereby the quality of the research published within can be traded off for greater quantity. This is a perfectly valid question, but its answer does not turn out to be terribly useful for assessing journals' relative research quality.

[5] In addition, the practical advantage of this size measure is its ease of use and ready availability. Because pages across journals differ a great deal in the number of words or characters they contain on average, a count of pages would have to be adjusted accordingly. An accounting of cross-journal differences in the average number of characters per article seems excessive.

Table 1

Characteristics of Select Rankings of Economics Journals

Ranking	Description of citation data	Controls					
		Size	Article age	Citation age	Citation source	Self-citations	Reference intensity
Thompson Reuters *JCR* Impact Factor	Citations in a year to articles published during the previous two years	✔	✔	✔			
ISI Web of Science *h*-index	Citations from journals in database, years chosen by user		✔				
Kalaitzidakis et al. (2003)	Citations in 1998 to papers published during 1994-98	✔	✔	✔	✔	✔	
Palacios-Huerta and Volij (2004)	Citations in 2000 to papers published during 1993-99	✔	✔	✔	✔		✔
Kodrzycki and Yu (2006)	Citations in 2003 to papers published during 1996-2003	✔	✔	✔	✔	✔	✔
Ambition-Adjusted Ranking	Citations in 2001-07 to articles published during 2001-07	✔	✔	✔	✔		

NOTE: A checkmark (✔) indicates that the ranking controls for the relevant factor. Size is measured variously as the number of papers, number of pages, or number of characters. Article age is controlled for by restricting the data to citations of papers published in recent years, as chosen by the ranker. Others have controlled for citation age by looking at citations from one year only. Self-citations are citations from a journal to itself. In the other rankings listed here, citation source is controlled for with a variant of the recursive method of Liebowitz and Palmer (1984). To control for reference intensity the recursive weights include the average number of references in the citing journals.

is open to interpretation. On the one hand, if one looks at citations to papers published in, say, only the previous year, the result would largely be noise: The various publication lags would preclude any paper's impact from being realized fully. Further, given the large differences in these lags across journals, the results would be severely biased. On the other hand, the further one goes back in time, the less relevant the data are to any journal's current research quality. Ideally, then, the data should go back just far enough to reflect some steady-state level of papers' impact while still being useful for measuring current quality.

Although all of the rankings listed in Table 1 restrict the age of articles, the Thompson Reuters *JCR* Impact Factor considers only papers published in the previous two years. Such a short time frame renders the information pretty useless for assessing economics journals, for which there are extremely large differences across journals in publication lags.[6] The other rankings listed in the table use citation data on papers published over a five- to eight-year period. For our ranking we have elected to use citations to journals over the previous seven-year period.

Control for the Age of Citations

Because any ranking is necessarily backward-looking, it should rely on the most recent expression of journal quality available, while at the same time having enough information to make the ranking meaningful and to minimize short-term fluctuations. To achieve this we look at citations made over a seven-year period to articles published during the same period. The standard practice has been to look only at citations during a single year to articles over some number of prior years. Because we are counting citations from a small number of journals, however, this would not be enough information to achieve our objectives.

Adjust for Citation Source

As we outlined in our introduction, the most important difference between our ranking and others is in its treatment of citation sources. While we agree with the premise that citation source matters, we do not agree that the most appropriate way to handle the issue is the application of the LP procedure. Therefore, we replace the LP procedure with a simple rule: We count only citations from the top seven general-interest journals as determined by the total number of non-self-citations per article they received in 2001-07.

Exclude Self-Citations

To ensure that a journal's impact reaches outside its perhaps limited circle of authors, self-citations—that is, citations from papers in a journal to other papers in the same journal—are usually excluded when ranking economics journals. Although self-citations are not necessarily bad things, the practice has been to err on the side of caution and eliminate them from every journal's citation count. In our ranking, however, self-citations are relevant only for the seven general-interest journals, which could put them at a severe disadvantage relative to the rest of the journals. Further, it's conceivable that the rate of bad self-citations differs a lot across the seven general-interest journals. If so, then a blanket elimination of self-citations would be unfair to some of the journals with relatively few bad self-citations and would affect the ranking within this subset of journals.

Because of our concerns, we do not control for journal self-citations in our ranking. Admittedly, this is a judgment call because it is not possible to know for each journal how many of the self-citations should be eliminated. We have, therefore, also produced a ranking that eliminates all self-citations. As we show, this affects the ordering, but not the membership, of the top five journals. We leave it to the user to choose between the two alternative rankings.

Control for Reference Intensity

As shown by Palacios-Huerta and Volij (2004), journals can differ a great deal in the average number of citations given by their papers. These

[6] According to Garfield (2003), the two-year time frame was chosen in the early 1970s because it seemed appropriate for the two fields of primary interest: molecular biology and biochemistry. This ad hoc time frame thought appropriate for these two fields has remained the standard more than 35 years later across all fields in the hard sciences, the social sciences, humanities, etc.

Table 2

Reference Intensities

	2000	2007
American Economic Review	1.0	1.0
Econometrica	0.6	0.9
Economic Journal	0.5	1.0
Journal of Political Economy	0.6	1.0
Quarterly Journal of Economics	0.9	1.2
Review of Economics and Statistics	0.5	1.0
Review of Economic Studies	0.8	1.0

NOTE: Reference intensity is the average number of references per article relative to that of the *American Economic Review*. The numbers for 2000 are from Palacios-Huerta and Volij (2004).

differences reflect the variety of attitudes and traditions across fields, and there is a tendency for the rankings of theory journals to suffer as a result. For example, according to Palacios-Huerta and Volij, in 2000 the average article in the *Journal of Monetary Economics* contained 80 percent more references than did the average across all articles, which would result in an upward bias for the rankings of journals that are cited relatively heavily in that journal. Similarly, the average articles in the *AER* and the *QJE* contained, respectively, 70 percent and 50 percent more references than average. At the other end, the average articles in the *Journal of Business and Economic Statistics*, the *AER Papers and Proceedings*, and the *International Journal of Game Theory* each contained only 40 percent of the average number of references.

The potential problem with differences in reference intensity is that journals receiving disproportionate numbers of citations from journals with high reference intensities would have an artificially high ranking. In effect, high reference intensity gives some journals more votes about the quality of research published in other journals. Indeed, as reported in Table 2, the differences in reference intensity across our seven general-interest journals were substantial in 2000. For 2007, however, using our citation dataset, which is more limiting than that of Palacios-Huerta and Volij (2004), reference intensities differed very little.[7] Further, adjusting for the differences that did exist would have had very little effect on our ranking.[8] Therefore, in the interest of simplicity and transparency, our ranking does not take differences in reference intensity into account.

AN AMBITION-ADJUSTED JOURNAL RANKING

We start with a list of 69 journals that does not include non-refereed or invited-paper journals (the *Journal of Economic Literature*, *Brookings Papers on Economic Activity*, and the *Journal of Economic Perspectives*). We treat the May *Papers and Proceedings* issue of the *AER* separately from the rest of the journal because, as shown below, it is much less selective than the rest of the *AER*. The list is by no means complete, but we think that it contains most, if not all, journals that would rank in the top 50 if we considered the universe of economics journals. Nonetheless, an advantage of our ranking is that, because it is independent of the set of included journals, it is very easy to determine the position of any excluded journal because one needs only to navigate the ISI Web of Science website to obtain the data for the journal.[9]

We looked at all citations during 2001-07 from articles in the seven general-interest journals to articles in each of the 69 journals. Note that, using the Web of Science terminology, articles do not include proceedings, editorial material, book reviews, corrections, reviews, meeting abstracts,

[7] One reason that Palacios-Huerta and Volij (2004) found larger differences in reference intensity is because they considered all papers published in a journal, including short papers, comments, and non-refereed articles. Our dataset, on the other hand, includes only regular refereed articles.

[8] If citations to journals for which the *QJE* tended to overcite were adjusted to the citation tendencies across the other general-interest journals, the rankings of the affected journals would be nearly identical.

[9] From the main page, search by the journal name using the default time span of "all years." Refine the results to include articles from 2001-07 only. Create a citation report, view the citing articles, and refine to exclude all but articles and anything from years other than 2001-07. Click "Analyze results" and rank by source title, analyze up to 100,000 records, show the top 500 results with a threshold of 1, and sort by selected field. Select the seven general-interest journals and view the record, yielding the number of citations to the journal from these sources.

biographical items, software reviews, letters, news items, and reprints.[10] Also note that the citations are all those that were in the database as of the day that the data were collected: November 13, 2008.

Table 3 includes the number of articles, the number of adjusted cites (adjusted to include only those from the seven general-interest journals), the impact factor, and the relative impact. The impact factor is simply the number of adjusted cites per article, whereas the relative impact divides this by the impact factor of the *AER*. It's worth pointing out once again that one should handle this and any other journal ranking with care. Saying that "the average article in journal A received more citations than the average article in journal B" is a long way from saying "an article in journal A is better than an article in journal B."

There are some general results apparent from Table 3. First, the five top-ranked journals—*QJE*, *JPE*, *Econometrica*, *AER*, and *REStud*—are clearly separate from the rest: The fifth-ranked *REStud* is indistinguishable from the *AER* while the sixth-ranked *Journal of Labor Economics* has 55 percent of the average impact of the *AER*. Further, within the top five, the *QJE* and *JPE* are clearly distinguishable from the rest, with the *QJE* well ahead of the *JPE*. Specifically, the *QJE* and *JPE* had, respectively, 78 percent and 41 percent greater impact per article than the *AER*.

Second, the journals ranked sixth through ninth, with relative impacts ranging from the aforementioned 0.55 for the *Journal of Labor Economics* to 0.40 for the *Economic Journal*, are clearly separate from the remainder of the list. From the tenth-ranked journal on down, however, there are no obvious groupings of journals in that relative impact declines fairly continuously.

Several journals introduced in recent years have been relatively successful at generating citations. Most prominently, the *Journal of Economic Growth*, which began publishing in 1996 and for which citation data are available starting in 1999, is the seventh-ranked journal. It is among the group ranked sixth through ninth that is not quite the elite but is clearly separate from the next tier. The 18th-ranked *Review of Economic Dynamics*, which began publishing in 1998 and for which citation data are available from 2001, has been another very successful newcomer. The *Journal of the European Economic Association* has established itself in an even shorter period of time. It began publishing in 2003 and is ranked a very respectable 31st.

At this stage an alert reader with strong priors will, perhaps, question our ranking on the basis of its inclusion of self-citations. After all, the *JPE* and *QJE*, our two top-ranked journals, are considered (at least anecdotally) to have a publication bias toward adherents of the perceived worldviews of their home institutions. If this supposition is true, then their rankings might be inflated by the inclusion of self-citations. As we show in Table 4, however, the supposition is false.

The first column of numbers in Table 4 gives the raw number of self-citations, while the second column gives self-citations as a percentage of total citations from the seven reference journals. The most important number for each journal is in the third column, the self-citation rate, which is the average number of self-citations per article. Among the top five journals, the most notable differences are that the self-cites are relatively rare in the *REStud*, whereas the *QJE* and *Econometrica* have the highest self-citation rates. The effect of eliminating self-citations is to slightly reshuffle the top five, without any effect on the aforementioned relative positions of the *QJE* and *JPE.* The most notable effect that the exclusion of self-citations has is on the rankings of the *EJ*, which drops from 9th to 17th place.

As outlined in the previous section, we think that the negatives from eliminating self-citations outweigh the positives. In the end, however, doing so would have relatively little effect on the resulting ranking. Nevertheless, the reader has both versions from which to choose.

TRENDS IN AMBITION-ADJUSTED RANKINGS

Table 5 reports the ambition-adjusted ranking for 2002, which is based on citations in 1995-2001 for articles published during the same period.

[10] The *AER Papers and Proceedings* is the exception to this.

Table 3

Ambition-Adjusted Journal Ranking, 2008

	Journal	Articles	Adjusted cites	Impact factor	Relative impact
1	Quarterly Journal of Economics	283	470	1.66	1.78
2	J of Political Economy	296	390	1.32	1.41
3	Econometrica	420	442	1.05	1.13
4	American Economic Review	644	601	0.93	1.00
5	Review of Economic Studies	292	271	0.93	0.99
6	J of Labor Economics	201	104	0.52	0.55
7	*J of Economic Growth (1999)*	*87*	*39*	*0.45*	*0.48*
8	Review of Economics & Statistics	456	192	0.42	0.45
9	Economic Journal	498	185	0.37	0.40
10	American Economic Review P & P	592	179	0.30	0.32
11	International Economic Review	336	95	0.28	0.30
12	J of Monetary Economics	449	121	0.27	0.29
13	Rand Journal of Economics	285	73	0.26	0.27
14	J of International Economics	400	100	0.25	0.27
15	J of Law & Economics	169	42	0.25	0.27
16	J of Economic Theory	713	175	0.25	0.26
17	J of Public Economics	606	133	0.22	0.24
18	*Review of Economic Dynamics (2001)*	*234*	*48*	*0.21*	*0.22*
19	J of Business & Economic Statistics	250	50	0.20	0.21
20	J of Finance	589	117	0.20	0.21
21	Games & Economic Behavior	492	93	0.19	0.20
22	J of Econometrics	601	104	0.17	0.19
23	European Economic Review	482	77	0.16	0.17
24	Review of Financial Studies	289	43	0.15	0.16
25	J of Financial Economics	496	70	0.14	0.15
26	J of Industrial Economics	166	23	0.14	0.15
27	J of Applied Econometrics	258	35	0.14	0.15
28	J of Human Resources	224	29	0.13	0.14
29	J of Law, Economics & Organization	146	18	0.12	0.13
30	J of Development Economics	461	55	0.12	0.13
31	*J of the European Econ Assoc (2005)*	*80*	*9*	*0.11*	*0.12*
32	J of Urban Economics	350	37	0.11	0.11
33	Scandinavian Journal of Economics	227	23	0.10	0.11
34	Oxford Economic Papers	229	21	0.09	0.10
35	J of Economic Behavior & Org	508	44	0.09	0.09
36	Economica	234	20	0.09	0.09

Table 3, cont'd

Ambition-Adjusted Journal Ranking, 2008

	Journal	Articles	Adjusted cites	Impact factor	Relative impact
37	J of Risk & Uncertainty	167	14	0.08	0.09
38	Oxford Bulletin of Econ & Statistics	229	18	0.08	0.08
39	*Macroeconomic Dynamics (1998)*	*177*	*13*	*0.07*	*0.08*
40	Economic Inquiry	360	26	0.07	0.08
41	Economic Theory	651	46	0.07	0.08
42	Econometric Theory	356	25	0.07	0.08
43	J of Money, Credit & Banking	390	26	0.07	0.07
44	Canadian Journal of Economics	349	22	0.06	0.07
45	*J of Economic Geography (2002)*	*106*	*6*	*0.06*	*0.06*
46	*J of Business*	*302*	*17*	*0.06*	*0.06*
47	J of Economic History	214	12	0.06	0.06
48	J of Health Economics	375	20	0.05	0.06
49	J of Economic Dynamics & Control	636	32	0.05	0.05
50	International J of Industrial Org	441	22	0.05	0.05
51	J of International Money & Finance	304	15	0.05	0.05
52	J of Financial & Quantitative Analysis	234	10	0.04	0.05
53	Regional Science & Urban Economics	219	9	0.04	0.04
54	Economics Letters	1,736	70	0.04	0.04
55	J of Mathematical Economics	280	11	0.04	0.04
56	J of Policy Analysis & Management	239	8	0.03	0.04
57	J of Environ Econ & Management	341	10	0.03	0.03
58	National Tax Journal	205	6	0.03	0.03
59	Public Choice	555	13	0.02	0.03
60	J of Regional Science	201	4	0.02	0.02
61	J of Macroeconomics	235	3	0.01	0.01
62	Papers in Regional Science	172	2	0.01	0.01
63	Southern Economic Journal	370	4	0.01	0.01
64	J of Banking & Finance	703	7	0.01	0.01
65	Economic History Review	110	1	0.01	0.01
66	Annals of Regional Science	241	2	0.01	0.01
67	Contemporary Economic Policy	189	1	0.01	0.01
68	Applied Economics	1,459	7	0.00	0.01
69	J of Forecasting	225	1	0.00	0.00

NOTE: The impact factor is the number of adjusted citations per article. A relative impact is the impact factor relative to that of the *American Economic Review*. Italics indicate a journal for which data are incomplete for some years between 1995 and 2007. For newer journals, the years that the citation data begin are in parentheses. The *Journal of Business* ceased operation at the end of 2006.

Table 4

Ambition-Adjusted Journal Ranking Excluding Self-Citations, 2008

	Journal	Self-citations	Percent self-citations	Self-citation rate	Relative impact	Change in rank
1	Quarterly Journal of Economics	128	27.2	0.45	1.96	0
2	J of Political Economy	70	17.9	0.24	1.75	0
3	Review of Economic Studies	56	20.7	0.19	1.19	2
4	Econometrica	152	34.4	0.36	1.12	–1
5	American Economic Review	204	33.9	0.32	1.00	–1
6	J of Labor Economics				0.84	0
7	*J of Economic Growth (1999)*				*0.73*	*0*
8	American Economic Review P & P				0.49	2
9	Review of Economics & Statistics	58	30.2	0.13	0.48	–1
10	International Economic Review				0.46	1
11	J of Monetary Economics				0.44	1
12	Rand Journal of Economics				0.42	1
13	J of International Economics				0.41	1
14	J of Law & Economics				0.40	1
15	J of Economic Theory				0.40	1
16	J of Public Economics				0.36	1
17	Economic Journal	77	41.6	0.15	0.35	–8
18	*Review of Economic Dynamics (2001)*				*0.33*	*0*
19	J of Business & Economic Statistics				0.32	0
20	J of Finance				0.32	0

NOTE: Citations are adjusted to exclude citations from the journal to articles in the same journal. The percent of self-citations is self-citations relative to total citations, while the self-citation rate is the number of self-citations per article. A journal's relative impact is its impact factor relative to that of the *American Economic Review*. Italics indicate a journal for which data are incomplete for some years between 1995 and 2007. For newer journals, the years that the citation data begin are in parentheses. The change in rank is the difference between Tables 3 and 4.

