JOURNAL OF APPLIED ECONOMETRICS
J. Appl. Econ., **13**, 613–626 (1998)

CAUSAL ORDERING AND 'THE BANK LENDING CHANNEL'

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SUMMARY

The bank lending channel implies the Federal Reserve can influence real income by controlling the level of intermediated loans. Using the notion of causality developed by Simon (1953) and the causal order methodology developed by Hoover (1990), I test for an operative bank lending channel in the transmission mechanism of monetary policy. I find loans *did* cause real income; there is evidence that a bank lending channel did exist in the 1960s. The data appears to show, however, that by the early 1990s the bank lending channel was no longer operative. © 1998 John Wiley & Sons, Ltd.

1. INTRODUCTION: DO BANK LOANS *CAUSE* REAL INCOME?

Typically, if economists believe that monetary policy affects real variables, they believe that interest rates are a primary channel between policy and real income. Yet the relative ineffectiveness of falling interest rates to generate a vigorous recovery from the 1990–1991 recession has heightened interest in additional channels for monetary policy. One alternative is the bank lending channel which Kashyap and Stein (1994, p. 222) define as follows:

> ... monetary policy can work not only through its impact on the bond-market rate of interest, but also through its *independent* impact on the supply of intermediated loans ... Put differently, monetary policy can have significant real effects that are not summarized by its consequences for open-market interest rates (cf. Mishkin, 1995).

In causal language, the bank lending channel says that changes on monetary policy cause changes in bank loans which cause changes in real output.

The bank lending channel implies that monetary policy has differential effects on firms which are more or less dependent on banks for finance. A local hardware store will be more affected by a reduction in bank loans than, say, IBM because the local hardware store has little access to alternative forms of finance (see Perez, 1996a for discussion of the relationship between firm size and access to alternative forms of finance). In this paper, I address another implication: the bank lending channel implies the Federal Reserve exerts some *control* over real income by controlling the level of intermediated loans traded in the economy. Independent of movements in interest rates, an increase in loans will raise aggregate output as bank dependent firms have increased access to working capital.

Evidence of the existence of a bank lending channel (see Eichenbaum, 1994; Kayshap and Stein, 1994; Fazzari, Hubbard, and Petersen, 1988; Gertler, 1988; Kashyap, Stein, and Wilcox,

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CCC 0883–7252/98/060613–14$17·50

Received 30 April 1996
Revised 15 May 1997

1993; Gilchrist, Bernanke, and Gertler, 1994; Oliner and Rudebusch, 1996) is elusive because of the frequently encountered problem of observational equivalence. Bank loans and real income are correlated whether the demand for loans responds to changes in income or real income responds to a change in loans (bank lending channel). To resolve the observational equivalence, I examine the econometric effects of structural interventions in the economy applying a methodology for determining causal order suggested by Hoover (1990).

The notion of causal order employed is due to Simon (1953); a variable L causes Y if control of L (actions that control the deep parameters of the process governing L) renders Y controllable. To illustrate, suppose that the data are generated by the following structure:

$$Y = a + bL + \varepsilon \tag{1}$$

$$L = c + \nu \tag{2}$$

where ε and ν are identically, independent, normal error terms. If a, b, c and the parameters describing the distribution of the error terms are 'deep' in the sense of Lucas (1976), then L causes Y in the sense that actions that control L through controlling the parameters of equation (2) permit control over Y, while independent actions to control Y through control of the parameters of equation (1) do not influence L.

This notion of causality must be distinguished from that most familiar to economists: Granger (1969) causality. Roughly, A Granger causes B if knowledge of A and its past history help to predict B even after taking account of its past history. But as Granger himself notes, Granger causality implies temporal predictability but does not address the issue of control: 'If Y [Granger] causes X, it does not necessarily mean that Y can be used to control X' (Granger, 1980, p. 351; see Hoover, 1988, ch. 8; Jacobs, Leamer, and Ward, 1979; Engle, Hendry, and Richard, 1983; Cooley and LeRoy, 1985 for critical discussions). The difference is important; for an analysis based on Granger causality can answer the question 'Does knowledge of the level of loans extended help the Federal Reserve predict real income?' But it cannot answer the question 'Should the Federal Reserve attempt to restrict the availability of loans in order to reduce real income?'

Implementation of the causal order methodology requires examination of the stability of the marginal and conditional distributions across interventions in the data generating process represented by the joint distribution of L and Y ($D(L, Y)$). $D(L, Y)$ can be factored in two ways: $D(Y|L)\, D(L)$ or $D(L|Y)\, D(Y)$, where $D(L|Y)$ is the distribution of L conditional on knowing Y and $D(L)$ is the distribution of L marginal of knowledge of Y. Both factorizations equally well describe the joint distribution; they are observationally equivalent under any single regime (cf. Basmann, 1965; Neftci and Sargent, 1978). Yet the first factorization corresponds in an obvious sense to the fact (as represented in equations (1) and (2)) that L causes Y in a straightforward way.

