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Proposals for Full-Reserve Banking: A Historical Survey from David Ricardo to Martin Wolf[1]

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Abstract

Full-reserve banking, which prohibits private money creation, has not been implemented since the 19th century. Thereafter, bank deposits became the dominant means of payment and have retained their position until today. The specific contribution of this paper is to provide a comprehensive outlook on the historical and contemporary proposals for full-reserve banking. The proposals for full-reserve banking have become particularly popular after serious financial crises.

Keywords: full-reserve banking, monetary reform, sovereign money, Chicago Plan, history

1. Introduction

Under full-reserve banking (FRB) private money creation is prohibited. Today it would mean that banks could no longer create new money in the form of bank deposits in the process of bank lending. In other words, every deposit would be backed by government money (i.e. cash, central bank reserves and government securities) or a commodity (e.g. gold). FRB aims at separating the payments system from the financing system, as well as separating monetary policy from credit policy.

FRB has been proposed and even implemented as a solution to financial instability a number of times in the past. Thus, the idea of monetary reform should be seen as a historical continuum. In the UK the Bank Charter Act of 1844 prohibited private money creation through fractional-reserve banking by requiring that bank notes (which were the prevailing means of payment) should be fully-backed by government money. The National Acts of 1863 and 1864 achieved the same goal in the US.

The prohibitions, however, did not include bank deposits, which slowly became the dominant means of payment. In the 1930s, the Chicago Plan was almost adopted in the US, but the FRB idea was watered down in the Banking Acts of 1933 (better known as the Glass-Steagall Act) and 1935. Instead of preventing private money creation in the form of bank deposits, the Banking Acts separated commercial and investment banking, provided deposit insurance and improved government's control over monetary policy and money supply. Currently there are no examples of economies where the majority of money does not come into existence as a consequence of bank lending.

Now, in the aftermath of the Global Financial Crisis (GFC), preventing private money creation in order to ensure financial stability has once again become a topical issue. For

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instance, Martin Wolf (2014a; 2014b), the chief economics commentator at the *Financial Times*, supports FRB openly; in 2015 the Green Party in the UK included FRB in its political agenda; Iceland's Prime Minister commissioned a report authored by Sigurjonsson (2015) on FRB; and bills to implement FRB have been put forward in the US and UK.

The scope of this paper is to chart the history of FRB proposals from the 19th century until today. I will focus on the FRB proposals put forward in the US and UK, although other countries are not entirely excluded. The reason for this is that the US and UK are at the heart of global (financial) capitalism and, since World War II, have been a key influence setting global financial standards through international institutions – such as the International Monetary Fund (IMF) and the Bank for International Settlements (BIS).

The discussion on the consequences of FRB is excluded from this paper. It would be too demanding in terms of length to go to the wider literature which debates the advantages and shortcomings of FRB. Nevertheless, it could be mentioned that, for example, Goodhart (1987; 1993), Kregel (2012), Dow et al (2015) and Fontana and Sawyer (2015) provide academic critiques of FRB.

The specific contribution of this paper is a comprehensive mapping exercise of the history of FRB proposals. Although Ronnie Phillips (1994a) laid down much of the groundwork – especially for the New Deal period – such a survey on historical and contemporary proposals for FRB has not been conducted before, especially including the recent new wave of FRB proposals sparked by the GFC.

As Table 1 below illustrates, there are different versions of FRB. A pure commodity standard was the first type to emerge in the 19th century. Sovereign money was proposed before the Great Depression in the 1920s and it has probably become the most popular alternative since the GFC. The Chicago Plan was of high standing academically and politically during the New Deal banking reforms in the 1930s. Deposited currency was an innovation of the mid-1980s. Narrow banking emerged as an alternative during the Savings and Loan Crisis of the late 1980s. The most recent newcomer is limited purpose banking in the mid-1990s. In the following sections I will go through and elaborate on these various FRB types in chronological order.

This paper is structured as follows. In section 2 I will present the first FRB proposals starting from David Ricardo. In section 3 I will move to the Chicago Plan outlined in 1930s during the New Deal banking reforms before discussing the FRB proposals of the latter half of the 20th century in sections 4 and 5. In section 6 I will present the recent new wave of FRB proposals following the GFC. Finally, in section 7, I will draw some concluding remarks.

Table 1. Different types of full-reserve banking

	Pure Commodity Standard	**Sovereign Money**	**Chicago Plan**	**Deposited Currency**	**Narrow Banking**	**Limited Purpose Banking**
Features	All money, including bank deposits, backed by a commodity such as gold (in all other types backed by government money).	Deposit banks can make loans only by attracting savings or using own capital.	Deposit banks provide only payments services and cannot make loans.	Full-reserve requirement applied only to certain deposits. Other (not fully-backed) deposits not guaranteed. Individuals choose which type of deposits to hold.	Banks' assets restricted to 'safe' by some standards.	Banks become unleveraged mutual funds. Banks' liabilities restricted to equity.
Notes	Associated to Austrian school.	Associated to Positive Money, New Economics Foundation and ecological economics.	Associated to 'old' Chicago school and monetarism.	For example, postal saving system or central bank accounts for the general public.	Less restrictive proposals not counted as FRB.	Instead of banks, all risks are born by investors.
Proposals	Ricardo (1824), Mises (1912), Hayek (1937), Rothbard (1962), Huerta de Soto (2009)	Soddy (1926; 1934), Currie (1934; 2004), Daly (1980; 2013), Rowbotham (1998), Huber and Robertson (2000), Yamaguchi (2010; 2011; 2014), Jackson and Dyson (2012), Kolehmainen et al (2013), Farley et al (2013), Wolf (2014a; 2014b), Lainà (2015b), Green Party UK (2015), Sigurjonsson (2015)	Knight et al (1933), Simons et al (1933), Fisher (1935), Douglas et al (1939), Simons (1948), Friedman (1948; 1960; 1969), Benes and Kumhof (2012; 2013)	Tobin (1985; 1987), Jessup and Bochnak (1992), Gruen (2014), Lainà (2015a)	Kareken (1986), Litan (1987), Spong (1996), DeGrauwe (2008), Kay (2009), Phillips and Roselli (2009), Flaschel et al (2010), Chiarella et al (2011)	Pollock (1993), Kotlikoff (2010), Cochrane (2014)

2. First Steps: David Ricardo and Others

The first proposal for FRB can be traced back to David Ricardo. In 1823, Ricardo (1824) drafted a 'Plan for the Establishment of a National Bank' in which he argued that money creation should be separated from lending by requiring the issuing department to hold 100 percent in gold reserves. Ricardo's plan was a full-reserve plan – but it accepted only gold as reserves. The plan was published in 1824, six months after his death.

Ricardo's (1824) plan was a **pure commodity standard** proposal. Unlike in a regular commodity standard – such as the gold standard effective until the 20th century – in a pure commodity standard *all money*, including bank deposits, is backed with the commodity. In a regular commodity standard only *base money* (i.e. cash and central bank reserves) is backed with the commodity.

According to Phillips (1994a), Ricardo's plan served as a guideline for the Bank Charter Act of 1844. As described earlier, the Bank Charter Act (passed in the UK in 1844) effectively implemented FRB. The Act required full-backing of bank notes – which were the dominant means of payment at the time. However, in addition to gold, as suggested by Ricardo (1824), notes could also be backed with government debt. Nevertheless, the Act did not cover bank deposits. Hence, over time banks were able to substitute bank notes with bank deposits. This, in addition to the fact that the Act was suspended whenever a real panic occurred in the subsequent 25 years, slowly led to the deterioration of FRB in the UK.

The National Currency Act of 1863 and the National Banking Act of 1864 implemented a FRB requirement for all national banks in the US. According to McCallum (1989, p. 318), these Acts required national bank notes to be 111.11 percent backed by government bonds (so it was even more than *full*-reserve banking as it imposed a 111.11 percent reserve requirement). Later, according to White (1983, p. 11), Congress imposed a 10 percent tax on any new issuance of bank notes by state-chartered banks. This led banks, both national and state-chartered, to reduce the issuing of bank notes. As in the UK, the US banks were, nevertheless, able to undermine the reform by increasing their issuance of demand deposits.

Ludwig von Mises (1912) presented his brief proposal for FRB arguing that there are two reasons why FRB should be adopted. Firstly, the use of fiduciary money (i.e. money that represents dual sides of a balance sheet) would be destabilising and, secondly and more importantly, human influence on the credit system would be eliminated. As cash and central bank reserves are also fiduciary money, it is quite obvious that Mises is arguing for a full-reserve gold standard (or some other metal standard). Thereby, the FRB proposal of Mises substantially resembles Ricardo's pure commodity standard of almost a century earlier.

The origins of later **sovereign money** proposals can be traced back to Frederick Soddy. He was a Nobel Prize winner in chemistry in 1921, but he was also an economist. Soddy (1926) pointed out the difference between real wealth (buildings, machinery etc.) and virtual wealth (money and debt). Real wealth is subject to inescapable entropy laws of thermodynamics (depreciation), while virtual wealth is subject only to laws of mathematics (compounding at the rate of interest instead of depreciating). As a solution to this imbalance Soddy (1926; 1934) suggested FRB. Soddy's economic views, however, were largely ignored by his contemporaries.

Even though it was implemented in the 19th century both in the UK and in the US, FRB was unable to endure as near-money emerged and finally replaced bank notes as the dominant means of payment. This near-money, known as bank deposits, continues to occupy the position of the main means of payment.

3. Chicago Plan: on the Policy Agenda

During Roosevelt's New Deal banking reforms, FRB re-emerged in the form of the Chicago Plan. The Chicago Plan was presented as a way out of the Great Depression as well as providing a long-term reform of the financial system. This section is divided into three subsections. First, I outline the proposals for the Chicago Plan. Second, I present legislative initiatives implementing the FRB principle. Third, I describe academic reactions to the Chicago Plan.

3.1 Proposals

The first version of the **Chicago Plan** was provided by Knight et al (1933) in the Chicago Memorandum of March 1933. The memorandum was from Garfield Cox, Aaron Director, Paul Douglas, Albert Hart, Frank Knight, Lloyd Mints, Henry Schultz and Henry Simons and it was signed by Frank Knight. All were members at the University of Chicago. Later Douglas became a senator and is still known in economics for the Cobb-Douglas production function. The recipient of the memorandum was Henry Wallace, the Secretary of Agriculture.

In short, the proposal would require FRB in currency and central bank reserves, which would be backed by government debt in the books of the Federal Reserve Banks. The detailed proposal included 1) federal ownership of the Federal Reserve Banks, 2) giving Congress the sole power to grant charters for deposit banking, 3) a two-year transition period for deposit banking, 4) creation of a new type of deposit bank institution with a 100 percent reserve requirement in notes and deposits at the Federal Reserve Banks, 5) abolition of reserve requirements for Federal Reserve Banks, 6) replacement of private credit with Federal Reserve Bank credit within a two-year transition period, and 7) restricting currency to only Federal Reserve notes. As deflation was the pressing economic problem of the time, one of the short-term objectives of the proposal was reflation (a term coined by Irving Fisher to indicate inflation after deflation) of wholesale prices by 15 percent, until a long-run currency-management rule could be established. As a long-run currency-management rule the group proposed different versions of the stabilisation of money supply (either total quantity M, total circulation MV, or per-capita total circulation MV/N; where M is the money supply, V is the velocity of circulation and N is the number of inhabitants).

According to Phillips (1994a), Wallace handed the Chicago Memorandum of March 1933 to President Roosevelt two and half weeks after his inauguration. The Chicago Plan was also sent to a number of other recipients including John Maynard Keynes. According to Phillips (1994a), Keynes briefly expressed his interest in the plan, but did not elaborate his views in more detail.

The second version of the Chicago Plan was provided by Simons et al (1933) in the Chicago Memorandum of November 1933. The memorandum was signed by the same group, but, according to Phillips (1994a), it was evidently written only by Henry Simons. The revised Chicago Plan included the same items as the March 1933 version, but added a simple rule for monetary policy and a price-level target set by Congress. It was argued that monetary policy should be subject to a rule instead of being discretionary. The goal could be, for instance, price stability, steady growth of the money supply, or some other goal specified by Congress. The proposal included neither deposit insurance, as deposits would already be fully secured by the reserves backing them, nor a central bank discount window – as banks would always be able to settle their payments and credit availability was not seen as a potential problem. In addition, the proposal rejected the gold standard.

Proponents of FRB can also be found within the US administration. In 1934, Secretary of Treasury Henry Morgenthau appointed Jacob Viner to assemble a group to come up with ideas involving money, banking and public finance. The group was referred to as the ‘Freshman Brain Trust’. It included, among others, Lauchlin Currie and Albert Hart, who were open advocates of FRB, and Jacob Viner who was at least sympathetic to it. Later that year, Currie became a personal assistant to the governor of the Federal Reserve Board, Marriner Eccles.

Lauchlin Currie (1934) submitted his proposal for FRB to Morgenthau in 1934. In Currie's **sovereign money** proposal, banks would initially meet the 100 percent reserve requirement with a non-interest-bearing note from the Federal Reserve Banks. The note could be left outstanding indefinitely or alternatively the note could be retired over a period of time from five to 20 years by turning over government bonds to the Federal Reserve Banks. As the discount window would be abolished, the money supply could only be affected by open market operations. Currie (1934) was against an independent monetary authority as he argued that democracy should apply to monetary policy as well. As his memo from 1938 reveals, Currie (2004) continued to develop the idea of FRB. Another proposal for FRB, emanating from within the administration, came from Gardiner Means (1933) who was working at the Department of Agriculture.

According to Sandilands (2004), Currie had a major influence on the administration version of the Banking Act of 1935. Phillips (1994a) argued that Currie did not, however, suggest FRB should be included in the administration version of the bill, as he saw it as politically unacceptable. According to Phillips (1994a), Currie compromised on the 100 percent reserve goal, and, in the end, his compromise prohibited any possibility of such a reform being achieved in the future. Nevertheless, Currie was able to include in the administration version of the bill that the Federal Reserve Board would have unlimited power to alter the reserve requirements – with a view to them eventually being raised to 100 percent. Senator Carter Glass, however, was able to rewrite the bill in Congress to limit the Fed's ability to raise reserve requirements higher than 30 percent. It goes without saying that this prohibited any attempt to raise the reserve requirement to 100 percent.

President Roosevelt and Irving Fisher, according to Phillips (1994a), were frequently in touch. Roosevelt requested Fisher to provide comments on his economic policies. Phillips (1994a) argued that Fisher first became aware of FRB as he was handed the Chicago Memorandum. Fisher was working on his own version of the Chicago Plan and provided a draft of his book *100% Money* to Roosevelt. Afterwards, according to Phillips (1994a), Fisher urged Roosevelt to consider the proposal a number of times. Roosevelt and Fisher continuously exchanged letters on FRB and Roosevelt even showed some interest in it, but he was not willing to embrace the reform as the bankers were opposed to it. Nevertheless, Roosevelt forwarded Fisher's draft to his Secretary of Treasury, Henry Morgenthau.

In 1935, Irving Fisher published his own version of FRB. Fisher's (1935) book *100% Money* was largely in line with the Chicago Plan, but it differed somewhat in its policy target. Fisher proposed a price-level stabilisation rule instead of stabilisation of monetary aggregates.

3.2 Legislation

Legislation to implement FRB was introduced during the New Deal reforms. It is worthwhile noticing that FRB was already made possible by the Emergency Banking Act of 1933. The Act permitted banks to offer deposit accounts backed with cash, central bank reserves or government bonds. In other words, these deposit accounts operated according to the FRB principle. There were, of course, other deposit accounts as well and, thus, only a small fraction of deposits became fully-backed by government money. For the banks, the full-reserve requirement of these accounts was easy to satisfy as the Fed flooded the banking system with excess reserves by changing its policy to issue reserves against almost any assets of the banks.

The idea of FRB was also practiced without legal obligations on bank-level. According to Phillips (1994b), John M. '100%' Nichols put the theory fully into practice by successfully operating a bank according to the FRB principle for over a decade.

There were also bills to fully implement FRB nationwide. According to Phillips (1994a), Henry Simons outlined and Robert Hemphill drafted a bill, largely based on the Chicago Memorandums, for Senator Bronson Cutting and Congressman Wright Patman. They introduced the bill S. 3744 'A bill to regulate the value of money' (H.R. 9855) in 1934. The goal of the bill was to correct the shortcomings of the Banking Act of 1933, which did not address the problem of the availability of credit and how to effectively control the money supply. As Phillips (1994a) put it: 'Deposit insurance made banks "safe" not by direct restrictions on their assets, but rather by the promise that the government would guarantee a percentage of the deposits in all banks, good and bad.' In other words, deposit insurance succeeded in stopping bank runs, but it did not address the second primary function of banks: funding the capital development of the economy.

The bill would have made lawful cash money and bank deposits fully-backed with either central bank reserves or government securities. The bill proposed 1) to segregate demand deposits from savings deposits; 2) to require banks to hold 100 percent reserves against their demand deposits; 3) to require banks to hold 5 percent reserves against savings deposits; 4) to set up a Federal Monetary Authority (FMA) with full control over the supply of currency, the buying and selling of government securities, and the gold price of the dollar; 5) to have the FMA take over enough bonds of the banks to provide 100 percent reserves against demand deposits; and 6) to have the FMA raise the price level to its 1926 level and keep it there by buying and selling government bonds.

Senator Cutting was, according to Phillips (1994a), personally disliked by President Roosevelt. This was one reason why the bill did not gain the support of the administration and, consequently, did not pass. Later, however, the bill was reintroduced as S. 2204. A significant blow to the FRB legislation came in May 1935, during the fierce debate over the Banking Act of 1935, when Senator Cutting died in an airplane crash. For the last time the proposal for FRB was introduced by Senator Nye, but his amendment was defeated. The Banking Act of 1935 was a watered down version of Cutting and Patman's bill and – although reforming some aspects, for instance, allowing the Federal Reserve to alter reserve requirements and making deposit insurance permanent – it did not reform money to become fully-backed by government money. Although the Chicago Plan was not adopted, it did have a significant influence on the New Deal legislation. To sum up, the Banking Acts of 1933 and 1935 gave the government better control over monetary policy and the money supply, but not full control over the money supply.

Phillips (1994a) gave four reasons why the FRB proposal was not adopted: 1) the administration blundered in its handling of the banking legislation as it did not keep Senator Glass up to date; 2) the public was ill-informed; 3) Senator Cutting died; and 4) the Banking Act of 1935 was not believed to be the final New Deal banking legislation. Phillips (1994a) added that bankers were against the Chicago Plan as it was seen to reduce their profits. They resisted any changes to the *status quo*, unless it could be demonstrated that the new system would be even more profitable. Whittlesey (1935, p. 23) was pretty much of the same opinion as he saw that the proposal was opposed because free services of banks would no longer be free, and bank owners would lose their main source of profits.

3.3 Academic Reactions

Only after the Banking Act of 1935 had passed did the Chicago Plan start to generate widespread academic interest. Most academic discussions were sympathetic to the plan: there were concerns about transition and details, but the goals were widely seen as desirable.

Douglas (1935), Whittlesey (1935), Hart (1935), Graham (1936) and Higgins (1941) advocated FRB but they emphasised different reasons. In Angell's (1935) version, the government would place a lien on the total assets of the banks equal to the value of new currency received. Service charges would be avoided by banks paying a specified amount to a common pool and then receiving money from the pool relative to their demand deposits.

Watkins (1938, 44) cited Keynes: 'Those (monetary) reformers, who look for a remedy by creating artificial carrying-costs for money through the device of requiring legal-tender currency to be periodically stamped at a prescribed cost in order to retain its quality as money, or in analogous ways, have been on the right track.' Watkins (1938, 44) argued that FRB would be the analogous way that Keynes meant, as it would raise service charges.

Douglas et al (1939) circulated a paper which claimed that FRB was supported by nearly 300 economists while disapproved by only 43. The paper was written by Paul Douglas, Irving Fisher, Frank Graham, Earl Hamilton, Willford King and Charles Whittlesey and it included many of the previous features of the FRB proposals. According to Allen (1977, p. 586), two years later the group also included John R. Commons and the supporters had grown to some 400 economists.

Hayek (1937), on the other hand, revived the **pure commodity standard** proposals. In his pure gold standard proposal, deposits should not be backed with government money, but only with gold. Otherwise Hayek's proposal resembled the original Chicago Plan.

The pure commodity standard type of FRB proposal is sometimes associated with 'free banking'. However, some free banking proposals are, by definition, excluded from being FRB proposals as there are no reserve requirements at all. Other proposals, such as Hayek's (1937), argued for 'free' banking with full gold backing. Apparently, 'free' means in this context 'free from any governmental control' as banks could not freely issue money.

Although FRB might sound like a radical solution now, at the time it was presented as a moderate alternative to the nationalisation of the whole banking system (see e.g. Simons 1948, pp. 332-333; Douglas 1935, pp. 184-187; and Watkins 1938, p. 11). Today it might also sound peculiar that demands for FRB came from the University of Chicago whose economics department is known for *laissez faire* policy prescriptions. According to Phillips (1994a), the founders of the Chicago School of Economics – Frank Knight, Henry Simons, Jacob Viner and Lloyd Mints – were indeed proponents of *laissez faire* in industry, but at the same time they did not question the right of the government to have an exclusive monopoly on money creation.

4. Post-World War II: Academic Developments

After World War II, the atmosphere for reform was again propitious. Congressman Jerry Voorhis introduced a bill H.R. 3648 in 1945 to create a Monetary Authority as the sole creator of money. According to Phillips (1994a), Voorhis worked closely with Fisher who, by 1946, had received over 1100 positive responses out of 4662 members of the American Economic Association willing to endorse FRB (with no response from most of the members). However, the end of the political possibilities for FRB came in the 1946 elections when Congressman Jerry Voorhis from California was defeated by Richard Nixon.

In academia FRB was, nevertheless, not abandoned. After Irving Fisher died, Henry Simons (1948) continued to argue for the **Chicago Plan** and Lloyd Mints (1950, pp. 186-87) suggested his proposal.

Maurice Allais presented his version of FRB in 1948 in French. His views were not published in English until 1987 in Allais (1987). Allais's proposal resembled previous versions of the full-reserve plan, but differed in some important respects. He argued that banks should be required to borrow long and lend short, whereas at the time (and still now) they borrowed short and lent long.

Friedman (1948) suggested eliminating the private creation of money and the discretionary control of the money supply by the monetary authority. This would also mean the elimination of the discount window. Friedman (1948) argued that the chief function of the monetary authority should be to create money to meet government deficits, or destroy money when the government has a surplus. In a later proposal, however, Friedman departed from this view.

Friedman's (1960) later proposal departed from the Chicago Plan by demanding that interest should be paid on reserves – because FRB would be, according to Friedman (1960), effectively a tax on the banking system. Friedman (1960, p. 74) argued that paying interest on reserves would reduce the incentive to evade the full-reserve requirement and to create near-monies. Friedman (1960, p. 65) also argued that holders of money balances and holders of government securities should be equally compensated. Friedman (1960, p. 70) saw 'no technical problem of achieving a transition from our present system to 100% reserves'.

Friedman (1969, p. 83) agreed with Simons's FRB plan, but for different reasons. Friedman's (1969, p. 83) aim was to reduce government interference in lending and borrowing and to allow greater freedom in the variety of borrowing and lending arrangements.

Rothbard (1962) argued that the central bank should be abolished and we should adopt a 'free banking' system. However, Rothbard suggested gold as the only eligible asset to back deposits. In other words, he proposed a **pure commodity standard**. Rothbard's 100 percent gold standard proposal is thus very similar to Hayek's (1937) proposal.

5. Turn of the Millennium: More Creative Ideas

After Friedman, FRB lost its interest in the academic world and among policy-makers for a couple of decades. Proposals for FRB were, however, revived at the turn of the millennium, which generated more creative proposals such as deposited currency, narrow banking and limited purpose banking. Of course, there were also more traditional proposals including government money or gold as the asset to back deposits.

James Tobin's (1985; 1987) **deposited currency** proposal included the establishment of a currency functioning according to the FRB principle, while allowing other deposits as well. Thus, Tobin's (1985; 1987) deposited currency can be seen as optional or 'limited' FRB. In other words, only a fraction (whose size would be determined by the actions of various economic agents) of demand deposits would function according to the FRB principle.

In addition to Tobin's deposited currency, there were also other 'limited' FRB proposals. Jessup and Bochnak (1992) proposed reviving the postal savings system. According to O'Hara and Easley (1979, p. 744), funds in the postal savings accounts could only be invested in government securities or placed in solvent national banks. Thus, the postal saving system can be seen as a limited implementation of FRB.

The turn of the millennium also saw proposals for **narrow banking** (sometimes called core banking). Narrow banking, a term coined by Litan (1987), allows any 'safe' asset to be the balancing item of bank deposits. The safe assets can be anything from central bank reserves to traditional bank loans such as mortgages – depending on the proposal. Indeed, some of the narrow banking proposals are so permissive that they could not be labelled as FRB proposals. However, Litan (1987), Kareken (1986) and Spong (1996) would impose such strict restrictions on bank assets that they *would* qualify as FRB proposals.

Gordon Getty, according to Ferguson (1993), wanted to replace the financial system controlled by the Fed with a parallel system of mutual funds. Pollock (1993), on the other hand, suggested reviving mutual savings and loan associations, which would restrict the funding of investments to equity or shares. These types of FRB proposals are labelled **limited purpose banking**. Mutual fund shares would be effectively money backed by the asset portfolio. There would be no government insurance and no guarantee of par value clearance. Instead of banks, individuals would carry the risks. This would be a full-reserve system, but neither in government liabilities nor in commodities.

While Hotson (1985) and Schemmann (1991) wanted to carry out the **Chicago Plan** in a more modern context, Islamic banking was also discussed as an alternative way to organise the monetary system. According to Phillips (1994a, pp. 208-209), Islamic banking, which forbids charging interest, is also one type of FRB. Khan and Mirakhor (1985), Khan (1986; 1988) and Doak (1988) provide a detailed discussion on the connection between FRB and Islamic banking.

In 1998 Huerta de Soto (2009, ch. 9) proposed a **pure commodity standard** following a very liberal line of argument from the Austrian school. He proposed a FRB system which would offer total freedom of choice in currency; implement free banking; and abolish central banking. Thus, Huerta de Soto's proposal is built especially on the FRB proposals of Ludwig von Mises (1912), Friedrich Hayek (1937) and Murray Rothbard (1962) who opposed any monetary system in which the government would have significant influence on monetary policy – either through interest rates or the quantity of money. As Hayek (1937) and Rothbard (1962) demanded FRB only in gold, Huerta de Soto (2009, p. 739) made the same argument for a pure commodity standard although after the initial transition to a 100 percent gold standard he was willing to accept 'the spontaneous and gradual entrance of other monetary standards' as well.

Daly (1980), and other ecological economists, finally revived Soddy's (1926; 1934) **sovereign money** version of FRB. Rowbotham (1998) concentrated on a holistic analysis of the current monetary system and on the reasons for monetary reform, but he also presented his version of how to concretely implement the sovereign money system. According to Rowbotham (1998), the fraction of government money should be gradually increased either through government spending or basic income.

Huber and Robertson (2000) presented the first detailed proposal for sovereign money. Their main argument was that seigniorage revenue should be restored as the sole privilege of the government. Hence, all new money would be issued as public revenue and it would be spent into circulation by the government.

6. Aftermath of the Global Financial Crisis: Back to the Policy Agenda

The GFC sparked a new wave of proposals for, and academic research on, FRB. Recently, Martin Wolf (2014a; 2014b), the chief economics commentator at the *Financial Times*, supported FRB openly; the UK parliament debated on money creation; Switzerland is preparing a referendum on FRB; Iceland's Prime Minister commissioned a report on FRB; and bills to implement FRB have been put forward in the US and UK. FRB has indeed become a timely topic again.