The table also reports the change in rank between 2002 and 2008 for each journal. The first thing to note is the stability at the very top of the ranking, as the top six journals are exactly the same for the two periods. Beyond that, however, there was a great deal of movement for some journals.[11]

As mentioned earlier, because several new journals placed relatively well in the 2008 ranking, there will necessarily be some movement across the board as journals are bumped down the ranking by the entrants, none of which was ranked higher than 50th in 2002. In addition to the new journals, several journals made notable strides between 2002 and 2008. The *Journal of Law and Economics*, for example, moved from the 30th position in 2002 to the 15th position in 2008, while the *Journal of Financial Economics*, *Journal of Development Economics*, and *Journal of Industrial Economics* all moved into the top 30.

On the other hand, some journals experienced significant downward movement in their ranking. Three—the *Journal of Monetary Economics*, *Rand Journal of Economics*, and *Journal of Human Resources*—fell out of the top ten. Although the first two of these fell by only five positions, the

[11] The Spearman rank-correlation coefficient for the 2002 and 2008 rankings is 0.79.

Table 5

Change in Journal Ranking, 2002 to 2008

Journal	Rank 2002	Change 2002 to 2008	Journal	Rank 2002	Change 2002 to 2008
Quarterly Journal of Economics	1	0	J of Urban Economics	36	4
J of Political Economy	2	0	*J of Business*	*37*	*–9*
Econometrica	3	0	J of Health Economics	38	–10
American Economic Review	4	0	National Tax Journal	39	–19
Review of Economic Studies	5	0	J of Development Economics	40	10
J of Labor Economics	6	0	Regional Science & Urban Economics	41	–12
J of Monetary Economics	7	–5	J of Industrial Economics	42	16
Rand Journal of Economics	8	–5	J of Economic Behavior & Org	43	8
Review of Economics & Statistics	9	1	Scandinavian Journal of Economics	44	11
J of Human Resources	10	–18	J of Policy Analysis & Management	45	–11
Economic Journal	11	2	Canadian Journal of Economics	46	2
International Economic Review	12	1	J of Economic History	47	0
J of Economic Theory	13	–3	J of Risk & Uncertainty	48	11
American Economic Review P & P	14	4	International J of Industrial Org	49	–1
Games & Economic Behavior	15	–6	*J of Economic Growth (1999)*	*50*	*43*
J of Money, Credit & Banking	16	–27	J of International Money & Finance	51	0
J of Business & Economic Statistics	17	–2	Economics Letters	52	–2
J of Public Economics	18	1	J of Mathematical Economics	53	–2
J of Econometrics	19	–3	J of Financial & Quantitative Analysis	54	2
European Economic Review	20	–3	Public Choice	55	–4
J of International Economics	21	7	Southern Economic Journal	56	–7
J of Finance	22	2	*Macroeconomic Dynamics (1998)*	*57*	*18*
Review of Financial Studies	23	–1	Contemporary Economic Policy	58	–9
J of Applied Econometrics	24	–3	J of Macroeconomics	59	–2
J of Law, Economics & Organization	25	–4	J of Banking & Finance	60	–4
Econometric Theory	26	–16	Economic History Review	61	–4
Economica	27	–9	Papers in Regional Science	62	0
Economic Theory	28	–13	Applied Economics	63	–5
J of Economic Dynamics & Control	29	–20	J of Forecasting	64	–5
J of Law & Economics	30	15	J of Regional Science	65	5
Economic Inquiry	31	–9	Annals of Regional Science	66	0
J of Financial Economics	32	7	*Review of Economic Dynamics (2001)*	*67*	*49*
J of Environ Econ & Management	33	–24	*J of the European Econ Assoc (2005)*	*68*	*37*
Oxford Economic Papers	34	0	*J of Economic Geography (2002)*	*69*	*24*
Oxford Bulletin of Econ & Statistics	35	–3			

NOTE: Italics indicate a journal for which data are incomplete for some years between 1995 and 2007. For newer journals, the years that the citation data for these journals begin are in parentheses. The *Journal of Business* ceased operation at the end of 2006.

Journal of Human Resources fell from the tenth all the way to the 28th position. Still, no journal fell by as much as the *Journal of Money, Credit, and Banking*, which was the 16th-ranked journal in 2002 but the 43rd-ranked one in 2008. Finally, three journals—the *Journal of Economic Dynamics and Control*, *Economic Theory*, and *Econometric Theory*—dropped from among the 20th- to 30th-ranked journals to outside the top 40. Although it is well beyond our present scope to explain the movement in journal ranking over time, at least some of the movement appears to have been due to the entrant journals. The most successful of the entrants can be described in general terms as macro journals, and their effects on the positions of incumbent journals in the field do not seem to have been nugatory.

SUMMARY AND CONCLUSION

There is no such thing as "the" correct journal ranking. All journal rankings, even those using the seemingly objective LP procedure, are sensitive to the subjective decisions of their constructors. Whether it's the set of journals to consider, the ages of citations and articles to allow, or the question of including self-citations, a ranking is the outcome of many judgment calls. What would be most useful for the profession is an array of rankings for which the judgment calls are clearly laid out so that users can choose among them. Ideally, decisions of this sort would be made on the basis of the criteria by which the rankings are constructed, rather than whether or not the outcome of the ranking satisfied one's imperfectly informed priors. Clear expressions of the inputs and judgments would be of great use in achieving this ideal.

Our ranking is a contribution to this ideal scenario. We have chosen a clear rule for which citations to use and have laid out exactly what we have done with our citation data to obtain our ranking. Some of our judgments, such as not controlling for reference intensity, are a nod to transparency and ease of use over precision. Also, by including self-citations we have chosen one imperfect metric over another purely on the grounds of our own judgment. On the other hand, we have shown that the effects that these judgments have on our ranking are not major. Finally, given that Wall (2009) has shown that large mental error bands should be used with any journal ranking, we would have been comfortable with even more imprecision than we have allowed.

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Do Donors Care About Declining Trade Revenue from Liberalization? An Analysis of Bilateral Aid Allocation

Javed Younas and Subhayu Bandyopadhyay

Many developing-country governments rely heavily on trade tax revenue. Therefore, trade liberalization can be a potential source of significant fiscal instability and may affect government spending on development activities—at least in the short run. This article investigates whether donors use aid to compensate recipient nations for lost trade revenue or perhaps to reward them for moving toward freer trade regimes. The authors do not find empirical evidence supporting such motives. This is of some concern because binding government revenue constraints may hinder development prospects of some poorer nations. The authors use fixed effects to control for the usual political, strategic, and other considerations for aid allocations. (JEL F35, H0)

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After successive Uruguay Round negotiations and the creation of the World Trade Organization in the 1990s, many developing countries chose to dismantle their trade barriers and open their economies to international competition. Transition to free trade may involve substantial short-run costs for developing governments, especially in terms of a decline in tax revenues. Many developing countries rely heavily on trade tax revenue, and a reduction or elimination of these taxes may be a source of their fiscal instability. To the extent that public spending is targeted at useful programs (e.g., schools, infrastructure, health), the transition to free trade initially may result in a significant loss for a poor nation. In the long run, if liberalization is successful, these problems would be expected to be addressed both by provision of better private markets and rising revenues from different sources (income and sales taxes or possibly trade taxes owing to the volume effect) as a result of rising national income levels. However, even in the case of potentially successful liberalization, the donors may be concerned about the short-run budgetary implications of trade liberalization for the poorest of nations.

In principle, even in the short run, revenue losses from trade liberalization may be offset by turning to less-distortionary alternative sources of revenue. This approach requires good governance and an efficient domestic tax system; however, the evidence for this alternative is somewhat disheartening. For example, Baunsgaard and Keen (2005) argue that middle- and low-income countries fail to achieve substantial tax reforms to replace the lost trade revenue by revenue from other sources. They find that middle-income countries recovered 45 to 60 cents from other sources for every one-dollar loss in trade tax revenue, whereas low-income countries could recover no more than 30 cents for each lost dollar. Khattry

Javed Younas is a visiting assistant professor in the Department of Economics at Central Michigan University. Subhayu Bandyopadhyay is a research officer and economist at the Federal Reserve Bank of St. Louis and a research fellow at the Institute for the Study of Labor (IZA), Bonn, Germany. The authors thank Howard Wall for several helpful comments and suggestions.

and Rao (2002) find that in low-income countries revenue constraints remain even after a decade of trade reforms, and they emphasize the need for a fiscally realistic development strategy in the post-liberalization period. In a broader analysis of the limitations of trade policy reform in developing countries, Rodrik (1992) argues that tariff reduction at the cost of fiscal considerations can have disastrous consequences. He cites the examples of Turkey and Morocco, where trade taxes were reimposed because of fiscal problems.

The logic of compensating trade-liberalizing developing nations is consistent with the foreign aid objectives of reducing poverty and promoting economic development, captured in the Development Assistance Committee (DAC) guidelines for poverty reduction of the Organisation for Economic Co-operation and Development (OECD).[1] Moreover, donor nations may also be driven by the motivation to pursue their own economic interests in their potential export markets (see Dudley and Montmarquette, 1976; Neumayer, 2003; and Younas, 2008). Indeed, aid in "bailing out" liberalizing nations may also relate to the self-interest motive outlined in these contributions. Donors may worry that fiscal crisis may halt or reverse trade liberalization, which would not benefit the donors' export interests. Therefore, to maintain trade relations, they may compensate developing nations that experience a decline in trade tax revenues.[2]

Despite the sizeable literature in this broad area of trade and foreign aid, empirical analysis of the impact of trade liberalization and declining trade revenues on foreign aid allocation is sparse.[3] Most studies focus on the political and strategic interests of donors; others analyze their developmental and humanitarian concerns; and some investigate both aspects.[4] Recent studies have explored other aspects of donors' aid allocation, such as colonial ties of aid-recipient countries and support to donor countries in U.N. voting (Alesina and Dollar, 2000; Burnside and Dollar, 2000; and Kuziemko and Werker, 2006). Dollar and Levin (2004) find that overall more aid has been allocated to poor countries that have reasonably good economic governance. They find, however, that this pattern is somewhat different between bilateral and multilateral donors. We complement the literature by empirically investigating the effect of declining trade revenue on aid allocation decisions.

We estimate the effects of revenue collection (from import duties and international trade taxes) on aggregate bilateral aid allocation given by 22 DAC-member countries of the OECD to 52 aid-recipient countries over the 1991–2003 period.[5] We use fixed effects to control for the usual political, strategic, and other considerations for aid allocations. Our central finding is that there is no statistically significant evidence that supports the hypothesis that donors compensate for trade revenue losses of the recipients.

The remainder of the paper is organized as follows. The next section provides the empirical model and methodology, followed by the data description section. The third section presents the estimation results, and the final section contains our summary and conclusion.

THE EMPIRICAL MODEL AND METHODOLOGY

Three goals guide our sample selection. First, we include only middle- and low-income aid-recipient countries because past studies conclude that they face the highest uncompensated loss of tax trade revenue from trade liberalization

1 For example, see the guidelines at www.oecd.org/dataoecd/47/14/2672735.pdf.

2 Bagwell and Staiger (2001) analyze reciprocal and mutually advantageous trade liberalization agreements.

3 There are, however, some theoretical contributions on this issue. For example, Lahiri and Raimondos-Møller (1997) present a model in which recipient nations compete to give tariff concessions to donors to receive a larger share of foreign aid.

4 See, for example, McKinlay and Little (1977 and 1979), Maizels and Nissanke (1984), Dowling and Hiemenz (1985), Trumbull and Wall (1994), Wall (1995), Neumayer (2003), and Bandyopadhyay and Wall (2007).

5 The sample of aid-recipient countries is constrained by the non-availability of consistent yearly data, and most data are available only for years after 1990. Since many studies find that containment of communism rather than development concerns was a major factor for providing aid during the Cold War era, we limit our analysis to the post-Cold War period (see Boschini and Olofsgård, 2007).

(Baunsgaard and Keen, 2005; Khattry and Rao, 2002; Rodrik, 1992).[6] Second, we exclude Israel and Egypt from the data because both countries receive a disproportionately higher amount of aid from the United States, largely based on their strategic locations in the Middle East. Third, we limit our analysis to the post-Cold War period because containment of communism rather than development concerns was a major factor for providing aid during the Cold War era (see Boschini and Olofsgård, 2007).

Our empirical model of bilateral aid from 22 DAC-member countries of the OECD to 52 aid-recipient countries takes the following form:

(1)

$$\ln(baid)_{it} = \beta_0 + \beta_1 \ln(pop)_{it} + \beta_2 \left[\ln(pop)_{it}\right]^2 + \beta_3 \ln(inc)_{it} + \beta_4 \left[\ln(inc)_{it}\right]^2 + \beta_5 \ln(mor)_{it} + \beta_6 \ln(rights)_{it} + \beta_7 \ln(imd)_{it} + \beta_8 \ln(ttr)_{it} + \beta_8 \ln(maid)_{it} + \beta_i + \lambda_t + \mu_{it}.$$

where i = aid-recipient countries, t = years, and the following apply to each recipient country:

baid = total real bilateral aid
pop = population size
inc = per capita income
mor = the infant mortality rate
rights = level of political rights and civil liberties
imd = real revenue from import duties
ttr = real revenue from international trade taxes[7]
maid = total real multilateral aid
β_i = recipient-specific fixed effects
λ_t = year dummy variables
μ_{it} = error term

Aid and per capita income may be either substitutes or complements. They will be substitutes if compassion or altruism is the driving force. In this case, more aid is given when per capita income falls. In addition, we also introduce a squared term for per capita income to determine whether this effect increases for poorer nations. Population is included to capture the difference in recipient-country size (Bandyopadhyay and Wall, 2007). The sign of its coefficient will suggest whether a population-related bias exists in aid allocation.

Per capita income alone may not be an adequate reflection of economic need, especially in view of high income inequalities in several recipient countries. This prompted our use of the infant mortality rate, which relates to the concept of individual well-being. This variable is also used in the existing literature as a measure of a nation's well-being (Trumbull and Wall, 1994; Wall, 1995; Bandyopadhyay and Wall, 2007; Younas, 2008).[8-10] The political rights and civil liberties variable, which is used as a proxy for human rights, captures the donor's perception of the objective function of the recipient government: If a recipient government values human rights, the perception may be that it puts a higher weight on its peoples' welfare and would use the aid to improve their well-being.

We expect to see a negative relation between revenue from international trade taxes (or from import duties) and aid if donors either (i) compensate for revenue losses from trade liberalization or (ii) reward the nations that engage in such liberalization. We note, however, that these two motives are distinct in principle but observationally equivalent. Following Younas (2008) we also include real multilateral aid to a recipient

[6] The World Bank (2006) classifies aid-recipient countries into high-income non-OECD, middle-income, and low-income countries.

[7] Revenue from international trade taxes and from import duties is not included simultaneously in the regression because of very high correlation between them.

[8] The World Bank (2006) defines the infant mortality rate as the number of infants who die before 1 year of age per 1,000 live births in a given year.

[9] Per capita income captures economic needs, whereas infant mortality signifies physical needs (Trumbull and Wall, 1994; Wall, 1995; and Bandyopadhyay and Wall, 2007). Bandyopadhyay and Wall (2007) note that although economic and physical needs are clearly correlated in the long run, they do not necessarily move in the same direction over shorter periods.

[10] Correlations among the independent variables are not high, with the exception of that between per capita income and the infant mortality rate. To check whether multicollinearity poses a problem, eigenvalues for correlations among explanatory variables were tested and found to be low.

country.[11] Bilateral and multilateral aid may be either substitutes or complements. They will be substitutes if donor nations reduce their aid allocation to a recipient that also receives aid from multilateral agencies. They will be complements if donor nations provide more aid to maintain their political influence on a recipient.

To control for the usual political, strategic, and other considerations for aid allocations by donors, we introduce recipient-specific fixed effects in the model. Finally, we include time dummy variables that are common to all aid recipients within a given year. Time dummies control for events such as a flood or a drought within a particular year, which may lead to an aid spike for the corresponding year.[12] Moreover, all regressions are estimated using feasible generalized least squares allowing for recipient-specific heteroskedasticity.

Because most explanatory variables vary across a wide range (such as population size, per capita income, international trade tax revenue, and revenue from import duties) and exhibit skewed distributions, we use the natural log of all variables. Also, we use a log-log model to help reduce outlier effects, and the resulting coefficients are interpreted as elasticities.