The alternative factorizations have different stability properties. Suppose that equations (1) and (2) represent the true model; L causes Y in Simon's sense. In this simple example, the value of L is the only element of equation (2) (the L process) relevant to the determination of Y. An intervention in the L process (e.g. a change in the parameter c or the parameters governing the error term ν) would render $D(L)$ and $D(L|Y)$, as well as $D(Y)$ unstable, while the $D(Y|L)$ would remain stable across an intervention in the L process. Suppose that the intervention were in the Y process. $D(Y)$ would be unstable (as would $D(Y|L)$ and $D(L|Y)$), but $D(L)$ would remain stable since Y does not enter equation (2). An analytical derivation of these results and a full menu of stability conditions is available from the author. (Hoover, 1990 and Hoover and Sheffrin, 1992 provide detailed analysis of a similar case and note the changes that are necessary to account for examples that involve cross-equation restrictions among the parameters of (1) and (2).)

The conditional and marginal distributions can be interpreted as regression equations and subjected to econometric analysis of stability in the face of known interventions.[1] The narrative history is examined to identify interventions in the Y and L processes. The pattern of structural breaks in the regressions corresponding to the conditional and marginal distributions provides evidence of the underlying causal order. One way causation is characterized by three out of four regressions showing instability across an intervention. If L and Y are mutual causes, then all four regressions would show instability in the face of an intervention in either process. Furthermore, causal independence results in poor estimation of the conditional regressions. (Hoover, 1991 and Hoover and Sheffrin, 1992 are empirical illustrations of this methodology and provide further details.) Finding either L causing Y or L and Y serving as mutual causes is evidence in favour of the bank lending channel. Otherwise control of L does not render control of Y, and the bank lending channel does not exist.

Stability, as it is used here, is closely related to Engle and Hendry's (1993) notion of superexogeniety which is required to sustain conditional inference in processes subject to interventions. The parameter constancy tests used to determine causal order are similar to those suggested by Engle and Hendry (1993, p. 130) to test for superexogeniety and invariance. The presence or absence of superexogeniety of conditional distributions in the face of particular classes of interventions constitutes evidence needed to infer causal direction. But as Hoover (1991, p. 395) notes, the evidence on the invariance or non-invariance of the marginal distributions is also crucial for causal determination.

The causal inferences made here are based on the structural stability of various conditional and marginal regressions. Whenever one finds a statistical instability there is a possibility that it is due to specification error. Relatively short sample periods resulting in statistical power below what one might wish, make this problem more worrisome. I try to avoid the misclassification of statistical instability as true interventions of coordinating information between marginal regressions, conditional regressions, and the narrative history. Only if all information implies a particular causal order is it taken as convincing.

The remainder of the paper develops empirical evidence of structural stability that appears to indicate that while the bank lending channel may have exerted some effect in the late 1960s, causal order analysis of commercial/industrial loans and output indicates the bank lending channel is no longer a quantitatively important part of the monetary transmission mechanism. The bank lending channel may still be important to the distribution of output among more and less bank dependent firms, but independent of interest rates, aggregate output appears not to be affected by the level of commercial/industrial loans extended.

[1] Regressions provide empirical estimates of the means and variance of these distributions. Other moments of the distribution, however, may also be of interest. Granger and Newbold (1986) develop the notion of Granger causality, and the difference between testing for causality using various different moments is clarified. However, under the assumption of normality, the distributions are characterized by mean and variance and regression analysis is sufficient for causal order analysis.

2. APPLICATION OF CAUSAL ORDER METHODOLOGY

To get a baseline specification against which to identify structural breaks the narrative history is examined for tranquil periods during which there were no interventions in the data-generating process determining Y and L. Candidate interventions include episodes of non-price rationing of loans, war-time build-ups, oil shocks, price controls, etc. Two tranquil periods with no significant interventions are identified: 1952.1 to 1958.4 and 1982.4 to 1988.4. Prior to 1952 the US economy was subject to interventions due to the Korean War, including loan controls and war-time military expenditures.[2] This period was followed by a serious round of non-price rationing in 1959 when the T-bill rate rose above the regulation Q ceilings and disintermediation occurred. The result was two-fold, first, the economy went into recession. Second, depository institutions turned to negotiable certificates of deposits as an alternative source of liquidity, gradually changing the L process. 'It is little wonder that several econometric studies concerned with the relations among financial aggregates, inflation, and interest rates detect — or are clouded by — a break of continuity in the early 1960s' (Wojnilower, 1980, p. 285). This period marks the end of the first tranquil period, and the beginning of the first volatile period.

The late 1960s, 1970s, and early 1980s are littered with potentially important interventions including: the '1966 credit crunch', the Vietnam war build-up, federal pressure to ration loans in 1969, wage and price controls in the early 1970s, oil shocks in both 1974 and 1979, and the change of monetary policy operating procedures in 1979 and the third quarter of 1982.