Firstly, in this section, I outline the contemporary proposals for FRB. Then, I describe legislative initiatives and civil movements advocating FRB. Finally, I present academic modelling of FRB.

6.1 Proposals

Positive Money probably presents the most detailed version of FRB so far in Jackson and Dyson (2012). Positive Money's **sovereign money** proposal is written in the UK context and it has been endorsed by *Financial Times* columnist Martin Wolf (2014a). Kolehmainen et al (2013) is my co-authored proposal which adapts a sovereign money proposal for Finland.

Jackson and Dyson (2012) argue that money should be an asset to the holder, but not a liability to anybody. Contrary to previous FRB proposals, Jackson and Dyson (2012) and its former version Dyson et al (2011) suggest that deposits should be treated off-balance sheet in accounting. That is, all deposits would be held in custody at the central bank (although they also provide an alternative treatment where deposits would be held on-balance sheet at the central bank). They argue that coins in the US are actually treated in this way even today.

The transition from the current banking system to FRB would be done in an overnight switchover in Positive Money's proposal. Jackson and Dyson (2012) adopt Currie's (1934) proposal that demand deposits would be replaced in the balance sheets of banks with a 'conversion liability', which banks would have to repay to the central bank over a ten-to-20-year period of time. The objective of the conversion liability would be to reclaim seigniorage revenue from previously issued deposits back to the government. Thus, their proposal is in line with Huber and Robertson's (2000) previous proposal.

In Jackson and Dyson's (2012) system there would be two types of bank accounts. Current accounts called 'transaction accounts' and savings accounts called 'investment accounts'. No money would be actually held in savings accounts as the money would be transferred from an economic agent's current account to a bank's 'investment pool', which is the bank's current account for making loans. Savings accounts are thus promises by banks to pay money after a certain period. Jackson and Dyson (2012) introduce as a catch-all requirement that a bank must be able to repay the total sum of its current accounts at any time. This would effectively prevent any money creation by banks.

Jackson and Dyson (2012) propose that an independent body would decide how much new money should be created in order to prevent political abuse. The newly created (destroyed) money would simply be added to (subtracted from) the government's budget and, subsequently, a political body such as parliament would decide how the newly created money would be used (collected). Basically, there are four alternatives: increase government spending, cut taxes, make direct payments to citizens or pay off the national debt. Additionally, in order to avoid a credit crunch in some circumstances money could be created by lending it to banks on the condition that they re-lent it to the real economy. The monetary policy target would be unaffected unless decided otherwise. That is, the independent body responsible for money creation would target inflation.

Besides supporting Positive Money's FRB proposal in Wolf (2014a), Martin Wolf also came out with his own proposal. Otherwise Wolf's (2014b) proposal resembles to a large extent Positive Money's sovereign money proposal but it would also strongly increase capital requirements.

Herman Daly (2013) follows the arguments of Frederick Soddy (1926; 1934) and Lauchlin Currie (1934; 2004). He justifies FRB by arguing that it would better service a non-growing or de-growing economy. In addition, he argues that seigniorage revenue should entirely go to the government. In his sovereign money version of FRB monetary policy should be subject to parliamentary decision-making instead of being independent. Farley et al (2013) continue Daly's ecological justification of FRB.

Mayer's (2013a) proposal concentrates on the euro area and turns the established order of the EU Banking Union upside down. EU Banking Union means the establishment of a Single Supervisory Mechanism (SSM), Single Resolution Mechanism (SRM) and common deposit insurance scheme for the euro area (and an opt-in possibility for non-euro area EU states). Until now only SSM has been achieved as the ECB took over financial supervision of the largest banks from national supervisors in November 2014. SRM, which may require laborious change of the EU Treaties, is only being planned. Moreover, common deposit insurance has been postponed into the indefinite future.

Mayer (2013a) argues that the EU Banking Union should have been established starting from common deposit insurance, then SRM and finally SSM. Instead of governments guaranteeing bank deposits, Mayer suggests that FRB should be adopted to make deposit insurance obsolete. After that, according to Mayer (2013a), establishment of SRM and SSM would be more straightforward and the EU Banking Union would be more functional.

In addition, Mayer (2013b) provides seven accounting options for the central bank for how new money can be brought into circulation under FRB. For example, new money could be issued through negative equity. This would mean changing only the liabilities side of the central bank's balance sheet when issuing money. As the central bank cannot go bankrupt, it can operate with negative equity without any problems.

The idea of **deposited currency** was revived after the GFC by Gruen (2014) with his elaborate proposal. In Lainà (2015a) I make a similar proposal to allow central bank accounts[2] for all economic agents in Finland.

Also **narrow banking** has been recently proposed as a solution by DeGrauwe (2008), Kay (2009) and Phillips and Roselli (2009). In DeGrauwe's (2008) proposal narrow banks would be precluded from investing in equities, derivatives and complex structured products. Nevertheless, he does not explicitly determine the assets valid for backing deposits. Phillips and Roselli (2009) would allow government securities – in addition to central bank reserves – as the balancing assets. DeGrauwe (2008) suggests that maturity mismatch would not be allowed for any financial institutions other than narrow banks (i.e. the average duration of other financial institution's liabilities should equal the average duration of their assets). According to DeGrauwe (2008), if only a few countries would implement narrow banking, the banks of these countries would face a competitive disadvantage. Consequently, DeGrauwe (2008) demands also international coordination in order to avoid a regulatory race-to-the-bottom.

Kotlikoff (2010), on the other hand, suggests **limited purpose banking**, a variant of FRB in which each pool of investments made by a bank would be turned into a mutual fund. This would mean that there would be no maturity mismatch between a bank's assets and liabilities. In other words, banks would not be leveraged at all and they would be pure intermediaries between borrowers and lenders. Kotlikoff (2010) admits that it could lead to irrational collective exuberance (financial instability), but he argues that risks and rewards would be better aligned. Banks could not fail as they are not leveraged. Losses would be

[2] According to Godley and Lavoie (2006, p. 102), in some countries individuals are allowed to hold deposits at the central bank and, thus, already have a deposited currency.

absorbed by investors. The Bank of England's former governor Mervyn King (2010) discusses FRB and shows cautious support for it – especially for Kotlikoff's version.

Also Cochrane (2014) argues for limited purpose banking. As bank deposits are run-prone liabilities of banks, Cochrane (2014) argues that banks should be funded 100 percent with equity. According to Cochrane (2014), technology is already available for allowing everybody to sell assets (such as equities) and obtain fully-backed money instantly. Cochrane(2014) sees capital requirements as inefficient regulation and proposes taxing short-term bank debt instead in order to test whether run-prone liabilities are really worth having around. Furthermore, Cochrane (2014) argues that the central bank should include everybody as its counterparties when issuing reserves.

6.2 Legislation and Civil Movements

After Congressman Jerry Voorhis was defeated by Richard Nixon in the 1946 elections, there had not been any legislative initiatives to implement FRB in the US until the GFC. However, in 2011 Congressman Dennis Kucinich introduced a bill H.R. 2990 'National Emergency Employment Defense Act' (NEED Act) to implement FRB in the US. The draft version of the bill was known as the American Monetary Act. The bill, however, failed to pass.

In 2010 in the UK, a Member of Parliament, Douglas Carswell, introduced a short bill 'Financial Services (Regulation of Deposits and Lending)' which, in effect, would implement FRB in the UK. Unsurprisingly, the bill did not pass. Positive Money (2013) has drafted a much more detailed bill to implement FRB in the UK, but it has not been introduced yet.

The UK parliament, nevertheless, debated on money creation for the first time in 170 years on 20 November 2014. The debate was entitled 'Money Creation & Society'. Although no voting on legislation followed the debate, it certainly raised awareness of the monetary system and its alternatives among members of the UK parliament. Indeed, in the following year, the Green Party UK (2015) included FRB in their political agenda in their general elections manifesto.

Iceland is considering how to concretely put the idea of FRB into practice. Iceland's Prime Minister, Sigmundur David Gunnlaugsson, commissioned a report authored by Frosti Sigurjonsson (2015). The report has a chance to lead to legislation which would implement FRB in Iceland.

Sigurjonsson's (2015) report is very similar to Jackson and Dyson's (2012) proposal, but it gives more precise numbers. For instance, Sigurjonsson (2015) suggests a 45-day minimum maturity or notice period for time deposits. He would also set the interest rate on the conversion liability equal to the average current interest rate on demand deposits in order to avoid making banks better or worse off than in the current system.

Worldwide there are a number of political parties, NGOs and civil movements demanding FRB. Reforming money to function according to the FRB principle is one of the main goals of the following political parties: Green Party (UK), Money Reform Party (UK), Canadian Action Party (Canada), Humanwirtschaftspartei (Germany), Alternativet (Denmark) and Democrats for Social Credit (New Zealand). In Switzerland, Vollgeld-Initiative (Sovereign Money Initiative in English) is a project preparing a referendum on adopting FRB.

The International Movement for Monetary Reform is an umbrella organisation for national NGOs and civil movements propagating the idea of FRB. In addition to Positive Money in the UK, there are many national NGOs and civil movements advocating FRB, for instance, American Monetary Institute (US); Progressive Money (Canada); Sensible Money (Ireland); Fair Money (Australia); Positive Money NZ (New Zealand); Monetative (Germany); MoMo (Switzerland); Ons Geld (Netherlands); Monnaie Honnête (France); Moneta Positiva

(Italy); Dinero Positivo (Spain); Boa Moeda (Portugal); Dinero Justo (Puerto Rico); Positiva Pengar (Sweden); Gode Penge (Denmark); Betra Peningakerfi (Iceland); and Suomen Talousdemokratia (Finland).

6.3 Academic Modelling

Although in recent years there has been a revival of interest in FRB, it has so far been modelled little and with mixed methods. Indeed, it was never formally modelled until the GFC. After the GFC, FRB has been modelled in a dynamic stochastic general equilibrium (DSGE) framework, in a system dynamics framework, in a dynamic multiplier framework and in a stock-flow consistent (SFC) framework. Regardless of the diverse modelling approaches, according to the results, the consequences of adopting FRB seem to be widely positive. Next I will briefly go through these modelling results.

Benes and Kumhof (2012) conducted their study at the IMF and used the methodology of neoclassical economics – DSGE modelling – to reach the same conclusions as Irving Fisher (1935) almost eight decades earlier. According to Benes and Kumhof (2012), FRB would 1) provide better control of money supply and bank credit, which are a major source of business cycle fluctuations; 2) eliminate bank runs; 3) reduce public debt; and 4) reduce private debt. Furthermore, they found that output would increase by almost 10 percent and inflation could be dropped to zero without causing any problems. Later, Benes and Kumhof (2013) revised their paper but the results remained unchanged.

Yamaguchi (2010) modelled the NEED Act, and later refined the modelling in Yamaguchi (2011; 2014), using accounting system dynamics approach. Yamaguchi (2010; 2011; 2014) found that, in stark contrast to the current monetary system, under FRB government debt can be liquidated without triggering recession, unemployment or inflation.

Flaschel et al (2010) and later Chiarella et al (2011) showed in a dynamic multiplier framework that FRB provides a more stable financial environment than the current fractional-reserve banking system – even if appropriate monetary policy is conducted. Furthermore, they showed that under FRB a sufficient loan supply can be guaranteed (and that bank runs do not occur, which should be obvious, since the logic of FRB makes bank runs redundant).

Most recently, in Lainà (2015b) I modelled FRB in a SFC framework popularised by Godley and Lavoie (2006). I found that FRB can accommodate a zero growth economy and provide both full employment and zero inflation. In addition, FRB would not cause credit crunches or excessively volatile interest rates. Not surprisingly, money creation through government spending would lead to a temporary increase in real GDP and inflation. More surprising, however, is that money creation would also lead to a permanent reduction in consolidated government debt.

Until now, there are only a few attempts to model FRB – and even those have been conducted very recently. The results of various modelling methods seem to be tentatively promising – at least for proponents of FRB. However, for more general conclusions, more modelling is required.

7. Concluding Remarks

This paper provided a comprehensive outlook on historical and contemporary proposals for FRB. FRB was first proposed by David Ricardo in 1823. Ricardo's proposal served as a guideline for the Bank Charter Act which implemented FRB in the UK in 1844. Two decades later, FRB was also implemented in the US. Nevertheless, bank deposits slowly replaced bank notes fully-backed with government money. Since then, bank deposits have remained the dominant means of payment.

The FRB proposals have become particularly popular after serious financial crises, especially the Great Depression and the GFC – which both sparked a number of proposals for FRB. The supporters of FRB included many prominent economists such as Irving Fisher, Milton Friedman, Herman Daly and James Tobin. One of the most recent proposals came from Martin Wolf, the chief economics commentator at the *Financial Times*.

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A Commentary on Patrizio Lainà's 'Proposals for Full-Reserve Banking: A Historical Survey from David Ricardo to Martin Wolf'

Currency School versus Banking School: An Ongoing Confrontation

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Patrizio Lainà's paper, and our commentary here, reflect the perennial battle between the Currency and the Banking Schools.[1] The main contention of the Currency School is that the functions of money creation and financial intermediation not only are, but should be, separable, and only became entwined by a (reversible) accident of history whereby commercial banking developed on a fractional reserve basis in Europe, (i.e. an example of path dependence).

Thus Ronnie Phillips (1995) opens his book on '*The Chicago Plan and New Deal Banking Reform*', which is the main source of parts of Lainà's paper, as follows:

> 'In 1823 the great economist David Ricardo drafted a "Plan for the Establishment of a National Bank" that was published in February 1824, six months after his death. The document opened with the following statement:
>
> "The Bank of England performs two operations of banking, which are quite distinct, and have no necessary connection with each other: it issues a paper currency as a substitute for a metallic one; and it advances money in the way of loans, to merchants and others. That these two operations of banking have no necessary connection, will appear obvious from this – that they might be carried on by two separate bodies, without the slightest loss of advantage, either to the country, or to the merchants who receive accommodation from such loans (Ricardo, 1951, vol. 4, 276)"'

Phillips then goes on to show that Henry Simons, one of the founders of the Chicago Plan, explicitly modelled that on the 1844 Bank Act. Thus Phillips writes, (1995, p. 17):

> 'At one point Simons notes:
>
> "Your remark about the Bank of England reminds me that I got started toward this scheme of ours about ten years ago, by trying to figure out the possibilities of applying the principle of the English Act of 1844 to the deposits as well as to the notes of private banks. This Act would have been an almost

[1] The hey-day of this controversy was the 19th century. The best surveys are to be found in Arnon (2011) and Fetter (1965). The Currency School triumphed in the 1844 Bank Act, but the Banking School regained ascendancy by the end of that century. Currency School ideas have since resurfaced, perhaps temporarily, in the aftermath of the financial crises in the 1930s and in 2007/8.

> perfect solution of the banking problem, if bank issue could have been confined to notes (Simons to Fisher, January 19, 1934, Simons Papers)."
>
> 'Indeed a comparison of David Ricardo's "Plan for the Establishment of a National Bank," which served as a guide for the 1844 legislation, with the November 1933 Chicago memorandum indicates a striking similarity on several key points.'

One of the reasons sometimes put forward by Currency School advocates for this separation, though not emphasised by Lainà, is the claim that money creation should be a State monopoly,[2] so that having much of such creation done by private sector banks is, in some senses, an inappropriate transfer of seignorage from the public sector to private sector bodies. A problem with this position is that many of these same economists would probably also endorse the (invalid) Karl Menger (1892) theory of the creation of money as a *private sector* market response to the constraints of bartering, in which story the government only plays a subsidiary role.[3] Holding both positions simultaneously would seem to be logically inconsistent.

In any case the proposed separation of money creation and financial intermediation then leads on to the question of what should then determine the level and growth of the separately provided money stock. Here there is a stark divide between Currency and Banking School supporters. The Currency School supporters, almost to a man, propose rules, but a wide variety of rules: a gold standard rule (Ricardo, 1824), a k-percent rule (Friedman, 1960), a price level rule (Fisher, 1935), or an inflation target, or whatever the politicians want. Even nowadays, when there is an unusual degree of harmony around the maintenance of a 2% inflation target, there are those who challenge whether this target should be replaced by something else. Whereas supporters of the Currency School prefer rules, there remains much debate amongst them over which rule to adopt. Banking School proponents prefer discretion and flexibility.[4] No rule can take account of all eventualities. To Banking School adherents the financial system is evolutionary, not static,[5] and a rule adopted in one set of circumstances may soon become out-dated and inappropriate.

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[2] See Gaitskell (1933, pp 377-379) Here Soddy's moral argument is discussed by Gaitskell: 'The issuer of money gets "something for nothing" and it therefore should be the prerogative of the state to engage in such activity'. Also 'manufacture of currency used to be the privilege of the crown', the community alone should reap the benefits which the creator of money obtains. Also see Wolf (2014), Fisher (1935); and Jackson and Dyson (2013).

[3] Thus Desan (2014, p. 27) writes on 'The conventional creation story' that,

> 'Many narratives stage its start in the wild simplicity of an early world. In that conjured space, exchange was a murky broth of barter. People traded all sorts of objects among themselves – grain, gold, cows and hides, promises, services, cider, and salt. In the fluid mix of exchange, they found silver and gold especially easy to give and take. Metal gradually rose like fat to the surface, becoming a favored medium and marker of value as it passed endlessly from hand to hand. People cut silver and gold into pieces to make the process easier and more regular; disks of the commodity became coin. Its brokers were buyers and sellers converging upon pieces of precious metal to mediate each transaction and, ultimately, to create prices in a common medium.
>
> Content changes and the government assists as society becomes more complicated or bankers become more powerful – but the medium has a constancy across all those details that is clearly sourced in the primal spring of exchange.'

Also, see Goodhart (1998).

[4] For earlier examples, see Tooke (1844), Laidler (1972) and Arnon (2011, Chapter 12). For some more recent examples, see Modigliani (1977), Tinbergen (1952), Goodhart (1989) and Greenspan (1997).

[5] This can be argued by referring to the continuous evolution of banking legislation in the UK and the US as *The Economist* (2015) author writes: 'Another important issue for academics to consider is that the financial sector is not static. Each crisis induces changes in behaviour and new regulations that prompt market participants to adjust (and to find new ways to game the system).'

The 1844 Bank Charter Act subsequently had to be suspended during crises, and soon ceased to operate as initially intended. To Currency School supporters this was due to a (somewhat accidental) shift from notes to bank deposits as the main component of money. To Banking School adherents, there was nothing accidental about this shift; if the authorities try to impose constraints on the private sector's access to liquidity, it will attempt to innovate its way around that.[6] Crises have invariably found strict rules of money creation to be wanting (e.g. 1907 in the USA, 1914 in London, see Roberts, 2013), and have led to calls for 'a more elastic currency'. The adoption of 'full-allotment' by the ECB, and the introduction of a whole gamut of schemes by the Fed and Bank of England, to allow the banking system, and near-banks, to obtain the liquidity that they craved during the Great Financial Crisis (GFC), 2008/9, were typical examples of the application of Banking School principles.

It is somewhat odd in some ways that the 2008/9 GFC has called forth greater interest in FRB. Central Banks responded flexibly in unconventional ways to the GFC, leading to a huge increase in the monetary base. Had a rule-based money creation been in place, would we have got through it as well as we did? (Banking School) opponents of Currency School monetary rules believe that such rules will tend to be too inflexible, and quite often too deflationary.[7] For example, Ann Pettifor, (2013, p. 22), states that,

> 'Linking all current and future activity to a fixed quantity of reserves (or bars of gold, or supplies of fossil fuel) limits the ability of the (public and private) banking system to generate sufficient and varied credit for society's purposeful and hopefully expanding economic activity.'

Currency School advocates might respond by claiming first that, without a fractional-reserve banking system there would have been no crisis in the first place, and second that, with a price level, or even an inflation, target the money supply should have been forced to increase even more than it did.[8] Perhaps. One cannot help noting that many of those who prefer rules were worried that the increase in the money base was excessive, and would cause serious inflation if not now, then sometime in the future.

Be that as it may, the Banking School may lose a few battles (as in 1844), but usually wins the war. One reason for this is that the monetary authorities like to maintain discretionary control, and do not much want to be constrained by the rules that academic economists propose. Per contra, academic economists generally prefer rules to discretion. Even Tobin (1985; 1987) flirted with narrow bank proposals. Besides the time inconsistency argument, economists can devise rules that provide 'optimal' welfare in the context of their own models, which they naturally wish to proselytise.

Perhaps the main problem for followers of the Currency School (and FRB) is that, in order for their proposals to work, there needs to be a clear, hard and fast, distinction between 'money' and 'near-money'.[9] Let us take an example. Suppose that narrow, FRB, banks were

6 Tooke (1856), Arnon (2011, Chapter 12).

7 Allen (1993, p. 715, footnote 49), writing on 'Irving Fisher and the 100% Reserve Proposal', records that Keynes declined Fisher's plea to become an 'advocate'.

> 'In my judgment deflation is in the near future a much more dangerous risk than inflation. I am afraid of your formula because I think it would, certainly in England, have a highly deflationary suggestion to a great many people. Apart from that, I am satisfied that in British conditions anyhow… we can obtain complete control over the quantity of money by means much less capable of exciting unfavourable comment and opposition." (Letter from Keynes to Fisher, July 7, 1944 [Yale].)'

8 Jackson and Dyson (2013).

9 Prior to 1844 the Currency School supporters did not recognise deposits as having any importance as circulating medium, while the Banking School advocates stressed the fact that only controlling bank notes was not sufficient (Fetter, 1965, p. 187).

established, while other (risky) banks continued to be allowed to offer seven-day time deposits, as now. Banks would still be able to make loans by writing up both sides of their balance sheet, only in the form of short-dated time deposits rather than demand (sight) deposits. Borrowers would have to wait a week before accessing their funds, but that is a short time for most purposes. It would, of course, make the use of credit cards considerably more expensive, since retailers would have to wait before getting paid, (n.b. debit cards could only be issued by narrow, FRB banks).

Such a system would be even more systemically dangerous than at present. As noted in Goodhart's earlier papers (1987 and 1993), private sector agents would shift the bulk of their liquid funds to risky banks during normal times. Such time deposits would have a higher return, better ancillary services (book-keeping, investment advice, access to credit, etc.), and could normally be switched back into claims on a narrow bank easily and just-in-time to make necessary payment.[10] *Per contra*, when fears about the solvency of risky banks arose during crises, (n.b. the standard Currency School proposal is to withdraw all deposit insurance and the public sector safety net from the risky banks), there would be a rush by private sector agents to switch funds from risky banks to FRB banks. Such a system would become even more terrifyingly pro-cyclical, indeed a recipe for disaster.

So, in order for the FRB system to work safely, banks would have to be banned from offering any form of liquid, short-maturity liability. Perhaps a one or three month time deposit, with a rigorously enforced (how?) penalty for early encashment, should be the most liquid liability which the risky banks could be allowed to offer. And what about marketable certificates of deposit? Would it be possible to impose a sharp and deep dividing line between the sight deposits of the FRB banks and the necessarily illiquid liabilities of the risky banks? And if such a division could be achieved, would not other intermediaries rush in to fill the gap? In a system without controls on international capital movements, and with electronic banking, banks situated abroad could still provide a full range of commercial banking in the domestic currency, transferring funds to and from the FRB banks instantaneously as and when a payment needed to be made. Even in a closed economy, a variety of non-banks (e.g. IT companies) could move easily to fill the gap in the liquidity spectrum needed to make the FRB, Currency School program work.

Henry Simons recognised this problem clearly, (unlike several of his colleagues, e.g. Irving Fisher; Simons comes across in Ronnie Phillips book as a particularly sensible economist). Thus Phillips (1995, pp 89-90) wrote:

> 'In a letter to Paul Douglas, Simons added the post-script:
>
> "Have been a little upset lately about the banking scheme – trying to figure out how to keep deposit banking from growing up extensively outside the special banks with the 100% reserves. Just what should be done, for example, to prevent savings banks (a) from acquiring funds which the depositors would regard as liquid cash reserves or (b) from providing through drafts a fair substitute for checking facilities? After all, it is important that the reform which we propose should be more than nominal! The problem can be dealt with, of course; but just what is the best combination of expedients?

[10] Governments have frequently provided narrow-type banks providing safe-keeping and payment functions, usually as a service to the otherwise un-banked poorer segments of the population. The Post Office Savings Bank (POSB) in the UK is an example. As a generality such narrow banks have rarely prospered when in open and free competition with commercial banks.

> Perhaps you will have some suggestions to pass on" (Simons to Douglas, January 25, 1934, Simons Papers).
>
> Continuing concern is also emphasized in a letter to Fisher in which he wrote: "Much is gained by our coming to regard demand deposits as virtual equivalents of cash; but the main point is likely to be lost if we fail to recognize that savings-deposits, treasury certificates, and even commercial paper are almost as close to demand deposits as are demand deposits to legal-tender currency. The whole problem which we now associate with commercial banking might easily reappear in other forms of financial arrangements. There can be no adequate stability under any system which permits lenders to force financial institutions into effort at wholesale liquidation, and thus to compel industry to disinvest rapidly – for orderly disinvestment on a large scale is simply impossible under modern conditions. Little would be gained by putting demand deposit banking on a 100% basis, if that change were accompanied by increasing disposition to hold, and increasing facilities for holding, liquid 'cash' reserves in the form of time-deposits. The fact that such deposits cannot serve as circulated medium is not decisively important; for they are an effective substitute medium for purposes of cash balances. The expansion of demand deposits,[11] releasing circulating medium from 'hoards,' might be just as inflationary as expansion of demand deposits – and their contraction just as deflationary; and the problem of runs would still be with us" (Simons to Fisher, July 4, 1934, Simons Papers).'"

Although Simons recognised the problem, neither he, nor anyone else, to the best of our knowledge, has ever managed to resolve it. It is the Achilles heel of the Currency School, and most proponents deal with it by ignoring it.

If it were possible to maintain such a gap between narrow money and illiquid risky bank liabilities there could be further structural problems. When a bank makes a loan, it expects the money to be spent and usually end up in another bank. But, unless it is expanding far faster than average, it will expect to get back its share of the available high-powered-money. Under the 'risky' bank system that reflux[12] could be expected to be far less; indeed that is the intention. If so, all lending which occurs at the initiative of the borrower, up to a pre-committed limit set by the bank, e.g. credit cards, overdrafts, etc., would either have to be withdrawn or made more expensive and less attractive. Indeed, there are concerns that such a system, with all risky bank lending financed by long-term liabilities, would not be in a good position to meet the short-term, working capital needs of industry. Thus Phillips, (1995, pp. 149-50), notes that,

> 'Thomas was concerned, however, that the 100 percent reserve system might lead to the total abolition of short-term lending, which would present difficulties for business borrowing over the business cycle (Thomas 1940, 318). During an economic downturn, a fall in loans would lead to increases in reserves of the lending institutions, thereby decreasing the money supply. This is why, Thomas notes, that the institutions offering savings and time

[11] Sic. He presumably meant 'time deposits'.

[12] Concern about the extent of such reflux is another hallmark of Banking School theorists, notably Fullarton (1844).

deposits must be investment trusts without the privilege of making short-term loans (Thomas 1940, 319). The result, Thomas argued, would be to "drive a large volume of such borrowing into the field of trade credit."'

Moreover the longer term liabilities that the risky banks could offer would, because they would be less liquid, require a higher rate of interest. Such extra costs would get passed onto the borrower. So, the FRB system would result in a system of risky bank loans that would be both more expensive and less flexibly available. The private sector would find its access both to liquid assets and to borrowing from banks impaired. A FRB (Chicago Plan) supporter would, no doubt, claim that this would be a small price to pay if financial stability could be thereby ensured. But that would require a whole panoply of restrictions on the issuance of near-moneys, and/or banking abroad, and it is doubtful that they could be deployed.