Before proceeding to estimation, we address the possibility of simultaneous causation between aid and per capita income. It may be argued that per capita income of a recipient may be endogenous because it not only affects the donor's decision to provide aid but also may be affected by the flow of aid.[13] Wooldridge (2003) states that if we assume that the error term, μ_{it}, is uncorrelated (a standard assumption) with all past endogenous and exogenous variables, then lagged endogenous variables in simultaneous models are treated as predetermined variables and are uncorrelated with μ_{it}.[14] Following that technique, we use a 1-year-lagged value for all independent variables in our econometric model. This makes sense as information to the donors about a recipient is available only with some time lag (Younas, 2008). Thus, our empirical model in equation (1) takes the following form:

(2)

$$\begin{aligned}\ln(baid)_{it} &= \beta_0 + \beta_1 \ln(pop)_{i,t-1} + \beta_2 \left[\ln(pop)_{i,t-1}\right]^2 \\ &+ \beta_3 \ln(inc)_{i,t-1} + \beta_4 \left[\ln(inc)_{i,t-1}\right]^2 + \beta_5 \ln(mor)_{i,t-1} \\ &+ \beta_6 \ln(rights)_{i,t-1} + \beta_7 \ln(imd)_{i,t-1} + \beta_8 \ln(ttr)_{i,t-1} \\ &+ \beta_8 \ln(maid)_{i,t-1} + \beta_i + \lambda_t + \mu_{it}.\end{aligned}$$

DESCRIPTION OF DATA

The data for aggregate net bilateral aid to 52 recipient countries for the 1992–2003 period are from OECD *International Development Statistics* (OECD, 2005).[15] The data contain aid given for development purposes only and do not include grants, loans, and credits for military purposes.[16] Data for multilateral aid are also from the same source.

Data for revenue from international trade taxes and import duties are from *Government Finance Statistics* (International Monetary Fund [IMF], 2005) and *World Development Indicators*

[11] The multilateral aid is given by the World Bank, the International Monetary Fund (IMF), and the United Nations, including their regional branches.

[12] Inclusion of time-specific dummy variables allows each time period to have its own intercept for aggregate time effects that affect all recipients. Also, one time-specific dummy must be dropped to avoid perfect collinearity. We also drop one recipient-specific dummy in the fixed effects model for the same reason.

[13] Most literature on aid shows that aid does not cause growth. Thus, there is little reason to believe that there would be reverse causation from aid to per capita income.

[14] Maizels and Nissanke (1984), while citing Maddala (1977), state that "all estimation techniques, including 2SLS [two-stage least squares], are designed to deal only with the contemporaneous simultaneity and the lagged endogenous variables are treated in simultaneous models as predetermined variables along with other exogenous variables in the system." Therefore, if aid flows can be assumed to affect a country's economic performance with some time lag, the problem of simultaneous bias is considerably lessened.

[15] Bilateral aid is from 22 DAC-member countries of the OECD (Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, and the United States). Eight aid-recipient countries in our dataset received no aid or negative aid (net payer) for 1 or a maximum of 2 years in the 12 yearly time periods in our sample. We placed a value 0 for such observations.

[16] Following Neumayer (2003) and Younas (2008), we converted the aid data into constant year-2000 U.S. dollars using the unit value of the world import price index; data are from the United Nations Conference on Trade and Development (2005).

([*WDI*], World Bank, 2006). The revenue data are given in the national currency of each country. We have converted these data into U.S. dollars using the exchange rate for each country for each year.[17] As we did for the aid data, we also converted international trade tax revenue and import duties into constant year-2000 U.S. dollars using the unit value of the world import price index.[18] The data appendix shows the countries' average data on trade tax revenue variables, both in levels and also as ratios of total tax revenue. According to *WDI* (World Bank, 2006), taxes on international trade include import duties, export duties, profits of export or import monopolies, exchange profits, and exchange taxes, whereas import duties comprise all levies collected on goods at the point of entry into the country. The levies may be imposed for revenue or protection purposes and may be determined on a specific or ad valorem basis, as long as they are restricted to imported products.[19]

Per capita income is measured by per capita gross domestic product (purchasing power parity) at constant year-2000 U.S. dollars. Data for per capita GDP, population, and infant mortality rates are obtained from *WDI* (World Bank, 2006).[20] We use indices for political rights and civil liberties produced by Freedom House (2006) as a proxy for human rights measure. "Political rights" refer to the freedom of people to participate in the political process by exercising their voting rights, the right to organize political parties to compete for public office, and the ability to form an effective opposition and elect representatives who devise public policies and are accountable for their actions. "Civil liberties" entail the freedom of expression and religious belief, the prevalence of rule of law, the right to form unions, the freedom to marry, and the freedom to travel. It also signifies the autonomy of citizens without interference from the state. These two indicators are derived from a cross-country survey every year. Each of these indices is measured on a scale from 1 (best) to 7 (worst) points. Following the literature on aid (see, for example, Trumbull and Wall, 1994; Wall, 1995; Neumayer, 2003; and Younas, 2008), we constructed a combined freedom index by adding indices of political rights and civil liberties, and then reverted that index so that it ranges from 2 (worst) to 14 (best) points.

Figures 1 and 2 show the correlations between real bilateral aid and revenue from international trade taxes and revenue from import duties, respectively. The correlation pattern gives a crude idea that aid is concentrated mostly toward recipients with low revenue from import duties and international trade taxes. The correlation pattern of bilateral aid and ratios of revenue from international trade taxes to total tax revenue and the ratio of revenue from import duties to total tax revenue in Figures 3 and 4 also seem to suggest a somewhat similar pattern.

RESULTS

The correlations suggest higher aid allocation to countries experiencing a decline in international trade revenues. To ascertain the existence of any significant econometric relationship, we run regressions by simultaneously controlling for all explanatory variables in our model to find their individual effects on aid.

Model without Fixed Effects

We first estimate the model under the restriction that fixed effects for donors' considerations for aid allocations do *not* matter ($\beta_i = 0 \forall_i$). All regressions are estimated using feasible generalized least squares allowing for recipient-specific heteroskedasticity. The results are presented in

[17] Data for exchange rates are from *International Finance Statistics* (IMF, 2004).

[18] A few countries in our sample do not have complete observations for revenue from international trade taxes and import duties for all 12 yearly time periods.

[19] Because it cannot be stated with certainty that a decline in trade tax revenue is always the consequence of trade liberalization, we regressed the ratio of trade tax revenue to total tax revenue on the trade openness index devised by Sachs and Warner (1995). In addition, we used the economic globalization index of Dreher (2006). The results show that both trade openness and economic globalization cause a decline in trade tax revenue in developing countries (these results are available from the authors on request). Although these findings may not be conclusive, our study mainly aims to analyze whether donor countries compensate recipient countries for declining trade tax revenue.

[20] Some data values are missing for some countries for infant mortality rates. Because infant mortality rates change slowly over time, values for missing observations are interpolated by calculating averages from available values.

Figure 1

Correlation Between Aid and Revenue from Import Duties

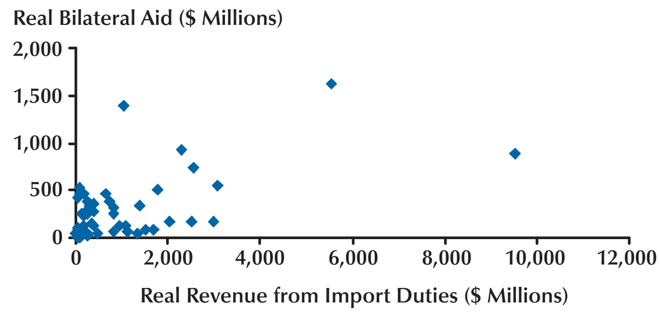
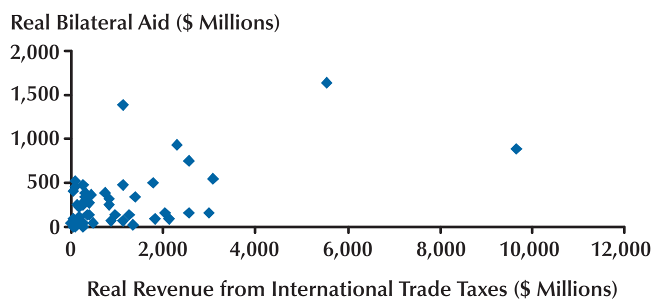


Figure 2

Correlation Between Aid and Revenue from International Trade Taxes



columns 1 and 2 of Table 1. Surprisingly, the effect of the key variable of interest—revenue from import duties—is positive and statistically significant at the 10 percent level. This peculiar result is somewhat confusing as it seems to suggest that more aid goes to recipients experiencing an increase in trade tax revenue. However, we cannot rely on this result because without controlling for fixed effects, the estimates will be biased and inconsistent. This effect can be more prevalent in our study because past literature reports on aid conclude that bilateral donors' political, strategic, and other considerations also determine their aid allocation decisions for such countries.

On the other hand, the impact of all other variables except political rights and civil liberties is statistically different from zero. According to *t*-statistics, their coefficients are significant at the 1 percent level. The hill-shaped relationship between aid and population suggests a bias in aid allocation toward less-populated developing countries (Bandyopadhyay and Wall, 2007). This also implies that countries such as India, China, and Pakistan remain at a disadvantage in garnering more aid because of their large populations. Per capita income and infant mortality rates appear to be important indicators of bilateral aid allocation. A 1 percent increase in the infant mortality rate has an impact of a 0.24 percent increase in aid allocation. The positive but quadratic relationship between aid and per capita income sug-

Figure 3

Correlation Between Aid and Ratio of Revenue from Import Duties to Total Tax Revenue

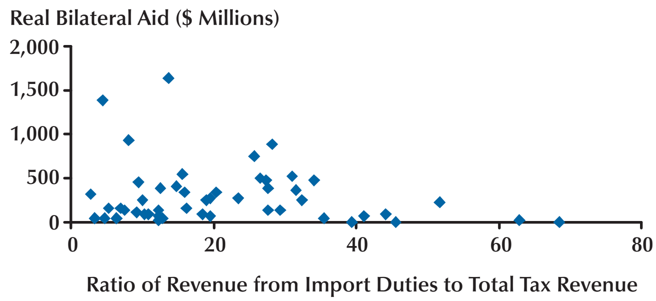
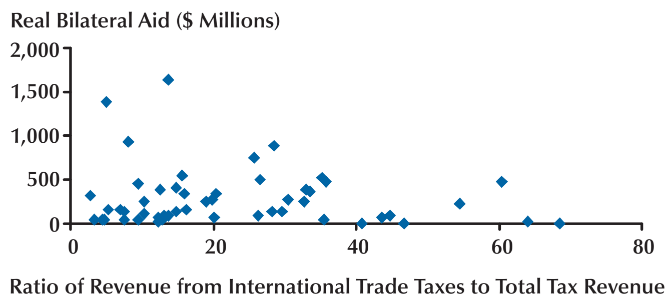


Figure 4

Correlation Between Aid and Ratio of Revenue from International Trade Taxes to Total Tax Revenue



gests that the donors do not favor the poorest of developing nations, although this pattern is reduced toward the higher end of the income scale. The level of aid is not responsive to political rights and civil liberties.

The positive and significant coefficient on multilateral aid suggests that donor countries provide more aid to recipient countries that also receive aid from multilateral agencies.[21] This result is also consistent with a previous finding (Younas, 2008). Omitting revenue from import duties from the regression and including revenue from international trade taxes yields similar results (Table 1, column 2).

Model with Fixed Effects

Now we estimate the model without imposing the restrictions that the fixed effects are all zero (columns 3 and 4 of Table 1). This is the preferred model because it controls for the donors' usual political, strategic, and other considerations of aid allocations. We find that only per capita income, its squared term, and multilateral aid are statistically significant. Trade revenue variables are statistically insignificant in both of the fixed-effects regressions. This suggests that donors do not appear to consider recipients' trade revenue levels in their aid allocation decisions.

[21] Because bilateral donors provide funds to multilateral agencies (e.g., World Bank, IMF, United Nations), their likely influence over aid allocation decisions of these agencies may also be a factor that affects this result.

Table 1

Dependent Variable: ln(*Bilateral aid*)

Independent variables	Without fixed effects [1]	Without fixed effects [2]	With fixed effects [3]	With fixed effects [4]
ln(*Population*)	0.972	0.925	1.952	1.969
	(4.67)***	(4.40)***	(0.71)	(0.69)
[ln(*Population*)]^2	–0.019	–0.017	–0.140	–0.148
	(3.11)***	(2.85)***	(1.55)	(1.61)
ln(*Per capita income*)	6.254	6.062	11.714	11.825
	(7.49)***	(7.24)***	(4.93)***	(4.86)***
[ln (*Per capita income*)]^2	–0.408	–0.397	–0.784	–0.796
	(7.57)***	(7.35)***	(5.14)***	(5.11)***
ln(*Infant mortality*)	0.244	0.215	0.033	0.039
	(3.04)***	(2.61)***	(0.13)	(0.15)
ln(*Political and civil rights*)	0.045	0.055	–0.113	–0.105
	(0.63)	(0.77)	(0.79)	(0.74)
ln(*Import duties revenue*)	0.059	—	0.059	—
	(1.81)*		(0.88)	
ln(*International trade tax revenues*)	—	0.067	—	0.069
		(2.02)**		(0.96)
ln(*Multilateral aid*)	0.154	0.146	0.074	0.075
	(5.70)***	(5.32)***	(3.17)***	(3.26)***
Recipient fixed effects	No	No	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Estimated coefficients	20	20	71	71
Wald chi-square	1,121.94	1,084.11	3,433.25	3,408.02
Log likelihood	–767.10	–766.62	–550.42	–552.62
Observations	555	555	555	555

NOTE: Estimated using feasible generalized least squares allowing for recipient-specific heteroskedasticity. ***, ** and * indicate significance at the 1, 5, and 10 percent levels, respectively. See text for detailed explanations of columns 1 through 4.

This evidence is somewhat disheartening because past studies have found that most revenue losses from international trade taxes in developing countries are not compensated from other domestic sources (Baunsgaard and Keen, 2005; Khattry and Rao, 2002; Rodrik, 1992). The likelihood-ratio test also rejects the null hypothesis that the fixed effects are all zero, implying that this is the statistically superior model. It is also the preferred model because it controls for donors' usual political, strategic, and other considerations of aid allocations.

As a robustness check, we also derive the estimation from another angle. It is possible that donors may consider the ratio of revenue from import duties to total tax revenue and/or the ratio of revenue from international trade taxes to total tax revenue in making aid allocation decisions. For this purpose, we omit the revenue variables in levels and include the ratios (see Table 2). Interestingly, the findings without fixed effects for both import duties and international trade taxes in ratios now show the expected negative sign and that they are also statistically significant (columns 1 and 2). However, when we introduce fixed effects in this context, we again find no significant relation (columns 3 and 4). Based on

Table 2

Dependent Variable: ln(*Bilateral aid*)

Independent variables	Without fixed effects [1]	Without fixed effects [2]	With fixed effects [3]	With fixed effects [4]
ln(*Population*)	0.737	0.777	1.238	–0.548
	(3.82)***	(4.04)***	(0.50)	(0.23)
[ln(*Population*)]^2	–0.012	–0.013	–0.127	–0.106
	(2.05)**	(2.23)**	(1.56)	(1.34)
ln(*Per capita income*)	7.785	7.664	7.137	7.240
	(10.03)***	(9.65)***	(3.79)***	(3.82)***
[ln (*Per capita income*)]^2	–0.498	–0.492	–0.523	–0.526
	(9.76)***	(9.44)***	(4.30)***	(4.29)***
ln(*Infant mortality*)	0.512	0.463	0.032	0.019
	(6.46)***	(5.71)***	(0.13)	(0.07)
ln(*Political and civil rights*)	0.109	0.107	–0.257	–0.275
	(1.68)*	(1.64)	(1.98)**	(2.10)**
Import duties revenue/tax revenue	–0.014	—	–0.009	—
	(6.06)***		(1.61)	
International trade tax revenue/tax revenue	—	–0.012	—	–0.005
		(5.53)***		(0.88)
ln(*Multilateral aid*)	0.166	0.158	0.086	0.088
	(6.33)***	(5.86)***	(3.51)***	(3.62)***
Recipient fixed effects	No	No	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes
Estimated coefficients	20	20	71	71
Wald chi-square	1,745.43	1,600.40	4,121.71	4,189.03
Log likelihood	–734.99	–738.44	524.33	–526.12
Observations	555	555	555	555

NOTE: Estimated using feasible generalized least squares allowing for recipient-specific heteroskedasticity. ***, ** and * indicate significance at the 1, 5, and 10 percent levels, respectively. See text for detailed explanations of columns 1 through 4.

likelihood-ratio tests, the fixed-effects model is the preferred specification. This further strengthens our finding that aid is not responsive to declining trade revenues in developing countries.

CONCLUSION

Although trade liberalization results in greater economic efficiency and growth, it is also a potential source of fiscal instability in developing countries because they rely heavily on revenue from trade taxes. There is a realization among developed nations that trade-related technical and financial assistance should be extended to mitigate detrimental effects of trade reforms in developing countries.

This article examines whether this trade revenue compensation motive is observed in donor behavior. We use aggregate bilateral aid data from 22 DAC countries to 52 aid-recipient countries over the 1991-2003 period. Using fixed effects to control for donors' political, strategic, and other considerations, we find no significant relationship between aid allocation decisions and trade revenues of recipient nations. This suggests that the governments of developing nations may face significant short-run challenges in the form of revenue constraints as a result of declining revenue collections from trade liberalization.