The second tranquil period covers the fourth quarter of 1982 to the fourth quarter of 1988. The end of the second tranquil period is marked by reports of dramatic tightening of loan standards at lending institutions in 1989–1990, the introduction of the Basle Accord, and the Iraqi invasion of Kuwait. There exist a few possible interventions during the second tranquil period (including the failure and bailout of Continental Illinois Bank, problems at Savings and Loan institutions, the emergence of fiscal deficits, and several changes in tax policy). These are of minor importance to the regressions under consideration evidenced by the statistical stability of the regressions within the tranquil period described below.

Out of necessity, all regressions are specified over a relatively short sample. Although the specification tests used may lack power (in favour of stability) under these conditions, there is no alternative. The tranquil periods are determined by history and the parsimonious regressions must be specified under those conditions. Any unavoidable subjectivity of the choice of tranquil periods is reduced both by a careful reading of the historical and institutional record and by cross-checking that record with a battery of statistical tests for stability.

2.1 The Data

The causal investigation focuses on quarterly observations of real gross national product (Y) and real commercial–industrial loans (L). The 3-month T-bill rate (TB) is included to control for the traditional monetary transmission mechanism operating through interest rates. With the inclusion of TB, the Y conditional regression ($D(Y|L, TB)$) is conditional on all information, and

[2] Although these expenditures and controls continued on past 1952, stability tests of the regressions considered do not show structural changes prior to 1955. Although these expenditures and controls continued past 1952, in order to preserve degrees of freedom, the tranquil period is assumed to run from 1952. This assumption appears to be warranted as stability tests do not show structural change before 1955 and the substantive results are not changed when data from 1952 to 1954 is eliminated.

vice versa. the Y marginal regression ($D(Y|TB)$) is the regression of Y marginal of L (including all lags), and vice versa. The data are from the July 1993 update of Citibase covering the years 1950 to 1993. Commercial paper from statistical reports of the Federal Reserve Board is removed from the Citibase measure of L in order to concentrate the investigation on a narrow bank lending channel. All data are seasonally adjusted.[3]

2.2 Tranquil Period Regressions

The specifications in the tranquil periods are found using the Hendry and Richard (1982) 'general-to-specific' modelling procedure. Ramanathan (1992) and Mizon (1995) describe this procedure; Phillips (1988) describes its merits and Hoover and Perez (1996) provide simulation evidence that it is an effective search strategy. Unrestricted distributed lag regressions of the difference of the dependent variable on a constant, four lags to the difference of the dependent variable, the current and four lags of the difference of the independent variables under consideration, and the level of all variables included in the regression lagged once are estimated.

Both levels and differences of the right-hand-side variables are included in error correction form to allow both short-run and long-run dynamics to be modelled. Over the entire sample (1952.1–1991.1) Johansen cointegration tests are not conclusive, rejecting the hypothesis of cointegration in three out of five model specifications.[4] Therefore, the long-run dynamics are not imposed on the data but derived through the general-to-specific modelling procedure.

Other variables could be included in the regressions explaining movements of L and Y. Exclusion of these variables is only of consequence if interventions in these variables result in structural changes in the L and Y process analogous to those used to identify the causal order and thus cause a mischaracterization. The robustness of the results shown below has been checked by inclusion of the inflation rate and the federal deficit. In all cases, the results are analogous. The inflation rate and the deficit are not included with as many lags as the core variables to conserve degrees of freedom. In most regressions for the tranquil period discussed below, the inclusion of inflation and the deficit does not change the parsimonious specifications. In those where inflation and/or the deficit enter with significance the stability results are not affected. Due to the increase in the importance of the deficit during the 1980s, it is included in the regression analysis in the second tranquil period. The stability results appear robust to the inclusion of the deficit and the inflation rate. Close coordination of the interventions identified in the narrative history with the timing of the statistical instability further substantiates the validity of the implications drawn from the following specifications.

The over-parameterized error-correction model is restricted down to a more parsimonious specific regression explaining the data at least as well as the general regression. The restrictions include not only exclusion restrictions but also equality restrictions across variables resulting in the inclusion of transformed variables not defined in the general model such as second

[3] Although I would have preferred to use seasonally unadjusted data, they are not available for the appropriate series over the entire time period. As Ericsson, Hendry, and Tran (1994) show in the case of a money demand equation for the United Kingdom, an equation estimated with seasonally adjusted data is encompassed by one estimated with non-seasonally adjusted data, and may lead to different inferences. Unfortunately we do not have the luxury of using seasonally unadjusted data here.

[4] I look at five possible model specifications: with and without an intercept and a trend in both the cointegrating equation and the VAR specification. The two specifications which imply cointegration are no intercept or trend in either the cointegrating equation or the VAR and with an intercept in the cointegrating equation only.

differences.[5] In all cases a battery of Chow, normality, ARCH, and serial correlation tests is conducted to guarantee proper specification and stability of the baseline regressions. As a further check of in-sample stability, plots of recursively estimated coefficients are examined.