Furthermore (investment) banks facilitate the smooth functioning of capital markets by using their balance sheets elastically to offset sizeable discrete shifts in market flows, e.g. by underwriting and managing IPOs in the primary market and by acting as market makers in secondary markets. If investment banks could not vary their books flexibly by adjusting their needs in short-term money markets, then capital markets would become less liquid with significantly higher transactions costs. The contribution of liquid secondary markets was also highlighted by Lavington (1934)[13] in his book *The English Capital Market*. He argued that liquidity draws forth more investment, as opposed to a less liquid market which would deter the risk averse investors, increasing financing costs. Keynes (1934, Chap. 12) also recognised the importance of liquid secondary markets despite the risk of speculation creating instability. The alternative would be increased hoarding and less available financing capital due to illiquid markets.

One of the great attractions of the Currency School (Chicago Plan, FRB, narrow bank) proposals, however, is their elegant simplicity. Separate money creation and intermediation between savers and lenders. Control money creation through some rule, and then leave intermediaries free to compete without government regulation or support. We have tried to demonstrate why this simple version is flawed. But you cannot beat something with nothing. What alternative structure does the Banking School have to offer? This is somewhat more difficult to set out clearly since the Banking School has traditionally been rather fuzzier in its proposals.[14] This is partly because Banking School adherents have been more inclined to work backwards from practical empirical observation towards general principles, whereas Currency School have tended to work forwards from certain theoretical axioms to more general conclusions. But, perhaps, the Banking School can be represented as generally holding the following beliefs, (Arnon, 2011):

- In the spectrum of liquid assets there is no clear gap between monetary assets and quasi-monetary assets;
- The determination of the money stock (any definition) is largely endogenous;
- Causation runs as much from the macro-economy to money, as vice versa;
- Credit creation is the key link between money and the real economy; control over credit creation is vital, but difficult.

[13] Also see Toporowski (2005).
[14] Viner (1932, p. 220) and Fetter (1965, p. 191).

Instead of rules the Banking School tends to aver that the various monetary aggregates should respond flexibly to the 'needs of trade'.[15] This is best done by having the Central Bank set its interest rate to achieve some macro-economic target, (e.g. the Gold Standard, via the Palmer Rules, but not blindly following them), or an Inflation Target (via the Taylor Reaction Function, again using judgment), and then meeting all commercial banks' demand for reserves at this chosen rate. Thus the monetary base becomes an endogenous variable, and the money multiplier works in reverse to determine H, *not* M. Similarly, given the official short rate set by the Central Bank, commercial banks should set their terms and conditions for lending, and then meet the needs of all potential borrowers who can satisfy those conditions.

This is, of course, broadly how modern monetary systems work, i.e. according to Banking School precepts. But there is an inherent problem with it; this is that borrowers and banks tend to behave pro-cyclically, getting over-excited and over-optimistic in booms and too risk averse in busts.

Banking School supporters used to think that they had a well-designed response to this, in the guise of the 'Real Bills Doctrine'.[16] Unlike nowadays where most bank lending is property related, in the 19th century under the Anglo-Saxon banking model, most bank lending went to industry to finance trade, inventories and working capital. [17] If trade increased, then output would increase in line, and under the Quantity Theory, the monetary aggregates could increase alongside without any increase in prices.[18]

The opposite of a 'real bill' was (not a 'nominal bill' but) a speculative, or a 'finance' bill, drawn not against productive trade and output, but for a speculative investment in an asset which was hoped to rise in price. Thus the counterpart instrument (to the choice of interest rate) which a Central Bank would use, prior to the 1930s, to maintain financial stability was to assess the quality of private sector bill finance in the money market, and to discriminate against low-quality finance bills, e.g. both by refusing to discount them and by communicating its warnings to the relevant market participants.

This 'real bill' doctrine had numerous advantages. At a time when data were scarce and Central Banks were, in most cases, unable to supervise commercial banks directly, it played to a Central Bank's strength as the key player in the money market. The doctrine unified macro-monetary policy, (an increase in M based on expanded trade will not be inflationary), and financial stability policy (preventing an expansion of 'finance' bills will constrain boom and bust), to a degree never subsequently achieved.

But the real bills doctrine was, unfortunately, wrong.[19] In a boom, trade can expand and unemployment fall, beyond the sustainable level leading to subsequent upwards pressures on prices and inflation. Much more important, if a severe depression should occur, trade will contract to a point where the Central Bank will not be presented with sufficient trade bills to discount to generate enough cash/liquidity to return the economy to equilibrium. The

[15] See Arnon (2011) for 19th Century discussions on this. More modern writers include Kaldor (1986), and Moore (1981; 1988).

[16] For a concise guide to this doctrine, see Green (1989). It was a key pillar of the Banking School, e.g. Fullarton (1844) and Tooke (1844), and has been consistently attacked by the Currency School from Ricardo and Thornton (1802) onwards.

[17] In the 19th, and earlier centuries, increases in government expenditures were usually war-related. Wars did not increase real incomes and output. Hence bank lending to governments was regarded as inherently inflationary.

[18] 'If the loans or discounts are advanced on proper banking securities, for short periods, the reflux of the notes, if any have been issued, will be equal to the efflux, leaving the circulation unaltered. If, indeed, the transactions of the district, or the trade of the country generally, require more instruments of exchange, a larger amount of notes would remain out; but this increase of the outstanding circulation would be the effect of increased transactions and prices, and not the cause of them' (Tooke, 1848, p. 194. Also see, p. 185).

[19] Although wrong, it had some illustrious forebears, notably Smith, (1776, Chapter 2) 'Of Money'; also see Arnon (2011, Chapter 2).

Federal Reserve System had been created in 1913 specifically to operate along procedures established by the Banking School 'real bills' doctrine.[20] There is, alas, little doubt that a, somewhat slavish, adherence to the 'real bills' doctrine by the Fed played a significant role in the intensification of the Great Depression (1929-33). Part of the attraction of the Currency School's Chicago Plan in the 1930s was not only the purported merits of the scheme itself, but also a general acceptance of the claim that (in the USA at any rate) the 'real bills' doctrine of the Banking School had not only failed, but had failed disastrously.[21]

In so far as there is a current analogue to the 1930s failure of the 'real bills' doctrine, it probably lies in the failure of bank regulation to prevent the prior boom and subsequent GFC bust in 2008/9. Currency School supporters express doubt that regulation can ever achieve a satisfactory balance between control and evolutionary growth. Would it not be better to have a tightly managed, protected, core payments/monetary system, and then allow intermediation outside those limits to be largely unregulated, and unsupported by any safety net? The Currency School arguments are certainly seductive, which is, of course, why they persist in 2015, just as they did two centuries ago.

Indeed the Vickers Report in the UK (2011) is suffused with Currency School ideas. To an adherent of the Banking School, the attempt to separate money from near-money assets, or money issuing entities from other intermediaries will prove illusory and self-defeating. And the idea that the money-issuing bodies should be protected by the public sector safety net, while the rest, e.g. the risky investment banks, can be left to the mercy of the market and the operation of special resolution and bankruptcy laws, is just wrong. Recall that Lehman Bros did not offer demand deposits, and in 2008 was not even classified as a bank!

For the Banking School the essential requirement is that the quality, i.e. maturity, risk, etc., of an intermediary's assets should match that of its liabilities. If the liabilities are very short-term and of fixed value, then the assets should also be liquid, subject to little price variation, with enough equity backing to meet any expected declines in asset prices.[22]

A basic problem has been that the banking sector has departed massively from such Banking School precepts. Instead of making short-term, self-liquidating loans to industry, it is now making long-term, illiquid, property-related, mortgage loans to individuals, and on commercial real estate. Meanwhile banks' own liquid assets were massively run-down under

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[20] See Meltzer (2003, pp 69-71); Bordo and Wheelock (2013); Calomiris (2013); Sissoko (2015).

[21] Lloyd Mints (1945), a Chicago-based monetary economist, wrote in the Preface of his book, *A History of Banking Theory*, that,

> 'Monetary theory is a matter of paramount importance in a free-market economy; but, to the present time, banking legislation has been too much controlled, in the United States at any rate, by the belief that a restriction of the banks to the making of loans for bona fide commercial purposes will automatically provide for all needed variations in the means of payment. This belief, which I have called the "real-bills doctrine," is utterly subversive of any rational attack on the problem of monetary policy. If there is a central theme in what I have written, it is that this doctrine is unsound in all its aspects.'

[22] In the April 1861 edition of *The Economist*, in the article on 'How to read Joint Stock bank accounts', Walter Bagehot (1861) warned against judging a bank primarily on the adequacy of its capital and reserves. Rather, 'we should add together all the liabilities of the bank – its circulation, its drafts, and its deposits: see what the total is carefully; and then we should compare it with the amount of cash, loans to bill brokers, Government securities, and other immediately tangible and convertible assets which the bank has in hand. If the available money bears a good proportion to the possible claims, the bank is a good and secure bank'. On the question of 'the specific proportion between the cash reserve and the liabilities of the bank to the public', Bagehot refused to 'lay down any technical or theoretical rule upon it'. The cash ratio must be allowed 'to vary in some degree with the nature of the bank's business'. Not for Bagehot rigid control of the banking system through operations on the cash base and a stable multiplier. But then, the Banking School was a family matter for him; he had married Eliza Wilson, daughter of James Wilson, an early member of the Banking School and founder of *The Economist* in 1843. See Arnon (2011, p. 245).

the influence of the myth that any bank can always borrow necessary cash from wholesale markets. Their equity capital was, furthermore, way below that necessary to ride out a housing bust.

There is overwhelming evidence that the GFC, and indeed most post-WWII financial crises, have been the result of an interaction between cycles in the housing market and bank credit expansion, see the many papers by Jorda, Schularick and Taylor.[23] It is this nexus that needs fundamental reform, (in addition to the needed rebuilding of equity capital and liquid assets). Indeed one effect of the Vickers Report will be to focus the assets of the ring-fenced retail banks even more heavily on residential mortgages, thereby making the system even riskier.

One reason why there has been so little attention paid to the deleterious effect of allowing banks to fill their portfolios with long-dated, illiquid mortgages was that the encouragement of house-ownership has been, in most developed countries, a major plank of government policy. So, getting banks to finance home-ownership was consciously encouraged by (various aspects of) government policy (Wallison, 2015). Rather than do a thorough review of housing finance, with the aim of returning bank balance sheets to their traditional composition, it is easier to leave the housing/bank credit nexus untouched while blaming investment bank 'culture' and toxic exotic derivatives for the GFC, and supposing that (hesitant) moves towards separation in the banking sector will do the trick. It will not do so.

Meanwhile, the perennial battle between the Currency and the Banking Schools will continue, as the contents of Lainà's paper and our commentary on it indicate.

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[23] For example, see Jordá, Schularik and Taylor (2014; and 2015).

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A Hayekian Explanation of Hayek's 'Epistemic Turn'

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Abstract

The present essay aims to account for F.A. Hayek's oft-noted 'turn' away from technical economics to concerns of a more philosophical nature. In particular, the paper seeks an explanatory principle that reconciles various elements of both continuity and discontinuity in Hayek's intellectual development, especially with respect to the evolution of his arguments concerning economic fluctuations. The essay uncovers such an explanatory principle in Hayek's own methodology of sciences of complex phenomena. According to this principle, an inquirer who confronts phenomena too complex for adequate explanation on the basis of current knowledge must move to a more general, albeit less testable, explanation. This is precisely what occurred in the evolution of Hayek's thought concerning trade cycles. The concluding section considers the implications of the argument for the extensive secondary literature on Hayek's 'transformation'.

Keywords: Hayek, business cycle theory, methodology, complex phenomena, pattern prediction, explanation of the principle

1. Continuity and Discontinuity in the Intellectual Development of F.A. Hayek

It has been often noted – including by the man himself (Hayek, 1964b [1967], p. 91) – that F.A. Hayek's career as a 'very pure and narrow economic theorist' came to a rather abrupt end, sometime around the publication of 1941's *The Pure Theory of Capital* (Hayek, 1941 [2007]) and that his subsequent career led 'into all kinds of questions usually regarded as philosophical' (Hayek, 1964b [1967], p. 91). However, opinions differ concerning the exact nature and extent of what Bruce Caldwell (1988; 2004) called 'Hayek's transformation'.[1] Indeed, the questions about whether and which of Hayek's scientific, methodological, and philosophical attitudes were more or less continuous across the arc of his career remains perhaps *the* central issue in Hayek scholarship – what did Hayek give up, and what did he retain, when he 'transformed'? The secondary literature is rife with arguments concerning this or that aspect of Hayek's early writings that were purportedly rejected – or retained – over the course of the subsequent development of his ideas, and with attempts to draw more general conclusions concerning the unity of Hayek's thought from such vignettes.

I have made two, confessedly minor, contributions to this literature. My 'Hayek the Apriorist?' (Scheall, 2015a) argues against Terence Hutchison's (1981) well-known 'Hayek I'

[1] Jack Birner (1999) coined the phrase 'epistemic turn' to describe (his own view of) Hayek's transformation. I have suggested elsewhere that 'Hayek's "epistemic turn" was less a *turn* than an *enlargement* of elements that were always present in his thought' (Scheall, 2015b, p. 103). Nonetheless, for reasons that should become clear as the argument of the paper progresses, I have adopted 'epistemic turn' rather than 'transformation' in the title of the essay.

thesis that Hayek's early career in economics bore the mark of a commitment to methodological apriorism (the paper is largely silent concerning Hutchison's thesis that there was a Popperian 'Hayek II'). Elsewhere, I have argued that one can infer from several of Hayek's post-transformation writings an 'epistemic' theory of industrial fluctuations that is closely related to his famous business cycle theory of the interwar years (Scheall, 2015b). Both Hayek's earlier and later accounts attribute economic fluctuations to actors ignorant of knowledge required to prevent disequilibrating effects. However, I also emphasise in the latter paper certain discontinuities between the theoretical foundations of Hayek's interwar business cycle theory and his later 'epistemic' account.

In the present paper, I am less interested in contributing further to the already extensive literature on purported continuities and discontinuities in Hayek's intellectual development. Rather, my aim here is to find an explanatory principle that unifies seemingly disparate elements of Hayek's thought already uncovered in the literature. In particular, I'm looking for an explanation of both the continuities and discontinuities posited in the aforementioned 'Hayek's Epistemic Theory of Industrial Fluctuations' (Scheall, 2015b). The main thesis advanced in the present paper is that one can discover, in Hayek's own methodology of sciences of complex phenomena, an 'explanation of the principle' of both (a) the continuity concerning the causal role in propagating economic fluctuations of action based on an inadequate epistemic basis and (b) the discontinuity in the theories he adopted at different times to explain the sources, and disequilibrating economic effects, of action founded on deficient knowledge. In the concluding section, I discuss how this same principle might unify other seemingly irreconcilable continuities and discontinuities discussed in the existing secondary literature.

The present paper builds a Hayekian explanation of Hayek's epistemic turn[2] upon the premises that 1) Hayek's early career was dedicated to the investigation of some very complex phenomena, namely, those of the business cycle, using the tools of technical economic analysis then available.[3] Hayek thought that this project ultimately failed, at least in part, because the complexity of the relevant phenomena outran the explanatory capacities of these tools. 2) Hayek (1964a [1967], p. 29; also see 1961 [2014], p. 381-382) spent much of his subsequent career developing a methodology of sciences of complex phenomena, according to which the inquirer confronted with phenomena of a degree of complexity beyond explanatory tractability must take refuge in a system of 'higher-level generalities' that subsume some of the otherwise inexplicable complexities of the relevant phenomena. 3) Hayek's post-turn writings indicate that his thought had moved from the theoretical plane of

[2] Strictly speaking, the argument of the current paper contributes to an explanation of merely an *aspect* of Hayek's epistemic turn, i.e., the development of his thought concerning industrial fluctuations, and it is only insofar as this development is representative of his broader intellectual evolution that the argument contributes to an explanation of his 'transformation' or 'epistemic turn'. No very robust argument will be offered here for connecting the changes in his thought concerning fluctuations with his broader intellectual evolution. Indeed, quite the opposite – it is not for nothing that the phrase 'epistemic turn' appears in scare quotes in the paper's title. As we will see, it is probably impossible to explain more than particular aspects of Hayek's career, and any attempt to infer from such specific episodes some more general conclusion regarding the continuity of Hayek's thought is specious. It is (in part) the fact that the present paper both exemplifies and emphasises certain limitations on explanations of a scholar's intellectual development that distinguishes it from the remainder of the existing literature on Hayek's transformation.

[3] 'Technical economics' was Hayek's own term for what he took himself to be doing prior to the publication of 'Economics and Knowledge' (Hayek, 1937 [1948]): 'This brings me to what in my personal development was the starting point of all these reflections, and which may explain why, though at one time a very pure and narrow economic theorist, I was led from technical economics into all kinds of questions usually regarded as philosophical. When I look back, it seems to have all begun, nearly thirty years ago, with an essay on "Economics and Knowledge"' (Hayek, 1964b [1967], p. 91).

generalities about the phenomena of industrial fluctuations, to the higher-level methodological plane of generalities about the latter kind of theoretical generalities.

In short, Hayek's methodology of sciences of complex phenomena contributes to an explanation of the development of his thought concerning industrial fluctuations. According to this methodology, an inquirer who runs into phenomena too complex for adequate explanation on the basis of current knowledge must move to a more general, albeit less testable, explanation. This is precisely what happened in the evolution of Hayek's thought concerning economic fluctuations.

2. The Argument of 'Hayek's Epistemic Theory of Industrial Fluctuations' Revisited

The current essay was originally conceived as part of a larger project that included my 'Hayek's Epistemic Theory of Industrial Fluctuations' (Scheall, 2015b). It was only relatively late in the development of this project that I split what was becoming a very long (and, thus, probably unpublishable) single essay into two stand-alone papers. Although these articles advance distinct theses, in order to appreciate the present paper, it is important to have some understanding of the argument of 'Hayek's Epistemic Theory', an essay that considers the development over some forty-plus years of Hayek's thought concerning economic fluctuations.[4]

I defend three theses in 'Hayek's Epistemic Theory', the most central being that, if one combines the main arguments of Hayek's 'Economics and Knowledge' (Hayek, 1937 [1948]), 'The Use of Knowledge in Society' (Hayek, 1945 [1948]), and 'The Pretence of Knowledge' (Hayek, 1975 [1978]), the result is a (sketch) of a theory that attributes industrial fluctuations to action based on deficient knowledge. More carefully, in 'Economics and Knowledge', Hayek (1937 [1948]) developed an epistemic conception of economic equilibrium – or, as he later preferred to call it, economic 'order' (Hayek, 1968 [1978]) – according to which 'equilibrium exists to the extent that economically-relevant beliefs of individual market participants are mutually consistent and accurate with respect to the external facts' (Scheall, 2015b, p. 115). The problem left unresolved in 'Economics and Knowledge' concerns the mechanism(s) by which the empirically observable tendency toward equilibrium in this epistemic sense is either facilitated or impeded.[5] In 'The Use of Knowledge in Society', Hayek (1945 [1948]) argued that a system of freely adjusting prices is necessary to facilitate the tendency toward coordination of relevant beliefs. In the absence of exogenous interference, the price system *qua* epistemic mechanism operates – not perfectly, but well enough (Scheall, 2015b, p. 105, footnote 1) – to facilitate the tendency toward belief coordination. Finally, in his 1974 Nobel Prize lecture, Hayek (1975 [1978]) argued that policymakers do indeed regularly interfere with the price system. As I put the point in 'Hayek's Epistemic Theory',

> 'blinded by a false ["scientistic"] methodology, the economic policymaker is led into a "pretence of knowledge" upon which she acts, unawares – indeed, convinced otherwise – of the irrelevance and inadequacy of her epistemic position. Policymakers are misled into the false belief that they can possess both the theoretical and empirical knowledge required of effective

[4] I use the terms *industrial* (*economic*) *fluctuations*, *business* (*trade*) *cycles*, and *episodes of disequilibrium* interchangeably despite certain differences in the technical meanings of these terms. I justify this stipulation at some length in 'Hayek's Epistemic Theory' (Scheall, 2015b, p. 102, footnote 2).

[5] 'Experience shows us that something of this sort does happen, since the empirical observation that prices tend to correspond to costs was the beginning of our science' (Hayek, 1937 [1948], p. 51).

> macroeconomic management by the combination of a methodology that accords special status to measurable parameters, a [Keynesian] theory that makes the relevant parameters those that just happen to be measurable, and the statistical techniques for the analysis of the aggregative variables in which the latter [Keynesian] theory trucks.
>
> What's more, when policymakers pretend to possess the relevant economic knowledge and make policy on the basis of this pretence, their decisions typically impede, either directly or indirectly, the price system's knowledge-coordinating function.[6] It suffices to hamper the operation of the tendency toward equilibrium for those in a position to do so to intervene on the basis of knowledge that they don't in fact possess in a way that fetters the adjustment of the price system to changes in economic circumstances. Such is Hayek's epistemic theory of industrial fluctuations' (Scheall, 2015b, p. 109).

Beyond defending this theory-sketch, I further argue in 'Hayek's Epistemic Theory' that ignorance plays a similar part in Hayek's famous early theory of the cycle as it does in his subsequent epistemic account. The problem of action in the economy on an inadequate epistemic basis is a motive for, and causal factor in, both accounts. The epistemic theory assigns ignorant action a *general* causal role in propagating fluctuations: dis-coordination will follow wherever the price system is exogenously manipulated (directly or indirectly) on the basis of knowledge inadequate to facilitate the epistemic function of freely-adjusting prices. It just so happens that, according to Hayek, it is policymakers who most often (and with comparatively more general, and thus, more dire consequences) interfere with the price system on a deficient epistemic basis. On the other hand, Hayek's early theory assigns ignorant action a more specific causal role in the cycle. In order to avoid industrial fluctuations, bankers must possess knowledge of the so-called 'natural' rate(s) of interest that would balance the supply of voluntary savings with the demand for loans. However, the natural rate cannot be known, so bankers cannot know, at any particular time, whether they are artificially increasing (or decreasing) the supply of loanable funds, i.e., expanding (or contracting) credit, beyond the supply of voluntary savings. Thus, it turns out that, despite certain fundamental discontinuities between the two theories,[7] the earlier theory is a special case of the later, more general, epistemic account, at least with respect to the causal role that each theory assigns to action founded on deficient knowledge.

Thus, Hayek's concern for the economic effects of ignorant action remained continuous (and, indeed, only grew more general) throughout his multiple attempts to explain industrial fluctuations, but the foundations of these theories were markedly discontinuous. My concern in the present paper is to show that, in his methodology of sciences of complex phenomena, Hayek (consciously or otherwise) articulated a principle that explains both these continuities and discontinuities.

[6] Hayek had what I've called elsewhere a 'non-standard conception of knowledge' (Scheall, 2015c) according to which *knowledge* and *belief* are, unless otherwise qualified, synonymous.

[7] The earlier theory is based on the standard Walrasian general equilibrium concept, while the later account starts from Hayek's epistemic treatment of economic order (née 'equilibrium'); the interwar theory incorporates the Böhm-Bawerkian treatment of capital from which the epistemic theory abstracts entirely.

3. The Implications of Hayek's Methodology of Sciences of Complex Phenomena for an Explanation of His 'Epistemic Turn'

Hayek (1945 [1948], p. 80) argued that there are two varieties of knowledge: 'a little reflection will show that there is…a body of very important but unorganized knowledge which cannot possibly be called scientific in the sense of knowledge of general rules: the knowledge of the particular circumstances of time and place'.[8] This distinction between theoretical knowledge (of 'general rules') and empirical knowledge (of 'particular circumstances of time and place') is essential to Hayek's methodology of sciences of complex phenomena. The possibility of a 'full' explanation or of a detailed prediction of particular events,[9] requires that the inquirer possess both kinds of knowledge to a sufficient extent: '[s]uch prediction will be possible if we can ascertain…all the circumstances which influence those events. We need for this both a theory which tells us on what circumstances the events in question will depend, and information on the particular circumstances which may influence the event in which we are interested' (Hayek, 1961 [2014], pp. 376-377).[10] In order to explain some phenomena, we need theoretical knowledge of the causal structure that gives rise to the phenomena and empirical knowledge of the values the causal elements assume at the relevant time and place. If our knowledge of both theory and data are 'full' relative to our explanatory purposes, we will be able to generate full explanations and precise predictions of particular events.

For Hayek, the sciences of complex phenomena were those disciplines in which this condition of epistemic adequacy does not hold.[11] Complex phenomena consist of a large number of elements interconnected (both to each other and to the external environment) in such a way as to give rise to an emergent order that possesses 'certain general or abstract features which will recur independently of the particular values of the individual data, so long as the general structure…is preserved' (Hayek, 1964a [1967], p. 26). Naturally, given our cognitive limitations, as the elements (and, thus, their internal and external interconnections) grow in number, our ability to acquire theoretical knowledge of the structure from which the relevant order emerges, diminishes. Similarly, it becomes more difficult to acquire sufficient empirical knowledge of the relevant data *given otherwise adequate theoretical knowledge*.[12] In the sciences of complex phenomena, because our knowledge of either theory or data (or both) is limited, we cannot generate full explanations or detailed predictions of particular

[8] See my 'Brief Note Concerning Hayek's Non-Standard Conception of Knowledge' (Scheall, 2015c) for more on Hayek's epistemology.

[9] Hayek accepted that explanations and predictions are logically symmetrical (Hayek, 1955 [1967], p. 9, footnote 4). I use the terms interchangeably.

[10] To say that an explanation is 'full' is not to say that it explains every aspect of the phenomena under investigation. According to Hayek (1952, p. 182), an explanation 'can never explain everything to be observed on a particular set of events'. Instead, explanatory 'fullness' should be thought of as sensitive to contextual considerations. An explanation that is full in one scientific context may be comparatively empty in another.

[11] The sciences of complex phenomena include theoretical psychology (Hayek, 1952), economics, linguistics (Hayek, 1967, p. 72), geology, evolutionary biology, and the astrophysical sub-disciplines that investigate the formation of stars and galaxies (Hayek, 1967, p. 76); as well as 'cybernetics, the theory of automata or machines, general systems theory, and perhaps also communications theory' (Hayek, 1955 [1967], p. 20).

[12] For his own part, Hayek emphasised what I call the 'data problem' in the sciences of complex phenomena, i.e., the difficulty of acquiring adequate empirical knowledge of the values taken by the causal elements at the relevant time and place. In 'Lesser Degrees of Explanation' (Scheall, 2015d), I argue that Hayek's methodology implies that the sciences of complex phenomena may also suffer from a distinct 'theory problem', i.e., the difficulty of acquiring adequate theoretical knowledge of the relevant causal elements, the internal interrelations between the proper subsets of these variables, and the external relations between the subsets of elements and the environment.

events: we are limited to 'explanations of the principle' and 'pattern predictions' of greater or lesser 'degree'.[13]

All of this is to say that Hayek (1964a [1967], pp. 25-27) essentially defined the sciences of complex phenomena in terms of these epistemic difficulties. A full explanation of some complex phenomena *requires a sufficiently complex model*.[14] This means that any attempt to theorise about complex phenomena as if they were simple – i.e., any attempt to use a simple model consisting of few variables to account for phenomena the adequate explanation of which require a complex model consisting of many variables – will ultimately fail. That is, Hayek's methodology predicts that we should observe patterns of failure wherever tools appropriate only for the analysis of simpler phenomena are applied to complex phenomena.

However, this does not mean that the scholar who encounters phenomena too complex for the extant analytical tools must throw up their hands in defeat. The answer to the problem of complexity, according to Hayek's methodology, is to develop a system of 'higher-level theories' that subsume some of the otherwise inexplicable complexities of the relevant phenomena.