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APPENDIX

Data for Trade Tax Revenue Variables (Country Averages, 1991-2003, $ Millions)

Countries	Real revenue from international trade taxes	Real revenue from import duties	Ratio of international trade tax to total tax revenue	Ratio of import duties revenue to total tax revenue
Algeria	2,021.8	2,021.8	16.2	16.2
Argentina	2,122.6	1,685.4	13.8	10.3
Bhutan	1.2	0.9	4.4	3.2
Bolivia	67.7	67.7	9.5	9.5
Botswana	279.5	279.4	35.4	35.4
Brazil	2,534.6	2,533.8	5.5	5.5
Bulgaria	183.9	165.7	10.4	9.2
Burundi	35.5	25.2	26.2	18.5
Cameroon	310.0	263.3	33.0	27.6
China	5,552.2	5,552.2	13.6	13.6
Colombia	820.5	813.1	10.3	10.1
Congo DR	79.5	73.5	35.3	31.0
Côte d'Ivoire	1,124.9	640.6	60.4	34.1
Croatia	491.8	491.8	13.0	13.0
Dominican Republic	872.0	820.6	43.7	41.0
Ethiopia	247.8	188.0	35.7	27.4
Hungary	1,119.8	1,119.8	12.3	12.3
India	9,643.6	9,540.5	28.5	28.1
Indonesia	1,126.5	1,041.3	4.9	4.5
Jordan	436.3	409.1	33.6	31.6
Kenya	321.0	321.0	16.0	16.0
Latvia	29.3	29.3	3.4	3.4
Lithuania	43.7	43.0	4.8	4.6
Madagascar	174.8	167.3	54.6	51.7
Malaysia	1,839.5	1,536.4	13.1	10.9
Maldives	40.6	40.1	64.1	62.9
Mauritius	259.7	251.4	40.8	39.3

SOURCE: *Government Finance Statistics* (IMF, 2005) and *World Development Indicators* (World Bank, 2006).

APPENDIX, cont'd

Data for Trade Tax Revenue Variables (Country Averages, 1991-2003, $ Millions)

Countries	Real revenue from international trade taxes	Real revenue from import duties	Ratio of international trade tax to total tax revenue	Ratio of import duties revenue to total tax revenue
Mexico	2,977.3	2,971.2	7.1	7.0
Moldova	20.9	14.5	9.5	6.3
Morocco	1,407.9	1,405.8	20.5	20.5
Nepal	119.6	118.0	32.7	32.4
Nicaragua	54.9	54.9	14.9	14.9
Oman	129.6	129.6	12.3	12.3
Pakistan	1,766.4	1,766.4	26.6	26.6
Papua New Guinea	259.4	196.7	30.5	23.5
Paraguay	152.8	150.9	20.1	19.7
Peru	750.7	747.7	12.6	12.5
Philippines	2,559.4	2,545.9	25.8	25.7
Poland	2,306.7	2,306.7	8.1	8.1
Romania	406.5	404.8	7.7	7.6
Seychelles	103.7	103.7	68.6	68.6
Sierra Leone	30.7	30.3	44.8	44.2
South Africa	842.9	810.6	2.9	2.8
Sri Lanka	405.7	398.8	19.9	19.5
St. Vincent and the Grenadines	33.9	33.1	46.6	45.6
Syria	1,274.0	1,061.8	14.8	12.3
Thailand	3,083.6	3,080.9	15.7	15.7
Tunisia	967.2	947.7	28.3	27.8
Uruguay	201.3	178.3	7.5	6.6
Venezuela	1,336.3	1,332.4	12.4	12.4
Yemen	337.2	334.9	29.5	29.3
Zimbabwe	281.1	277.3	19.1	18.9

SOURCE: *Government Finance Statistics* (IMF, 2005) and *World Development Indicators* (World Bank, 2006).

Supply Shocks, Demand Shocks, and Labor Market Fluctuations

Helge Braun, Reinout De Bock, and Riccardo DiCecio

The authors use structural vector autoregressions to analyze the responses of worker flows, job flows, vacancies, and hours to demand and supply shocks. They identify these shocks by restricting the short-run responses of output and the price level. On the demand side, they disentangle a monetary and nonmonetary shock by restricting the response of the interest rate. The responses of labor market variables are similar across shocks: Expansionary shocks increase job creation, the job-finding rate, vacancies, and hours; and they decrease job destruction and the separation rate. Supply shocks have more persistent effects than demand shocks. Demand and supply shocks are equally important in driving business cycle fluctuations of labor market variables. The authors' findings for demand shocks are robust to alternative identification schemes involving the response of labor productivity at different horizons. Supply shocks identified by restricting productivity generate a higher fraction of impulse responses inconsistent with standard search and matching models. (JEL C32, E24, E32, J63)

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Hall (2005) and Shimer (2004) argue that the search and matching model of Mortensen and Pissarides (1994) is unable to reproduce the volatility of the job-finding rate, unemployment, and vacancies observed in the data.[1] A growing literature has attempted to amend the basic Mortensen-Pissarides model to match these business cycle facts.[2] Although most of this literature considers shocks to labor productivity as the source of fluctuations, some authors invoke the responses to other shocks as a potential resolution (see Silva and Toledo, 2005). These analyses are based on the assumption that either the unconditional moments are driven to a large extent by a particular shock or the responses of the labor market to different shocks are similar. This article takes a step back and asks, What are the contributions of different aggregate shocks to labor market fluctuations and how different are the labor market responses to various shocks? The labor market variables we analyze are worker flows, job flows, vacancies, and hours. Including both worker flows and job flows allows us to analyze the different conclusions authors have reached on the importance of the hiring versus the separa-

[1] Also see Andolfatto (1996).

[2] See, for example, Hagedorn and Manovskii (2008) and Mortensen and Nagypál (2005).

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tion margin in driving changes in employment and unemployment. Including aggregate hours relates our work to the literature on the response of hours to technology shocks.

We identify three aggregate shocks—supply shocks, monetary shocks, and nonmonetary demand shocks—using a structural vector autoregression (structural VAR, or SVAR). Restrictions are placed on the signs of the dynamic responses of aggregate variables as in Uhlig (2005) and Peersman (2005). The first identification scheme we consider places restrictions on the short-run responses of output, the price level, and the interest rate. Supply shocks move output and the price level in opposite directions, while demand shocks generate price and output responses of the same sign. Additionally, monetary shocks lower the interest rate on impact; other demand shocks do not. These restrictions can be motivated by a basic IS-LM-AD-AS framework or by New Keynesian models. The responses of job flows, worker flows, hours, and vacancies are left unrestricted.

The main results for the labor market variables are as follows: The responses of hours, job flows, worker flows, and vacancies are qualitatively similar across shocks. A positive demand or supply shock increases vacancies and the job-finding and job-creation rates, and it decreases the separation and job-destruction rates. As in Fujita (2004), the responses of vacancies and the job-finding rate are persistent and hump shaped. Furthermore, the responses induced by demand shocks are less persistent than those induced by supply shocks. For all shocks, changes in the job-finding rate are responsible for the bulk of changes in unemployment, although separations contribute up to one half of the change on impact. Changes in employment, on the other hand, are mostly driven by the job-destruction rate. As in Davis and Haltiwanger (1999), we find that job reallocation falls after expansionary shocks, especially demand-side shocks. We find no evidence of differences in the matching process of unemployed workers and vacancies in response to different shocks. Finally, each of the demand-side shocks is at least as important as the supply-side shock in explaining fluctuations in labor market variables.

There is mild evidence in support of a technological interpretation of the supply shocks identified by restricting output and the price level. The response of labor productivity is positive for supply shocks at medium-term horizons, whereas it is insignificantly different from zero for demand shocks. To check the robustness of our results, we modify the identification scheme by restricting the medium-run response of labor productivity to identify the supply-side shock, while leaving the short-run responses of output and the price level unrestricted. This is akin to a long-run restriction on the response of labor productivity used in the literature (see Galì, 1999). Consistent with the first identification scheme, technology shocks tend to raise output and decrease the price level in the short run. Labor market responses to supply shocks under this identification scheme are less apparent. In particular, the responses of vacancies, worker flows, and job flows to supply shocks are not significantly different from zero. Again, the demand-side shocks are at least as important in explaining fluctuations in the labor market variables as the supply shock.

We also identify a technology shock, using a long-run restriction on labor productivity, and a monetary shock, by means of the recursiveness assumption used by Christiano, Eichenbaum, and Evans (1999). Again, we find that the responses to the technology shock are not significantly different from zero. The responses to the monetary shock are consistent with the ones identified above. The contribution of the monetary shock to the variance of labor market variables exceeds that of the technology shock.

We also analyze the subsample stability of our results. We find a reduction in the volatility of shocks for the post-1984 subsample, consistent with the Great Moderation literature. The main conclusions from the analysis above apply to both subsamples.

Finally, we use a small VAR that includes only non-labor market variables and hours to identify the shocks. We then uncover the responses of the labor market variables by regressing them on distributed lags of the shocks.[3] Our findings are robust to this alternative empirical strategy.

[3] This procedure is used by Beaudry and Portier (2004) to analyze the effects of news shocks identified in a small VAR including only an index of stock market value and total factor productivity on other variables of interests, such as consumption and investment.

Our results suggest that a reconciliation of the Mortensen-Pissarides model should equally apply to the response of labor market variables to demand-side shocks. Furthermore, the response to supply-side shocks is much less clear cut than implicitly assumed in the bulk of the literature. In a related paper (Braun, De Bock, and DiCecio, 2006) we further explore the labor market responses to differentiated supply shocks (see also López-Salido and Michelacci, 2007).

Our findings suggest that the "hours debate" spawned by Galì (1999) is relevant for business cycle models with a frictional labor market à la Mortensen-Pissarides. In trying to uncover the source of business cycle fluctuations, several authors have argued that a negative response of hours worked to supply shocks is inconsistent with the standard real business cycle (RBC) model. These results are often interpreted as suggesting that demand-side shocks must play an important role in driving the cycle and are used as empirical support for models that depart from the RBC standard by incorporating nominal rigidities and other frictions. We provide empirical evidence on the response of job flows, worker flows, and vacancies. This is a necessary step to evaluate the empirical soundness of business cycle models with a labor market structure richer than the competitive structure typical of the RBC models or the stylized sticky wages structure often adopted in New Keynesian models. The importance of demand shocks in driving labor market variables and the atypical responses to supply shocks can be interpreted as a milder version of the "negative response of hours" findings.

In the next sections, we describe the data used in the analysis and the identification procedure and then discuss our results. The final section contains the robustness analysis.

WORKER FLOWS AND JOB FLOWS DATA

Worker flows are measured by the separation and job-finding rates constructed by Shimer (2007). Their construction is summarized in the next subsection. The following subsections discuss job flows—which are measured by the job-creation and job-destruction series constructed by Faberman (2004) and Davis, Faberman, and Haltiwanger (2006)—and the business cycle statistics of the data.

Separation and Job-Finding Rates

The separation rate measures the rate at which workers leave employment and enter the unemployment pool. The job-finding rate measures the rate at which unemployed workers exit the unemployment pool. Although the rates are constructed and interpreted while omitting flows between labor market participation and nonparticipation, Shimer (2007) shows that they capture most of the behavior of both the unemployment and employment pools over the business cycle. The advantage of using these data lies in their availability for a long time span. The data constructed by Shimer are available from 1947, whereas worker flow data including nonparticipation flows from the Current Population Survey (CPS) are available only from 1967 onward.

The separation and job-finding rates are constructed using data on the short-term unemployment rate as a measure of separations and the law of motion for the unemployment rate to back out a measure of the job-finding rate. The size of the unemployment pool is observed at discrete dates t, $t+1$, $t+2$, etc. Hirings and separations occur continuously between these dates. To identify the relevant rates within a time period, assume that between dates t and $t+1$, separations and job finding occur with constant Poisson arrival rates s_t and f_t, respectively. For some $\tau \in (0,1)$, the law of motion for the unemployment pool $U_{t+\tau}$ is

$$\dot{U}_{t+\tau} = E_{t+\tau}s_t - U_{t+\tau}f_t, \quad (1)$$

where $E_{t+\tau}$ is the pool of employed workers and $E_{t+\tau}s_t$ are the inflows and $U_{t+\tau}f_t$ the outflows from the unemployment pool at $t+\tau$. The analogous expression for the pool of short-term unemployed $U^s_{t+\tau}$ (i.e., those workers who have entered the unemployment pool after date t) is:

$$\dot{U}^s_{t+\tau} = E_{t+\tau}s_t - U^s_{t+\tau}f_t. \quad (2)$$

Combining expressions (1) and (2) gives

$$(3) \qquad \dot{U}_{t+\tau} = \dot{U}^s_{t+\tau} - \left(U_{t+\tau} - U^s_{t+\tau}\right) f_t.$$

Solving the differential equation using $U^s_t = 0$ as the initial condition yields

$$U_{t+1} = U_t e^{-f_t} + U^s_{t+1}.$$

Given data on U_t, U_{t+1}, and U^s_{t+1}, the last expression is used to construct the job-finding rate, f_t. The separation rate then follows from

$$(4) \qquad U_{t+1} = \left(1 - e^{-f_t - s_t}\right) \frac{s_t}{f_t + s_t} L_t + e^{-f_t - s_t} U_t,$$

where $L_t \equiv (U_t + E_t)$ is the labor force. Notice that the rates s_t and f_t are time-aggregation–adjusted versions of U^s_{t+1}/E_{t+1} and $(U_t - U_{t+1} + U^s_{t+1})/U_{t+1}$, respectively. The construction of s_t and f_t takes into account that workers may experience multiple transitions between dates t and t+1. These rates are continuous-time arrival rates and the corresponding probabilities are $S_t = (1 - e^{-s_t})$ and $F_t = (1 - e^{-f_t})$, respectively.

Using equation (4), observe that if $(f_t + s_t)$ is large, the unemployment rate, U_{t+1}/L_t, can be approximated by the steady-state relationship $u_{t+1} \cong s_t/(s_t + f_t)$. As shown by Shimer (2007), this turns out to be an accurate approximation to the actual unemployment rate. We use this approximation to infer changes in unemployment from the responses of f_t and s_t in the SVAR. To gauge the relative importance of the job-finding and separation rates in determining unemployment, we follow Shimer (2007) and construct the following variables:

- $s_t/(s_t + f_t)$ is the approximated unemployment rate;
- $\bar{s}_t/(\bar{s}_t + f_t)$ is the hypothetical unemployment rate computed with the actual job-finding rate, f_t, and the average separation rate, $\bar{s}$;
- $s_t/(s_t + \bar{f})$ is the hypothetical unemployment rate computed with the average job-finding rate, $\bar{f}$, and the actual separation rate, s_t.

Inflows into the employment pool are measured by the job-finding rate and not, as in Fujita (2004), by the hiring rate. The hiring rate sums all worker flows into the employment pool and scales them by current employment. Its construction is analogous to the job-creation rate defined for job flows. The response of the hiring rate to shocks is in general not very persistent, as opposed to that of the job-finding rate. This difference is due to the scaling. We discuss this point in more detail below.

Job Creation and Job Destruction

The job flows literature focuses on job-creation (JC) and job-destruction (JD) rates.[4] Gross job creation sums employment gains at all plants that expand or start up between t–1 and t. Gross job destruction, on the other hand, sums up employment losses at all plants that contract or shut down between t–1 and t. To obtain the creation and destruction rates, both measures are divided by the averages of employment at t–1 and t. Davis, Haltiwanger, and Schuh (1996) construct measures for both series from the Longitudinal Research Database (LRD) and the monthly Current Employment Statistics (CES) survey from the Bureau of Labor Statistics (BLS).[5] A number of researchers work only with the quarterly job-creation and job-destruction series from the LRD.[6] Unfortunately, these series are available only for the 1972:Q1–1993:Q4 period.

This paper uses the quarterly job flows data constructed by Faberman (2004) and Davis, Faberman, and Haltiwanger (2006). These authors splice together data from (i) the Manufacturing Turnover Survey (MTD) from 1947 to 1982, (ii)

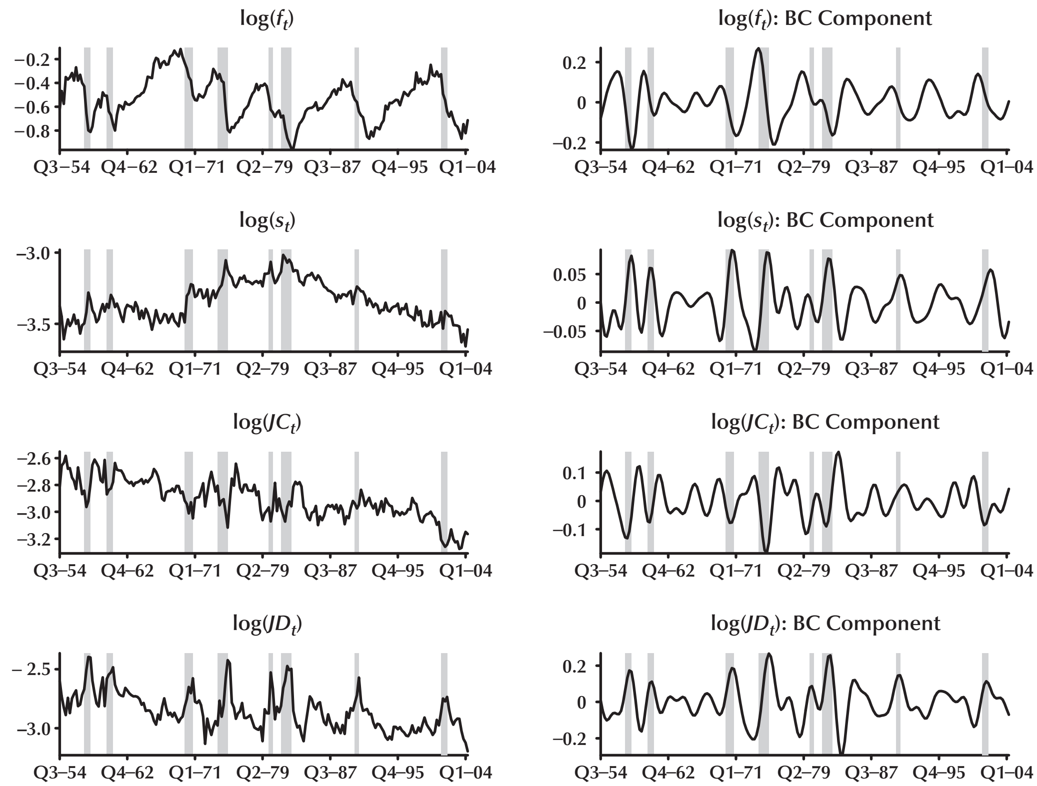
[4] See Davis and Haltiwanger (1992), Davis, Haltiwanger, and Schuh (1996), Davis and Haltiwanger (1999), Caballero and Hammour (2005), and López-Salido and Michelacci (2007).