The two factorizations of $D(L, Y)$ are interderivable. Thus, if one knew $D(L|Y)$ and $D(Y)$, one could compute $D(Y|L)$ and $D(L)$ or vice versa. In practice, there is no guarantee that specifications based on the separate factorizations will be perfectly consistent. But there is a tradeoff between analytical consistency and power to detect breaks. Given the limitations of the data, the case for maximizing power is more compelling; I have therefore chosen to ignore the analytical restrictions between the two factorizations and to treat each separately. This procedure gives each factorization its best shot at capturing the relevant information. However, this procedure results in alternate factorizations which are inconsistent, and thus not exactly equivalent, in that an independent variable may be present in $D(Y)$ and not in $D(L|Y)$ or $D(L)$. Hoover's (1990) result does not necessarily hold if the two factorizations are not exactly equivalent and so inferences could be different. Thus the following results only imply a particular causal order.

2.3 Parsimonious Regressions

For all parsimonious regressions the F-test cannot reject that they are a valid restriction of the general specification with P-values of at least 0·25 (see Table I for specifications and summary statistics for all regressions). In fact, $\bar{R}^2$ is at least as higher for all regressions and the standard error of regression is lower in all cases. It should be noted that the marginal Y regression was unstable following 1957 and thus was specified for 1952.1–1957.4.

For each regression the nulls of normality, no serial correlation (two lags), and no ARCH (one lag) cannot be rejected. Only the serial correlation test for the Y marginal (1952.1–1958.4) regression had a p-value less than 10% (=0·07). All coefficients are significant at the 5% level and all regressions pass a Chow test for stability in 1955.2 and 1957.4 for the first tranquil period (1956.4 for the Y marginal regression) and 1986.1 and 1987.4 for the second tranquil period at the 5% significance level. Visual examination of the coefficient plots implies no instability.

2.4 Stability Evidence from Volatile Periods

The volatile period between 1959 and 1982 is riddled with possible interventions in the L and Y processes. The logic of causal inference requires discriminating interventions — ones that clearly appear in either the L or the Y process, but not both. Examination of the historical record shows that, while there are a number of interventions, only the credit crunches of 1969 and the late 1980s are discriminating in the required sense (see Perez, 1996b for a full description of the historical record).

The Credit Crunch of 1969

Quickly following the 1966 credit crunch, the US economy entered another period of rapidly expanding loan demand and supply. Banks continued to increase the supply of loans and pass

[5] The specification search methodology permits the data to conform to the most parsimonious dynamic form without *a priori* theoretical constraint. When it appears that coefficients are equal, it sometimes proves to be a valid restriction to include second or fourth differences. This is quite common in the applied literature on general-to-specific specification searches (see Hendry and Ericsson, 1991, p. 25 for an example). Fourth differences are naturally interpretable as seasonal effects. The seasonal filters employed by the original data collectors may also induce dynamic restrictions that lack an obvious economic interpretation.

Table I. Parimonious regressions

1952.1–1958.4

D(L|Y, TB):	$\Delta L_t = -146{\cdot}9 + 0{\cdot}63\Delta L_{t-4} + 0{\cdot}20\Delta Y_t - 0{\cdot}53L_{t-1} - 0{\cdot}14Y_{t-1} + 5{\cdot}81TB_{t-1}$
D(L|TB):	$\Delta L_t = 6{\cdot}9 - 0{\cdot}63\Delta L_{t-2} + 0{\cdot}21(L_{t-3} - L_{t-5}) + 7{\cdot}97(TB_t - TB_{t-4}) - 2{\cdot}24TB_{t-1}$
D(Y|L, TB):	$\Delta Y_t = 667{\cdot}4 + 2{\cdot}43\Delta L_t - 1{\cdot}05(L_{t-1} - L_{t-3}) - 1{\cdot}80\Delta L_{t-4} + 24{\cdot}64(TB_t - TB_{t-4}) + 2{\cdot}54L_{t-1} - 0{\cdot}65Y_{t-1} - 38{\cdot}22TB_{t-1}$
D(Y|TB):	$\Delta Y_t = 212{\cdot}9 + 52{\cdot}33\Delta TB_t + 20{\cdot}74(TB_{t-1} - 2TB_{t-3} + TB_{t-4}) - 0{\cdot}26(Y_{t-1} - Y_{t-5}) - 0{\cdot}11Y_{t-1}$