> 'Though we may never know as much about certain complex phenomena as we can know about simple phenomena, we may partly pierce the boundary by deliberately cultivating a technique which aims at more limited objectives—the explanation not of individual events but merely of the appearance of certain patterns or orders. Whether we call these mere explanations of the principle or mere pattern predictions or higher-level theories does not matter. Once we explicitly recognize that the understanding of the general mechanism which produces patterns of a certain kind is not merely a tool for specific predictions but important in its own right, and that it may provide important guides to action (or sometimes indications of the desirability of no action), we may indeed find that this limited knowledge is most valuable' (Hayek, 1964a [1967], p. 40).

As I will show in the next section, Hayek ultimately came to believe that the analytical tools he used in his early business cycle project were simply inadequate for the complexity of the phenomena he was trying to explain. Moreover, Hayek's epistemic theory of industrial fluctuations is precisely the sort of more general, higher-level, theory that, according to his methodology, a scholar must develop in order to deal with the problem of otherwise intractable complexity.

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[13] See 'Lesser Degrees of Explanation' (Scheall, 2015d) for a discussion of Hayek's mostly implicit 'theory of predictive degree'. A truly precise prediction of a particular event – i.e, a prediction of degree 1 – would rule out all but one event and would be falsified by the appearance of any of the prohibited events. To say that explanations of the principle and pattern predictions are of lesser degree is to say that they rule out fewer events and, thus, are more difficult to falsify.

[14] '[T]here seems to exist a fairly easy and abstract way to measure the degree of complexity of different kinds of abstract patterns. The minimum number of elements of which an instance of the pattern must consist in order to exhibit all the characteristic attributes of the class of patterns in question appears to provide an unambiguous criterion…[W]hen we consider the question from the angle of the minimum number of distinct variables a formula or model must possess in order to reproduce the characteristic patterns of structures of different fields (or to exhibit the general laws which these structures obey), the increasing complexity as we proceed from the inanimate to the ('more highly organized') animate and social phenomena becomes fairly obvious' (Hayek, 1964a [1967], p. 25-26).

4. The Failure of Hayek's Early Theory of the Cycle and his Response

Hayek eventually came to recognise the chief defect of his early business-cycle project to be an incompatibility between the complexity of economic-cyclical phenomena and the limited explanatory capacities of the extant analytical tools. In the introductory sections of *The Pure Theory of Capital* – the culminating piece of his technical research program on the trade cycle – Hayek (1941 [2007]) acknowledged that, despite the tremendous intricacy of the models developed therein, *The Pure Theory* was not nearly elaborate enough to express the complexity of the phenomena under investigation. In order to appreciate this point, it is important to review the difficulties Hayek encountered in the development of the business-cycle project that was so very central to his early career as an economic theorist.

Hayek's earliest writings in technical economics[15] aimed to clarify the foundations of the theoretical framework upon which he would build the trade-cycle theory exposited in the companion pieces *Monetary Theory and the Trade Cycle* (Hayek, 1933 [2008]; originally published in German in 1929) and *Prices and Production* (Hayek, 1931; 1935 [2008]). It was the development of an appropriate concept of intertemporal equilibrium and, later, a theory of capital adequate to the problem of industrial fluctuations that would prove most intractable in this regard.

Hayek (1928 [1984]) was quite aware that Walras' static general equilibrium framework was an imperfect tool upon which to base a theory of the cycle in a dynamic, money- and capital-using economy. Nonetheless, when he came to consider the methodology of cycle theories in *Monetary Theory*, he argued that the goal of unifying an explanation of the cycle with the then-accepted corpus of economic theory required the Walrasian framework (Hayek, 1933 [2008], pp. 18-19). The uniqueness of Hayek's early theory lies in the fact that, with the introduction of assumptions concerning money and the activities of bankers in the creation of credit, cyclical fluctuations can be generated out of an otherwise perfectly-adjusting equilibrium framework.

However, in the 1933 essay 'Price Expectations, Monetary Disturbances, and Malinvestments', Hayek (1933 [1984], p. 136) argued against this view that the superimposition of monetary assumptions on the skeleton of Walrasian equilibrium sufficed to generate an adequate explanation of the cycle. This method 'press[es] the problems into the strait-jacket of a scheme which does not really help to solve them'. Instead, what was needed was 'a development of our fundamental theoretical apparatus which will enable us to explain dynamic phenomena…I am now more inclined to say that general theory itself ought to be developed so as to enable us to use it directly in the explanation of particular industrial fluctuations' (Hayek, 1933 [1984], pp. 137-138). A new treatment of 'general theory' – i.e., equilibrium theory – was necessary to account for cyclical phenomena.

Hayek (1937 [1948]) subsequently developed, in 'Economics and Knowledge,' the unique conception according to which equilibrium exists to the extent that the relevant beliefs of individual market participants are both inter-subjectively consistent and accurate with respect to external conditions. He employed this framework throughout *The Pure Theory of Capital*, but '…repeatedly apologizes for doing so. Although he clearly considers the new definition to be an advance over those found in earlier models, he also suggests that equilibrium analysis in general is, at best, *preparatory* to a more advanced causal analysis of economic phenomena' (Caldwell, 2004, p. 224; italics in the original; also see Chapter Two of Hayek, 1941 [2007], pp. 31-51). That is, Hayek judged his epistemic concept of equilibrium,

[15] These early essays have been translated and anthologised, either in Hayek (1984) or in the relevant volumes of Hayek's *Collected Works*.

more suitable though it was than the traditional treatment, to be yet too simple for a full explanation of fluctuations.

Beyond its inadequate equilibrium-theoretical foundations, *Prices and Production* incorporated Böhm-Bawerk's concept of the 'average period of production', a measure of the temporal duration of the economy's capital structure. It was this element that was to receive the harshest criticism from both Hayek and his peers in the years following the initial articulation of the early cycle theory. As Hayek soon came to recognise, it is possible to define an average period of production without circularity only under severely restricted assumptions. When these conditions are relaxed, the definition of the average production period becomes circular in that it is both determined by, and a determinant of, the interest rate (Hayek, 1936 [2008], pp. 497-498; White, 2007, p. xxii).

The theory of fluctuations offered in *Prices and Production* was intended to be – and, given the circumstances of its rushed preparation, could only be – a mere sketch of an elaborated explanation of the cycle.[16] But, as it became clear that the simplifications of the latter book, especially with regard to the temporal element embodied in the period of production concept, 'evaded so many essential problems that the attempt to replace it by a more adequate treatment…raised a host of new questions which had never been really considered and to which answers had to be found' (Hayek, 1941 [2007], p. 4), Hayek was unable to proceed immediately to a more detailed account of the cycle. The theory of capital upon which the analysis of *Prices and Production* was founded was too simple: 'I can see in the simplified form in which I had to use it in my former book it may be more misleading than helpful' (Hayek, 1939, p. 7; quoted in White, 2007, p. xxii). The consequences of the simplifications of *Prices and Production*, especially with regard to capital, could not be ignored (Hayek, 1941 [2007], p. 4).

The further theoretical gaps in *Prices and Production* include, but are not limited to, a theory of the bust or depression phase of the cycle, or as Hayek (1932 [1984], p. 137) called it, a theory of the 'economics of decline', and a theory of what it means to maintain capital intact over time. Hayek attempted to settle this latter question on a number of occasions (see Hayek, 1935 [1939]; 1936 [2008]; 1941 [2007]). Indeed, he dedicated the better part of the 1930s to reconstructing Böhm-Bawerk's theory of capital so as to make it a more appropriate basis for an explanation of the cycle. Hayek's initial reaction to the shortcomings of his early cycle theory was to attempt to develop a model complicated enough to account for the complexity of the phenomena.

However, Hayek was far from satisfied with the results of this protracted endeavour. The preface to *The Pure Theory of Capital* is little more than an extended apology for the inadequacies of the theory developed therein *despite its massive complexity*. In particular, Hayek (1941 [2007], p. 5) perceived the flaws of *The Pure Theory* to lie 'in the fact that…it leaves some problems of real importance unsolved'. Though '[i]t would undoubtedly be highly desirable…that this should be done once and for all…I can only plead that I have grappled honestly and patiently with what even now appears to me to be by far the most difficult part of economic theory, and that the present book with all its shortcomings is the outcome of work over a period so prolonged that I doubt whether further effort on my part would be repaid by the results' (Hayek, 1941 [2007], p. 5). Indeed, the limited discussion of the trade cycle such as it appears in the fourth part of *The Pure Theory* remains 'condensed and sketchy' (Hayek, 1941 [2007], p. 5) despite the fact that an elaboration of an improved theory of the cycle was the original motivation for writing the book!

[16] On the circumstances of the hurried preparation of Hayek's invited LSE lectures in 1931 (ultimately published later in the same year as *Prices and Production*), see Hayek (1931; 1935 [2008], pp. 191-196) and Caldwell (2004, pp. 171-173).

For Hayek, in the last analysis, attempting to explain the cycle by way of sufficiently complex analytical tools led only to a theory that nevertheless remained too simple to adequately account for the complexity of the phenomena – and which was, for this reason, 'probably of necessity false' (Hayek, 1964a [1967], p. 28) – while at the same time being 'so damned complicated it's almost impossible to follow' (Hayek, 1994, p. 141). Stated plainly, it seems that Hayek's early business-cycle project failed to bear the fruit he expected of it (at least in part) because, while still failing to express the complexity of the phenomena, the analysis he developed started to outrun his cognitive capacity to keep track of it. Hayek had taken the extant tools as far as he could – which may have been as far as they could have been taken by anyone – but not far enough to complete the capital theory project, much less the elaborated theory of the cycle.[17] Of course, failure is what we should expect when tools appropriate only for the analysis of simpler phenomena are applied to more complex phenomena. Hayek's methodology implies an explanation of the failure of his early business cycle project.

Beyond this, as we have seen, Hayek's methodology implies that the proper response to such complexity-induced failure is the development of a more general, albeit less testable, higher-level explanation of the principle that permits the theorist to abstract from some of the complexity of the phenomena under investigation. Hayek's epistemic theory represents just such a higher-level explanation. With respect to the causal role each assigns to action based on inadequate knowledge, the epistemic theory includes his early theory of the cycle as a special case – i.e., the epistemic theory is more general than Hayek's interwar theory. This move (which I'm *not* arguing was necessarily a conscious or deliberate one[18]) allowed Hayek to rise above the very intricate details that weighed so heavily upon his early account. Whatever the possible demerits of the epistemic theory of economic fluctuations, it certainly abstracts from many of the intractable intricacies – particularly those concerning capital (which figures nowhere in the epistemic theory) – that undermined Hayek's technical economic account of the cycle.

It is interesting to note the central role that Hayek's 'methodology of sciences of complex phenomena' plays in the epistemic theory itself. Recall that the latter is built upon Hayek's epistemic conception of economic order, his treatment of the price system as a knowledge-coordinating mechanism, and his argument that scientistic methodology encourages policymakers to interfere with the price system, and thereby, to interrupt its epistemic function. According to Hayek's (1975 [1978]) argument in 'The Pretence of Knowledge', scientism treats the very complex phenomena of economic equilibrium as if they are simpler than they are in fact. The methodological element in Hayek's epistemic theory attributes episodes of disequilibrium to action, under the pretence that the complexities of economic phenomena are cognitively tractable with the standard tools of technical economic

[17] This raises the interesting question whether Hayek would have adopted the more sophisticated analytical tools of, say, modern complex systems theory, had they been available at the time. This is a difficult counterfactual to evaluate if for no other reason than that Hayek did not seem to possess the competency required to fruitfully apply the most sophisticated mathematical tools of his own day, much less some yet more refined tools that would not be developed for a half-century following his emigration from the field of technical economics. Who knows what Hayek would have done if both he had been sufficiently adept and more advanced tools had been available during the 1930s?

[18] Hayek's epistemology emphasises the crucial role of knowledge of which we are not 'explicitly aware', but that we 'merely manifest…in the discriminations which we perform' (Hayek, 1952, p. 19). This is 'tacit' knowledge (Polanyi, 1966) or 'knowledge how' as opposed to 'knowledge that' (Ryle, 1946). Given the prominent role that Hayek assigns to tacit knowledge, it is perfectly in keeping with his epistemology that his development of the more general, less testable, epistemic theory was not a purposeful response to the failure of his early cycle theory. It is possible that Hayek's shift to the epistemic theory was merely manifest in the discriminations he performed and that he was not explicitly aware of the consistency of this shift with his methodology.

analysis. More to the point, Hayek's argument is that action – usually, political action – intended to maintain (or restore) a state of economic equilibrium, or of 'full employment', which is founded on belief in a simpler-than-required theory of the economy, is likely to interfere with the equilibrating tendency of the price system and, thus, to lead further away from, rather than closer to, equilibrium.

This move to the higher-level epistemic theory involves a shift from the theoretical plane of generalisations about economic phenomena, to the methodological plane of generalisations about the former kind of theoretical generalisations. Hayek's methodology of sciences of complex phenomena, which takes as its elements the relations between theories and *their* elements, is of a yet higher order than the theories it encompasses.[19] The elements of the epistemic theory are not themselves economic phenomena, but beliefs about economic phenomena and the mechanisms by which such beliefs might be coordinated.[20] Thus, the methodology of sciences of complex phenomena both explains, and can be interpreted as, a reaction to the failure of Hayek's early business cycle project.

5. Concluding Remarks: The Implications of the Argument for the Literature on Hayek's 'Transformation'

The present paper is the second part of a multi-essay project that addresses, and aims to account for, various continuities and discontinuities in Hayek's thought concerning the business cycle. In the first part of this project – 'Hayek's Epistemic Theory of Industrial Fluctuations' (Scheall, 2015b) – I argue that Hayek's concern for the causal role in propagating economic fluctuations of actions based on an inadequate epistemic foundation was continuous (and only became more general) over the arc of his career, but that he adopted distinct tools at different times to account for the causes and economic effects of ignorant action. In the present paper, I have attempted to show that Hayek's own methodology of sciences of complex phenomena implies an explanatory principle that reconciles both the former permanence and the latter break in Hayek's intellectual development. When a scholar attempts to account for massively complex phenomena such as those of industrial fluctuations with tools appropriate only for the explanation of simpler phenomena, the proper response is the development of a more general, higher-level, explanation of the principle such as Hayek's epistemic theory of industrial fluctuations.

In this concluding section, it remains merely to draw out the implications of this multi-essay project for the secondary literature on Hayek's transformation. First, a brief overview of this literature – or, at least, of its 'greatest hits' – is in order. The existing secondary canon divides fairly neatly between arguments for an irreconcilable break in Hayek's intellectual development, and interpretations of Hayek's writings as essentially consistent across a broad swathe of his career.

Terence Hutchison (1981) was the first to posit a fundamental discontinuity in Hayek's thought. According to Hutchison, 'Hayek I' was (something like) a Misesian

[19] 'The term "higher level regularities" which I have used to describe the content of such statements about the general character of an order is meant to indicate that it does not refer to relations between particular elements of such an order, but only to relations between relations, or even relations between relations between relations between the elements' (Hayek, 1961 [2014], p. 381).

[20] However, this being said, although Hayek's epistemic theory is a 'higher-level' account, it remains entirely a micro explanation and does not run afoul of Hayek's well-known distaste for macro explanations of social phenomena (for evidence of this antipathy, see, e.g., Hayek, 1975 [1978]). The epistemic account explains fluctuations in terms of the conditions necessary to coordinate the beliefs, and of the causes and effects of the ignorant actions of, *individual actors*, and makes no attempt to aggregate, index, or average beliefs across multiple actors.

methodological apriorist, while 'Hayek II' favoured a methodology more in line with Popperian philosophy of science.[21] In the early 1990s, Bruce Caldwell engaged in a somewhat tendentious debate with Hutchison concerning the 'Hayek II' thesis (Caldwell, 1988; 1992a; 1992b; for Hutchison's response, see Hutchison, 1992; and for Caldwell's later reflections on the 'skirmish', see Caldwell, 2009). Caldwell rejected Hutchison's particular taxonomy, but not the notion that there was a significant discontinuity in Hayek's thought. According to Caldwell (1988; 2004), Hayek's transformation consisted of a rejection of the standard notion of equilibrium in favour of the epistemic conception of economic order. Like Caldwell, Nicolai Juul Foss (1995) argued that Hayek traded the customary treatment of equilibrium for the epistemic conception, but insisted that this transformation was more subtle and less sudden than Caldwell would have it.

Several other scholars have made notable contributions to the 'fundamental discontinuity' side of the secondary literature. Steve Fleetwood (1995) posited three different 'Hayeks' distinguished in terms of unique ontological attitudes that Hayek allegedly adopted at different times. Ulrich Witt (1997) defended an account of industrial fluctuations consistent with Hayek's theory of spontaneous orders, but argued for a discontinuity between these aspects, as developed in Hayek's own writings. According to Jack Birner (1999), Hayek's 'epistemic turn' consisted of the rediscovery and subsequent emphasis of themes prevalent in Hayek's (1920 [1991]) early work on cognitive psychology that were largely ignored in his technical economics. More recently, Erwin Dekker (2014) has argued that the several social cataclysms of the interwar years (and immediately after) led to an extension of Hayek's curricular interests beyond technical economics.

On the other side of this division reside arguments that aim to establish the fundamental unity of Hayek's intellectual development. Perhaps the most well-known and representative example of this literature is Gerald O'Driscoll's (1977) *Economics as a Coordination Problem*. Recent excursions to the Hayek archives at Stanford University's Hoover Institution have led Bruce Caldwell (Manuscript) to cast some doubt on his prior thesis concerning Hayek's once-and-for-all rejection of the toolbox of the technical economist. Peter Boettke (2015) has argued, not only for the continuity of Hayek's methodological thought, but also, for its fundamental consistency with Mises's methodological project.

Regardless of the obvious significance of several of these contributions, it is part of the purpose of the present multi-essay project to exhibit certain fundamental weaknesses in the secondary literature on Hayek's intellectual development. In the concluding section of

[21] As indicated in the introductory section of the present essay, I argue in 'Hayek the Apriorist?' (Scheall, 2015a) that Hutchison's 'Hayek I' thesis is untenable. Hayek and Mises defended mutually inconsistent notions of *a priori* knowledge such that, even if Hayek had accepted methodological apriorism (a claim he always denied), it would have meant something different to him than it meant to Mises. Jeffrey Friedman (2013) has recently argued that Hayek held multiple mutually-inconsistent epistemologies at different times in his career. This too is an untenable thesis. In order to accept Friedman's argument that there is an inconsistency between the fallibilist-'interpretivist' epistemology of *The Sensory Order* (Hayek, 1952) and the (allegedly) infallibilist-'non-interpretivist' epistemology of 'The Use of Knowledge in Society' (Hayek, 1945 [1948]), one has to believe, not only that Hayek changed his epistemological views between 1945 and 1952, but also that he was almost quite literally *schizophrenic* in his ever-dithering epistemological attitudes. Hayek's fallibilism in *The Sensory Order* is also apparent in the 1920 essay (Hayek, 1920 [1991]) upon which (as Friedman acknowledges) it was explicitly based. So, either (as implied by Friedman's argument) Hayek was a fallibilist in 1920, an infallibilist in 1945, and a fallibilist again in 1952, or – as seems far more likely – when Hayek wrote of 'knowledge' in the 1945 essay, he intended the word in its fallibilist sense and Friedman's interpretation of Hayek as an infallibilist in the same essay is a misreading. If this is right, then the inconsistency alleged (and the epistemological schizophrenia implied) by Friedman disappears. In any case, either Hutchison or Friedman (or, I would argue, both) must be wrong: Hutchison argued that Hayek only became a thoroughgoing fallibilist with the publication of "Economics and Knowledge" in 1937 (Hayek, 1937 [1948]) and remained one for the balance of his career; Friedman claimed that Hayek was a fallibilist over the entire course of his career with the exception of a brief infallibilist interlude in 1945.

'Hayek's Epistemic Theory', I defend the view that the paper bears no significant implications for the question whether Hayek's thought was more or less continuous across time. My point there was that, in a career as long and multifaceted as Hayek's, we should expect to observe neither maximal unity nor utter chaos. All we can do, then, is to argue whether Hayek's development was *more* or *less* continuous though the years. However, no one in the relevant secondary literature has offered a *standard* or *definition* of continuity according to which the degree of diachronic unity of Hayek's thought might be inter-subjectively evaluated. In the absence of some such standard, there can be no (knowable) fact of the matter concerning the degree to which Hayek 'transformed' beyond the arguments of his youth: 'there are only [unstated] competing definitions of continuity according to which the question is answered in different ways, and [implicit] judgments of value as to which definitions are more acceptable than others. Such arguments are more likely to reflect the interests of the individual scholars who defend them than anything of significance for Hayek's career' (Scheall, 2015b, pp. 118-119).[22]

The present paper establishes two points of further relevance to an immanent criticism of the secondary literature on Hayek's intellectual development. First, as should be apparent from the argument of the present paper, several of the central contributions to this literature can *at best* be considered preparatory to a more advanced analysis. Even if we possessed some definition of general intellectual continuity by which related judgments might be inter-subjectively appraised, to leap from an argument for this particular continuity or that specific discontinuity in Hayek's thought to some more general conclusion, would remain dubious so long as the possibility remained open of unifying these disparate elements in terms of some explanatory principle. If some such principle could be discovered in Hayek's own methodological writings, then all to the better for a higher-order unification of his canon.

The same principle adduced in the current paper to reconcile the continuous and discontinuous aspects of the development of Hayek's thought concerning industrial fluctuations, is capable of similarly unifying other apparently incongruent elements posited in the secondary literature. Consider the seeming inconsistency between O'Driscoll's (1977) argument for the continuity of the development of Hayek's writings on spontaneous order out of the early business-cycle project, and Caldwell's (1988; 2004) argument that Hayek rejected the general equilibrium framework of his early theory of fluctuations for a fundamentally inconsistent conception of economic order as coordinated knowledge. The continuity posited by O'Driscoll regarding Hayek's unending concern for the coordination and dis-coordination of knowledge, would seem to be easily reconcilable with the break posited by Caldwell in the different conceptions of economic equilibrium / order. The Walrasian treatment of general equilibrium was inadequate relative to the epistemic conception of economic order for the elucidation of the causes and effects of knowledge dis-coordination, so – in keeping with the principle that a scholar whose concerns outrun the capacities of the extant tools of analysis must settle for a more general, higher-order, explanation of the principle – Hayek traded the Walrasian concept for the epistemic one.

[22] Given the contentious nature of both the topics that Hayek typically engaged and his distinctive arguments, it should be explicitly acknowledged that there is *political* value in any answer to the question concerning the continuity of these arguments. If it could be established that Hayek argued indiscriminately, with little internal consistency across time, it would seem to provide some, albeit perhaps weak, grounds to those inclined to dismiss his perspective(s); but, by the same token, this move from the discontinuity of Hayek's canon to a rejection of one or more of his positions would be blocked if it could be shown that Hayek's arguments were in fact continuous throughout his career. Alternatively, if one likes some aspect(s) of Hayek's writings more than some other(s), the transformation question fuels arguments that his thought either *evolved* or *devolved* beyond his early views.

Similarly, consider the inconsistency between Birner's (1999) thesis that the epistemological themes of Hayek's (1920 [1991]; 1952) cognitive psychology, which figure centrally in his later work, are absent from his early business-cycle project, and my own claim in 'Hayek's Epistemic Theory' (Scheall, 2015b) that epistemic considerations are important to the early business-cycle project, albeit not emphasised to the extent they came to be in the epistemic account of industrial fluctuations. Again, it may well be that these theses are less at odds than they appear at first glance. It is quite possible that epistemic concerns were omnipresent throughout Hayek's career, but that because the Walrasian equilibrium construct at the heart of the early cycle theory was a tool too simple to express the complexity of the phenomena of human ignorance, these epistemic concerns were minimised in the early account. We might then interpret Hayek's awkward (and somewhat *ad hoc*) attempt to shoehorn bankers' ignorance of the natural rate of interest into the early theory, as a doomed effort to deal with the causes and effects of the complex phenomena of human ignorance with tools far too simple for such an analysis. Needless to say, Hayek's epistemic conceptions of both economic order and the price system, as well as his anti-scientistic methodology of sciences of complex phenomena, are both better suited to the complexity of human ignorance and emblematic of the principle that explanatory progress is possible under circumstances of complexity-induced failure only if the scholar settles for a more general, less testable, higher-order explanation. Thus, the apparent inconsistency between Birner's account and my own disappears when examined through the lens of the relevant Hayekian methodological principle.

Beyond this failure to seek explanatory principles that might unify seemingly irreconcilable aspects of Hayek's intellectual development, the present paper points to a second shortcoming of the secondary literature. There is a tendency, in attempts to explain (some aspect or other of) Hayek's career, to try to go beyond the sort of limited explanations of the principle that Hayek himself thought were possible in the sciences of complex phenomena. That is, there is a tendency to pretend to a full explanation of (some aspect or other of) Hayek's career, where only a limited explanation of the principle may be possible.

There are several reasons to think that the phenomena of Hayek's career – and the phenomena of scholarly inquiry, more generally – qualify as complex in Hayek's sense. First, Hayek argued that all *mental* phenomena are complex in the relevant sense. Thus, to the extent that the phenomena of scholarly inquiry are mental phenomena, they qualify as complex. Second, Hayek argued for the complexity of *social* phenomena. So, to the extent that the phenomena of scholarly inquiry are social, they qualify as complex.[23] Moreover, if these phenomena are a consequence of some combination of mental *and* social phenomena – an assumption that seems perfectly reasonable – then the phenomena of scholarly inquiry must be yet more complex than if they were either purely mental or purely social. A full explanation of the phenomena of scholarly inquiry would have to account, not only for the complexity of their mental elements and their social elements in isolation, but also for the *interactions* of these elements.[24] Thus, a full explanation of any particular aspect of Hayek's career – much less of its entirety – would seem to be impossible on Hayek's own lights.

If this is right, then any attempt to explain some part of Hayek's intellectual development must be accompanied with a frank acknowledgement of the limits of such an

[23] See footnote 11 above. Both theoretical psychology and the social disciplines qualify as sciences of complex phenomena.

[24] The possibility that the investigation of scholarly inquiry – i.e., methodology itself – might be, in Hayek's sense, a science of complex phenomena is explored in my unpublished manuscript 'Kinds of Scientific Rationalism: The Case for Methodological Liberalism' (Scheall, Manuscript).

explanation.[25] More to the point, if we are limited to explanations of the principle of this or that aspect of Hayek's career, the question whether his intellectual development was *more* or *less* continuous would seem to evaporate. We can't fully explain this development in its entirety, but are limited to explanations of the principle of particular aspects of Hayek's career. No more general conclusion can be legitimately inferred, given our limited theoretical and empirical knowledge of the mental and social circumstances from which Hayek's scholarly arguments emerged.

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[25] Alternatively, of course, it might be argued either that the phenomena of scholarly inquiry – and, therefore, of Hayek's intellectual development – do not qualify as complex phenomena or that, complex though these phenomena may be, Hayek was simply wrong about the impossibility of full explanations of complex phenomena. Needless to say, no such arguments appear anywhere in the literature concerned with explaining Hayek's transformation.

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A Reflection on the Samuelson-Garegnani Debate

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Abstract
This paper argues that Samuelson's criticisms of Sraffa mainly concentrated on Sraffa's claim that the propositions of his book (Sraffa 1960) did not depend on the assumption of constant returns to scale. Garegnani's defence of Sraffa against Samuelson's criticisms remained ineffective because Garegnani's own interpretation of Sraffa's prices as classical 'centre of gravitation' or 'long term' prices requires constant returns to scale assumption. The paper goes on to critique Garegnani's interpretation of Sraffa and the classical economics to show that Garegnani's interpretation of Sraffa and the classical economics is highly problematic and that Samuelson's criticism of Sraffa does not hit the target because Sraffa's prices are not necessarily 'equilibrium' prices and therefore there is no need of returns to scale assumption in his theory.