[5] As pointed out in Blanchard and Diamond (1990) these job-creation and -destruction measures differ from true job creation and destruction as (i) they ignore gross job creation and destruction within firms, (ii) the point-in-time observations do not take into account job-creation and -destruction offsets within the quarter, and (iii) they fail to account for newly created jobs that are not yet filled with workers.

[6] Davis and Haltiwanger (1999) extend the series back to 1948. Some authors report that this extended series is (i) somewhat less accurate and (ii) tracks only aggregate employment in the 1972:Q1–1993:Q4 period (see Caballero and Hammour, 2005).

Figure 1

Worker and Job Flows: Levels and Business Cycle Components



NOTE: The business cycle component is extracted with a BP(8,32) filter. Shaded areas denote the NBER recessions.

the LRD from 1972 to 1998, and (iii) the Business Employment Dynamics (BED) from 1990 to 2004. The MTD and LRD data are spliced as in Davis and Haltiwanger (1999), whereas the LRD and BED splice follows Faberman (2004).

A fundamental accounting identity relates the net employment change between any two points in time to the difference between job creation and destruction. We define $g_{E,t}^{JC,JD}$ as the growth rate of employment implied by job flows:

$$(5) \qquad g_{E,t}^{JC,JD} \equiv \frac{E_t - E_{t-1}}{\left(E_t + E_{t-1}\right)/2} = JC_t - JD_t.$$

The data spliced from the MTD and LRD of the job-creation and -destruction rates constructed by Davis, Faberman, and Haltiwanger (2006) pertain to the manufacturing sector. However, over the period 1954:Q2–2004:Q2, the implied growth rate of employment from these job flows data, $g_{E,t}^{JC,JD} = (JC_t - JD_t)$, is highly correlated with the growth rate of total nonfarm payroll employment,

$$g_{E,t} \equiv \left[\frac{E_t - E_{t-1}}{0.5\left(E_t + E_{t-1}\right)}\right]: Corr\left(g_{E,t}^{JC,JD}, g_{E,t}\right) = 0.89.^{7}$$

As in Davis, Haltiwanger, and Schuh (1996), we also define gross job reallocation as $r_t \equiv (JC_t + JD_t)$. Using this definition we examine the reallocation effects of different shocks in the SVARs. We also look at cumulative reallocation.

[7] The correlation of $g_{E,t}^{JC,JD}$ with the growth rate of employment in manufacturing is 0.93.

Table 1

Correlation Matrix of Business Cycle Components

	f	*s*	*JC*	*JD*	*u*	*v*	*h*	*APL*	*y*
f	6.27 [5.54,6.99]	–0.48 [–0.63,–0.29]	–0.04 [–0.24,0.15]	–0.53 [–0.66,–0.39]	–0.98 [–0.99,–0.96]	0.95 [0.93,0.97]	0.96 [0.93,0.98]	0.20 [–0.03,0.4]	0.88 [0.81,0.92]
s		2.55 [2.21,2.99]	–0.55 [–0.68,–0.37]	0.86 [0.78,0.91]	0.54 [0.39,0.66]	–0.62 [–0.73,–0.49]	–0.48 [–0.61,–0.33]	–0.63 [-0.77,–0.44]	–0.67 [–0.78,–0.53]
JC			4.26 [3.61,4.97]	–0.58 [–0.7,–0.41]	0.08 [–0.10,0.26]	0.04 [–0.17,0.24]	–0.11 [–0.3,0.09]	0.53 [0.33,0.68]	0.14 [–0.11,0.36]
JD				6.73 [5.89,7.59]	0.53 [0.40,0.63]	–0.65 [–0.76,–0.53]	–0.53 [–0.66,–0.39]	–0.70 [–0.82,–0.54]	–0.72 [–0.84,–0.58]
u					7.27 [6.39,8.24]	–0.95 [–0.96,–0.93]	–0.95 [–0.97,–0.92]	–0.18 [–0.38,0.01]	–0.86 [–0.90,–0.81]
v						8.84 [8.13,9.78]	0.95 [0.94,0.97]	0.34 [0.14,0.53]	0.94 [0.9,0.96]
h							1.10 [1.01,1.19]	0.17 [–0.06,0.38]	0.89 [0.84,0.93]
APL								0.65 [0.56,0.77]	0.58 [0.43,0.7]
y									1 [NA]

NOTE: Standard deviations (relative to output) are shown on the diagonal. All series were logged and detrended using a BP(8,32) filter. Block-bootstrapped confidence intervals in brackets.

Business Cycle Properties

Figure 1 shows the levels and business cycle components[8] of worker and job flows along with the National Bureau of Economic Research (NBER) recession dates. Table 1 reports correlations and standard deviations (relative to output) for the business cycle component of worker flows, job flows, the unemployment rate (u), vacancies (v), hours per capita (h), average labor productivity (APL), and output (y).[9] The job-finding rate and vacancies are strongly procyclical, with correlations with output of 0.88 and 0.96, respectively. Job creation is moderately procyclical (0.14). The separation (–0.67), job-destruction (–0.72), and the unemployment (–0.86) rates are countercyclical. The diagonal of Table 1 reports volatilities. The job-destruction rate (6.73) is one and a half times more volatile than the job creation rate (4.26). The job-finding rate (6.27) is twice as volatile as the separation rate (2.55). Notice that the job-destruction and separation rates are positively correlated (0.86), whereas the job-creation and job-finding rates are orthogonal to each other (–0.04).

Table 2 reports correlations of the three unemployment approximations described in the subsection "Separation and Job-Finding Rates" with actual unemployment, as well as the standard deviations of the three approximations (relative to actual unemployment). The steady-state approximation to unemployment, $s_t/(s_t + f_t)$, is very accurate and the job-finding rate plays a bigger role in determining unemployment. The contribution of the job-finding rate is even larger at cyclical frequencies.[10]

[8] We used the band-pass filter described in Christiano and Fitzgerald (2003) for frequencies between 8 and 32 quarters to extract the business cycle component of the data.

[9] See the appendix for data sources.

[10] Shimer (2005) uses a Hodrick-Prescott filter with smoothing parameter 10^5. His choice of an unusual filter to detrend the data further magnifies the contribution of the job-finding rate to unemployment with respect to the figures we report.

Table 2

Contribution of the Job-Finding and Separation Rates to Unemployment: Levels and Business Cycle Components

	Levels			Business cycle component		
	$s_t/(s_t + f_t)$	$\bar{s}/(\bar{s} + f_t)$	$s_t/(s_t + \bar{f})$	$s_t/(s_t + f_t)$	$\bar{s}/(\bar{s} + f_t)$	$s_t/(s_t + \bar{f})$
$Corr(x,u_{t+1})$	0.99	0.85	0.79	0.99	0.93	0.74
	[0.99,1]	[0.76,0.92]	[0.64,0.87]	[0.99,1]	[0.90,0.95]	[0.62,0.82]
$Std(x)/Std(u_{t+1})$	1.01	0.69	0.49	1.03	0.79	0.31
	[1,1.03]	[0.6,0.82]	[0.42,0.58]	[1.01,1.05]	[0.73,0.86]	[0.28,0.36]

NOTE: The business cycle component is extracted with a BP(8,32) filter. Block-bootstrapped confidence intervals in brackets.

SVAR ANALYSIS

This section describes the reduced-form VAR specification and provides an outline of the Bayesian implementation of sign restrictions. The variables included in the SVAR analysis are the growth rate of average labor productivity ($\Delta\log(Y_t/H_t)$), the inflation rate ($\Delta\log(p_t)$), hours ($\Delta\log(H_t)$), worker flows, job flows, a measure of vacancies ($\Delta\log(v_t)$), and the federal funds rate ($\log(1 + R_t)$). Worker flows are the job-finding and separation rates constructed in Shimer (2007). Job flows are the job-creation and job-destruction series from Faberman (2004) and Davis, Faberman, and Haltiwanger (2006). Sources for the other data are given in the appendix. The sample covers the period 1954:Q2–2004:Q2. To achieve stationarity, we linearly detrend the logarithms of the job flows variables. The estimated VAR coefficients corroborate the stationarity assumption.

Consider the following reduced-form VAR[11]:

$$Z_t = \mu + \sum_{j=1}^{p} B_j Z_{t-j} + u_t,\ E\left(u_t u_t'\right) = V, \tag{6}$$

where Z_t is defined as

$$Z_t = \begin{bmatrix} \Delta\log\left(\frac{Y_t}{H_t}\right), \Delta\log\left(p_t\right), \log\left(H_t\right), \\ \log\left(f_t\right), \log\left(s_t\right), \log\left(JC_t\right), \\ \log\left(JD_t\right), \log\left(v_t\right), \ln\left(1 + R_t\right) \end{bmatrix}'.$$

The reduced-form residuals (u_t) are mapped into the structural shocks (ε_t) by the structural matrix (A_0) as follows: $\varepsilon_t = A_0 u_t$. The structural shocks are orthogonal to each other, i.e., $E(\varepsilon_t \varepsilon_t') = I$.

We identify the structural shocks using prior information on the signs of the responses of certain variables. First, we use short-run output and price responses to distinguish between demand and supply shocks (see section "Price and Output Restrictions"). In the section "Robustness," alternatively supply-side technology shocks are identified by restricting the medium-run response of labor productivity. As an ulterior robustness check, we also combine long-run and short-run restrictions more commonly used in the literature.

Implementing Sign Restrictions

The identification schemes are implemented following a Bayesian procedure. We impose a Jeffreys (1961) prior on the reduced-form VAR parameters:

$$p\left(B,V\right) \propto \left\| V \right\|^{-\frac{n+1}{2}},$$

where $B = [\mu, B_1, \ldots, B_p]'$ contains the reduced-form VAR parameters and n is the number of variables in the VAR. The posterior distribution of the reduced-form VAR parameters belongs to the inverse Wishart-Normal family:

$$\left(V \middle| Z_{t=1,\ldots,T}\right) \sim IW\left(T\hat{V}, T-k\right), \tag{7}$$

$$\left(B \middle| V, Z_{t=1,\ldots,T}\right) \sim N\left(\hat{B}, V\left(X'X\right)^{-1}\right), \tag{8}$$

[11] Based on information criteria, we estimate a reduced-form VAR including two lags, i.e., $p = 2$.

Table 3

Sign Restrictions: Demand and Supply Shocks

	Demand shocks		
Variable	Monetary	Other	**Supply shocks**
Output	↑1-4	↑1-4	↑1-4
Price level	↑1-4	↑1-4	↓1-4
Interest rate	↓1	↑1	—

where $\hat{B}$ and $\hat{V}$ are the ordinary least squares estimates of B and V,T, is the sample length, $k = (np + 1)$, and X is defined as

$$X = \left[x_1',\ldots,x_T'\right]',$$
$$x_t' = \left[1, Z_{t-1}',\ldots,Z_{t-p}'\right]'.$$

Consider a possible orthogonal decomposition of the covariance matrix, i.e., a matrix C such that $V = CC'$. Then CQ, where Q is a rotation matrix, is also an admissible decomposition. The posterior distribution on the reduced-form VAR parameters, a uniform distribution over rotation matrices, and an indicator function equal to zero on the set of impulse response functions (IRFs) that violate the identification restrictions induce a posterior distribution over the IRFs that satisfy the sign restrictions.

The sign restrictions are implemented as follows:

1. For each draw from the inverse Wishart-Normal family for (V,B), we take an orthogonal decomposition matrix, C, and draw one possible rotation, Q.[12]
2. We check the signs of the impulse responses for each structural shock. If we find a set of structural shocks that satisfies the restrictions, we keep the draw. Otherwise we discard it.
3. We continue until we have 1,000 draws from the posterior distribution of the IRFs that satisfy the identifying restrictions.

[12] We obtain Q by generating a matrix X with independent standard normal entries, taking the QR factorization of X, and normalizing so that the diagonal elements of R are positive.

PRICE AND OUTPUT RESTRICTIONS

The basic IS-LM-AD-AS model can be used to motivate the following restrictions to distinguish demand and supply shocks. Demand shocks move the price level and output in the same direction in the short run. Supply shocks, on the other hand, move output and the price level in opposite directions. On the demand side, we further distinguish between monetary and nonmonetary shocks: Monetary shocks lower the interest rate on impact, whereas nonmonetary demand shocks do not. The interest rate responses are restricted to one quarter, and the output and price-level responses are restricted to four quarters. These restrictions are similar to the ones used by Peersman (2005).[13] The identifying restrictions are summarized in Table 3.

Figures 2 and 3 report the median, 16th, and 84th percentiles of 1,000 draws from the posterior distribution of acceptable IRFs of nonlabor market variables, labor market variables, and other variables of interest.[14] Recall that labor market variables are left unrestricted. The response of output is hump shaped across shocks and more persistent for supply shocks. The response of hours is positive for all shocks and the response of labor productivity is positive for supply shocks.

For the response of the labor market variables displayed in Figure 3, the following main observations emerge:

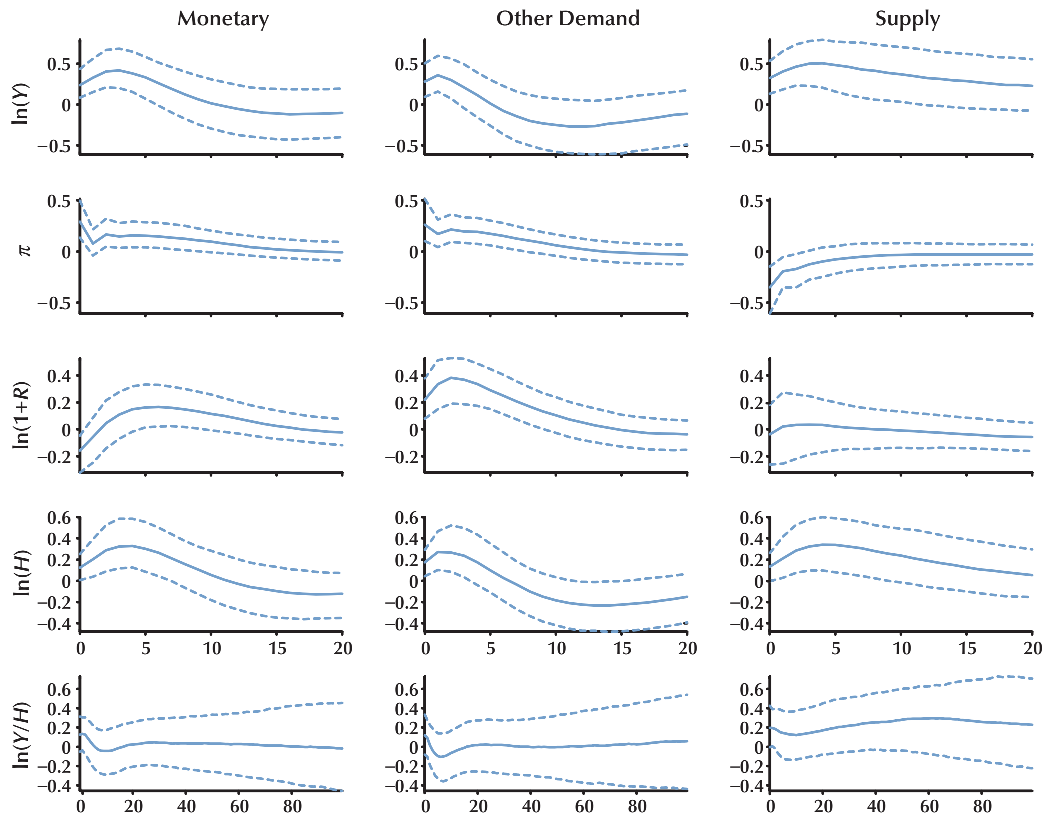
- **Similarity Across Shocks.** The responses of labor market variables are qualitatively similar across shocks. Supply shocks generate more persistent, although less pronounced, responses than demand shocks. Supply shocks induce a larger fraction of atypical responses of labor market variables, such as an increase in job destruction on impact.
- **Worker Flows, Unemployment, and Vacancies.** The job-finding rate and vacan-

[13] Peersman (2005) additionally restricts the response of the interest rate for supply shocks and the response of the oil price to further disentangle supply shocks.