1982.4–1988.4

D(L|Y, TB):	$\Delta L_t = -454 - 0{\cdot}39(L_{t-2} - L_{t-4}) - 0{\cdot}25(Y_t - Y_{t-2}) + 0{\cdot}24\Delta DEF_{t-1} + 0{\cdot}07Y_{t-1} + 16{\cdot}73TB_{t-1} - 0{\cdot}43DEF_{t-1}$
D(L|TB):	$\Delta L_t = -345{\cdot}2 - 0{\cdot}35(L_{t-2} - L_{t-4}) + 7{\cdot}06\Delta TB_{t-4} + 0{\cdot}20L_{t-1} - 18{\cdot}09TB_{t-1} - 0{\cdot}28DEF_{t-1}$
D(Y|L, TB):	$\Delta Y_t = 18{\cdot}25 - 0{\cdot}69(L_{t-1} - L_{t-3}) - 0{\cdot}48\Delta Y_{t-1} + 14{\cdot}10(TB_t - TB_{t-2}) + 7{\cdot}54TB_{t-1}$
D(Y|TB):	$\Delta Y_t = 488{\cdot}48 + 0{\cdot}32(\Delta Y_{t-2} - \Delta Y_{t-2}) + 11{\cdot}05(TB_t - TB_{t-3}) + 9{\cdot}92\Delta TB_{t-4} - 0{\cdot}09Y_{t-1} - 975TB_{t-1}$

Regression	R^2	SER	SSR	Norm.	ARCH	SC	Nested F	Chow	Forecast Chow
1952.1–1958.4									
D(L|Y, TB)	0·88	2·00	88·38	0·33	0·66	0·24	0·89	0·70	0·94
D(L|TB)	0·77	2·76	174·80	0·54	0·48	0·83	0·25	0·22	0·05
D(Y|L,TB)	0·87	6·61	875·03	0·57	0·79	0·50	0·71	0·66	0·30
D(Y|TB)	0·77	7·67	1117·40	0·57	0·88	0·07	0·67	0·80	0·90
1982.4–1988.4									
D(L|Y, TB)	0·58	11·62	2431·67	0·64	0·91	0·54	0·97	0·93	0·84
D(L|TB)	0·51	12·59	3013·90	0·96	0·30	0·83	0·57	0·07	0·96
D(YL|, TB)	0·64	16·42	4311·48	0·59	0·11	0·92	0·96	0·70	0·71
D(Y|TB)	0·46	18·32	6378·86	0·49	0·41	0·92	0·83	0·65	0·67

Notes:

- All coefficients are significant at the 5% significance level.
- P-values are reported for all tests. The null hypothesis of any test is rejected if the p-value is less than the significance level.
- SER is the standard error of regression and SSR is the sum of squared residuals.
- Norm. is tested with the Jarque–Bera normality test in E-views.
- ARCH is the F-version of the ARCH LM test in E-views with one lag.
- Serial correlation is tested with the F-version of the Bruesch–Godfrey serial correlation LM test in E-views with two lags.
- Nested F is the F test of the restrictions from the UDL to the parsimonious regressions.
- Chow is the F-version of the Chow test for a change in coefficients in 1956.2 or 1986.1.
- Forecast Chow is the F-version of the forecast Chow test for a change in coefficients in 1957.4 or 1987.4.

along any increase in the cost of funds to the borrower until June 1969 when they lifted the prime rate a full point, from $7\frac{1}{2}\%$ to $8\frac{1}{2}\%$. 'The resultant congressional outcry carried the message that banks should no longer count on being able to pass on higher money costs to their customers' (Wojnilower, 1980, p. 292). Administration officials expressly asked banks to use non-price means to ration loans (see Owens and Schreft, 1992, p. 19).

The 1969 credit crunch represents an intervention in the L process. Various Chow tests (Chow, Max-Chow, and constant base Chow) and parameter fluctuation tests (Sup-Wald, fluctuation 1, fluctuation 2, and plots of recursively estimated coefficients) are used to determine whether a regression shows instability, and are summarized in Table II. A description of the tests and references are supplied in the Appendix. Both the $D(L|TB)$ and $D(L|Y, TB)$ regressions show structural breaks as a result of the 1969 credit crunch. A Sup-Wald test run from 1965.1 to 1974.4 with a window of observation of 1967.4 to 1971.4 shows a local maximum implying a structural break at a 5% significance level for $D(L|TB)$ in 1970.3 and for $D(L|Y, TB)$ in 1969.2. The fluctuation 2 test reaches a maximum for $D(L|TB)$ in 1969.2. Coefficient plots for both regressions imply structural breaks. For example, Figure 1(a) shows that a parameter estimate from the $D(L|Y, TB)$ regression is relatively stable up to late 1968 after which the estimate significantly falls. Because of the large number of coefficient plots generated, only the most informative are reproduced (all others are available from the author upon request). Taken together the evidence implies that both the regressions of $D(L|Y, TB)$ and $D(L|TB)$ break in 1969–1970.