Keywords: classical economics, constant returns to scale, Garegnani, Samuelson, Sraffa, standard commodity

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1. Introduction

Sraffa's book (1960), *Production of Commodities by Means of Commodities*, is perhaps the most enigmatic theoretical work ever published in economic theory. Of course, all classics are somewhat puzzling and do give rise to a number of interpretations over a period of time. But Sraffa's book is special in this respect. Upon its publication, many contemporaries of Sraffa hailed it as a definite classic but at the same time acknowledged their inability to completely understand what the book was all about (e.g., Harrod, 1961). And Sraffa's complete silence on explicating its terse prose was of no help. Its destructive potential for orthodox economics, however, came to the fore in the famous capital theory debates between 'the two Cambridges' in the 1960s (see Harcourt, 1969), when one of the leaders of the orthodoxy, Paul Samuelson (see 'Symposium 1966'), admitted that Sraffa's proposition about 're-switching of techniques' proves that the orthodox parable regarding 'quantity of homogeneous capital' is wrong. Pierangelo Garegnani (see 'Symposium 1966') was one of the participants on the winning side of this debate. Soon after, however, the orthodoxy came to the conclusion that the destructive potential of Sraffa's book could be confined to this simplified 'parable' only, and that the more sophisticated general equilibrium theory, which does not need the notion of aggregate quantity of capital prior to price determination, remains unscathed. Frank Hahn (1975, 1982) further argued that Sraffa's book can be interpreted as a special case of inter-temporal general equilibrium, and so the orthodoxy need worry about it no more.

This notwithstanding, Paul Samuelson was perhaps the only leading orthodox economist who maintained a sustained critical interest in Sraffa's slim volume until the end of his life (Samuelson died in December 2009) and Pierangelo Garegnani (who passed away in October 2011) was always willing to engage Samuelson in debating all matters Sraffian. This included not only the matters that directly pertained to Sraffa's book but also the 'Sraffian' interpretation of classical economics, championed by Garegnani himself. The recent publication of some of the major exchanges between these two luminaries of their respective schools of thought under one cover (see Kurz, 2013) has given an occasion for me to reflect on this debate and hopefully raise some questions for further consideration.

Samuelson (2000) in his paper, 'Sraffa's hits and misses', specifically makes three criticisms of Sraffa (1960): (i) Sraffa's claim that he does not make any assumption regarding returns to scale is not true, (ii) the Standard commodity is a useless device, and (iii) the limitations of land and capital are underplayed in his theory.

2. On the Assumption Regarding Returns to Scale

On point (i), Samuelson's main attack focuses on Sraffa's example of the 'subsistence economy' in his Chapter 1. Samuelson argues that Sraffa's example of the 'subsistence' economy given by:

280 qr. wheat + 12 t. iron → 400 qr. wheat
120 qr. wheat + 8 t. iron → 20 t. iron

necessarily assumes constant returns. Why? Because, according to Samuelson, Sraffa's snapshot of this economy is in self-replacing state at the given scale of production, but this snapshot position must have been arrived at by the Darwinian competitive process that entails adjustment of the quantities produced. Therefore, it cannot be claimed that the system would arrive at such a position by the mechanism of quantity adjustment without the assumption of constant returns. For example, Samuelson argues that suppose Sraffa's snapshot of the economy revealed a disequilibrium position such as:

350 wheat + 15 iron → 500 wheat
90 wheat + 6 iron → 15 iron.

In this case, we have excess supply of wheat and excess demand for iron. Now the wheat industry would contract and the iron industry would expand to a self-replacing subsistence system, only if constant returns were assumed. Actually, Sraffa in his sole footnote of Chapter 1 specifically states that for a system of this *type*, i.e., a subsistence system, such adjustment must be possible. Samuelson partially quotes this footnote and goes on to add: 'Oops! Only in constant returns to scale technologies do proportions matter and *alone* matter! Otherwise scale and proportions interact to deny the quoted claim' (Samuelson in Kurz, 2013, pp. 18-19).

What is Garegnani's response to all this? Before we get to that let us look at the full quotation from Sraffa's (1960) footnote of Chapter 1 that is under contention:

> 'This formulation presupposes the system's being in a self-replacing state; but every system of the type under consideration is capable of being brought to such a state merely by changing the proportions in which the individual equations enter it. (Systems which do so with a surplus are discussed in §4ff. Systems which are incapable of doing so under any proportions and show a deficit in the production of some commodities over consumption even if none has a surplus do not represent viable economic systems and are not considered)' (Sraffa, 1960, p. 5).

Garegnani's (2007a) response is that Samuelson confuses Sraffa's argument about abstract mathematical operations on equations with changes in the real system itself: '… That of course is true, but it applies to proportions between actual outputs and not to proportions between *equations*, as Sraffa is careful to specify in the one word we italicised in this passage' (Garegnani in Kurz, 2013, p. 58, Garegnani's emphasis). This I find highly unsatisfactory. Sraffa clearly states that 'every system of the type under consideration is capable of being brought to such a state…'. Hence the reference is to the 'system', which is capable of being brought to such a state, and not simply a mathematical operation on equations.

So let us go back to Samuelson's disequilibrium example. Even when the system is in disequilibrium, *if it is a subsistence system* then the price ratio between the two industries are still well established. This is because it is a definitional property of a subsistence system that the values of each industry's inputs must be equal to the values of its output, i.e., no industry produces any surplus or deficit. This property establishes the exchange ratio between the two industries as 1 ton of iron for 10 quarters of wheat. Given this exchange ratio, it is clear that the iron industry is capable of reproducing itself by selling 9 tons of iron for 90 quarters of wheat. But then the wheat industry is left with 9 tons of iron and 410 quarters of wheat. But it can combine 9 tons of iron only with 210 quarters of wheat and thus 200 quarters of wheat must go to waste. Thus the inputs of the wheat industry contract by 3/5 times. Now it is clear that this system will be again in a self-replacing state if the output of the wheat industry turns out to be 300 quarters of wheat, i.e., the industry displays constant returns to scale. What if the wheat industry is characterised by diminishing returns to scale? In this case, the system will cease to be a 'subsistence system' and turn into a 'surplus system' with a surplus of wheat output. Similarly, if increasing returns prevail then the system would turn into a deficit system. Sraffa, in his parenthetical note, states that he deals with such surplus systems in §4ff. and does not discuss the deficit system because it does not have historical viability. Thus it is clear that it is a mathematical property of a subsistence system, i.e. 'the system of the *type* under consideration' that must be characterised by constant returns to scale, otherwise slight vibration in the system would convert it into either a surplus or a deficit system. This, however, does not mean that the system producing surplus must also display constant returns, as it is clear that if the wheat industry displays diminishing returns then the system turns into a surplus system, a *type* that Sraffa discusses in §4ff. Hence Samuelson's choice of the example of a 'subsistence system' for an attack on Sraffa's claim that he makes 'no assumptions regarding returns to scale' was misplaced (see Sinha, 2007 for a more detailed response to Samuelson on this point).

Garegnani's response to Samuelson on this point, however, is a *non sequitur*. My interpretation gets further support from a draft of the footnote written by Sraffa in March 1956:

> Note to p. 4. 'The statement in this form applies only to a system which is in a self-replacing state. But any system, to be consistent, must be capable of being brought to such a state merely by changing the proportions in which the several equations enter it. If this is not possible there may be a deficit or a surplus, but no equality' (Sraffa-Papers D3/12/71: 5).

Now, what could the phrase, 'If this is not possible', mean? If Garegnani's interpretation was correct then the question of it being not possible does not arise – such mathematical operations on the equations must always be possible. But Sraffa's point is that the system may not revert back to a subsistence system if the industries were not characterised by constant returns.

But what about the system that produces a surplus? Samuelson interprets Sraffa's surplus system to be in a steady or a stationary state. This is because of two reasons: (a) Sraffa's surplus system is characterised by a uniform rate of profits across industries; hence his system is assumed to be in equilibrium and (b) the prices of both the inputs and outputs are the same in his equations. Again, this does not sit well with the evidence in Sraffa's book. A stationary state, in the classical sense, prevails when the rate of profits in the system reaches its minimum or when it is assumed that all 'surplus' is consumed by the capitalist class. Now, Sraffa consistently works out the movements of prices when the rate of profits is increased from the notional zero per cent to its maximum possible value. Furthermore, nowhere in the book does Sraffa assume that all the surplus is consumed by the capitalists. Thus this rules out a stationary state assumption. As far as steady state is concerned, it implies a constant returns assumption, which is denied by Sraffa. Furthermore, a steady state requires that employment of labour must rise at the same rate as capital is accumulating. This requires either a compatible theory of labour supply (e.g., a compatible theory of population) or an assumption of unlimited labour supply at the given wages – neither are even mentioned in Sraffa's book.

Garegnani, however, does not specifically dispute that Sraffa's system is in equilibrium, but argues that his 'equilibrium' is of a different nature than the case of either the steady state or the stationary state. Here Garegnani introduces his own reading of classical economics, which is not present in Sraffa's book, and interprets Sraffa's system to be implicitly following such a reasoning. In a nutshell, Garegnani's argument is that there is a 'core' of classical theory, which is designed to determine prices and the rate of profits in a surplus producing system (also see Garegnani 1984, 1987, 1990a). This core is characterised by three *given* variables: (i) total gross outputs or 'social product', (ii) technique of production or 'techniques' and (iii) 'real wage'. The 'social product' and 'techniques' together are supposed to determine the 'labour employed' and the 'labour employed' along with the 'real wages' determine 'necessary consumption'. Now 'social product' minus 'necessary consumption' determines 'surplus – share other than wages'. Given all this, the 'core' of the theory is supposed to determine the prices and the rate of profits in this system.

According to Garegnani, the core of the theory can be formulated in mathematical terms and precise answers to the determination of prices and the rate of profits could be found. However, as far as the determinations of the givens of the core, i.e., the 'social product', 'techniques' and 'real wages', are concerned, they are determined in a broad socio-historical context, which cannot be formulated in precise mathematical terms and so is also the case with the rebound effect of these variables on each other:

> 'On the one hand, we have the necessary quantitative relations, which competition entails between commodity prices and distributive variables and, which, in their comparative simplicity, are of a nature allowing for a mainly deductive treatment. On the other, we have the circumstances determining what we have described as the 'intermediate data': the subsistence or, more generally, the wage, the outputs, the technical conditions of production. These circumstances were seen to be closely related to institutional and historical factors, which, because of their complexity and variability according to circumstances, prevented deducing the corresponding variables from a few basic principles as was possible for prices and profits in the "core"' (Garegnani, in Kurz, 2013, p. 52).

Let us now look at the nature of Garegnani's '*givens*' carefully. Let us first take the given 'social product'. Is this an observed gross output vector after a production cycle is over? Garegnani's answer is, no:

> '… no economist had previously supposed the economy to ever actually *be* in equilibrium position, or more generally in a position of rest, except by fluke: *gravitation around such positions and not achievement of them* being what was always thought relevant for the positions of the economy in the focus of the analysis' (Garegnani, 2012, pp. 1429-30, emphasis in original).

By 'given social product' Garegnani means a vector of physical outputs that corresponds to a supposedly known vector of 'effectual demands'. This is because, according to Garegnani, the prices that the classical economists determine are the 'long-term' equilibrium prices – in the sense that these prices would hold only when the set of outputs adjusts to a given set of 'effectual demands'. Garegnani distinguishes this notion of equilibrium from the equilibrium arrived at by the simultaneous interaction of the supply and demand *functions* of the neoclassical variety. In the classical case, according to Garegnani, what is known is the set of 'effectual demands', i.e., the demand points and not demand functions and the *given* output is assumed to be equal to those given demand points. But if the given demand points are points in price-quantity space, then those demand points must also represent a set of prices, which is how 'effectual demand' is defined by Adam Smith. So the problem of solving for 'long-term' prices simply does not arise, since they must also be known along with the known 'effectual demands'. As Garegnani himself admits:

> 'The second difference is that the natural price—corresponding to an equilibrium price in modern terms—far from being an unknown to be determined by those 'supply and demand' as in neoclassical theory—is there a *given* for the very definition of the demand, the single quantity' (Garegnani in Kurz, 2013, p. 55, original emphasis).

In Adam Smith's (1981 [1776]) case, however, these demand points are not known data. His claim is that at any given moment a given set of outputs would be associated with a set of effectual demands, and if the output set is not equal, one-to-one, with its associated effectual demand set then a gravitation mechanism comes into play which adjusts supplies to the given set of effectual demands and the prices that represent those demand points can be discovered by knowing the natural rates of wages, profits and rent. *The argument implicitly assumes a linear technique or constant returns with no possibility of substitution between factors* (see Sinha, 2010a; c for details).

Garegnani, on the other hand, needs to know the effectual demand points because he needs to write the production equations such that its outputs are exactly equal to the effectual demands – so that no adjustment in outputs is required and thus no assumption, regarding returns to scale, is needed. Garegnani foists such an interpretation on classical economics in order to bring it in line with Sraffa's equation system, which, as Sraffa claims, does not make any assumption regarding returns to scale:

> However, as we shall see, a *separate determination of outputs* is possible and was in fact associated with the different classical theory of distribution considered above—and this is precisely what underlies Sraffa's assumption

> of given outputs and the independence of his analysis from constant returns to scale (Garegnani, 1990a, p. 129, emphasis in original).

An implication of this, however, turns out to be that the main problem of the theory becomes redundant. To avoid this, Garegnani reinterprets the classical notion of 'effectual demand' as a vertical straight line in quantity-price space: '… naturally leads … to a determination of outputs also independent of any such functions and, accordingly, *separate from that of prices*…' (Garegnani in Kurz, 2013, p. 51, emphasis added). In this case, the determination of the equilibrium price would require the solution of the 'given output' equations. But this reinterpretation of the classical notion of 'effectual demand' rules out the possibility of any classical gravitation mechanism, of which Garegnani himself makes so much. If the 'effectual demand' is a vertical straight line in quantity-price space, then any shortfall (given the 'effectual demand') in the quantity supplied in the market must lead to 'market price' rising to infinity, and in the converse case 'market price' falling to zero. If 'market prices' are supposed to be actual prices at which commodities do exchange in the market (when the system is not in equilibrium) then the rationing function of the rise in prices on quantity demanded cannot be denied.

The problem with Garegnani's reasoning becomes evident once we interrogate the grounds on which Garegnani claims that the vector of 'effectual demands' is known:

> 'These will be, to begin with: (1) the level of aggregate income and activity; (2) the technical conditions of production (governing, among other things, the outputs of means of production); (3) the distribution of the social product among the social classes (and therefore, in terms of the classical theories, the level of the independent distributive variable), since different classes generally spend their income on different commodities' (Garegnani, 1990a, p. 129).

It is, however, clear that all the three items in Garegnani's list can be known only if we know the vector of gross outputs produced. Otherwise, what does it mean to know 'the level of aggregate income and activity'? Similarly, how can one know what would be the total demand for the commodities that function as inputs, unless we know what has been the total utilisation of inputs in the system and the savings decisions of the capitalists (assuming workers don't save)? Moreover, the levels of aggregate incomes of either the workers or the capitalists cannot be known unless the vector of outputs is known. In other words, Garegnani's argument runs in a circle: the vector of outputs is supposed to be equal to the vector of effectual demands, which in turn is supposed to depend on the vector of outputs!

Garegnani, however, has another explanation of the idea of 'given output'. In this explanation the 'given output' is supposed to be the average of several years of actual outputs, which is considered to be the long-period equilibrium output, towards which the actual outputs are supposed to be adjusting or gravitating to: '… (the actual magnitude corresponding to it [Sraffa's system], would, if anything, be a moving average calculated over *several* years)' (Garegnani, 1990b, p. 350, emphasis in original). Garegnani determines this to be the feature that distinguishes the classical equilibrium from the steady or stationary state positions. He argues that the steady or stationary state equilibriums do not allow any adjustment for the empirical system, whereas the 'long-term' equilibrium notion allows 'the possibility of a correspondence between theoretical and observable variables' (Garegnani in Kurz, 2013, p. 81). Here the 'theoretical' long-term position is acknowledged to be an 'ideal' and not an actual or 'empirical' position:

> 'The "normal position" may be taken as a typical instance of Pareto's "ideal phenomena" in economics, centred as it is on Adam Smith's "central price", to which "the prices of all commodities are continually gravitating" (1776, I: 51) and therefore providing what Pareto calls here a "general or average fact"' (Garegnani, 1990b, p. 95, n. 72).

Nevertheless, the problem remains. If the ideal outputs are the average of several years of past outputs, then what are the inputs that go along with those outputs in the equations? If they are also the averages of the past years of inputs, then this amounts to assuming constant returns for the industries. The distance between Garegnani's interpretation of Sraffa's equations and Sraffa's own position becomes apparent when one juxtaposes Garegnani's above statement with Sraffa's characterisation of his equations as 'the *actual economic system of observation*' (Sraffa, 1960, p. 22, emphasis added) or as '[a] year's operations can be *tabulated* as follows …' (Sraffa, 1960, p. 3, emphasis added). These statements clearly refer to particular empirical data rather than 'general or average fact'.

Now, let us take the second item on the list of givens, *viz.* the 'techniques'. Garegnani nowhere explains what he means by the term 'techniques'. Are they production functions, which specify all the levels of outputs that would be produced with changes in inputs, or are they simply an observed set of inputs utilised to produce the observed set of outputs? The former case implies some assumption regarding returns to scale, e.g., Leontief technique with constant returns implicit in it; whereas the latter case is a one-point observation (such as Sraffa's 'actual economic system of observation') and cannot predict how outputs would behave with changes in inputs. Since Garegnani accepts that, most likely, the observed input-output data will not be in the classical centre of gravitation, the problem with the 'given output' position turns out to be this: even *if* we know what the output set must be, we still have no way of knowing what the input set must go along with that output set, unless we know the production function.

We now take up 'real wages', the last item of Garegnani's 'givens'. Unless we assume homogeneous labour throughout the economy and minimum subsistence wages, the idea of given 'real wages' remains quite vague. First, assume labour to be homogenous. In that case, if wages are higher than the minimum subsistence, then it is quite plausible that the wage baskets of different workers would reflect their individual tastes, so what meaning could be given to the 'given real wage'? Now, if labour is not homogeneous then how do we reduce a goldsmith's wage to an ironsmith's wage and down to a bricklayer's wage? What kind of comparison could one make in real terms? The classical economists proposed to reduce heterogeneous labour to homogeneous labour by using the given wage differentials as the reduction factor. But these wage differentials make sense only in terms of 'money wages' and not in terms of 'real wages'. Thus the total labour employment, in the sense of total homogeneous labour-time employed in the system, depends on the total wage bill of the system.

Now, to cut through this Gordian Knot, we must take the vectors of inputs and outputs as *observed* data after a production cycle. But then, according to Garegnani, such an observed data would, most likely, *not* be equal to the set of 'effectual demands' and thus prices associated with the observed system will *not* be equal to the 'long-term' classical equilibrium prices, which are characterised by the uniform industrial rate of profits. Thus the only way to reconcile the idea of the 'long-term' classical equilibrium prices with Sraffa's equations is to ascribe *linear techniques* to Sraffa's equations. But of course, Sraffa emphatically denied making such an assumption.

But how could Garegnani go wrong on such a straight forward theoretical issue? As a matter of fact, in his Ph.D. dissertation, which was completed in 1958, Garegnani explicitly admitted that the classical economists assumed constant returns:

> 'We can here remember that Smith and Ricardo's theory of price is *founded on the assumption of constant returns to scale* for manufactures, while the position of the margin in agriculture is given since it is treated as broadly determined by the level of accumulation and population' (Garegnani, 1959, p. 29, f.n. 2, emphasis added).

Obviously, at this stage Garegnani did not know Sraffa (1960). It was only after 1960 that Garegnani found it necessary to expunge the constant returns to scale assumption from classical economics to bring it in line with Sraffa (1960). But the task proved to be akin to squaring a circle.

Instead of directly interpreting classical economics – particularly the gravitation mechanism – and discovering the underlying constant returns assumption in it, as he had done in his Ph.D. dissertation, Garegnani now chose to analyse the significance of the returns to scale assumption in modern economics. He argued that the returns to scale assumption is linked with price determination in modern economic theory, due to its particular theory of income distribution. In modern economic theory, wages and the rate of profits are determined simultaneously with prices and if an economy is not in steady-state equilibrium then changes in the scale of production would affect the demands for factors and hence change the factor prices or income distribution, which in its turn affect the technique chosen and the cost of production of commodities. Garegnani argues that the assumption of wages given from outside, and independent of the scale of production in classical economics, takes away the main role of returns to scale in the theory of prices and therefore it is not needed by the classical theory.

Though it is true that the assumption of 'given wages' takes away the impact of changes in wages and the technique on prices, it is clearly not the whole story regarding the returns to scale assumption. The returns to scale assumption is mainly an assumption regarding the 'given technique', i.e., how physical changes in inputs are related to physical changes in outputs. And, as Garegnani recognises, classical economists did discuss the question of variable returns in the context of economic growth – such as Ricardo's treatment of diminishing returns in agriculture and Smith's treatment of increasing returns in manufacturing. He, however, then goes on to notice the absence of *variable returns* in the classical treatment of the gravitation mechanism and concludes from there that the assumption of returns to scale, as such, is irrelevant to the classical theory of price determination:

> 'Let us in fact suppose for a moment the presence also in those classical authors of neoclassical-like demand functions for the products, and consider the two elements that could cause Marshallian returns to be variable and accordingly make those functions be relevant there also. The first such element is changes in factor prices functionally linked to changes in relative outputs. The exogenous wage eliminates that element with regard to the division between wages and non-wage shares of the product and, to that extent, allows for a determination of prices separate from outputs and independent of demand functions we have assumed.

> The second element is non-constant physical returns to proportional changes of labour and capital: i.e. either *decreasing* physical returns to scale because of the scarcity of land (affecting in Ricardo the division the product between rent on the one hand and profits plus wages on the other) or *increasing* physical returns because of an increase in the division of labour. However, Ricardo treated decreasing returns from land, just as Smith had treated the increasing returns from division of labour: as relevant, that is, only for the comparatively large output changes involved in capital accumulation and growth. Unlike what happens in neoclassical theory, Smith and Ricardo could therefore leave physical returns to scale quite naturally aside when dealing with relative prices in a given position of the economy, with the kind of comparatively small output changes generally involved in that specific analysis' (Garegnani in Kurz, 2013, pp. 52-53).

It is, however, elementary that the absence of variable returns does not imply an absence of the returns to scale assumption, as such, if there are changes in output – all it implies is a *presence* of constant returns to scale, an assumption Garegnani had well understood in his Ph.D. dissertation. It should also be noted that the 'comparatively small output changes' that Garegnani refers to above are the precise conditions for which the neo-classical supply functions are well defined – i.e., they are well defined only in the neighbourhood of the equilibrium point, e.g., any large change in output would clearly break the Marshallian assumption of *ceteris paribus*. Thus to suggest that returns to scale are irrelevant to 'small output changes,' is a red herring.

So, is Samuelson right in attributing this assumption to Sraffa? The answer is: no. Both Garegnani and Samuelson make the mistake of interpreting Sraffa's prices as 'equilibrium' prices. Sraffa, however, is quite clear that if the reader interprets his prices as 'equilibrium' prices then she will ascribe the constant returns assumption to his equations but he, as a matter of fact, does not make any such assumption, which implies that the interpretation of his prices as 'equilibrium' prices is not legitimate: 'Anyone accustomed to think in terms of the equilibrium of demand and supply may be inclined, on reading these pages, to suppose that the argument rests on a tacit assumption of constant returns in all industries. … In fact, however, no such assumption is made' (Sraffa, 1960, p. v). As I have argued in various places (e.g., Sinha, 2010a; 2012; 2013; 2014; Sinha and Dupertuis 2009a; b), the condition of a uniform rate of profits in Sraffa's equations is not a reference to 'equilibrium condition' but rather it is a logical corollary of the assumption of uniform 'commodity-money' wages given from outside the equation system irrespective of the condition of 'equilibrium'. Since Sraffa's equations represent *tabulation* of observed inputs and outputs of an empirical system, it is completely independent of the notion of 'equilibrium' and 'change' and hence completely independent of the notion of returns to scale. Garegnani's attempt to insert the notion of equilibrium in Sraffa's equations, but at the same time not allow the notion of returns to scale in it, introduces a contradictory theoretical positioning that he naturally was never able to resolve, as is evidenced by his admittance of 'comparatively small output changes' contrasted against Sraffa's emphatic position that '[n]o changes in output and (at any rate in Parts I and II) no changes in proportions in which different means of production are used by an industry are considered' (Sraffa, 1960, p. v).

This brings us to a fundamental methodological difference between Garegnani's approach to economics and Sraffa's. Garegnani remained wedded to the methodology of mechanical causation, which gives rise to the ideas of 'forces of gravitation' and 'equilibrium'. Sraffa rejected mechanical causation on the grounds that one can never be certain about relations based on causation; e.g., the cost of production of wheat would be affected very differently if an increase in its demand came at the cost of a fall in the demand for rice or a fall in the demand for boots. In the first case (with a fall in the demand for rice) where no additional land needs to be brought under cultivation, no Ricardian diminishing returns would kick in; however, in the second case (with a fall in the demand for boots) most likely, the Ricardian diminishing returns on land would kick in as additional marginal land would need to be brought under cultivation. Thus a mathematical causal function – relating output to cost – is not possible. This led Sraffa to reject counterfactual reasoning, which is at the heart of all functional relations. Sraffa on the other hand maintained that a geometrical theory, which eschews causation, can be developed in precise mathematical terms, if it relates variables that can be measured in quantitative terms. As Sraffa explains in one of his unpublished notes:

> 'The general confusion in all theories of value (except Marx probably) must be explained by the failure to distinguish between two entirely distinct types of questions and the universal attempt of solving them both by one single theory.
> The two questions are:
> 1) What determines the [*difference* in the ?] values at which various commodities are exchanged in a given market on a given instant?
> 2) What determines the *changes* in the values of commodities at different times? (e.g. of *one* commodity) …
>
> The first problem gives rise to a geometrical theory, the second to a mechanical one' (Sraffa-Papers, D3/12/7: 115).

His famous equation, $r = R(1 - w)$ is of a geometrical nature. It relates the rate of profits of any given empirical system, to the productivity of the system as a whole and the wages specified in terms of the Standard commodity. In this case, the relationship between the rate of profits and the wages in terms of the Standard commodity is of a similar nature to the relationship of one angle with the other two angles in any Euclidian triangle – given two angles the third angle can always be deduced precisely. In Sraffa's case, given any empirical system of inputs and outputs, its R is determined independently of the values of r and w, and so for any given value of w, r of the system is precisely determined – or for any given value of r, w of the system is precisely determined. But these relations hold only for the given data of inputs and outputs, they *do not predict* how r would be affected in time t+1, if trade unions succeed in winning a rise in wages in time t. Answers to such questions require precise knowledge of relations between these variables in terms of causation. Sraffa's equations only tell us that in time t+1 we again get new inputs and outputs data, for which $r = R(1 - w)$ must again hold.