[14] The acceptance rate is 66.6 percent.

Figure 2

Price Restriction: IRFs for Non-Labor Market Variables and Hours (percent): Demand and Supply Shocks



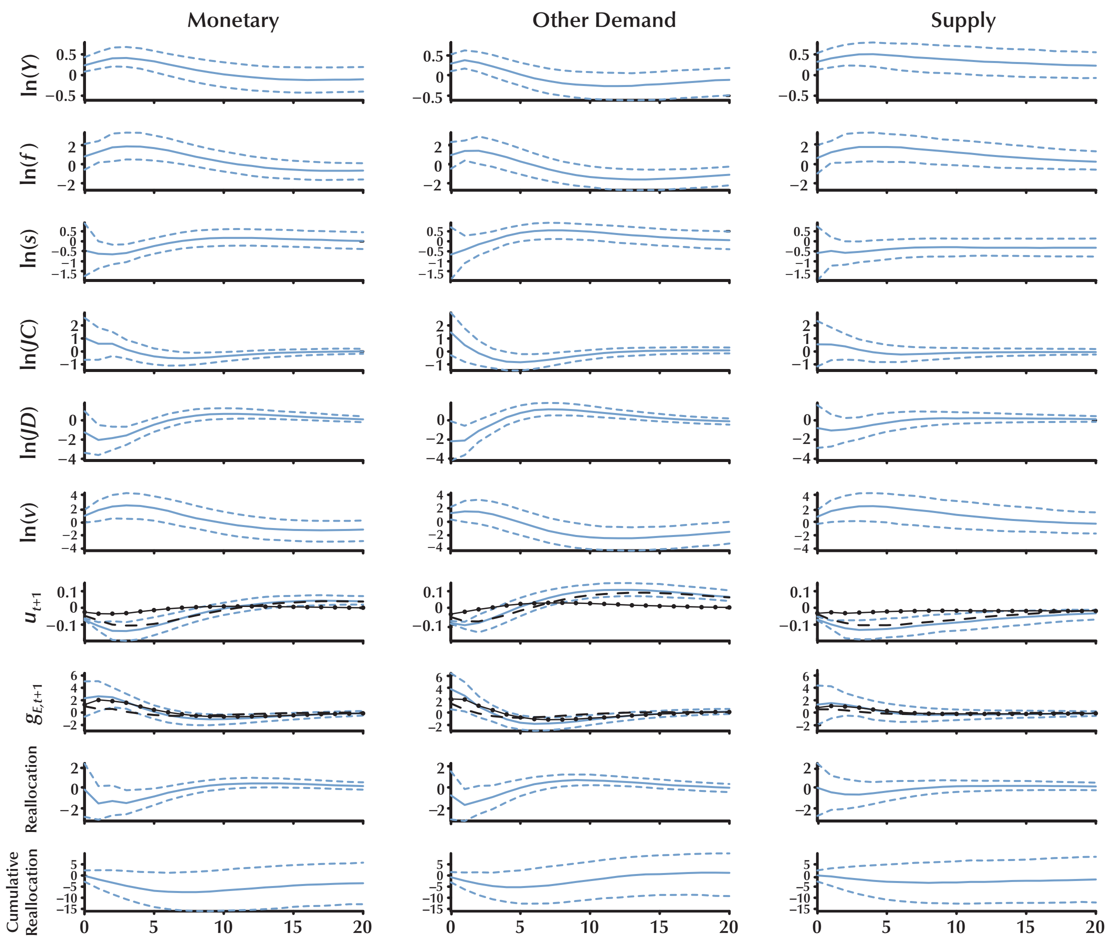
NOTE: Median (solid line), 16th and 84th percentiles (dashed lines) of posterior distributions.

cies respond in a persistent, hump-shaped manner. Separations are less persistent. In response to demand shocks, the unemployment rate decreases for 10 quarters and overshoots its steady-state value. In response to supply shocks, the unemployment rate decreases in a U-shaped way, displaying a more persistent response and no overshooting. The response of the unemployment rate to all shocks is mostly determined by the effect on the job-finding rate, as displayed by the black dashed line in the unemployment panel of Figure 3. However, the separation rate contributes up to one half of the total effect on impact, as shown by the black dotted line. The largest effect on unemployment is reached earlier for the separation rate than for the job-finding rate.

- **Job Flows, Employment Dynamics, and Job Reallocation.** The response of employment growth is driven largely by job destruction (black dotted line in the employment growth panel of Figure 3). The responses of the job-destruction rate are similar in shape to those of the separation rate, but larger in magnitude. The responses of the job-creation rate are the mirror image of the IRFs of the job-destruction rate. Job destruction responds to shocks twice as much as job creation does. A sizable number of the

Figure 3

Price Restriction: IRFs for Labor Market Variables (percent): Demand and Supply Shocks



NOTE: Median (solid line), 16th and 84th percentiles (dashed lines) of posterior distributions. Black lines in the u-panel are the contributions of the job-finding (dashed) and the separation rates (dotted) to unemployment. Black lines in the g_E-panel are the contributions of job creation (dashed) and job destruction (dotted) to employment growth.

responses of job flows to supply shocks involve a decrease in job creation and an increase in job destruction. All shocks increase the growth rate of employment and reduce reallocation. The drop in reallocation is more pronounced for demand shocks. We do not find a significant permanent effect on cumulative reallocation.

The similarity across shocks may support the one-shock approach taken in the literature studying the business cycle properties of the Mortensen-Pissarides model. Although the persistence of the effects differs, all shocks raise job finding, vacancies, and job creation; they lower separations and job destruction in a similar fashion. However, the difference in persistence across shocks casts doubts on a reconciliation of the Mortensen-Pissarides model with the observed labor market behavior that is specific to a particular shock. The considerable fraction of atypical responses to supply shocks suggests that a further analysis of shocks different from the one we consider is necessary (see Braun, De Bock, and DiCecio, 2006; López-Salido and Michelacci, 2007).

The hump-shaped response of the job-finding rate and vacancies to shocks is not consistent with the Mortensen-Pissarides model and with most of the literature. This finding is in line with Fujita (2004), who identifies a unique aggregate shock in a trivariate VAR including worker flows variables, scaled by employment, and vacancies. This aggregate shock is identified by restricting the responses of employment growth (nonnegative for four quarters), the separation rate (nonpositive on impact), and the hiring rate (nonnegative on impact). Our identification strategy confirms these findings without restricting worker flow variables. Where we use the job-finding probability in our VAR, Fujita (2004) includes the hiring rate to measure worker flows into employment. The hiring rate measures worker flows into employment scaled by the size of the employment pool. The job-finding rate measures the probability of exiting the unemployment pool. Although both arguably reflect movements of workers into employment (see Shimer, 2007), the difference in scaling leads to a different qualitative behavior of the two series in response to an aggregate shock. The response of the job-finding rate shows a persistent increase. Fujita's hiring rate initially increases but quickly drops below zero because of the swelling employment pool.

The mildly negative effect on cumulative reallocation is at odds with Caballero and Hammour (2005), who find that expansionary aggregate shocks have positive effects on cumulative reallocation.

For monetary policy shocks, the IRFs of aggregate variables are consistent with Christiano, Eichenbaum, and Evans (1999), who use a recursiveness restriction to identify a monetary policy shock. However, Christiano, Eichenbaum, and Evans (1999) obtain a more persistent interest rate response and inflation exhibits a price puzzle, i.e., inflation declines in response to an expansionary monetary policy shock. The latter difference is forced by our identification scheme. The job flows responses are consistent with estimates in Trigari (2009) and the worker flows and vacancies responses with those in Braun (2005).

The last row of Figure 2 shows the IRFs of labor productivity for 100 quarters. Average labor productivity, which is unrestricted, displays a persistent yet weak increase in response to supply shocks. On the other hand, productivity shows no persistent response to demand or monetary shocks. The medium-run response of labor productivity to supply shocks is consistent with a technology shocks interpretation.

Table 4 reports the median of the posterior distribution of variance decompositions, i.e., the percentage of the j-periods-ahead forecast error accounted for by the identified shocks. The forecast errors of output and labor productivity are driven primarily by supply shocks. Interestingly, the demand shocks have a greater impact on labor market variables than the supply shock. The greater importance of demand shocks suggests that more attention should be paid to shocks other than technology in the evaluation of the basic labor market search model.

A vast and growing literature analyzes the response of hours worked to technology shocks in VARs. Shea (1999), Galì (1999, 2004), Basu, Fernald, and Kimball (2006), and Francis and Ramey (2005) argue that hours decrease on impact in response to technology shocks. This result is at odds with the standard RBC model, which implies an increase in hours worked in response to a positive technology shock. The conclusion drawn is that the RBC model should be amended by including nominal rigidities, habit formation in consumption and investment adjustment costs, a short-run fixed proportion technology, or different shocks.[15] Our results on the importance of demand shocks in driving labor market variables and on atypical responses of these variables to supply shocks can be interpreted as an extension of the negative hours response findings, though in a milder form.

The last column in Table 4 shows the variance contributions of the shocks at business cycle frequencies. The contribution of shock i to the total variance is computed as follows:

[15] Christiano, Eichenbaum, and Vigufsson (2004), on the other hand, argue that the negative impact response of hours to technology shocks is an artifact of overdifferencing hours in VARs.

Table 4

Variance Decompositions for Output and Price Restrictions: Percentage of the *j*-Periods-Ahead Forecast Error Explained by Monetary, Other Demand, and Supply Shocks

	4			8			20			32			Business cycle		
	Monetary	Other demand	Supply	Monetary	Other demand	Supply	Monetary	Other demand	Supply	Monetary	Other demand	Supply	Monetary	Other demand	Supply
Output	9.5	6.4	13.4	8.1	5.8	14.4	7.1	7.0	12.3	6.7	6.9	11.5	9.3	12.5	9.8
	[2.7,23.8]	[1.6,19.9]	[3.8,30.9]	[2.5,21.3]	[2.4,13.4]	[4.4,32.5]	[2.8,15.5]	[2.5,16.7]	[3.9,28.2]	[2.8,15.1]	[2.7,6.9]	[3.7,26.3]	[3.4,20.6]	[4.3,25.9]	[2.9,23.5]
Inflation	6.8	8.6	9.5	8.4	9.6	8.5	8.7	9.6	9.0	9.1	10.2	9.3	6.1	12.4	10.3
	[2.1,18.6]	[2.4,24.8]	[2.3,27.8]	[2.5,21.3]	[2.8,26.4]	[2.4,23.7]	[2.9,21.5]	[3.7,23.4]	[3.3,21.3]	[3.4,19.7]	[4.2,10.2]	[3.7,20.9]	[1.7,16.1]	[4.3,29.5]	[3.4,25.2]
Interest rate	4.8	19.6	5.2	6.9	20.0	5.4	7.7	17.7	7.0	8.0	16.3	8.0	6.5	15.5	5.8
	[1.6,11.7]	[5.4,29.6]	[1.3,18.3]	[2.4,14.8]	[6.4,38.4]	[1.6,14.9]	[3.0,17.1]	[6.9,32.3]	[2.5,15.0]	[3.3,16.4]	[6.9,16.3]	[3.3,15.7]	[2.3,14.3]	[4.7,33.6]	[1.6,14.4]
Hours	8.4	7.8	8.5	8.9	6.0	10.9	8.6	9.0	10.8	9.1	9.2	10.4	9.3	13.8	9.1
	[1.2,26.6]	[1.4,26.5]	[1.4,27.4]	[1.8,25.5]	[1.9,17.1]	[2.2,29.8]	[2.8,19.2]	[3.0,19.2]	[3.2,25.1]	[3.1,18.9]	[3.5,9.2]	[3.8,22.8]	[2.6,24.1]	[4.0,31.6]	[2.6,23.1]
Job finding	9.3	6.8	8.2	9.4	6.1	10.4	8.5	10.7	10.0	8.6	11.2	10.1	9.5	12.8	9.2
	[2.0,27.8]	[1.7,22.8]	[2.2,25.7]	[1.9,26.3]	[2.2,16.0]	[2.5,28.7]	[2.7,18.7]	[4.1,21.6]	[3.2,25.4]	[3.1,18.6]	[4.4,11.2]	[3.5,23.8]	[2.6,24.3]	[3.5,31.1]	[2.8,22.5]
Separation	8.7	6.3	8.8	8.5	8.5	10.0	8.8	10.0	10.8	8.6	9.5	11.0	9.3	10.9	8.7
	[2.7,22.5]	[1.7,18.0]	[2.4,22.3]	[3.2,19.8]	[3.3,17.7]	[3.3,21.9]	[3.8,16.7]	[4.2,18.5]	[4.0,21.1]	[3.8,15.3]	[4.3,9.5]	[4.4,20.7]	[3.6,22.0]	[3.4,27.6]	[2.7,29.8]
Job creation	7.2	9.7	7.2	8.6	12.6	7.8	9.6	12.5	8.4	9.6	12.5	8.4	7.3	11.4	6.2
	[2.1,20.9]	[3.5,21.7]	[2.5,19.0]	[3.4,18.7]	[5.4,22.8]	[3.1,18.0]	[4.3,20.0]	[5.7,22.1]	[3.6,17.4]	[4.4,19.5]	[5.7,12.5]	[3.7,17.3]	[2.3,18.7]	[4.4,24.1]	[1.8,15.7]
Job destruction	11.5	10.3	6.6	11.4	11.9	7.5	11.3	12.7	7.5	11.2	12.8	7.7	9.5	13.3	6.3
	[3.8,27.8]	[2.7,26.1]	[2.2,17.9]	[4.1,24.9]	[4.1,24.8]	[3.0,17.5]	[4.4,25.3]	[4.9,25.3]	[3.1,16.5]	[4.4,24.9]	[5.1,12.8]	[3.2,16.1]	[2.9,22.8]	[3.6,28.7]	[1.8,16.3]
Vacancies	9.0	5.5	7.9	8.5	6.3	9.3	9.0	11.5	8.6	9.3	11.4	8.8	9.7	12.5	8.6
	[1.0,27.0]	[1.1,19.4]	[1.1,25.9]	[1.4,25.5]	[2.5,14.6]	[1.6,27.8]	[3.0,18.3]	[4.3,24.3]	[2.5,22.0]	[3.3,18.7]	[4.7,11.4]	[3.3,20.8]	[2.3,23.0]	[3.9,27.4]	[2.3,22.0]
Average labor	4.4	4.8	7.7	4.6	5.7	7.4	4.5	5.4	6.6	4.5	5.5	7.4	7.5	10.5	8.8
productivity	[1.0,16.5]	[1.4,15.7]	[1.4,27.9]	[1.4,14.9]	[1.6,16.6]	[1.6,24.7]	[1.3,14.3]	[1.5,17.0]	[1.6,22.7]	[1.3,14.5]	[1.5,5.5]	[1.7,24.2]	[2.8,17.0]	[3.6,20.8]	[2.6,21.1]

NOTE: The last column presents the variance contributions at the business cycle frequency (see text). Numbers in brackets are 16th and 84th percentiles obtained from 1,000 draws.

Table 5

Matching Function Estimates for Output and Price Restrictions: Elasticities and Matching Efficiency

	Constant returns to scale		No constant returns to scale		
	α_v	A	α_v	α_u	A
Monetary	0.39 [0.38,0.40]	3.35 [3.30,3.58]	0.27 [0.24,0.30]	0.44 [0.42,0.47]	0.81 [0.65,1.14]
Other demand	0.39 [0.38,0.40]	3.33 [3.24,3.52]	0.27 [0.25,0.31]	0.46 [0.44,0.48]	0.92 [0.77,1.29]
Supply	0.41 [0.41,0.42]	3.69 [3.61,3.86]	0.27 [0.25,0.31]	0.43 [0.43,0.44]	0.85 [0.72,1.14]
All	0.40 [0.40,0.41]	3.54 [3.49,3.69]	0.25 [0.25,0.29]	0.43 [0.43,0.44]	0.75 [0.69,1.01]
Data	0.40 [0.40,0.41]	3.55 [3.44,3.66]	0.25 [0.25,0.29]	0.43 [0.43,0.43]	0.74 [0.70,1.02]

NOTE: Median of the posterior distribution; 16th and 84th percentiles are in brackets.

- We simulate data with only shock i, say Z_t^i.
- We band-pass filter Z_t^i and Z_t to obtain their business cycle components, $(Z_t^i)^{BC}$ and $(Z_t)^{BC}$, respectively.
- The contribution of shock i is computed by dividing the variance of $(Z_t^i)^{BC}$ by the variance of $(Z_t)^{BC}$.

The three right panels of Table 4 show the variance contribution with the price-output restriction. The nonmonetary demand shock is the most important shock. The monetary and supply shocks contribute about equally to the business cycle variation of labor and non-labor market variables.

Matching Function Estimates

We investigate further the possibility of differential labor market responses to shocks by estimating a shock-specific matching function. In the Mortensen-Pissarides model, the number of hires ($f \times U$) is related to the size of the unemployment pool and the number of vacancies via a matching function, $M(U,V)$.[16] Assuming a Cobb-Douglas functional form, the matching function is given by

$$M(U,V) = AU^{\alpha_u}V^{\alpha_v}, \quad (9)$$

where α_v is the elasticity of the number of matches with respect to vacancies, α_u is the elasticity with respect to unemployment, and A captures the overall efficiency of the matching process.