Both $D(Y|TB)$ and $D(Y|L, TB)$ appear to break between 1965 and 1971. In general the wide standard error bands suggest some ambiguity in the precise timing of the breaks. In Figure 1(b) the plot from $D(Y|TB)$ appears to show a break around 1969. The only possible evidence of instability in the $D(Y|L, TB)$ regression is shown in Figure 1(c). However, the parameter change is not statistically significant as the estimates do not go outside the standard error bands until 1975. The Sup-Wald test for $D(Y|TB)$ reaches a maximum of 18·71 in 1970.2 and for $D(Y|L, TB)$ it reaches a maximum of 15·31 in 1971.2. The critical values provided in Andrews (1993) imply that $D(Y|TB)$ is unstable while $D(Y|L, TB)$ is stable (the null hypothesis of stability over the time period 1967.4 to 1971.4 cannot be rejected even at a 10% significance level).

The $D(L|Y, TB)$, $D(L|TB)$, and $D(Y|TB)$ regressions exhibit instability around 1969. The $D(L|Y, TB)$ regression appears stable through the 1969 credit crunch. Taken together this evidence is consistent with L causing Y and Y not causing L during the 1969 credit crunch: there appears to have been an operative bank lending channel.

1989–91

In the late 1980s, the Savings and Loan crisis was one of the dominant topics of conversation resulting in a desire to avoid the same problem in the banking industry. In response to heavy criticism from the public, policy makers and regulators urged banks to make only prudent loans (see Owens and Schreft, 1992, p. 39). The feeling that regulators were enforcing standards upon bank portfolios more strictly was passed on to borrowers as reports of tightening loan standards increased. Owens and Schreft (1992) report evidence of real estate developers nationwide being unable to get loans at any price and that commercial borrowers using real estate as collateral also had problems receiving loans. Compounding the effect of stricter regulations, the Basle Accord was signed, in late 1988, marking a move to risk-weighted capital standards where loans became a more costly asset to hold in a bank's portfolio.

Table II. Tests showing instability of specific regressions

Regression	CF Dates	Chow (1989.1)	1989.1 Dummy	SW(69–70)	CB	F1	F2	MC
1952.1–1958.4								
D(L|Y, TB)	65–66			49·18**	1959.4	10·2	NA	1·03
	69–70			1969.2		1959.3		1978.1
	71–74?							
D(L|TB)	65–66			17·63*	1960.3	2·61	2·98	1·02
	69–70			1970.3		1969.2	1969.2	1973.1
	71–74							
D(Y|L, TB)	59–60			15·31	1960.1	6·08	NA	2·23
	65–66			1971.2		1973.1		1975.1
	71–74			(no break)				
D(Y|TB)	59–60			18·71*	1958.2	4·90	5·08	1·07
	65–66			1970.2		1958.2	1958.2	1978.1
	69–70?							
	71–74							
1982.4–1988.4								
D(L|Y, TB)	89–90	0·01	0·002					
D(L|TB)	89–90	0·003	0·017					
D(Y|L, TB)	89–90	0·0001	0·001					
D(Y|TB)		0·42	0·18					

Notes:
- CF Dates = time periods where visual inspection of coefficient plots imply instability.
- Chow (1989·1) = p-value of F-test for the null hypothesis of constant parameters at 1989·1.
- 1989.1 Dummy = P-value for dummy variable equal to one after 1989.1.
- SW(69–70) = maximum and date of maximum for Sup-Wald test run over the period 1969–1970 described in the text. * = significant at 5%, ** = significant at 1%.
- CB = the date that constant-base Chow test implies instability.
- F1 = maximum and date of maximum for fluctuation 1 test.
- F2 = maximum and date of maximum for fluctuation 2 test. NA = too many parameters to run test.
- MC = maximum and date of maximum for Max-Chow test.

The narrative evidence of non-price rationing in 1990 is corroborated by the statistical evidence. Plots of the recursively estimated coefficients for D($L|TB$) and D($L|L$, TB) imply a structural break in 1989–90 (see Figures 1(d) and 1(e)). Figure 1(f) plots a recursively estimated coefficient from the D($Y|L$, TB) regression, depicting instability following 1989. This is not the case for the D($Y|TB$) regression (see Figure 1(g)) where no evidence of structural instability appears at any time.

Chu (1990) suggests at least 100 observations to run the Max-Chow, constant base Chow, and fluctuation tests. Instead, a simple Chow test for parameter change in 1989.1 corroborates the evidence from the coefficient plots. P-values for an F-test with null hypothesis of constant parameters are less than 1% for the D($L|Y$, TB), D($L|TB$), and D($Y|L$, TB) regressions, implying the parameters are not constant. The p-value for the D($Y|TB$) regression is 0·42, implying constant parameters across the 1989.1 intervention. Although the power of the Chow tests and