3. On the Irrelevancy of the Standard Commodity

On point (ii), Samuelson argues that 'Ricardo's goal is the *intertemporal and interspatial comparison* of price vectors, which tries to separate out *real* and *unreal* changes' (Samuelson in Kurz, 2013, p. 24). This identifies Ricardo's problem with the index number problem. Within this context, Samuelson finds Ricardo's search for an 'invariable measure of value' ill-defined and Sraffa's Standard commodity to be of no help. He goes on to raise questions about the existence of basic goods and, therefore, the Standard commodity – both in single-product and joint-production cases. He seems to be quite lost about the purpose of the Standard commodity either in the case of Ricardo or Sraffa. And reacting to Sraffa's statement that 'Standard system is a purely auxiliary construction' (Sraffa, 1960, p. 31), Samuelson asks: 'Toward what is it an "auxiliary"?' (Samuelson in Kurz, 2013, p. 28).

Garegnani accepts Samuelson's fundamental interpretive line that Sraffa's Standard commodity has something to do with Ricardo's search for an 'invariable measure of value'. He argues that one of the fundamental goals of Ricardo's theory was to prove Adam Smith's 'additive theory of value' wrong. For this purpose he needed to prove that a rise in wages would necessarily lead to a fall in the rate of profits. According to Garegnani, Ricardo tried to establish this proposition in a two-step manner. First, he assumed labour theory of value, which ensured that a rise in wages would have no impact on prices and thus on the value of the net output as well as the value of capital. This ensures that if the share of wages in the total net output rises, then this must lead to a fall in the general rate of profits. After this, Ricardo tried to work out the effect on prices of this change in the general rate of profits. But this second step could nullify the first result. To overcome this problem Ricardo tried to find an 'average commodity' or an 'invariable measure of value' that would ensure, if the prices and wages were measured in this *numéraire*, that the size of the net output remains constant when prices change from labour-values to 'prices of production'. And it is this problem that Sraffa's Standard commodity solves by reducing the n-commodity problem to a one-commodity problem: 'It seems hardly possible to deny that this particular change of coordinates system is a scientific achievement of some magnitude, in that it makes immediately visible a distributive process acting through thousands of intermediate prices' (Garegnani in Kurz, 2013, p. 65).

This reading of Ricardo's problem of the 'invariable measure of value' and the significance of Sraffa's Standard commodity are fraught with significant difficulties (see Sinha, 2010a and b for an alternative interpretation). First of all, Ricardo's two-step price solution, as described by Garegnani, cannot lead to only positive price solutions, some negative price solutions may also arise. Positive profits must be associated with unequal rates of industrial profits in the regime of labour theory of value, if the industries do not have uniform, organic composition of capital. Thus the rise or fall in the rate of profits, that Garegnani refers to, can only be the weighted average rate of profits of the system. Now, if this average rate of profits is imposed on every industry, then some prices must turn out to be negative (it is an implication of the Perron-Forbenius theorem). Let us now take up the relation of the Standard commodity with Ricardo's problem of the 'invariable measure of value' as explained by Garegnani. First of all, it should be clear, and definitely it was clear to Sraffa, that the Standard commodity is not needed to prove the inverse wage-profit relation in a Sraffa-type single-product equation system. It can be proved with any arbitrary *numéraire* that a rise in wages must lead to a fall in the rate of profits. This is because no price can rise more than wages as a result of a rise in wages – the price of a commodity could rise more than wages only if some of its means of production rose more than wages but this could not apply to the commodity that rose at the highest rate, therefore no price could rise more than wages. Thus Sraffa did not need to invent the Standard commodity to prove Ricardo's simple proposition within a Sraffa-type equation system. Of course, in the case of joint-production, the help of the Standard commodity is needed to prove this proposition, but then Ricardo was not concerned with the joint-production case.

But what about the proposition that Ricardo wanted to prove that a rise in wages would leave the size of the net output constant? The Standard commodity does not prove this proposition. Let's start with a zero rate of profits in both the actual and the Standard system. In this case, the labour theory of value will prevail and the value of the net output of the actual system and the Standard system will be equal. Now let's cut the wages to ¾ of the Standard net product. This divides the Standard net product into ¾ for the workers and ¼ for the capitalists. Now if ¾ of the Standard net product is applied as wages in the actual system, it will generate a different set of prices than the labour-value prices in the actual system, and there is no reason to believe that once ¾ of the Standard net product is taken as the wage share in the actual system, the remaining net output for profits would be equal to ¼ of the Standard net product. Actually, Sraffa (1960) is well aware of this:

> 'But while the share of wages will be equal in value to ¾ of the Standard national income, it does not follow that the share of profits will be equivalent to the remaining ¼ of the Standard income' (Sraffa, 1960, p. 23).

So why would Sraffa develop an artefact that does not do the job it was designed for? Actually, in Appendix D of his book, Sraffa makes it quite clear that the construction of the Standard system was integral to his book:

> 'It should perhaps be stated that it was only when the Standard system and the distinction between basics and non-basics had emerged in the course of the present investigation that the above interpretation of Ricardo's theory suggested itself as a natural consequence' (Sraffa, 1960, p.93).

My perusal of Sraffa's unpublished notes, from the period 1942 to 1944, has convinced me that the discovery of the Standard system and the Standard commodity was intimately related to proving the hypothesis that the ratio of the net output to capital remains constant while the rate of profits moves from zero to its maximum value, and that it was not related to Ricardo's problem. An implication of this discovery was to prove that the relationship of the Standard wages to the average rate of profit of the Standard system must also hold for the actual system as well, irrespective of the actual system being in equilibrium or not. And thus, Sraffa could proclaim that the Standard system shows that '…the rate of profits is a non-price phenomenon' (Sraffa-Papers, D3/12/53, quoted in Sinha, 2010; 2012). Thus, to answer Samuelson's question: the Standard system is an auxiliary to the real or actual system – it helps us discover the mathematical properties of the actual system.

On the question of whether at least one 'basic good' exists in the real world, Samuelson's argument is not very clear. In the case of a single production system, Samuelson claims that 'I believe in a plethora of independent *sub*-systems that are indecomposable. This denies BASICS' (Samuelson in Kurz, 2013, p. 26). And even if a 'basic good' exists, if its weight in the total economy is very low, then its choice as the index number for price changes would be highly dubious. Further on, in the case of joint-production, Samuelson refers to Manara (1980 [1968]), which argues that, in the general joint-production case, Sraffa's Standard commodity may not exist in the real space.

Garegnani, in response, argues that in Sraffa workers' necessities are 'basics' and, therefore, as long as labour is part of the production process there would be basics in the system. But the fact remains that Sraffa refrains from separating wages into workers' 'necessities' and 'superfluities' – for a good reason. It would be absurd to think that a worker's 'necessity' for potatoes would remain constant even when her consumption of steak is rising (Robinson, 1961). Sraffa, however, correctly suggest that changes in the techniques of production of workers' wage goods would still have impact on all the prices even though workers' wage goods are put in the limbo of non-basics. This is because, as workers' wage goods become cheaper or dearer, the money wages in terms of the Standard commodity would be accordingly adjusted, which will change the rate of profits and, therefore, all the prices in the system. But this does not mean that wages are basics in the sense that wage goods could be used to construct the Standard commodity. Garegnani further argues that:

> 'Also more generally, it seems inevitable to note that if we were to ignore the "basic" role of workers necessaries, and we were prepared to go along with Samuelson's present scepticism about other sources of basics, yet an inexistence of the latter would importantly affect the properties of the system (e.g. on the existence of a maximum rate of profits) and the reference to basic products—whether present or absent in any particular economy—could hardly be avoided in a satisfactory analysis of it' (Garegnani in Kurz, 2013 p. 66).

Now, as far as the existence of 'a maximum rate of profits' is concerned, Garegnani's above contention seems to be incorrect. One does not need the existence of a basic good to show that a maximum rate of profits must exist. Its existence depends on the fact that production requires some produced means of production. In other words, capital can never be completely reduced to only 'variable capital', to use Marx's terminology. But the existence of constant capital, as such, in the production process does not mean that it must be a 'basic' good.

It, however, appears to me that Samuelson's contention that there is perhaps no basic good in the real world, is too far-fetched. It would be hard to imagine that if we take the production equation of any good produced today and carry it backward far enough, we wouldn't find some oil or coal or iron or other building materials, etc. anywhere down the chain. Thus Garegnani correctly reminds Samuelson that in 1958 (Dorfman, Samuelson and Solow, 1958) Samuelson himself thought that a large part of national income was made up of basic goods (Garegnani in Kurz, 2013, pp. 66-67).

On the case of joint-production, however, Garegnani remains silent. This is because Sraffians had accepted Manara's critique in the general case of joint-production. Dupertuis and Sinha (2009) have, however, shown that this state of affairs existed because Sraffa's system was assumed to be in equilibrium. Once this unreasonable condition is lifted from Sraffa's equations then the Manara critique can be answered. So there is no need to give in to Samuelson on this point either.

4. On the Role of Demand on Prices

On point (iii) – the limitations of land and capital are underplayed in his theory – Samuelson has two arguments. One is a long-standing one, that when demand shifts from manufacturing to agricultural commodities, the diminishing returns on land would bite and rent would rise and prices of all commodities would be affected (Samuelson, 1978). The second argument is that the assumption of steady state, which, in his opinion, underlies the Sraffian equations, is not a state usually found in reality. When demand shifts from consumption to capital goods, e.g., a developing country deciding to increase future consumption at the cost of current, then the economy's trajectory will no longer be on the steady-state trajectory and, in this case, the own rates of interest of different commodities would differ and therefore the input prices in Sraffa's equations will not be equal to the output prices.

Since the first point is not explicitly made (though implied in his 'conclusion') Garegnani remains silent on this. On my reading of Sraffa, he would not deny the possibility of changes in prices due to changes in demand, *if those changes in demand cause quantities produced to change*. As a matter of fact, Sraffa explicitly denied ever making the argument that in his theory of price determination 'demand' plays only a passive role. In a letter to Arun Bose written in 1964, Sraffa wrote:

> 'I am sorry to have kept your MS so long—and with so little result.
>
> The fact is that your opening sentence is for me an obstacle which I am unable to get over. You write: "It is a basic proposition of the Sraffa theory that prices are determined exclusively by the physical requirements of production and the social wage–profit division with consumers demand playing a purely passive role." Never have I said this: certainly not in the two places to which you refer in your note 2. Nothing, in my view, could be more suicidal than to make such a statement. You are asking me to put my head on the block so that the first fool who comes along can cut it off neatly.
>
> Whatever you do, *please* do not represent me as saying such a thing' (Sraffa-Papers, C32, quoted in Sinha, 2007)

Bose's mistake was to assume that Sraffa's outputs were necessarily classical equilibrium outputs, and thus, as in classical gravitation process (see Ricardo, 1951 [1821], p.91), changes in demand patterns would only change the composition of equilibrium outputs but not their prices. It is clear to Sraffa that such a position implies the assumption of constant returns to scale or linear techniques – an attribution of that to his theory, he thinks, would amount to asking him to put his head on the block so that the first fool who comes along can cut it off neatly. Sraffa's position seems to be clear. At any moment, prices are determined by the empirical input-output data with the additional knowledge of 'money wages' in terms of the Standard commodity, irrespective of demand considerations. If, however, changes in demand patterns affect changes in outputs that change the equations, then prices would change – but those prices can again be determined by the given input-output data along with the knowledge of the 'money wages' at that moment.

On the second point of Samuelson, Garegnani's response is that the classical long-term equilibrium, which he attributes to Sraffa, is not the same as the steady- or stationary-state equilibrium. As explained above, Garegnani claims that steady- or stationary-state equilibrium does not allow any movement or adjustment of empirical quantities, whereas the long-period equilibrium allows the empirical outputs to adjust around it – it only claims that the equilibrium point generates a persistent force of attraction for empirical quantities to move towards it. But this does not answer Samuelson's charge. Samuelson's point is that when demand conditions shift, then the outputs of an economy must also move, and during this dynamic process the economy cannot follow 'steady state', which gives rise to unequal 'own rates of interest' across industries and, therefore, the prices of inputs cannot be taken as equal to prices of outputs in Sraffa's equations. Garegnani's answer to this is that it is not possible to analyse a system in motion – all one can do is a comparative-static analysis of a system based on long-period equilibrium:

> 'But for Sraffa, as well as for all neoclassical theorists up to the Pigous, Robertsons or Champernownes—up, that is, to what I have got used to calling the "Hicksian divide" in neoclassical theory, when, three or four decades ago, the new notions of equilibrium became dominant—there was no question of reproducing the "paths that economic systems [...] do follow". As again, Marshall had pointed out long before, and the predecessors of

> Hicks (1939), including Hicks (1932) himself, had in effect unanimously accepted: "dynamical solution in the physical sense of economic problems are unobtainable [so that] statical solutions afford starting points for such rude and imperfect approaches to dynamical solutions as we may be able to attain to" (Marshall 1898: 39). Normal positions and their comparison over time approved accordingly to be the essential constituents of such *attainable* "imperfect approaches"' (Garegnani in Kurz, 2013, p. 81).

Leaving aside the question of whether a dynamical system can be analysed or not, the fact of the matter remains that the 'own rates of interests' of the inter-temporal general equilibrium (GE) analysis are not directly translatable into Sraffa's industrial rates of profits. As I have shown in Sinha (2010a; and also Sinha and Dupertuis, 2009b) the translation of the inter-temporal GE equations into Sraffa's industrial equations is possible only on the *assumption* that the commodities' 'own rates of interests' are equal. There cannot be any possibility of prices of inputs being different from prices of outputs in Sraffa's equations. This can be checked by looking at Sraffa's Standard system. The global or the average rate of profits of the Standard system is determined by the physical input-output data without any knowledge of prices. Thus, it is a physical property of the system. Now, as long as prices of the inputs and outputs are taken to be the same, no matter what prices one ascribes to these equations, the value rate of profits would conform to the physical rate. However, if the input prices are taken to be different from the output prices, then the value of the rate of profits would not conform to its physical rate and hence contradict the physical property of the system.

Since Sraffa's system is an *ex-post* description of an economy, at any moment on its dynamic path the system can be represented by Sraffa's equations with the rate of profits being uniform. As a matter of fact, Sraffa states that:

> 'It can be said that in any actual economic system there is embedded a miniature Standard system which can be brought to light by chipping off the unwanted parts. (This applies as much to a system which is not in a self-replacing state as to one which is)' (Sraffa, 1960, p. 20).

Now, from his unpublished notes we have learnt that 'a system which is not in self-replacing state' refers to a dynamic system where some industries may be shrinking due to technical changes – such as industries producing those machines or raw materials that are exclusively used by old and dying out techniques. In such situations one cannot discover 'surplus output' in physical terms since all inputs cannot be deducted from outputs item-by-item, as some outputs would be found to be in lesser quantity than their use as inputs in the system. Sraffa maintained that all real systems are such, and claims that his analysis applies to such systems as well. Thus it would be incorrect to interpret Sraffa's system to be in 'equilibrium' or to argue from a Sraffian perspective, as Garegnani does, that an economic analysis is possible only for equilibrium conditions.

5. The Last Round

The last round of the debate between Samuelson (2007) and Garegnani (2007b) shows that these two highly sophisticated minds have been talking past each other. Samuelson's response completely ignores Garegnani's comments and goes on to mathematically work out an inter-temporal general equilibrium master model, which is capable of translating all kinds of models, including Sraffa's model, into a GE model, and shows that Sraffa-type systems also require the constant returns assumption and, in the presence of substitution possibilities, a demand shift would affect both distribution and prices in his system as well – of course, Samuelson's GE model takes total factor endowments as given and assumes full employment of all the factors as a condition for the solutions of his equations.

On the question of 'full employment', it should be noted that there is not one word spoken about it in Sraffa's book. As I have suggested above, Sraffa's equations are *ex-post* descriptions of an economic system after the harvest. All it tells us is how much of the total homogeneous labour-time was employed and the amount of produced means of production were used (we leave aside the complication of land) during the last production cycle. Since it takes either wages or the rate of profits given from outside, it has nothing to say on whether those wages could be associated with unemployed labour or not. Any statement of that sort would require a separate theory of either wages or the rate of profits. Samuelson's position is that distribution *must* be determined simultaneously with prices as in the GE model. Now, it is a problem for all Sraffians to either prove that the simultaneous determination of distribution and prices is theoretically flawed, or develop an alternative and more persuasive theory of distribution that separates the determination of distribution from the determination of prices. It is the second option that Garegnani emphasises by pointing to the classical economics as having that alternative.

Garegnani argues that the possibility of long-term unemployment coexisting with positive real wages is a major distinguishing feature of the classical economics in opposition to the neoclassical economics:

> 'The other issue regards the classical economists' theory of wages, the heart of the analysis and of its structure, as I have contended (Garegnani 2007, section I and II) and textually supported by the numerous well known "puzzles" which Smith and Ricardo's theory of wages raise for modern interpreters (Garegnani 2007, section Vc), or by my criticism of Samuelson's interpretation of chapter XXXI 'On Machinery' of the *Principles* (Garegnani 2007, section Vd)' (Garegnani in Kurz, 2013, p. 126).

Garegnani, in my opinion, stakes too much on Ricardo's 'Machinery' chapter, where Ricardo shows that introduction of machinery would throw out some previously employed workers:

> 'The discovery and use of machinery may be attended with a diminution of gross produce; and whenever that is the case, it will be injurious to the labouring class, as some of their number will be thrown out of employment, and population will become redundant, compared to the funds that are to employ them' (Ricardo, 1951, p. 390, quoted by Garegnani in Kurz, 2013, p. 78).

One should, however, not forget that Ricardo's problem of introduction of machinery takes place in a dynamic context:

> 'With every increase of capital and population, food will generally rise, on account of its being more difficult to produce. The consequence of a rise of food will be a rise of wages, and every rise of wages will have a tendency to determine the saved capital in a greater proportion than before to the employment of machinery. Machinery and labour are in constant competition,

> and the former can frequently not be employed until labour rises. … The demand for labour will continue to increase with an increase of capital, but not in proportion to its increase; the ratio will necessarily be a diminishing ratio' (Ricardo, 1951 [1821], p. 395).

Hence the introduction of machinery is mainly a problem of substitution between labour and machinery in the context of accumulation. Ricardo's position is that machinery is generally introduced to combat rising wages due to rising demand for labour. Introduction of machinery dampens the rise in demand for labour, but does not make it negative. As a matter of fact, Ricardo did not subscribe to Barton's claim that, under certain circumstances, such a tendency to replace labour with machines might be so strong that accumulation would lead to no increase in the demand for labour: 'It is not easy, I think, to conceive that under any circumstances, an increase in capital should not be followed by an increased demand for labour; the most that can be said is, that the demand will be in a diminishing ratio' (Ricardo, 1951 [1821], p. 396 f.n). This explains the last qualifier in Ricardo's quotation cited by Garegnani: '*compared to the funds that are to employ them*', i.e., labour will be thrown out in terms of per unit of total fund employed – but as the total fund itself is growing, the total labour employment would also be growing rather than declining. It should also be kept in mind that Ricardo abstracts from the 'rising productivity' or 'technological improvement' aspects of machinery. In the case of rising productivity due to technological change, the rate of profits would rise leading to a rising rate of accumulation and, consequently, rising wages. This would contradict Ricardo's position that the system has a secular tendency to move towards the stationary state.

Both Adam Smith and Ricardo develop their theories of wages in a dynamic context where population mechanism plays a crucial role, which brings into play the forces of demand for and supply of labour in the determination of wages (see Sinha, 2010 for my interpretation and also Hicks and Hollander, 1977). Garegnani ignores the role of population dynamics in the classical theory of wages and, therefore, I remain unconvinced of his interpretation. One way of relating the 'given wage' notion in the classical theory of price determination, would be to separate the dynamic and static contexts. Wages are determined in a dynamic context, whereas the natural prices are determined in the static context. Thus wages can be taken as given at any moment for the static equations of price determination. If this is acceptable, then the time involved in the classical gravitation mechanism must be understood as 'logical time' and not 'historical time'. But, in that case, Garegnani's gambit of using 'statistical average of past several years' as the centre of gravitation outputs, would lose theoretical support.

6. Concluding Remark

It seems to me that though Garegnani had a better intuitive understanding of Sraffa's overall project than Samuelson, he unfortunately tried to build his castle on sand. It was not for nothing that Sraffa, in the very opening sentences of his 'Preface,' had warned his reader not to bring the baggage of equilibrium to his book and had drawn the logical connection between the idea of equilibrium and the assumption of constant returns. Garegnani, instead of solving the puzzle of 'uniform rate of profits' in Sraffa's equations, took the easy way out by assuming classical equilibrium for Sraffa's equations. But then there was no logical way out of the assumption of constant returns. Now, as we have seen above, Garegnani had correctly interpreted classical theory of prices in his Ph.D. dissertation when he acknowledged that 'Smith and Ricardo's theory of price is founded on the assumption of constant returns to scale

…' (Garegnani, 1959, p. 29, f.n. 2). Apparently, it was Sraffa's (1960) statement in the 'Preface', where Sraffa relates his 'standpoint' to that of the old classical economists from Adam Smith to Ricardo without any further elaboration, that led Garegnani to change his mind and attribute 'equilibrium' to Sraffa (1960) and no assumption of constant returns to classical economics to bring the two in line with each other. This created a contradiction at the heart of his interpretation of both Sraffa (1960) and classical economics and tied him up in knots. And this is where Samuelson, in my opinion, was able to checkmate him. But this was no checkmate to Sraffa, as Sraffa did not assume either equilibrium or constant returns.

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The Political Power of Economic Ideas: Protectionism in Turn of the Century America

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Abstract

One of the main economic debates taking place in late-nineteenth and early-twentieth-century America was between supporters of protectionism and advocates of free-trade policies. Protectionists won this debate, as the 1897 Dingley Tariff raised tariff rates to record highs. An analysis of this outcome highlights the overlapping interests of Republican politicians and business groups. Both of these groups endorsed particular economic arguments in favour of protectionism. Contemporary studies by academic economists informed the debates surrounding protectionist policies at this time, and also analysed the impacts of these policies. Evidence from politicians, business owners, and economists provides a broad view of who favoured protectionist policies in turn-of-the-century America. This analysis also focuses on how the impacts of these policies were studied and presented in contemporary academic and public discourse.

Keywords: protectionism, free trade, economic policy, Republican Party, wool industry

1. Introduction

The turn of the century was characterised by major shifts in American politics and in the US economy. Politically, the Progressive Era began, while the 'merger movement' reshaped the economy. Though the late-nineteenth century is often characterised as epitomising *laissez-faire* capitalism, the very end of the century saw the shift toward some of the strongest protectionist policies to ever exist in the United States. This paper explores the political power of economic ideas in the late-nineteenth and early-twentieth centuries, focusing on the shift toward protectionism.

The political power of economic ideas was clearly on display in the United States at the turn of the century. This time period offers instructive examples of the connections between the economic ideas of economists, politicians and private business interests. Protectionist ideas won the economic debates of this time, and shaped the discourse of politicians and business owners, as well as economic studies in academic journals. While there were two sides to this debate, the focus here is on the political and business-oriented arguments in favour of protectionism at this time, as others have written in depth about the development of free-trade policies (e.g. Irwin, 1996, among many others). Thus the focus here is on the arguments made by Republican politicians and protectionism-favouring business owners, more than their counterparts in the free-trade camp. But in the analysis of academic studies of the economic implications of these policies, the discussion is broadened to include studies that focused on economic issues under both of the alternating free-trade and protectionist policy regimes that existed over the course of the 1890s.

The US economy was hit by a severe depression in the mid 1890s. For several decades leading up to this time the Republican Party enjoyed national power and imposed their vision of protectionism on the economy. But when the Panic of 1893 led to the mid-1890s depression, the Democrats held the executive office and implemented their preferred free trade legislation. This allowed the Republicans to attribute the depression to the Democrats' endorsement of free trade.

Certain groups within the business community echoed these arguments, with the woollen manufacturing industry being a prominent example. When the Republicans returned to power in 1897, they imposed some of the strongest protectionist legislation the United States has ever seen. Though there are important subtleties underlying the political economy issues of this time, it is clear that politicians were very direct in their advocating of specific economic ideas in order to restructure the economy along partisan lines. The wool manufacturing industry was one of the major beneficiaries of this protectionist legislation. An analysis of the wool industry's trade journal offers examples of how these business owners engaged with the debate over free trade versus protectionism.

Contemporary academic economists studied these developments in detail. Multiple studies focused on how various economic issues were impacted by the shifts from protectionist to free-trade policies, and *vice versa*, through the first couple decades of the twentieth century. There are both differences and similarities between the ways that politicians, businessmen, and academic economists expounded upon these issues. This paper looks at evidence from each of these sectors – political, commercial, and academic – and traces the use of economic concepts by people from each of these groups. Protectionism was at the forefront of powerful economic and political concerns at this time, and this shaped the discourse of politicians, business owners and academic economists during this transformative period in US history.

The intention here is not to take sides in this debate, nor to explore the merits or problems with protectionist policies. Elsewhere I argue that expectations surrounding the Dingley Tariff did encourage renewed investment toward the end of the 1890s depression (Bent, forthcoming). In contrast, this paper is concerned with intellectual history and analyses the economic discourse on protectionism that was taking place at this time.

2. Protectionist Politicians

Turn-of-the-century politicians tended to adhere to an overly-simplified dichotomy of free trade versus protectionism, filling speeches with grandiose rhetoric but not publicly working through the implications of these ideas in a rigorous way. An interesting counterexample, however, is offered by Representative Dingley, who developed the protectionist tariff legislation of 1897. A contemporary *New York Times* article, for example, discussed Dingley's estimates of the revenues that the tariff would generate for the federal government, providing quantitative support for the rhetoric employed by his party (Anon, 1897). Tariffs provided a significant source of income for the government at this time, since there was no federal income tax. Academic studies on the effects of tariff legislation in the early twentieth century also complemented the Republicans' protectionist rhetoric. For example, the Dingley Tariff Act is argued to have supported the interests of prominent businesses, as discussed in detail below.

Stern (1971) highlights the connections between the interests of the Republican Party and the business community at the turn of the century. This was seen to be a mutually beneficial relationship, as business leaders helped fund the Republican Party while the

Republicans returned the favour by enacting tariff bills that shielded American businesses from foreign competition:

> 'Contributing to the growing harmony of the party and to its growing efficiency as a vote-garnering machine were the augmented financial resources available to Republican leaders increasingly inclined to view the G.O.P. as primarily a business-enterprise-promotion agency dedicated to the determination of tariff schedules by the protectionist beneficiaries themselves' (Stern, 1971, p. viii).

The relationship between the turn-of-the-century Republican Party and the business community can also be seen in the economic ideas espoused by Republicans. While Democrats advocated the implementation of free-trade policies – seen with the lower tariff rates under President Cleveland from 1894-96 – Republicans had an economic worldview that more directly favoured particular business interests. 'Espoused by protectionist Republicans was a type of *laissez-faire* economic philosophy conceding a plentitude of government power for intervention in the operation of the highly esteemed free-enterprise system through the imposition of tariff duties for the enrichment of industrialists' (Stern, 1971). The Republicans promoted tariffs in order to protect American industries from foreign competition. They also argued that protectionist policies would decrease the federal deficit through increased tariff revenues (Anon, 1897).

As economic recovery followed the Republicans' return to the White House in 1897 and the signing into law of the strongly protectionist Dingley Tariff Act, Republicans were able to claim the recovery as being due to their policies. This enthusiasm was captured by contemporary observers following the Republicans' victory in the 1896 election:

> 'A crushing weight has been lifted and rolled away, and the business world has begun to adjust itself to a state of freedom and security which it has not known for years. Dread of immeasurable disaster no longer locks up reserves and paralyzes enterprise, and new contracts involving many millions have become binding since the election' (*Dun's Review*, 1896, as quoted in Faulkner, 1959, p. 161).