Under the assumption of constant returns to scale (CRS), i.e., $\alpha_u + \alpha_v = 1$, the job-finding rate can then be expressed as

$$\log(f_t) = \log(A) + \alpha_v\left(\log(v_t) - \log(u_t)\right). \quad (10)$$

If we do not impose CRS, then

$$\log(f_t) = \log(A) + \alpha_v \log(v_t) - \alpha_u \log(u_t). \quad (11)$$

To consider the effect of the shocks identified above on the matching process, we obtain a sample of 1,000 draws from the posterior distributions of A and the elasticity parameters estimated from artificial data. Each draw involves the following steps:

- a vector of accepted residuals is constructed as if the shock(s) of interest were the only structural shock(s);
- this vector of accepted residuals and the VAR parameters are used to generate artificial data, $\tilde{Z}_t$;

[16] Petrongolo and Pissarides (2001) survey the matching function literature.

Table 6

Sign Restrictions: Demand and Supply Shocks

	Demand shocks		
	Monetary	Other	Supply shocks
Productivity	Not ↑33-80	Not ↑33-80	↑33-80
Output	↑1-4	↑1-4	—
Price level	↑1-4	↑1-4	—
Interest rate	↓1	↑1	—

- unemployment is constructed using the steady-state approximation $\tilde{u}_{t+1} \cong \tilde{s}_t/(\tilde{s}_t + \tilde{f}_t)$ from the artificial data;
- $\log(\tilde{f}_t)$ is regressed on either $\log(\tilde{v}_t)$ and $\log(\tilde{u}_t)$ (not assuming CRS) or $\log(\tilde{v}_t/\tilde{u}_t)$ (under the CRS assumption).

The artificial data constructed using only monetary shocks, for example, induce a posterior distribution for the elasticity parameters and A for a hypothetical economy in which monetary shocks are the only source of fluctuations.

Table 5 reports the median, 16th, and 84th percentiles of 1,000 draws from the posterior distributions for the price-output identification scheme. The first two columns show the estimates for α_v and A when CRS are imposed. The CRS estimates suggest that aggregate shocks do not entail a differential effect on the matching process. The estimated efficiency parameters are somewhat lower for monetary and demand shocks than for the supply shock, but the median estimates differ by less than 5 percent. The last three columns of Table 5 show the unrestricted estimates for α_v, α_u, and A. Estimates of α_v and α_u across shocks are close and the sum of the coefficients is around 0.70, corresponding to decreasing returns to scale. There are no significant differences in the median estimates of the efficiency parameter A.

ROBUSTNESS

We analyze the robustness of our results by considering medium-run and long-run restrictions on productivity to identify technology shocks. Subsample stability and a minimal VAR specification to identify the shocks of interest are also considered.

Restricting the Medium-Run Response of Labor Productivity

Pushing the technological interpretation further, we identify supply shocks as ones that increase labor productivity in the medium run. The short-run responses of output and the price level are left unrestricted. This allows us to capture, as supply shocks, news effects on future technological improvements (see Beaudry and Portier, 2006). Also, this restriction is similar to the long-run restrictions used in the literature (see Galì, 1999). We will analyze the latter in the next subsection. The advantage of a medium-run restriction is that it allows the identification of the other shocks within the same framework as above.

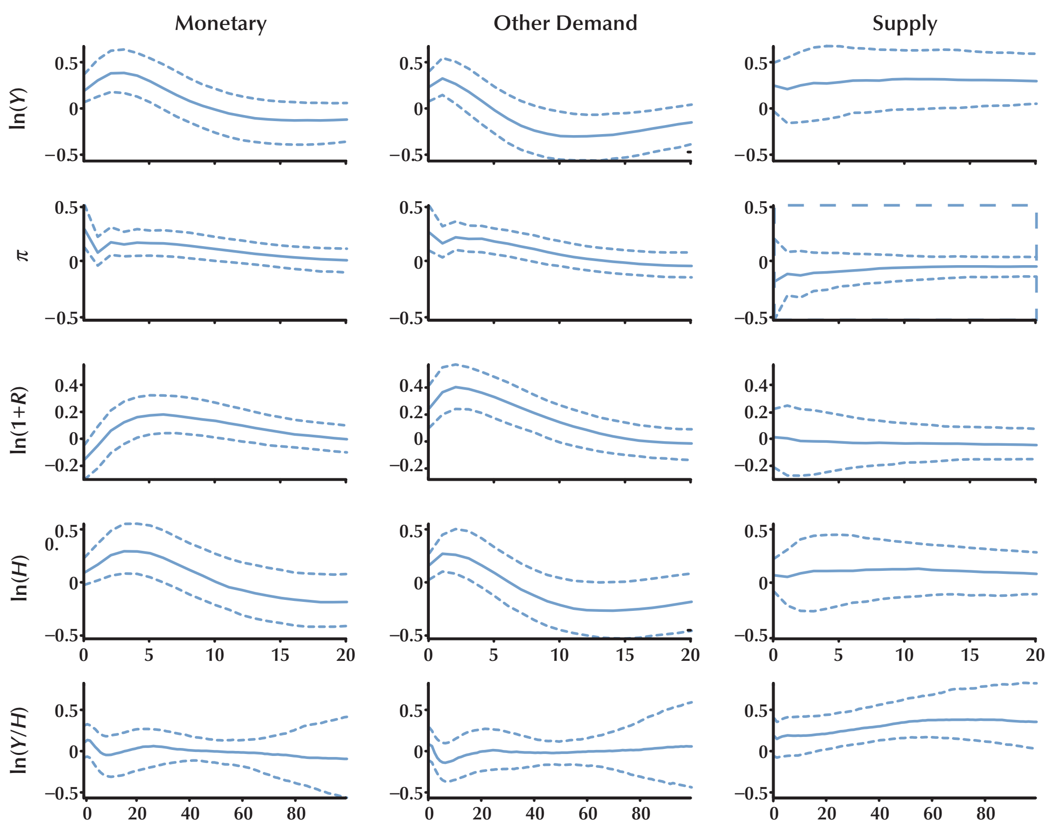
In particular, a technology shock is required to raise labor productivity in the medium run, i.e., throughout quarters 33 to 80 following the shock. On the other hand, demand-side shocks are restricted to have no positive medium-run impact on labor productivity, while affecting output, the price level, and the interest rate as above (see the previous section "Price and Output Restrictions"). (The identifying restrictions are summarized in Table 6.) This restriction is similar, in spirit, to the long-run restriction on productivity adopted by Galì (1999). Uhlig (2004) and Francis, Owyang, and Roush (2008) identify technology shocks in ways similar to our medium-run productivity restriction. According to Uhlig (2004), a technology shock is the only determinant of the k-periods-ahead forecast error variance. Identification in Francis, Owyang, and Roush (2008) is data driven and attributes to technology shocks the largest share of the k-periods-ahead forecast error variance.

Figures 4 and 5 report the median, 16th, and 84th percentiles of 1,000 draws from the posterior distribution of acceptable IRFs to the structural shocks.[17] By construction, the demand-side shocks identified satisfy the restrictions in the

[17] The acceptance rate is 11.7 percent.

Figure 4

Labor Productivity Restriction: IRFs for Non-Labor Market Variables and Hours (percent): Demand and Supply Shocks



NOTE: Median (solid line), 16th and 84th percentiles (dashed lines) of posterior distributions.

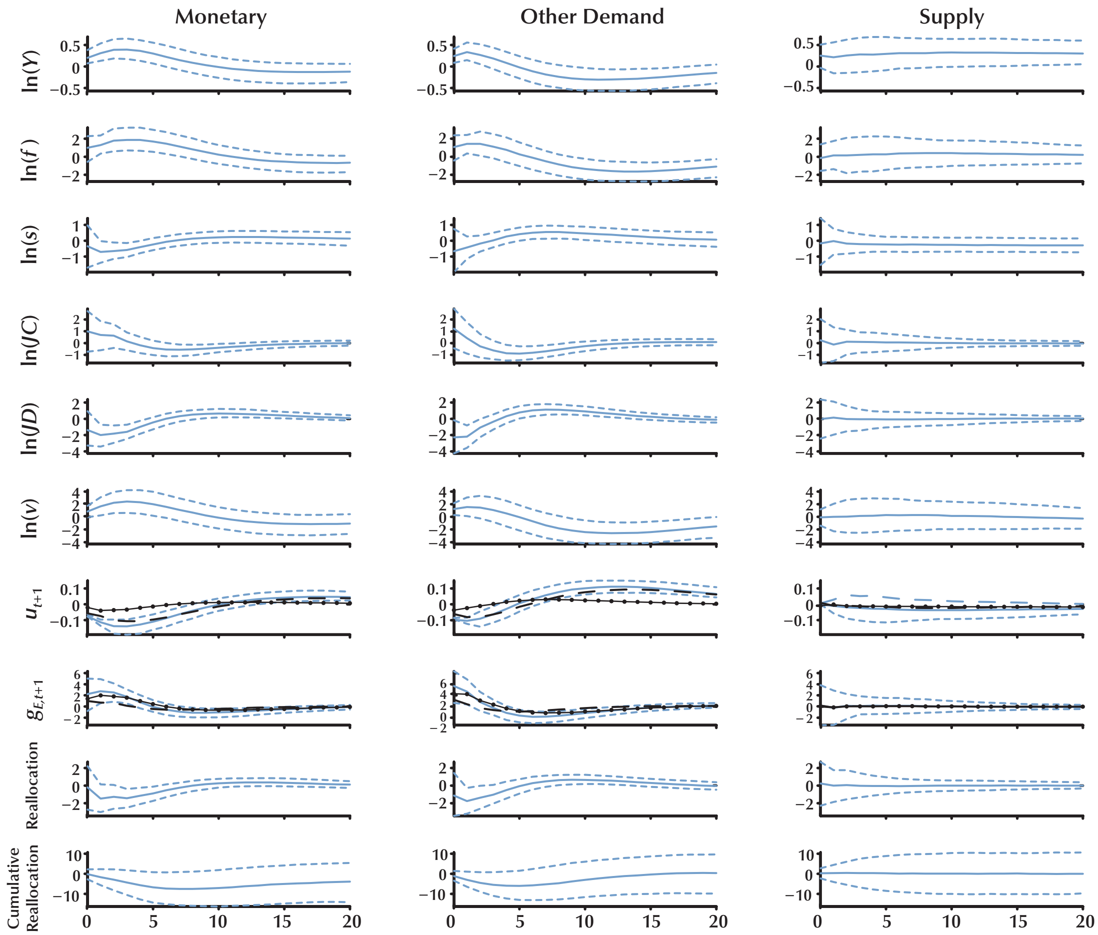
previous section as well. The responses of all variables to demand-side shocks and of output and inflation to supply shocks are almost identical to the ones above. A sizable fraction (49.3 percent) of the supply shocks identified by restricting productivity in the medium run generate short-run responses of output and prices of opposite sign. The responses of the labor market variables to the supply shocks are smaller in absolute value than under the previous identification scheme. Furthermore, a sizable fraction of the responses of labor market variables point to a reduction in employment and hours and an increase in unemployment.

For the variance decompositions displayed in Table 7, the two demand shocks are more important than the supply shock in driving fluctuations in labor market variables at different horizons. This is also true for the variance contributions at business cycle frequencies.

Table 8 shows the matching function estimates under the labor productivity identification scheme. The estimates are very similar to our benchmark analysis. Now, only the efficiency of the matching process in response to nonmonetary demand shocks is lower than the corresponding estimate for the supply shock under CRS.

Figure 5

Labor Productivity Restriction: IRFs for Labor Market Variables (percent): Demand and Supply Shocks



NOTE: Median (solid line), 16th and 84th percentiles (dashed lines) of posterior distributions. Black lines in the *u*-panel are the contributions of the job-finding (dashed) and the separation rates (dotted) to unemployment. Black lines in the g_E-panel are the contributions of job creation (dashed) and job destruction (dotted) to employment growth.

Table 7

Variance Decompositions for Productivity Restrictions: Percentage of the *j*-Periods-Ahead Forecast Error Explained by Monetary, Other Demand, and Supply Shocks

	4			8			20			32			Business cycle		
	Monetary	Other demand	Supply	Monetary	Other demand	Supply	Monetary	Other demand	Supply	Monetary	Other demand	Supply	Monetary	Other demand	Supply
Output	8.0	5.4	7.6	7.2	5.3	8.3	5.8	6.5	10.4	5.1	5.7	10.9	8.8	12.3	7.5
	[2.2,21.5]	[1.3,16.0]	[1.4,24.3]	[2.0,18.5]	[2.3,10.8]	[2.0,26.4]	[2.2,13.0]	[2.6,14.5]	[3.1,26.0]	[2.0,11.5]	[2.3,12.9]	[3.8,26.2]	[3.4,19.6]	[4.7,25.1]	[2.0,19.4]
Inflation	7.0	8.7	6.9	9.1	10.6	7.6	10.2	10.7	8.3	10.7	11.5	8.2	6.6	13.9	8.8
	[2.4,19.0]	[2.7,23.7]	[1.8,22.3]	[2.9,20.8]	[3.4,24.7]	[2.2,20.6]	[3.8,21.8]	[4.1,22.6]	[2.8,28.9]	[4.5,20.3]	[5.0,21.8]	[3.0,18.2]	[2.0,15.0]	[4.7,28.8]	[2.4,22.4]
Interest rate	4.5	19.5	5.6	6.5	20.5	6.4	7.3	20.5	8.0	7.9	16.8	8.3	6.4	16.0	6.2
	[1.8,10.4]	[6.5,41.4]	[1.3,18.3]	[2.4,13.8]	[7.4,29.7]	[1.7,16.7]	[3.0,16.8]	[7.4,32.7]	[2.6,16.8]	[3.3,15.9]	[7.7,28.5]	[3.1,17.3]	[2.4,13.7]	[6.0,33.9]	[1.8,16.8]
Hours	7.1	8.0	6.4	7.3	6.1	6.8	9.0	6.1	7.3	10.4	11.2	7.3	8.8	14.8	7.8
	[0.9,24.5]	[1.4,25.8]	[1.1,21.4]	[1.2,25.4]	[2.0,16.9]	[1.5,20.8]	[3.4,18.8]	[4.0,20.1]	[2.2,18.6]	[3.8,20.1]	[4.9,20.9]	[2.5,17.3]	[2.5,22.2]	[4.8,32.2]	[2.1,20.5]
Job finding	9.5	7.3	6.1	9.8	6.4	6.6	8.5	6.4	7.8	9.1	11.8	7.9	9.5	13.8	8.0
	[2.9,27.0]	[1.9,20.9]	[1.4,19.6]	[2.6,25.0]	[2.5,14.7]	[1.7,19.9]	[3.2,18.7]	[4.7,21.5]	[2.4,19.4]	[3.5,19.5]	[5.1,21.7]	[2.7,19.1]	[3.3,22.8]	[4.2,30.6]	[2.1,20.5]
Separation	8.9	6.2	7.4	8.5	8.7	8.4	8.8	8.8	9.3	8.9	10.1	9.3	9.9	11.2	8.1
	[2.7,21.8]	[1.9,19.4]	[2.1,20.5]	[3.2,18.7]	[3.6,18.3]	[2.9,19.9]	[4.2,16.0]	[4.7,18.7]	[3.8,19.0]	[4.8,15.4]	[5.0,17.2]	[3.9,19.2]	[3.7,20.8]	[3.7,27.3]	[2.6,19.4]
Job creation	7.4	9.3	7.4	8.8	12.4	8.2	9.8	12.4	8.4	9.9	12.5	8.5	7.1	11.6	6.6
	[2.2,23.2]	[3.4,21.1]	[2.8,19.5]	[3.6,20.1]	[5.7,21.9]	[3.3,18.7]	[4.3,20.4]	[6.3,21.6]	[3.6,17.8]	[4.4,20.1]	[6.5,21.4]	[3.8,17.6]	[2.4,19.2]	[4.6,23.5]	[2.1,16.6]
Job destruction	11.2	10.5	6.9	10.9	11.9	7.7	11.1	11.9	7.8	11.1	13.2	7.9	9.3	13.7	7.1
	[4.2,26.1]	[2.9,26.1]	[2.2,18.6]	[4.4,23.4]	[4.5,24.6]	[2.9,17.6]	[4.8,23.1]	[5.5,25.3]	[3.0,16.8]	[5.0,23.0]	[5.7,25.4]	[3.2,16.7]	[3.4,20.8]	[4.1,29.7]	[1.8,16.6]
Vacancies	8.8	5.2	6.5	7.6	6.5	7.0	8.6	6.5	7.8	9.0	12.3	8.4	9.1	13.0	8.0
	[1.2,26.8]	[1.1,19.8]	[0.8,21.4]	[1.6,24.5]	[2.5,14.7]	[1.4,21.2]	[3.0,18.5]	[4.9,23.5]	[2.5,18.6]	[3.4,19.5]	[5.3,23.3]	[3.1,17.8]	[2.7,22.7]	[4.6,28.3]	[2.0,19.7]
Average labor	4.9	4.5	7.6	5.2	5.7	8.0	5.5	5.7	7.7	5.0	5.7	8.1	7.4	9.9	8.5
productivity	[0.9,17.8]	[1.4,12.8]	[1.5,26.0]	[1.4,15.1]	[1.7,15.4]	[1.7,25.4]	[1.4,15.5]	[1.7,16.2]	[1.5,25.4]	[1.4,14.0]	[1.5,15.4]	[1.7,26.0]	[2.7,16.4]	[4.0,20.8]	[2.7,20.9]

NOTE: The last column presents the variance contributions at the business cycle frequency (see text). Numbers in brackets are 16th and 84th percentiles obtained from 1,000 draws.