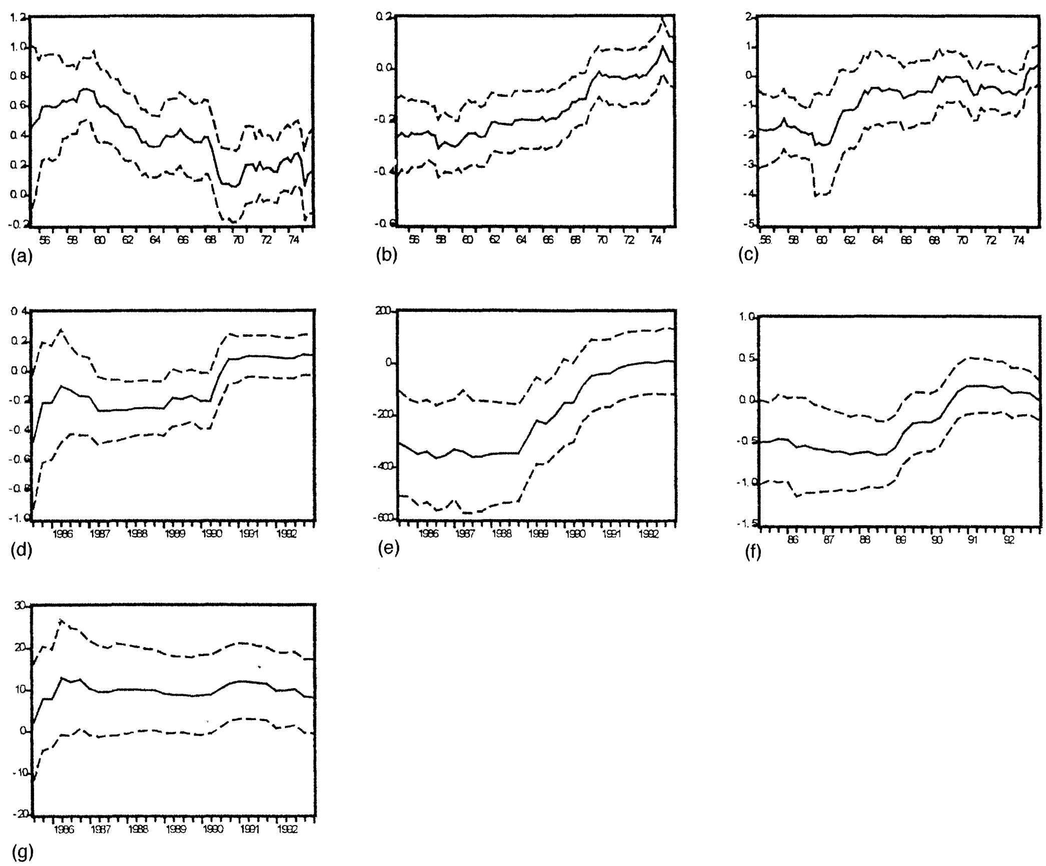

Figure 1. Recursive coefficient plots. (a) L-conditional 1952.1–1975.4: $L(t-4)$ – L(t – 5), (b) Y-marginal 1952.1–1975.4: $Y(t-1) - Y(t-5)$, (c) Y-conditional 1952.1–1975.4: $L(t-4)$ – L(t – 5), (d) L-conditional 1982.4–1993.1: $Y(t) - Y(t-2)$, (e) L-marginal 1982.4–1993.1: constant, (f) Y-conditional 1982.4–1993.1: $Y(t-1) - Y(t-2)$, (g) Y-marginal 1982.4–1993.1: $TB(t-4)$–$TB(t-5)$. Note: Solid line is coefficient estimate, dashed lines are standard error bands. Coefficient plots imply a structural break if recursively estimated parameter goes outside the standard error band of a previous estimate

coefficient plots to identify structural breaks in short samples is of some concern, the p-value for Y marginal is far higher than that for the other three regressions.

To further support these results, invariance tests suggested by Engle and Hendry (1993) are performed. A dummy variable is included in the $D(Y|L, TB)$ regression. If the parameters in the $D(Y|L, TB)$ regression are invariant to changes in the L process, conditioning on L should make the regime change insignificant; a dummy variable equal to one from 1989.1 on should be insignificant. In fact, this dummy variable is significant when included in the $D(Y|L, TB)$ regression having a p-value of 0·001. The same dummy variable when included in the $D(Y|TB)$ regression is insignificant (p-value = 0·1810), implying $D(Y|TB)$ regression is invariant to changes in the L process.

In the face of an intervention in the L process only the $D(Y|TB)$ regression is stable in 1989.1. The evidence is consistent with L not causing Y in this period.

3. CONCLUSION

The interpretion of the results is fairly straightforward. In the late 1960s, the evidence is consistent with the amount of loans extended playing a causal role in the determination of income independent of the level of interest rates. By Kashyap and Stein's (1994) definition, a bank lending channel did exist. By the late 1980s, however, evidence for this causal relationship appears to have disappeared, suggesting that the bank lending channel was no longer operative through commercial/industrial loans.