The Republicans were able to harness this renewed confidence in the American economy and argued that the recovery was due to their tariff legislation (White, 1939, p. 14). 'As the years succeeding [the Panic of] 1893 grew blacker and blacker, the staunch protectionists had the opportunity to cry: "We told you so; let us return to the policy of prosperity"' (Taussig, 1964, p. 323). After the Republicans returned to the White House and implemented the protectionist Dingley Tariff, the economy did indeed begin to recover. These connections between the actions and rhetoric of politicians and the commercial goals of businessmen highlight the political power of protectionist ideas during the late nineteenth century.

3. Protectionist Business Owners in the Wool Manufacturing Industry

Business leaders in the late nineteenth and early twentieth centuries advocated economic policies that would advance their narrowly focused interests. The wool industry offers a useful example of how this translated into the drive toward protectionism under McKinley's Republican administration. Woollen manufacturing was a major industry at this time, with powerful and politically-active mill owners located throughout the most important industrial centres of the United States. Under the contentious 1897 Dingley Tariff Act, wool was the item that was expected to bring in the most tariff revenue for the federal government (Anon, 1897). The interests of Republican politicians and of the wool industry overlapped in this respect at this time.

Wool manufacturers pushed for protectionist policies that would raise the duties levied on imported woollen goods. At face value, the logic behind this was simple: tariffs increased the prices paid for imported woollen goods, thus assuring domestically produced woollen goods' protection from foreign competition. However, in their pleas for duty increases, wool manufacturers did not simply press politicians to implement stronger protectionist measures. Instead, the evidence offers numerous cases when wool manufacturers called for *stable*, not prohibitively high, tariff levels. Still, the overall goal of mill owners was to get politicians to implement protectionist policies.

The most useful primary source for analysing the position of the wool industry *vis-à-vis* protectionist policy issues raised during the 1890s is the *Bulletin of the National Association of Wool Manufacturers*. Founded in 1864, this association was composed of the owners of wool manufacturing mills, mainly in the industrial centres of the northeastern United States. Their quarterly bulletin, published in Boston, Massachusetts (near wool manufacturing centres such as Lynn, Lowell, and Haverhill), included analyses of current tariff legislation, transcripts of presentations to Congress, and discussions of economic issues as pertained to the wool industry.

This *Bulletin* offers detailed descriptions of how wool manufacturers felt about protectionist policies during the 1890s and the early twentieth century. As mentioned above, they framed their concerns as centering round uncertainty regarding tariff policy, rather than simply advocating elevated levels of protection for their industry. This is seen, for example, in a statement given by Secretary S. N. D. North of the National Association of Wool Manufacturers to the Ways and Means Committee of the United States House of Representatives (North, 1897, pp. 63-64). North's arguments demonstrate both the association's adamance that free-trade policies would harm its interests, and the fact that uncertainty regarding tariff policy was harming their ability to effectively run their businesses.

The anti-free-trade stance taken by the wool manufacturers aligns with the economic arguments made by the Republican Party. During this period of relatively extensive international trade, wool manufacturers perceived free-trade policies as an 'evil,' while Republicans saw free trade as denying the government the opportunity to balance its budget through increased tariff revenues (North, 1897, pp. 63-64).

4. Economists' Studies of the Impact of Protectionist Policies

Economic ideas provoked intense debate during the politically and economically volatile decades of the late nineteenth and early twentieth centuries. This is apparent from the political record, where economic ideas – free trade versus protectionism – were at the forefront of the debates over tariff policies. The business community also weighed in by debating particular arguments put forth by economists.

Many of the contributions to this debate were empirical studies, analysing the impact of particular policies on specific industries or regions. Case studies were a preferred methodology for these types of studies, as discussed at length below. But there was the theoretical concept of pure free trade in economists' minds at this time, which is useful to note as it provides a reference point from which to gauge the degrees of protectionism that economists were arguing for or against. Taussig (1905) presents a detailed overview of the state of free-trade thinking in the first years of the twentieth century.[1] He describes how, during the first half of the nineteenth century, economists were more consistently in favour of liberalised trading regimes. Later in that century, countries from the United States to France to Russia increasingly implemented protectionist policies. In the mid nineteenth century, of all the issues economists studied 'the one least open to doubt seemed to be that, between nations as between individuals, free exchange brought about the best adjustment of the forces of production; and international free trade was regarded as the one most potent means of increasing the efficiency of labor' (Taussig, 1905, p. 29). But over the latter half of that century protectionism gained strength, and countries adopted policies that were 'inconsistent with a strict adherence to free trade' (Taussig, 1905, p. 30).

Taussig's approach to discussing these issues highlights the way that some economists envisioned a theoretical situation in which free trade shaped economic activity. In this view, protectionist policies are aberrations from this more perfect system. Free traders wanted to liberalise trade regimes to move toward such a system. But even Taussig suggested that: 'No doubt also the free-traders do not squarely face the difficulties of a transition to their system: the slowness with which capital and labor would have to be withdrawn from protected industries, and the prolonged period of unsettlement which would have to be undergone before final readjustment' (1905, p. 37). This suggests a theory-reality spectrum, with the ideal of free trade at one extreme and the economic, political, and social forces calling for protectionism at the other end.

Taussig was dismissive of calls for protection: 'As to most of the familiar arguments for protection, either all the economists are hopelessly in the wrong, or else the protectionist reasoning is hopelessly bad' (1905, p. 32). Still, Taussig (1905) engaged with protectionist arguments in detail. Much of this discussion was theoretical. But some of the claims of protectionists, Taussig argued, were especially suited to analysis through concrete examples, or case studies. The infant industry argument was an example of this reasoning (Taussig, 1905, pp. 46-47). Taussig argued that some issues in the protectionism versus free-trade debate were of general concern in economic theory: 'The benefits of imports and exports, the relations of domestic and foreign industry, wages, foreign cheap labor, surplus products, over-production, dumping' (Taussig, 1905, p. 47). In contrast to these general concerns that could be theorised, infant industry arguments were case-specific. When studying the impacts of protectionism on economic activity in the turn-of-the-century United States, other contemporary economists used case studies and undertook in-depth empirical analyses rather than keeping the debate at the level of theory.

Turn-of-the-century economists often studied the effects of economic policies by employing case-study methodologies. The lack of national-level statistics available at that time often restricted economists' analyses to more narrowly defined subjects. But this does not diminish the effectiveness of these studies. The debate over the impact of free-trade and protectionist policies required an appreciation of the nuanced effects that these policies have. This was the case even within particular industries, such as woollen manufacturing. Carpet wool, for example, was not produced domestically, so manufacturers who used this input argued that it should be imported freely (Taussig, 1934, pp. 300-301). But political pressure from wool producing states led to carpet wool being a dutiable good along with other categories of wool. Taussig (1934) discusses these types of tensions within particular industries, from wool to silk to sugar. While he expresses a clear preference for more open trade policies, his detailed case studies highlight the divergent interests and views even within certain industries.

[1] I am grateful to Eithne Murphy for directing me to Taussig (1905).

Other researchers at this time also employed case-study methodologies to analyse how different trade regimes affected particular parts of the US economy. For example, in order to study the impact of the mid-1890s low tariff rates on wool, Line (1912) narrows the focus of his study to the northwestern United States. Line's brief paper underscores how case studies can yield insights beyond the limitations of aggregate level analyses. Broader studies of wool production in western states observed that output remained high even during the low tariff years of the mid 1890s. Line looks beyond the aggregate-level statistics and argues that wool output was high even under the free-trade regime because farmers anticipated that Republicans would soon return to the White House and reinstate protectionist policies. Here the case study methodology offers deeper insights into economic behaviour than can be gleaned from broader statistical measures. This fits Morgan's (2014) definition of case studies, which offer 'a complex, often narrated, account that typically contains some of the raw evidence as well as its analysis, and that ties together the many different bits of evidence in the study,' thus offering analyses of particular depth and detail (p. 291).

Another study of the wool industry during the 1890s highlights how difficult it is to ascertain precisely what impact trade policy has on industries as complex as the growing and manufacturing of woollen products. Wright (1905) argues that:

> 'The extent to which our wool-grower is protected against the foreign wools by the tariff duties is a question often asked, but most difficult to answer. Ordinarily, the mere fact that a commodity is imported and the duty paid is taken as evidence that the price of the article in this country is raised to the full extent of the duty. This, however, presupposes that the article produced here and that imported are identical in quality. Yet it would be difficult to find another article which varies in so many respects as do different clips of wool. Fineness, elasticity, length, and strength of the fibre, working quality, and shrinkage, all enter into the question. Each separate fleece even may be sorted into six or eight different grades. It is obvious that under the circumstances the effects of system of duties like ours are not simple or easily analysed' (p. 619).

Wright goes on to argue that the complexity of the issues surrounding tariff policies and the wool industry '…must render any deductions uncertain. The attempt has been but to point out certain dominant tendencies, and the results to which, under given conditions, they lead' (1905, p. 645). He concludes that while the tariff did help producers of raw wool, other changes in domestic and global agricultural conditions negatively impacted wool producers. Ultimately, Wright concludes that 'The deeper one studies this industry of wool-growing, the better he will realize how varied is the guise which its competitors assume, how manifold are the factors which determine its course, and, above all, how difficult it is to control that course artificially' (1905, p. 645).

Other turn-of-the-century economists were more directly critical of the protectionist rates under the Dingley Tariff Act. Referring to the imposition of high duties on coarse wool, such as carpet wool, Taussig (1897) argued that this was '…a sop to states politically in doubt' (p. 596). Overall, Taussig argues that the Act was 'a source of sad disappointment' (1897, p. 598). Taussig was a perhaps the most prominent advocate of free trade at this time. But other economists were also critical of the extent of protection offered by late-nineteenth-century tariff rates. In his analysis of the turn-of-the-century paper industry, for example, Hess (1911) notes that 'The formation of the [paper] trust so soon after the enactment of the protective tariff act of 1897 has not been overlooked by those who are prone to regard the tariff as the cause of all evil' (p. 660). Thus the ability of some industries to gain monopoly power under protectionism was another concern some economists had with the contemporary tariff policy. Studies like those of Taussig and Hess contrast with the findings of Line, for example, and demonstrate that economists' findings fell on both sides of the free-trade versus protectionism debate.

It is also important to underscore the observation that there was no perfectly dichotomous split between advocates of pure free-trade policies and those in favour of much higher tariff rates. Taussig was clear about his pro-free-trade views. Other economists supported liberalising trade policies, but also argued in favour of certain aspects of protectionism. Atkinson (1903), for example, argued that luxury items were 'suitable subjects for revenue duties' while goods used in manufacturing should be exempt (p. 291). An across-the-board and sudden adoption of free trade, Atkinson argued, would be '…a change which no one proposes and which very few would advocate' (p. 281). Similarly, Beardsley (1901) argued that protectionist policies can be useful for promoting underdeveloped industries, but policymakers should be careful that industries do not use their favoured positions to become monopolies: 'The object of tariff legislation should be to furnish adequate protection to such industries as require it, without providing the opportunity for monopoly abuses. This object is certainly not fully attained by the present [Dingley] tariff law' (1901, p. 380).[2] Beardsley goes on to argue that '…if a protective tariff is to be maintained at all, those industries in which the costs of production for any reason are higher in this country than abroad furnish its proper field' (p. 386). Thus some economists held the view that protectionist policies did have a role to play for generating revenue and helping domestic industries, but it was counterproductive to push these policies too far.

While case studies were used to analyse the impacts of different trade regimes in the late nineteenth and early twentieth centuries, nationwide economic studies were also undertaken but were limited to subjects for which data was available. Willoughby (1901), for example, was able to study the integration of US industries at the national level because there were examples of companies that had nationwide reach and which could therefore be representative of national-level trends. That said, the limited data that were available led to Willoughby's adopting a methodology of extrapolating from several examples in order to paint a picture of national-level trends. Jenks' (1900) analysis of trusts exemplifies the barriers faced by this type of economic analysis as was undertaken at that time. After analysing the development of several trusts in the US economy over the preceding 20 years, he concludes that it is not feasible to draw broader lessons from national-level analyses. He concedes that 'Each case still needs to be studied by itself before any specific conclusion can be reached. No general conclusion is possible' (1900, p. 74).

Closson (1894) offers another example of the challenges facing economists who wanted to study nationwide economic trends. He analysed the unemployment situation faced by different regions of the United States following the Panic of 1893, when proponents of protectionism (e.g. Republicans and the National Association of Wool Manufacturers) argued that the depression was deepened and prolonged by the Democrats' more liberal trade regime. To study unemployment during this depression, Closson presents data produced from a '…circular of inquiry sent to public officials and other citizens of all cities of over twenty thousand inhabitants, and of many smaller places' (1894, p. 168). Despite the limitations of this approach, Closson's study yields useful regional insights. In order to gain a sense of the unemployment situation in Massachusetts alone, for example, Closson had to aggregate estimates from a range of sources, yet the overall unemployment situation in Boston could

[2] I am grateful to Eithne Murphy for directing me to the work of Atkinson (1903), Beardsley (1901), and Hess (1911).

still only be summarised as 'The number of the unemployed in Boston is uncertain' (1894, p. 168). Still, Closson's study offers insights into how unemployment was experienced and mitigated at this time, with descriptions of unemployment relief schemes organised by charities and municipalities. Such details are lost in aggregate-level analyses of historical unemployment rates (e.g. Romer, 1989).

While studies such as Closson's were undertaken by academic economists and published in specialised journals, business groups also participated in these economic debates. One of the more in-depth examples of this comes from the National Association of Wool Manufacturers' response to Émile Levasseur's *L'Ouvrier Américain*. Published in Paris in 1898, Levasseur's study covered the 'general industrial situation in the United States' at the end of the nineteenth century (Anon, 1898a, p. 224). His two volume study, '…comprising more than one thousand pages, are a monument to the painstaking industry, the ripe scholarship, and the scientific spirit of their distinguished author,' and presents 'the most complete picture that has yet been written of the contemporaneous social and industrial life and forces of this country' (Anon, 1898b, p. 205). These descriptions come from an overview of Levasseur's work presented in the *Bulletin of the National Association of Wool Manufacturers*. The September 1898 *Bulletin* contains an 18-page summary of Levasseur's study of US workers and their position in the economy at large. The *Bulletin* presents Levasseur's work as being an economic study of unparalleled depth and scope: 'Indeed there exists no more comprehensive exposition of the present industrial status of any country in any language. He has done for us what no one of our students or statisticians has attempted on any scale at all comparable in comprehensiveness' (Anon, 1898b, p. 206). The study is based on qualitative data collected during a five-month trip Levasseur took to the United States. Levasseur also employs quantitative data from sources such as the US census, and he covers issues as diverse as industrial concentration, the determining factors of wage rates, and the appeal that socialism holds for workers.

Following the synopsis of Levasseur's work, the 1898 *Bulletin* contains a more critical analysis of a particular point touched upon by Levasseur: protectionism. As discussed above, protectionism was one of the main concerns of US wool manufacturers at this time. They argued that unanticipated fluctuations in tariff rates made it difficult for them to foresee how competitive their products would be compared to imported products. If there was too much of this uncertainty, the wool manufacturers argued that they were unable to make sound investment decisions, thus undermining their ability to run their businesses profitably.

Levasseur was not sympathetic to such views. Instead, he openly advocated the adoption of free-trade policies. In the *Bulletin*'s critique of Levasseur's work, he is characterised as belonging to '…that school of economists which depreciates all restraint upon the freedom of international trade; and he believes that whatever may have been the case in the past the time has now come when protection is no longer necessary or advantageous to the United States' (Anon, 1898a, p. 224). Free trade advocates are said to cite Levasseur's work selectively in order to support their own political agenda, with the following line of reasoning when questioning the necessity of protectionism: if US manufacturers are producing enough goods such that exports are increasing, why is it necessary to continue to protect US manufacturers from foreign competition? But here it is not with the free-trade supporters that the *Bulletin* is most concerned – instead, its criticisms focus mainly on Levasseur's treatment of wages under protectionism.

The *Bulletin* begins its critique of Levasseur's argument by suggesting that he got his ideas about protectionism from radical elements of the proponents of protectionism. These sources included '…certain extracts from campaign speeches and documents, [which are] highly colored and charged for immediate effect upon an excited electorate' (Anon, 1898a,

p. 226). The more extreme proponents of protectionism, it is argued by the *Bulletin*, are emotionally charged in order to win votes, but 'average American protectionist[s]' are more reasoned in their support of protectionist policies (Anon, 1898a, p. 226). The *Bulletin* argues that the populist protectionists go too far in attributing the high wages of American workers solely to the protectionist policies put in place by the federal government. It is then suggested that if only Levasseur would listen to the more informed and sober protectionists, such as the wool manufacturers represented by the *Bulletin*, then he would see that they actually '…sympathize with M. Levasseur's impatience with those protectionists who attribute the whole of this advantage to the tariff policy of the government' (Anon, 1898a, p. 225). The *Bulletin* does, however, argue that US tariff policies supported the economic conditions which allowed for the relatively high wage rates seen at the turn of the century.

The basic argument set forth in the *Bulletin* is that protectionism does not explain the high wages in the United States, but it does help maintain them. 'A high tariff in the United States…makes permanently possible the highest standard of wages, in industries open to the competition of countries paying the lowest wages' (Anon, 1898a, p. 227). The assumption underlying this argument is then laid bare: 'The truth of the matter is that with the lapse of time and the spread of civilization the tendency is strongly towards the equalization of the conditions of production in all machine-using countries' (Anon, 1898a, p. 227). While this assumption of the convergence of wage rates in industrialised countries is simply asserted by the *Bulletin*, it is then argued that the '…chief advantage which we possess to-day over competing countries is our superior wage and the inducement to harder work which it carries' (Anon, 1898a, pp. 227-28). Other than high wages, '…the United States possesses no advantage which other nations cannot attain by the mere process of imitation' (Anon, 1898a, p. 228). Thus protectionism was argued to be necessary for the good of American workers as well as for manufacturers.

These passages present some of the key economic ideas underlying the reasoning put forth by American wool manufacturers as they argued in favour of protectionist policies at the end of the nineteenth century. These arguments are certainly being made in support of the interests of the wool manufacturing industry. But they also shed light on how economic debates surrounding free trade versus protectionism were taken up by the business community at this time, and even had a populist appeal.

5. Conclusions

The debates between advocates of free trade versus supporters of protectionism were defining features of economic thought at the end of the nineteenth century. This was a time of significant economic change in the United States. The merger movement resulted in the large-scale reorganisation of American industry (Lamoreaux, 1985), and the progressive era saw widespread social and political changes (Weinstein, 1968). The protectionist Dingley Tariff legislation grew out of the conditions facing the US economy during the mid to late 1890s. Republicans argued that this tariff act would promote stability in the economy through raising revenues for the federal government, thereby allaying fears that the government's deficit position was becoming untenable. Also, as discussed above, the Dingley Tariff was meant to protect American industries from foreign competition, with the intention that this would encourage businesses to invest and expand their economic activity more broadly.

The Smoot-Hawley Tariff Act of 1930 was also implemented during a time of economic and financial distress. Then and now observers linked the Smoot-Hawley Tariff to the worsening of the Great Depression. 'As a score of writers have pointed out, the world depression and the Smoot-Hawley Tariff are inextricably bound up one with the other, the latter being not only the first manifestation but a principal cause of the deepening and aggravating of the former' (Jones, 1934, p. 2, quoted in Eichengreen, 1986, p. 1). Many continue to argue that the Smoot-Hawley Tariff exacerbated the problems of the Great Depression (Bernanke, 2013, p. 4). Future research can explore what changes in US economic thought led to the different reactions toward protectionist policies from the 1890s to the 1930s.

It would also be useful to know more about what workers thought of these developments. Levasseur and the wool mill owners wrote about the effects that protectionism had on wages, but it would be interesting to know unions' thoughts on these issues. Friedman (1998) discusses how the '…strike wave of 1894 also came during a major political upheaval, as a severe depression and a powerful Populist challenge threatened the established political parties' (pp. 45-46). Then the '…strikes around 1900 came after this challenge was met, but they may have been encouraged by the readiness of Presidents McKinley and Roosevelt to support unions and collective bargaining' (Friedman, 1998, p. 46). It would be interesting to know whether unions saw protectionism as encouraging businesses to expand production and hire more workers, and how unions viewed the evidence that free trade lowered the prices of consumer goods for workers. This paper focuses on the views of politicians, business owners, and academic economists, but for a more complete understanding of this time period it would be necessary to say more about the views of workers and farmers.

What is clear from the above analysis is that politicians, business owners, and economists were all deeply concerned with the implications of trade policies, as the more liberal trade regime of 1894-96 gave way to the high-tariff years following McKinley's election. It is significant that the advocates of protectionism won these debates, such that their policies directed American stances on trade through the first decade of the twentieth century. Future research can study in greater depth the effects that these policies had on the US economy at this time, when mergers and progressivism began to reshape the American economy, and society more broadly.

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A Commentary on Peter Bent's 'The Political Power of Economic Ideas: Protectionism in Turn of the Century America'

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Peter Bent's paper 'The Political Power of Economic Ideas: Protectionism in Turn of the Century America' addresses an important question as to why the US in the late nineteenth and early twentieth century was still a champion of protectionism, and the role played by economic ideas in the outcome. Specifically, Bent focuses on the Dingley Act of 1897, which raised tariffs to unprecedented levels.

To know the reality of US trade policy in the past, the possible reasons for the trade policy stances of successive governments, and the economic arguments offered in support of their respective trade policy positions, are separate but overlapping issues that are of interest in their own right, but also of relevance for contemporary policy debates. In this regard, Bent's paper is to be commended for drawing our attention to the nature of the economic arguments deployed, and the forms of evidence used, in support of those positions. I say this even as I believe that he overestimates the role that economic ideas played in the eventual outcome; in other words, I would question his assertion that just because the US adopted the most protectionist policy in its own – admittedly protectionist – history, this indicates the victory of protectionist ideas. As the economic historian John Nye claims, when trying to understand the drivers of past policy 'political historians often overvalue the role of ideas in contrast to the more invisible, but longer term forces of economics and politics' (Nye, 2007, p. 110). That is not to say that ideas do not matter, they are indeed important (not least for propaganda purposes) but simply to question the view that they are all-determining in the policies adopted by governments. In this regard, I would also agree with Nye on the unique contribution of historical analysis to understanding past policies, to the extent that it attempts to reveal the constellation of interests, pressures and constraints, as well as economic ideas, that may have impelled governments to adopt the policies that they did. But, notwithstanding my quibble with Bent's view on how impactful economic ideas actually are on policy, I nevertheless applaud and appreciate what he has done in this paper and the seriously threatened tradition within which he is working.

Narrative accounts of economic events matter, not least today, where students of economics are woefully ignorant, not just of the history of their own discipline, but also of economic history. So just knowing that the US (which since post-World War II has been a champion of free trade), had a protectionist history can be revealing to many economists, reared as they have been on static models which assert (as a scientific principle) the universal benefits of free trade.[1]

Another reason why the protectionist stance of successive administrations in turn-ofthe-century USA is of such interest is because at that period in its history, the US was already emerging as the world's most productive economy – hardly one whose domestic economic activity required protection from more competitive forces overseas. This reality was

[1] See Bairoch (1993, p. 43) on his astonishment when discovering that those engaged in a contemporary debate on the merits of trade versus protectionism believed that the US had a free trade past.

not lost on many economists at that time, which made the logic of US policy harder to understand from the perspective of national economic interest. The Harvard economist Frank Taussig, when reviewing the 1897 Act, started his commentary by stating:

> 'Three or four years ago nothing seemed more improbable than the enactment of a measure affirming once more the principle of an all-embracing protection, and putting it into effect with a vigorous hand' (Taussig, 1897, p. 42).

Indeed, the protectionist policy stance of turn-of-the-century US administrations was viewed by some of its informed citizens as positively injurious to US interests, as another contributor to the debate at that time noted:

> '…have we not reached a point in our industrial condition when it may have become a more important problem how to keep the door open for our exports to foreign countries than how to close the door to foreign imports?' (Atkinson, 1903, p. 281).

So, if it is a case of protectionist ideas winning the debate, as Bent implies, what can account for such seeming economic irrationality? Is it a case of applying Joan Robinson's verdict (on the case of free trade in Britain) to protectionism in the US; that is to say, that it was an ideology that had outlived its usefulness, but one that still had a grip on the public consciousness, proving that ideologies can be difficult to counter with reasoned debate and that it takes time to replace one ideology with another.[2] Or, were there other forces at work, that could help us understand why successive US administrations found it so difficult to reverse a policy that had served it so well in its past.

While Bent does supply some context to the Dingley Act, the main thrust of his paper is focused on exploring the economic concepts employed by different interested parties, as well as presumably more disinterested economists. But, in the spirit of what I already said, which is that ideas are not enough to explain the twists and turns of government economic policy, I believe that it is useful to supplement his account with more details of the economic and political situation at that time, as related primarily by Frank Taussig. While not disagreeing with Bent that Taussig was a prominent free-trade advocate (which would have coloured his view of events), he was, nevertheless, a very informed commentator – one who knew intimately the minutiae of tariff legislation and the political, electoral and fiscal constraints that inevitably impacted on the shape of policy. So, specifically, as regards the 1897 Act, while he agreed that '[it] was the outcome of an aggressive spirit of protection' he was more circumspect in attributing this position to the 'verdict of the people' (Taussig, 1909a, p. 358). Indeed he claimed that the Democratic victory of 1894 had more solid ground for maintaining that the 'popular verdict had been against high handed protection than the Republicans in 1897 that it had been in favor [sic] of such a policy' (p. 358). Also, Taussig does not go as far as Bent, who characterises the legislative record of the Democrats in the previous administration as 'free trade'. Rather for Taussig, the preceding administration had simply begun a policy of trade liberalisation, moreover he deemed 'most of the steps in this direction [to be] feeble and faltering' (p. 318). The single exception to his judgement that the previous

[2] Joan Robinson was trying to explain why there had been such an outcry when Keynes turned apostate and argued for protection in Britain in the 1930s. Her conclusion was that the ideology of free trade (that served Britain so well in the nineteenth century) still reigned, despite the very different economic circumstances of the country, showing that an ideology can outlive its usefulness, as well pointing to the irrational element that is inherent in all ideologies (Robinson, 1962, p. 84).

Democratic party's legislative record on trade was 'anxiously conservative' was the decision to allow for 'the free admission of wool' (p. 321). All of the tentative steps in a liberalising direction that were adopted by the previous Democratic administration were reversed by the newly victorious Republican government. The starkest testament to this *volteface* was the Tariff Act of 1897, as introduced by Representative Dingley. Taussig, like Bent, acknowledged the importance of economic events in the Republican victory, namely the economic crisis that developed in the mid 1890s during the period of Democratic Party governance. However he denied that the Republican victory of 1896 reflected major public disillusionment with the policy of trade liberalisation. Instead, for him, the outcome of the 1896 election was fought and won on the question of monetary policy:

> 'Thus the election of 1896 turned on the question of the free coinage of silver. The popular verdict was clear on that question and that question only' (p. 322).[3]

Saying that, he still admitted the seeming inevitability of higher tariffs, given what he called the 'political complications of 1896-97', while maintaining at the same time that the Act of 1897 was excessive, even when judged by the political exigencies of the situation. This was so much the case that that it even disheartened those who supported the Republicans on the money issue (p. 358).