Table 8

Matching Function Estimates for Productivity Restrictions: Elasticities and Matching Efficiency

	Constant returns to scale		No constant returns to scale		
	α_v	A	α_v	α_u	A
Monetary	0.39 [0.39,0.40]	3.46 [3.29,3.53]	0.26 [0.24,0.32]	0.44 [0.42,0.46]	0.83 [0.64,1.29]
Other demand	0.38 [0.37,0.39]	3.18 [3.14,3.37]	0.26 [0.23,0.31]	0.43 [0.42,0.47]	0.80 [0.59,1.33]
Supply	0.39 [0.38,0.40]	3.42 [3.30,3.57]	0.26 [0.23,0.32]	0.45 [0.42,0.47]	0.79 [0.66,1.30]
All	0.39 [0.39,0.39]	3.40 [3.26,3.50]	0.25 [0.23,0.31]	0.43 [0.42,0.46]	0.73 [0.61,1.17]
Data	0.40 [0.40,0.41]	3.50 [3.44,3.65]	0.25 [0.24,0.29]	0.42 [0.42,0.44]	0.70 [0.65,1.01]

NOTE: Median of the posterior distribution; 16th and 84th percentiles in brackets.

Restricting Labor Productivity Using a Long-Run Restriction

Following Galì (1999), technology shocks are identified using long-run restrictions. Technology shocks are the only shocks to affect average labor productivity in the long run. The long-run effects of the structural shocks are given by

$$Z_\infty = \Theta \varepsilon_t,$$
$$\Theta \equiv \left[I - A(1)\right]^{-1}\left(A_0\right)^{-1}.$$

The identifying assumption boils down to assuming that the first row of matrix Θ has the following structure:

$$\Theta(1,:) = \left[\Theta(1,1) \quad 0_{1\times 9}\right].$$

Additionally, monetary policy shocks are identified by means of a recursiveness assumption as in Christiano, Eichenbaum, and Evans (1999) by assuming that the ninth column of A_0 has the following structure[18]:

$$A_0(:,9) = \left[0_{1\times 9} \quad A_0(9,9)\right]'.$$

This identification assumption can be interpreted as signifying that the monetary authority follows a Taylor-rule-like policy, which responds to all the variables ordered before the interest rate in the VAR.

Figure 6 shows the impulse responses to a technology shock. None of the responses of the labor market variables is significantly different from zero. Figure 7 shows the response to a monetary policy shock. The responses are consistent with the ones identified above.

Table 9 displays the variance decompositions at various horizons and at business cycle frequencies. Although monetary policy shocks contribute much less to the variance of output and productivity than the technology shocks, fluctuations in the labor market variables are to a much larger extent driven by the monetary shock.

Subsample Stability[19]

Several authors[20] document a drop in the volatility of output, inflation, interest rates, and

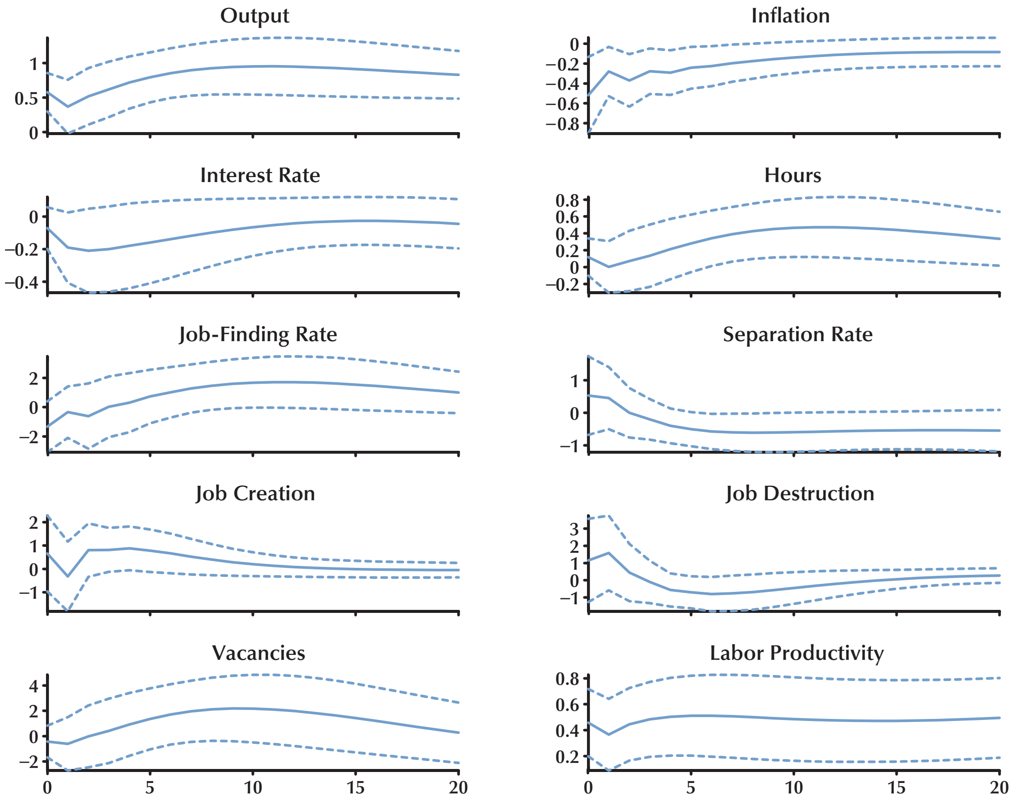
[18] Notice that there is one overidentifying restriction. The first element of ε_t would be just identified by imposing the long-run restriction. The identification of monetary policy shocks imposes one additional zero restriction.

[19] The full set of IRFs and variance decompositions for the two subsamples is available on request.

[20] See Kim and Nelson (1999), McConnel and Perez-Quiros (2000), and Stock and Watson (2003).

Figure 6

IRFs to a Technology Shock Identified with a Long-Run Restriction on Productivity



other macroeconomic variables since the early- or mid-1980s. Motivated by these findings, we estimate our SVAR with pre-1984 and post-1984 subsamples. The post-1984 responses have similar shapes, but are smaller than the pre-1984 and the whole-sample responses for all the shocks. This is consistent with a reduction in the volatility of the structural shocks. However, supply shocks have more persistent effects in the post-1984 subsample for both identification schemes. The responses of labor market variables to supply shocks identified by restricting productivity are insignificantly different from zero for both subsamples.

In terms of forecast error decomposition, supply shocks are the most important for output in the post-1984 subsamples; for hours, monetary shocks are the most important in the pre-1984 subsample, while in the post-1984 subsamples the three shocks we identify are equally important.[21] For worker and job flows, each demand shock is at least as important as the supply shock, across subsamples and identification schemes.

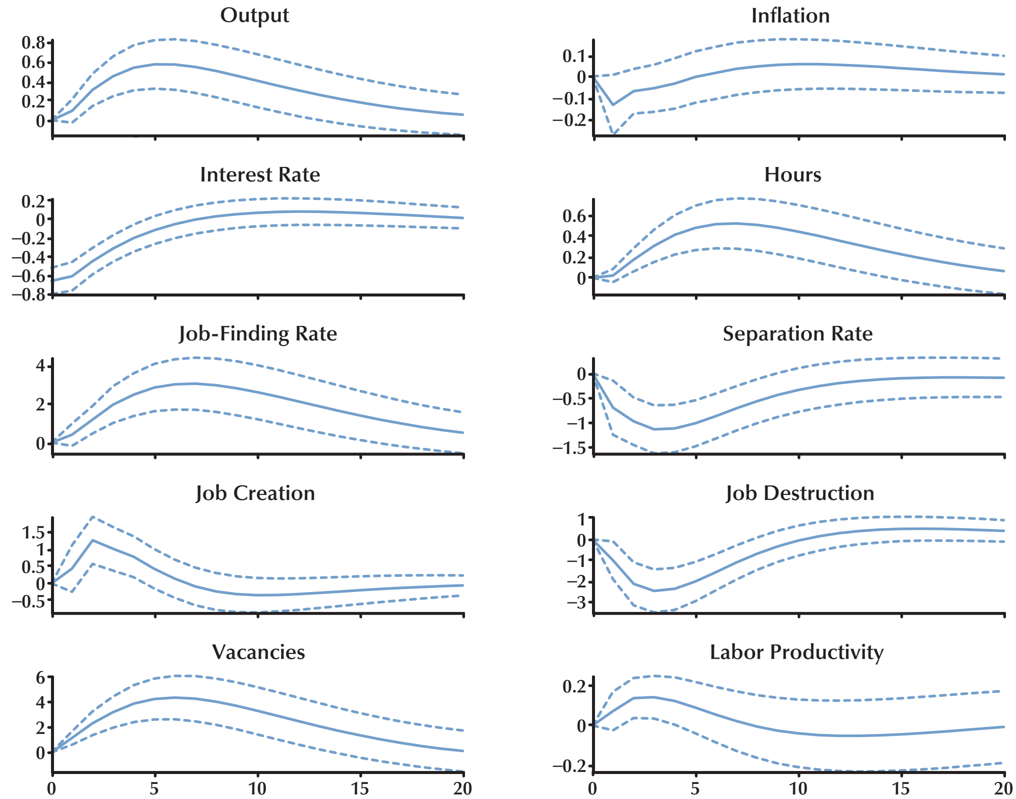
Small VAR

To further check the robustness of our results, we used a lower-dimensional VAR containing labor productivity, inflation, the nominal interest rate, and hours. Shocks are identified using the same sign restrictions as in the section "Price and Output Restrictions." For a draw that satisfies

[21] Our results on the increased importance in the later subsamples of supply shocks in accounting for the forecast error of output are consistent with Fisher (2006). On the other hand, for hours, Fisher (2006) argues that the importance of technology shocks decreased after 1982.

Figure 7

IRFs to a Monetary Shock Identified with a Contemporaneous Restriction



the identifying restrictions we run the following regression:

$$z_t = \alpha + \sum_{j=0}^{T} \beta_j^M \hat{\varepsilon}_{t-j}^{M} + \sum_{j=0}^{T} \beta_j^D \hat{\varepsilon}_{t-j}^{D} + \sum_{j=0}^{T} \beta_j^S \hat{\varepsilon}_{t-j}^{S} + v_{z,t},$$

where $\hat{\varepsilon}^M$, $\hat{\varepsilon}^D$, and $\hat{\varepsilon}^S$ denote the three shocks identified in the minimal VAR and z_t is one of the variables not contained in the VAR, i.e., vacancies, the job-finding rate, the separation rate, the job-creation rate, or the job-destruction rate. Also, α and $v_{z,t}$ denote a constant and an i.i.d. error term, respectively. The length of the moving average terms was set to $T = 30$. The impulse responses for the labor market variables are given by the respective β_j^i.

The conclusions are qualitatively similar to the ones reached above. However, the responses of the job-finding rate and vacancies to a nonmonetary demand shock are less persistent than in our benchmark analysis. Furthermore, the responses to supply shocks are even less pronounced than for the larger VAR specification discussed previously.[22] Again, demand shocks are as important as supply shocks in driving fluctuations of the labor market variables.

CONCLUSION

This paper considers alternative short-run, medium-run, and long-run restrictions to identify structural shocks in order to analyze their impact on worker flows, job flows, vacancies, and hours.

[22] The figures are available on request.

Table 9

Variance Decompositions for Recursiveness and Long-Run Restrictions: Percentage of the *j*-Periods-Ahead Forecast Error Explained by Monetary and Technology Shocks

	4		8		20		32		Business cycle	
	Monetary	Technology	Monetary	Technology	Monetary	Technology	Monetary	Technology	Monetary	Technology
Output	5.9 [3.3,9.6]	21.4 [10.9,41.5]	14.2 [8.1,19.7]	35.1 [22.0,51.7]	10.4 [5.4,14.3]	56.1 [39.4,66.9]	12.1 [4.1,11.6]	64.0 [47.4,73.8]	12.1 [7.7,23.6]	15.1 [6.0,25.2]
Inflation	1.2 [0.3,2.9]	25.7 [4.33,32.6]	0.9 [0.7,3.3]	26.4 [5.0,32.7]	1.4 [1.4,6.2]	25.8 [6.0,30.8]	3.3 [1.5,6.3]	24.9 [6.7,30.7]	3.3 [1.8,8.9]	30.3 [4.0,31.2]
Interest rate	50.5 [38.4,54.0]	5.7 [0.7,10.8]	32.9 [23.9,38.9]	6.1 [1.0,12.5]	26.3 [19.2,32.3]	5.4 [1.6,12.3]	32.3 [16.6,28.3]	6.6 [2.8,14.6]	32.3 [26.6,51.6]	6.1 [1.2,12.0]
Hours	4.4 [2.4,7.9]	1.3 [0.8,10.7]	17.1 [10.0,24.4]	6.9 [2.1,16.2]	18.4 [9.7,23.6]	21.4 [4.5,26.6]	14.6 [8.8,22.0]	22.3 [4.8,27.4]	14.6 [8.1,25.5]	7.9 [2.1,13.1]
Job finding	5.2 [2.9,9.1]	2.2 [1.1,9.7]	18.6 [11.4,27.0]	2.7 [1.9,11.1]	22.7 [12.7,28.5]	9.2 [3.0,17.9]	14.6 [12.1,27.3]	9.3 [3.3,18.4]	14.6 [8.1,26.1]	9.1 [2.1,13.6]
Separation	9.4 [5.6,13.9]	1.8 [1.2,7.5]	16.7 [9.7,21.4]	4.3 [2.4,9.9]	12.8 [7.5,17.2]	9.9 [3.5,15.5]	16.5 [6.1,14.6]	13.6 [4.3,20.3]	16.5 [9.7,24.8]	7.4 [2.0,12.1]
Job creation	4.9 [2.5,8.1]	3.3 [2.1,9.2]	4.9 [2.8,8.2]	5.5 [2.5,11.3]	5.4 [4.0,10.1]	5.5 [2.8,11.4]	6.6 [4.0,10.1]	5.5 [3.0,11.5]	6.6 [4.9,14.7]	4.1 [1.5,10.4]
Job destruction	9.9 [6.1,14.9]	3.4 [1.3,9.1]	17.6 [10.8,22.4]	4.2 [1.8,9.7]	16.2 [10.27,21.4]	4.4 [2.0,10.2]	13.3 [10.3,21.4]	4.7 [2.1,10.4]	13.4 [9.0,23.5]	5.3 [1.4,11.6]
Vacancies	10.6 [6.5,15.5]	0.5 [0.2,7.4]	25.0 [15.6,31.7]	3.0 [1.4,10.4]	21.7 [12.5,26.6]	6.3 [2.3,15.1]	17.3 [10.9,24.0]	6.2 [3.5,15.9]	17.3 [10.1,29.6]	7.5 [2.0,12.8]
Average labor productivity	2.1 [0.8,4.3]	37.1 [22.3,69.3]	1.6 [0.7,3.7]	41.2 [26.1,74.8]	0.8 [0.7,3.5]	42.4 [29.0,80.6]	6.2 [0.6,7.1]	47.0 [36.2,84.8]	6.2 [3.6,12.5]	25.7 [14.3,50.5]

NOTE: The last column presents the variance contributions at the business cycle frequency (see text). Numbers in brackets are 16th and 84th percentiles obtained from 1,000 draws.

We find that demand shocks are more important than supply shocks (technology shocks, more specifically) in driving labor market fluctuations. When identified by means of short-run price and output restrictions, supply shocks have effects that are qualitatively similar to those of demand shocks: Both demand and supply shocks raise employment, vacancies, the job-creation rate, and the job-finding rate while lowering unemployment, separations, and job destruction. These effects are more persistent for supply shocks. When identified by means of medium-run or long-run restrictions on labor productivity, supply shocks do not have a clear effect on the labor market variables.

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APPENDIX

Table A1 describes the data (other than the job flows and worker flows data) used in the paper and provides the corresponding Haver mnemonics. The data are readily available from other commercial and noncommercial databases, as well as from the original sources (Bureau of Economic Analysis, Bureau of Labor Statistics, Board of Governors of the Federal Reserve System).

Table A1

Other Data

Variable	Units	Haver (USECON)
Civilian noninstitutional population	Thousands, NSA	LN16N
Output per hour of all persons (nonfarm business sector)	Index, 1992=100, SA	LXNFA
Output (nonfarm business sector)	Index, 1992=100, SA	LXNFO
GDP: chain price index	Index, 2000=100, SA	JGDP
Real GDP	Billions chained 2000 $, SAAR	GDPH
Federal funds (effective) rate	Percent p.a.	FFED
Hours of all persons (nonfarm business sector)	Index, 1992=100, SA	LXNFH
Index of help-wanted advertising in newspapers	Index, 1987=100, SA	LHELP
Civilian labor force (16 years and older)	Thousands, SA	LF
Civilian unemployment rate (16 years and older)	Percent, SA	LR

The remaining variables used in the VAR analysis are constructed from the raw data as follows:

$$\Delta\log(p_t) = 4\Delta\log(\mathrm{JGDP}_t), H_t = \frac{\mathrm{LXNFH}_t}{\mathrm{LN16N}_t}, v_t = \frac{\mathrm{LHELP}_t}{\mathrm{LF}_t}.$$

Federal Reserve Bank of St. Louis
P.O. Box 442
St. Louis, MO 63166-0442