The credit crunches of 1969 and 1989 provide discriminating evidence on causal direction. In both credit crunches the evidence implies there was a significant disruption of the loan process. In 1969, the income process when conditioning on loans is stable across this intervention, however the income process marginal of loans is not, implying there is an operative bank lending channel. In 1989, the intervention of the loans process renders income process marginal of loans stable while income conditional on loans is unstable. Presumably, due to the financial innovations of the 1970s and 1980s, providing various alternatives to bank finance, the bank lending channel was no longer operative through commercial/industrial loans to real GNP as of 1990.

Although the bank lending channel appears no longer to be of aggregate importance (i.e. the availability of commercial/industrial loans does not cause aggregate output), there are still distributional questions to be addressed. If some firms are shut out of credit markets when monetary policy becomes more restrictive but other firms with access to alterative forms of credit pick up the slack (as suggested by Oliner and Rudebusch, 1996), then some firms win and some firms lose when the stance of monetary policy changes. Therefore, future research regarding the bank lending channel should proceed along these lines.

APPENDIX: STRUCTURAL BREAK TESTS

Two Chow tests are computed for each regression in the volatile period. The constant-base Chow test calculates a Chow test statistic for all possible dates in the volatile period, using the end of the tranquil period as the hypothesized break point. the first time the test statistic exceeds conventional critical values is taken as evidence of a structural break in the regression being tested. The constant-base Chow test is very useful at identifying the first break in a volatile period. Following the first break the test statistic is theoretically undefined.

The Max-Chow test calculates a Chow test statistic for all possible break points in the volatile period. Chu (1990) provides critical values for the Max-Chow and does not suggest the use of this test in samples with fewer than 100 observations.

Fluctuation tests calculate the difference between a regression's parameter estimates at each point in the sample and the estimates for the entire sample under consideration, $\beta_i^t - \beta_i^T$.[6] Excessive fluctuation of a parameter's estimates signifies a structural break in the regression. Two tests are used in this paper. Ploberger, Krämer, and Kontrus (1989) suggest one type of fluctuation test (for the purposes of this paper, fluctuation test 1) where the parameter estimate with the largest change is used to calculate the statistic (i.e. $\max_i (\beta_i^t - \beta_i^T)$). Chu (1990) suggests the use of the Euclidean norm of $\beta_i^t - \beta_i^T$ over all (i.e. $\|\beta_i^t - \beta_i^T\|$) to calculate the test statistic (for the purposes of this paper, fluctuation test 2) but does not suggest the use of this test with fewer than 100 observations. Both tests are calculated when possible and examined. In most cases they are very similar. For the Max-Chow, fluctuation test 1, and fluctuation test 2 distinct local maximums exceeding critical values are considered evidence of the existence and date of a structural break.

Perhaps the most useful information pertaining to the timing of breaks is obtained from plots of recursively estimated parameters. Although the visual inspection of recursive coefficients is not a formal test, it may be interpreted as a heuristically useful way of localizing structural breaks: structural breaks are signalled by a parameter's estimate going outside the standard error bands of a previous estimate. Mizon (1995, p. 135) refers to such an interpretation in his description of the general-to-specific methodology as a check for stability (see also Hoover and Sheffrin, 1992 and Hoover, 1991 for an applied example). This heuristic practice can also be thought of as viewing the internal details of the fluctuation tests which are based on the variability of coefficient estimates and for which there are formal statistics.

Finally, if the exact timing of a break is difficult to determine, the Sup-Wald test developed by Andrews (1993) and described in Hamilton (1994) is extremely useful. The Sup-Wald test splits a sample in two sub-samples at a particular date. Coefficient estimates from each sub-sample are compared to determine whether there was a significant change at the proposed break. The test statistic is calculated for all dates in a window of observation and the maximum is compared to critical values provided by Andrews (1993). For instance, suppose the stability of a regression run from 1950.1 to 1969.4 was questioned in the years 1960, 1961, or 1962. The Sup-Wald test compares the parameter estimates from each sub-sample on either side of the proposed break, i.e. the estimates from 1950.1 to 1960.1 versus the estimates from 1960.2 to 1969.4. The test statistic is calculated for all possible break dates in the window of observation, 1960.1 to 1962.4, and the maximum is compared to the critical value. The advantage of the Sup-Wald test is that it is able to test for a break in a specified time period. The sample and window of observation can be adjusted for very specific investigation when necessary. The maximum of the Sup-Wald test statistic is considered evidence of the existence and the date of a structural break if it is reached within the window of observation.

[6] Capital T signifies the whole sample estimate of the parameter. Lower-case t signifies the parameter estimate at time t. Subscribe i designates the element of the parameter vector under consideration.

ACKNOWLEDGEMENTS

I would like to thank Douglas Davis, David Harless, James Hartley, Kevin Hoover, Valerie Ramey, Kevin Salyer, Steven Sheffrin, participants of the UC Davis Macroeconomics Brownbag Workshop, and participants of the Virginia Commonwealth University Department of Economics seminar series for their helpful comments, and William Humphrey, Marcella Lawton, and Christopher McFadden for their excellent research assistance. I would also like to thank the anonymous referees for their insightful comments.

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