So if, according to Taussig, the move towards unprecedented protectionism was not wholly in keeping with the popular mood, what political forces could explain it? First, there was the complexion of the national legislature which, despite the Republican victory, did not give them free hand on 'either the tariff or currency' (Taussig, 1897, p. 44). Then there was the pressing issue of the Treasury deficit and urgent need for fiscal revenue (p. 45). So in 1897, the newly elected President, William McKinley, asked Congress to deal solely with import duties and fiscal revenue (p. 45). For Taussig, the issue of government finance should have been separate from that of industrial policy, even as he admitted that they were historically intertwined, both in the US and in most countries (p. 45). Later he supplied details of the extent of the fiscal crisis, which had been ongoing since 1894, which, for him, fundamentally reflected the inadequacy of federal legislation for the collection of fiscal revenue and, consequently, the over reliance on customs tariffs for revenue purposes. Nonetheless, the political debate on the issue of fiscal revenue was (inevitably) partisan, with Republicans attributing the federal deficit to the trade liberalising legislation of the previous Democratic administration (Taussig, 1909a, p. 325). A feature of the 1897 Act that may seem surprising was the measure that restored duty on the importation of raw wool, ostensibly against the economic interests of wool manufacturers, a group favoured by the Republicans.

Taussig's explanation was the Republican government's need to placate Senators from the far western ranching States, who held the balance of power. They did so by re-imposing duties on raw wool, which directly benefited the farmer (Taussig, 1897, p. 50). Of course, this inevitably led to escalating tariffs on manufactured woollen goods in order to compensate manufacturers for the increased cost of the raw material. According to Wright (1905, pp. 62425), wool manufacturers, though aware of the cost to them of increased duties on raw wool, consented (though not all) to this measure out of political necessity, feeling that it was the only hope they had of getting increased protection for their goods. And he claims that they

[3] The Democratic Party was seen as the silver party, or the party of cheap money, which best represented the economic interests of farmers and debtors. The Republicans were on the other side of the debate.

were not disappointed in the outcome, admitting that the Dingley Act offered them more protection than any previous tariff (p. 626).

So this reading of the reasons for the Dingley Act and its exact nature, would, in my view, place the emphasis on the government revenue crisis (and limited instruments for raising taxes), the complex character of the legislature, as well as what Taussig refers to as the 'old predilection' of the Republicans for protection.

Taussig, when trying to understand why US protectionism persisted for so long, went so far as to say that it was not the product of deliberate choice. He was not talking about the whole history of US protectionism, but what he called the extreme and intolerant protection that existed in turn of the century USA.[4] In his address to the American Economics Association in 1905, he attributed it to historical accident, by which he meant the Civil War which lasted from 1861 to 1865 (Taussig, 1905, p. 62). For him, the revenue exigencies of the Civil War resulted in very high tariffs and these were maintained afterwards because of:

> '… custom and iteration … The industries of the country have become habituated to it; and what is no less important, public feeling has become habituated to it' (pp. 62-63).

He compared protectionism in the US, and its public support, to free trade in England, in terms of being 'accepted doctrines' which gained extra hold because of the concomitant material prosperity that these countries enjoyed. For Taussig it was clearly a case of *post hoc ergo propter hoc*, and he felt that change in the legislation was not likely until that correspondence was broken (p. 63). He reiterated this point when talking about the Tariff Act of 1909, claiming that populations' thoughts become habituated to the legislative *status quo*, which he found to be, not only natural, but sensible (Taussig, 1909b, p. 8). The upshot of this is that abrupt policy shifts are both inexpedient and politically impossible, so that change has to happen in gradual steps. Yet, Taussig also explained the protectionist bias of US legislation at that time, as being a product of the US government structure, where the US Senate exercised greater power than the House, due to its smaller size and longer term. That the US Senate had a protectionist orientation was due to the greater subservience of the Senators to monied interest, and the unique representative nature of the Senate, where thinly populated States had disproportionate power (pp. 16-17). While he did not label this exercise of power as deliberate corruption, '… since there is no ground for suspecting anything more than direct contributions to party chests' (p. 32), he nevertheless alleged that the outcome was much the same as if there had been corruption.

When discussing the economic concepts deployed by protectionist politicians, Bent highlights that the Republican Party was seen as the party of US business interests, which, despite the rhetoric of free enterprise, believed that government should attend to the economic interests of industrialists. Even if US industry was more competitive than the international competition (as many economists at that time asserted), it would still have made sense for the Republican Party to champion protectionism – as long as it perceived that its business constituency was best served by this policy stance. This would have been the case if US industry made the most of its profits from supplying the US domestic market. In such a scenario, any policy measures that limited international competition enhanced those profits. But US business practices were, at that time, in the process of change, as US industrialists started selling more to overseas markets. Protectionism could hardly have served their

[4] Taussig (despite rejecting a universal argument for infant industry protection) does say that it is his belief that manufactures in the USA were advantageously promoted by restrictions on imports in the *early years* of the nineteenth century (Taussig, 1905, p.50).

business interests. What can explain their lack of political influence? Were they unaware of the policy stance that best served them? Were they insufficiently organised politically? Or was it still the case that the international market was less important to US business interests than the domestic one? Whatever the reason, one can only presume that the Republican Party must have been still catering to its predominant power base by adopting protectionist, rather than liberal, trade policies.

The prominence given to the government's role (obligation?) in imparting confidence to the business community is an interesting and important point, anticipating Keynes' emphasis on animal spirits as a key driver of economic activity. The importance of the revenue aspect of the Dingley Act has already been discussed, but I was struck by Bent's view that its quantitative estimation of expected tariff revenue was a (rare) example of a politician working through ideas in a rigorous way. For Taussig, the confidence with which Representative Dingley forecast the expected revenue yield from the legislation was misplaced, indeed he viewed all such predictions as little better than guesswork. His reasoning was simple, which was that dutiable imports fluctuated too much and too unexpectedly to provide any stable basis for such precise prophecies (Taussig, 1897, p. 67). But, the salient point, which resonates especially today, is that the quantification of social phenomena and quantitative predictions of related phenomena, while useful, need to be accompanied with an attendant social health warning, as all too often they are complicit in creating a dangerous illusion of precision and control.

As Bent says, the woollen industry was a powerful and politically active one, which had pushed for more protection against imported products. But, they exhibited their social responsibility by calling for stable, not prohibitively high tariffs. Their rhetorical restraint may also have reflected the heterogeneous nature of the woollen industry (including farming), and a realisation that there were upper limits to the price of wool and woollen products. The complex situation facing those in the woollen industry was elaborated on by both Taussig and Wright, with the former citing directly a formal communication from the Wool Manufacturers to the Ways and Means Committee on the rationale for their demands, which made reference to the heterogeneity of production conditions within the US (p. 49, footnote). For Wright (1905, p. 628), there was an upper limit to price that the wool tariff could effect, given the input needs of manufacturers and domestic supply constraints. Nevertheless, Taussig (1897, p. 52) estimated that the Dingley Act of 1897 pushed the effective net rate of protection of the woollen sector to an unprecedented 55 percent.

Bent mentions how the National Association of Wool Manufacturers defended their case for protection on the grounds that reduced uncertainty enabled them to make better business decisions, as well invoking the high wage argument. This was undoubtedly a clever (and still much used) tactic, to claim that legislative measures were not just in the interests of those lobbying for them, but also in the broader social interest. The case that they made seemed both reasonable and temperate; saying that tariffs facilitated the payment of higher wages, while not being wholly responsible for them. Of course, it was precisely the kind of argument that enraged Taussig, who while acknowledging its popular force, designated it 'claptrap' and furthermore asserted that most economists were in broad agreement with his position (Taussig, 1905, p. 35). For Taussig, the fundamental cause of high wages is labour productivity, which combines the benefits of high wages with low prices.[5] By contrast,

[5] This is still an article of faith among his neoclassical descendents. For example, Paul Krugman rubbished the concept of unfair or exploitative trade by invoking essentially the same argument; that is to say that it is average productivity in a country that determines aggregate wage levels. Or as he pithily put it referring to currently low paid labour in developing countries 'if they achieve Western productivity, they will be paid Western wages' (Krugman, 1998, p. 30).

maintaining high wages in a sector through protection, has the attendant ill of high domestic prices and the misallocation of resources (pp. 36-37).

As far as the views of economists are concerned, Bent alleges that economists fell on both sides of the free-trade versus protectionism debate, with many taking an intermediate position of favouring protection of certain sectors and free trade in other sectors. He correctly cites Frank Taussig as the most prominent of the free-trade advocates and one whose discussion was primarily theoretical. By contrast, according to Bent, most other economic studies at that time tended to be resolutely empirical, often concentrating on the effects of trade policy on particular sectors and regions. Because of the challenges posed by inadequate national data, these empirical studies were usually of a case study nature, which inevitably intertwined qualitative analysis with quantitative evidence to back their claims.

Taussig's theoretical exposition on trade was dismissive of most of the arguments of the protectionists, even as he acknowledged the real challenges posed by competition induced reallocation of resources, and the tendency of free traders to minimise this uncomfortable reality (Taussig, 1905, p. 37). The only concession that Taussig would make to protectionist claims was the infant industry argument, and even then he was reticent, asserting that it was not always successful, and that even where it seemed to have been successful, one could not preclude the possibility that other forces were responsible for a country's economically successful transformation (p. 49). He went so far as to allege that history showed, not only the insufficiency of the infant industry argument, but also its lack of necessity, in some instances. Essentially, for Taussig, the arguments for and against protection of young industries, could only be made inductively, on a case-by-case basis (p. 48). However, from his own inductive and historical research, he admitted that industry in the US had been facilitated by such a policy in the first half of the nineteenth century (p. 50).

The US economists whose names are synonymous with the American System (of which infant industry protection was such a prominent component), namely Alexander Hamilton and Henry Carey, argued for temporary protection until such time as industry reached its competitive maturity. Arguably, by the 1890s, much of US industry was mature enough to compete on the global stage, so this argument could no longer be invoked. So it is unsurprising that the argument for protection had to change, as it did, becoming one about workers' wages and the need to protect them from lower waged international competition. What is questionable is whether there were US economists invoking this argument. The evidence does not support this view.

Walker (1890), in his review of protection and protectionists in US history, made the well-worn point that there was no positive correlation between wages and cost of production, on the contrary, stressing that cost of production could be low when wages are high, and *viceversa*. He conceded that if a sector needed protection to defend its wage level, then it was so protected at 'the general charge' (p. 274), by which I take him to mean, at public expense.

Certainly none of the economists' writings cited by Bent invoke the wage argument as an economic rationale for protection, on the contrary, when they do refer to it, they invariably do so in a dismissive fashion. Taussig's views on this matter have already been mentioned. Hess called the wages defence argument 'wearisome' citing the relevance of 'the cost of labour per unit of output' (Hess, 1911, p. 672). And Atkinson was beating from the same drum when he asserted that:

> 'It is now an accepted truth, axiom, or principle in economic science that, in all arts to which modern science, invention, and mechanism can be applied, the highest rates of wages or earnings are recovered or derived from the sale

> of products made at the lowest cost of labor computed by the unit of product, either by the bushel, the pound, the ton, or the yard' (Atkinson, 1903, p. 281).

So it may be more accurate to attribute the high wage argument to industry appeals and Republican Party philosophy than to prevailing ideas among economists. This is the position of the economic historian Bairoch, who said that protectionism to safeguard American wage levels, was a central plank of Republican Party policy in the 1890s (Bairoch, 1993, p. 36).

So what were the arguments deployed by those economists who favoured protection, or some degree of protection? Bent claims that economists were on both sides of the protectionist debate, which I find to be a strong assertion in light of the evidence, as published in the predominant US economic journal at that time, the *Quarterly Journal of Economics.* Certainly Line (1912) comes across as the most protectionist of the economists cited, claiming that the removal of the duty on wool in the 1894 Tariff Act had had an adverse economic effect on sheep farmers in the North Western States of the US, and that it was only the anticipation of a reversal of this policy that enabled many farmers to survive. But Line also pointed to the decline of the industry in that region of the US, due to other forces at work, and doubted whether protection had any real impact on the process of change. Wright (1905), also acknowledged the assistance that tariffs provided to producers of raw wool, even as he felt that it was 'deceptive' (p. 645) in that the aid was less than one would expect given the size of the nominal tariff. Wright could be regarded as being less pro-protectionist than Line, to the extent that he did not believe such a policy could, King Canute style, hold back the other forces leading to the (inevitable) decline of the woollen sector. These forces that he identified were – improved global accessibility, changing demand patterns for textiles and alternative uses for land. The writings of Line and Wright have a distinct sectoral focus (albeit a sector with a lot of political clout) but Wright's geographical canvas was broader, in that he looked at the impact of policy on the wool industry across the whole of the US. This broader perspective, allied with the heterogeneous nature of US farming, may explain Wright's more pessimistic tone, when it comes to the efficacy of protectionism in assisting a sector.

Of the other economists mentioned by Bent, he put Hess (1911) on the free trade side of the debate, with most others occupying the middle ground between ardent free traders and realistic protectionists. But what is being discussed here are the arguments that they used, as no doubt, even free-trade advocates would not disagree that freer trade would be to the economic detriment of some sectors, even leading to their inevitable demise The issue then became one of whether policy should be used to prevent the decline of certain sectors and the economic and social justification for such measures. Hess was as narrowly focused as Line and Wright, in looking at the impact of trade policy on the paper industry, even as he came down on of the liberal side of the protectionist controversy. He defended his stance by claiming that, in some instances, no amount of protection will save 'unsuitably located mills' (Hess, 1911, p. 681), but the more forceful part of his argument was that such a policy penalised the consumer and, above all, led to serious environmental damage. Bent points out that Hess also alluded to the emergence of a monopoly (paper trust) soon after the enactment of the 1897 tariff, and that this tendency was one that concerned some economists.

The *zeitgeist* at that time was to be increasingly critical of monopolies, which were an emerging phenomenon on the US economic landscape. For Taussig, the more critical public attitude had to do with the belief that domestic producers were making 'unreasonable profits' Taussig (1909b, p. 7).This fed into the debate on protection at that time, to the extent that trade policy facilitated this process. Beardsley (1901) drew attention to the fact that most monopolies in the US were to be found in protected industries. He believed that so rapid had been the consolidation of industries in the sectors protected from foreign competition, that the tariff issue needed to be reconsidered. So as far as US trade policy was concerned at that time, Beardsley was arguing for more trade liberalisation. But Bent is correct in saying that Beardsley was not anti-protection *per se*, only fearful that it could facilitate monopolisation of industry to the social detriment. He defended tariffs for uncompetitive US industries,

> '… if a protective tariff is to be maintained at all, those industries in which cost of production *for any reason* [my emphasis] are higher in this country than abroad furnish its proper field' (Beardsley, 1901, p. 384).

This is, of course, straightforward protectionist sentiment of the sort that so depressed Taussig, since as he correctly said:

> '…any tariff so implemented as to equalise the cost of production between US producers and their international competitors would result in the complete stoppage of international trade' (Taussig, 1909b, p. 2).

So Beardsley, notwithstanding his concerns with the particularity of US trade policy and its effect on the structure of industry, notwithstanding his call for more trade liberalisation, still articulated a protectionist argument that ran contrary to orthodox trade theory, as expressed, not just by British classical and neoclassical economists, but also by their mainstream American heirs, such as Taussig.

Also, as far as the dangers of monopolies were concerned, their existence could not be exclusively attributed to protectionist trade policy. Indeed, rather presciently, Beardsley acknowledged that '… international competition could lead to international combinations' (Beardsley, 1901, p. 387). Likewise Commons (1892), whose views are of particular interest as one of the pioneers of the American Institutional School (natural heirs to the German Historical tradition), had an ambiguous view on protection, not least because he realised that different monopolies arose under different competitive conditions. For him, natural monopolies in agricultural land and transportation were facilitated by free trade. By contrast, other natural monopolies, in sources of raw materials and land used for manufacturing, benefited from protection to manufacturing. A common feature of all commercial regimes was that neither labour nor, what he referred to as free capital, ever gained. The beneficiaries were always different types of natural monopolies, by virtue of their ownership of scarce resources. From a policy perspective, he did not attach much significance to the free trade versus protectionist debate as he felt it was a side issue.

> 'The controversy between protectionists and free traders is a matter of inferior significance. Whichever side wins, natural monopolies absorb the gain' (Commons, 1892, p. 484).

Accordingly, given his social concerns, his primary policy recommendation was for a special tax on speculative holdings of scarce natural resources, allied with free trade in raw materials, whereas he remained silent of the issue to free trade or protectionism in manufactures.

Bent cites Atkinson (1903) as another example of an economist whose stance on trade policy was intermediate between the extremes of free trade and protection. I agree that Atkinson was not making a case for absolute free trade, but the tenor of his article was very much in a liberalising direction. In what was an impressive piece of empirical analysis, utilising country-wide census data, Atkinson attempted to show that a liberal trade regime would not have detrimental economic and social effects, in terms of its impact on employment.

> 'When the facts become established, the dread of business depression predicted, if any complete revision of the duties on imports is undertaken, may be wholly removed' (Atkinson, 1903, p. 282).

And later:

> 'I think it will prove impossible for any sincere student of the subject to designate one million persons, out of the twenty-nine now occupied for gain, whose industry would be seriously or adversely affected, even if all duties on foreign products of like kind were all at once removed – an act which no one proposes' (p. 292).

While he did allow for tariffs on what he deemed to be luxury goods, the thrust of his article was designed to show that the traded sector of US industry did not need protection, that much 'injudicious' protection hindered exports and that the bulk of the employed population were adversely affected by a policy stance that added to their cost of living.

Of course, empirical work like that of Wright, Hess, Beardsley and Atkinson did not address the essence of the free-trade argument, as articulated above all by Taussig, which was that free trade benefited countries independent of its impact on particular sectors. Whether a certain sector expanded or declined with the change to a liberal trading regime was neither here nor there. Free trade was considered an enlightened policy stance because of its expected impact on domestic resource utilisation and prices. The work of the aforementioned economists at that time was designed, to different degrees, to show that the US could withstand foreign competition, possibly even thrive from it, precisely because it had become such a productive, and consequently, competitive economy. The closest to an economy-wide analysis was the work of Atkinson, and his use of Census data was innovative as well as revealing.

Indeed, it could be considered a foretaste of the type of empirical economic analysis that was to come, even if his approach was static and he did not deploy statistical techniques. However, the accusation of Taussig, which was that such reasoning was effectively reflective of mercantilist thinking, is valid. That is not to say, however, that mercantilist reasoning is incorrect or 'crass' as Taussig (1909b, p.9) suggested. But it does leave us with the perennial question. Why was the US still defending and practising protectionist policies at a time in its economic history when it appeared to have no need to do so, according to either classical, neoclassical or mercantilist logic?

In this regard, a very interesting contributor to the debate was Powers (1899), who advanced a subtle but powerful argument on the determinants of US trade policy and reasons why he believed it was in the process of change. He spoke of the impact of the public mood, by which he meant the culture and ideas and temper of the nation. And he predicted that the US would move away from protectionism and towards free trade as the public mood was in the process of changing. For Powers, the protectionist stance of US trade policy for much of the 19th century was not due to the persuasiveness of the infant industry argument, nor that of the subsequent pauper labour argument (though he was not disparaging those arguments, simply claiming that they were irrelevant). Instead, he asserted that they were the outgrowth of unique special conditions of US post-independence history, such as the need for nation building and the creation of a national identity. (In this, he sounds remarkably like Taussig, in his reference to historical accident to explain US protectionism). But Powers goes further in his reference to national moods, which he acknowledged exasperated the economists but which he claimed could be useful and necessary. For him economic arguments are a *product* of policy, not a *cause* (Powers, 1899, p. 372). So he predicted a change in the public mood more favourable to liberal trade, not least because of US military engagements with Spain and the Philippines at that time, which would change the national imagination. Also,

> 'The very growth of foreign commerce itself, from whatever cause, tends to create powerful and organized interests, restive of commercial restraint and forming a hitherto lacking counterweight to the closely organized manufacturing interests which have so successfully supported the policy of protection' (p. 376).

To my mind, Powers presents a very subtle and powerful argument about the development of national consciousness which can be deployed in favour of whatever particular policy stance is useful.

The intellectual arguments used to defend the protectionist *status quo* in turn-of-thecentury USA were: confidence and/or reduction in uncertainty (by business and Republican politicians); the protection of wages of US labour (by business and Republican politicians); and the decline of certain sectors in the absence of protection (Line, 1912). Most economic commentators argued for reduced protectionism and increased trade liberalisation. As Bent correctly notes, most of these studies were empirical, frequently case studies, and their arguments ranged from: the inevitability of change which protectionism could not halt (Wright, 1905); sectoral gains and environmental benefits (Hess, 1911); reduction in monopoly power (Beardsely, 1901); and the competitive strength of the overall US economy (Atkinson, 1903). I agree with Bent that the most forceful advocate for the free trade position was Taussig, and that moreover, his arguments were primarily theoretical, even as he was acutely aware of the reality of the economic, political and legislative landscape. What is noteworthy about those economists whose empirical work supported greater trade liberalisation, is that they did not address the issue in terms that theoretical economists regarded as valid. So whether they called for greater liberalisation because, in their view, the US economy was competitive enough to prosper in such an environment, or conceded the protectionist case in instances where US sectors were insufficiently competitive (as did Beardsley, 1901), their implicit emphasis on competitiveness would have been rejected by contemporary neoclassical economists, as it was by Taussig. Almost all neoclassical economists, on both sides of the Atlantic, supported not just the policy of free trade, but the principle as well.

> 'The essence of the doctrine of free trade is that prima facie international trade brings a gain, and that restrictions on it presumably bring a loss' (Taussig, 1905, p. 65).

Neoclassical economists (then as now) defended free trade, as a beneficent doctrine with universal application, on the basis of the principle of comparative advantage. Since it was presumed that all countries had a comparative advantage in some sector, discussions on competitiveness were extraneous. A country would be competitive in sectors where it had a comparative advantage and uncompetitive in sectors where it had a comparative disadvantage. But the country as a whole would be economically richer if competitive forces (like free trade) resulted in an economic structure that reflected this imperative. Its resources would be put to the best possible use and consumers would have the benefit of low priced imports. Those who questioned this principle were classed as not belonging to the neoclassical school.

This division among academic economists, which was generally influenced by their economic method, manifested itself in the UK (the home of Classical Political Economy) during the Tariff reform controversy of 1903.[6] Those economists who came out publicly in defence of free trade (to the extent of penning a letter to the *Times*) were invariably economic theorists, whereas those who favoured a return to tariffs were, by and large, economic historians. Taussig took issue with contemporary German economists (of the German Historical School) for their failure to accept established theory on the doctrine of free trade.

> 'In their view there is no established theory and no reason for ascribing greater validity to the doctrine of free-trade than to that of protection. It is all a matter of advantage – or disadvantage in the given case' (p. 57).

What upset Taussig about the position of the German Historical School (GHS) was that they denied the existence of established theory on economic matters and embraced a policy of relativism and opportunism when it came to a country's trade policy stance. He attributed this unfortunate tendency to their intellectual deficiencies claiming that 'On all such questions of principle, we often find a sad lack of clear-cut reasoning among our German colleagues' (p. 57). Furthermore, 'this defect does not show itself solely in the protective controversy. It appears in almost every part of the economic field, as soon as the more difficult problems are touched' (pp. 57-58). For him such a tendency confirmed that they were writers of *'… the second rank*' [my emphasis] (p. 58). I do not think that it is presumptuous to say that what made economists of the GHS second rank, in the eyes of Taussig was, not just their failure to theorise in the neoclassical manner, but their refusal to accept that, in economics, there was any established theory with [almost] universal applicability. Their form of scholarship was hampered by being too wedded to concrete facts.[7]

For the economists cited by Bent, who waded in on the free trade-protectionist debate, facts, both of a quantitative and qualitative nature, were a central part of their arguments. Most argued for a more liberal US trade policy regime on the basis of what they considered to be the reality of US competitiveness. For whatever reason (the compilation of vested interests, political constraints, fiscal issues, inertia, public mood), protectionism continued to be a feature of US trade policy. By contrast, after World War II, the US started to liberalise its trade and became a powerful vocal advocate for the merits of a free-trade policy. What can explain such a Pauline conversion? For some, it could be the changed nature of the political and social constraints that the US faced. Moreover (invoking the relativist logic of the GHS) such a conversion made sound economic sense, given the extent of US industrial supremacy after the war. However, there are those who would attribute the changed US policy stance to the triumph of sound economic ideas and improved economic methods, which must have eventually convinced legislators of the rightness and righteousness of free trade. Douglas Irwin is a case in point.

[6] The Tariff reform controversy was a product of an unsuccessful campaign launched by Joseph Chamberlain (the Colonial Secretary in the government of the day) in 1903 to abandon Britain's freetrade policy for a policy of imperial preference.

[7] Unsurprisingly, many years later a similar accusation of lacking theory and being obsessed with facts was levelled at American Institutional economists by their neoclassical peers. This was played out in titfor-tat articles in the *American Economic Review*, which culminated in Friedman's famous, and arguably influential, article that laid down methodological prescriptions for how economics should be practised (see Friedman, 1953).

> 'Free trade … remains as sound as any proposition in economic theory which purports to have implications for economic policy is ever likely to be' (Irwin, 1996, p. 8).

And Irwin is not alone. Esteemed neoclassical economists from Paul Samuelson to Paul Krugman have vigorously defended what they believed to be the veracity of the principle underpinning the view that free trade is an unmitigated good for all [countries] concerned.

All of which brings me to the issue of economic method. A feature of economic analysis that Bent highlights was the ubiquity of case studies, not least because of a lack of national statistics. While he acknowledged that the lacuna of data inevitably constrained the focus of empirically-oriented economists, he nevertheless emphasised how effective such studies could be in terms of revealing an analysis of more depth. However, he also refers to the challenge of extrapolation posed by limited data. I agree with what he says about the limits and merits of case studies. Undoubtedly researchers today inhabit a qualitatively different environment to those economists who were writing about the impact of trade policy in turn-of-the-century USA. Not least, there is a rich supply of national statistics, an increased array of statistical procedures (to facilitate extrapolation) and amazing computing power. So such increased resources should facilitate improved economic analysis. Yet, quite apart from what the global financial crisis did to the professional reputation of economists, there is a contrary example more pertinent to the topic of this paper – the debate over free trade versus degrees of protectionism. It is now finally acknowledged (though very often reluctantly) the role that protectionism played in the economic success of South East Asian economies. Yet for a long time economists failed to recognise just how interventionist governments in these countries actually were, some going so far as to erroneously hail them as exemplars of freetrade orthodoxy, while others claimed that they industrialised and developed despite protectionism.[8] What can account for such evidential blindness? One possible reason is hegemony of a single method in contemporary economic research. For many contemporary economists, empirical proof of cause is a statistically significant coefficient attached to the relevant explanatory variable. Quite apart from the challenge of finding a variable (or variables) to adequately portray the complexity of a country's trade policy, as well other data and statistical challenges, this is a frighteningly blinkered view of evidence. It creates misplaced confidence that one has found economic causes simply because (imperfectly) measured aspects of social reality appear to be correlated. My point here is not to rubbish statistical analysis or to argue for a return to case study methods, but to highlight the dangers of relying exclusively on one form of evidence. Statistical analysis derives its force by selecting comparable, numerically expressed observations of a few aspects of social reality, and looking for patterns and correlations in the objects selected and measured. This is sometimes called a thin description of social reality, reflecting the uni-dimensional nature of the observations and the fact that only certain features of social behaviour are selected and studied. By contrast, case studies start from the assumption that social and economic life is multi-dimensional and multi-connected and seek to integrate different forms of evidence into a coherent narrative. Far from being unscientific, case studies are a different way of doing science, a different way of finding out about the world. Indeed Morgan (2014) not only defends the case study as a respected epistemic genre, she claims that case studies are eminently suitable as part of social science research, where controlled experiments are impossible. If past and recent history has shown us anything, it is that there are many paths to knowledge, and, given the complexity of the

[8] For example, Paul Krugman in his very successful international economics textbook speaks of pseudoinfant industries, claiming that some protected industries may have become competitive for reasons that have nothing to do with protection (Krugman et al, 2012, p. 188).

social world, we cannot expect enlightened economic policies if our approach to studying social and economic phenomena is unnecessarily narrow.

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