A Patchwork Cat
A few thoughts on the Phillips Curve

Christopher Willmore
1997

"The cat came back
The very next day
The cat came back
We thought he was a goner
But the cat came back
He just couldn't stay away."
-Traditional

The Phillips curve is the Frankenstein's monster of economic theory - a patchwork blend of many trends and models, brought to life by a freak flash of inspiration\(^1\) and built in the form and semblance of a proper model. For nearly forty years it has walked among us, its monumental stature and promise of powerful results ensuring its continued popularity among the economic community. That its predictions often fail to coincide with reality do not matter - no sooner does the creature fall under attack than its supporters rally to its side, adding new factors to its equations and conditions to its hypotheses until Phillips and Fact coincide, and the monster is free to rise and lumber on once more - for a time.

Clearly, this cannot go on indefinitely. There will come a point where we must all acknowledge that no matter how much it may look like a theory, that no matter how much we may wish this relationship to be as simple, as beautiful and as clear as we were told it was, even the most blind among us will have to conclude that a quilt of corpses does not make a man. A sickly tree brings forth bad fruit, and as J.K. Galbraith\(^2\) so eloquently put it, 'a theoretical argument that

\(^1\) And a driving need to fill the Keynesian gap for a theory of inflation
\(^2\) (Galbraith, 1997), p 94
rests on an atheoretic foundation is likely to run into trouble sooner or later’. There must be some limit to the amendments we may tack on to this relationship - the present 'expectations-augmented' Phillips curve, introduced by M. Friedman in 1968, bears little resemblance to the 1958 original and relies on variables so abstract, ephemeral and difficult to measure as to make it almost useless for any policy decisions, even assuming its essential validity. Still, the field cheers on, and many economists swear by the Curve with an almost-religious faith in its predictive abilities.

It is imperative to revise our thinking about the Phillips relationship. If we do not do so now, we will be forced to do so later, when the model collapses under its own weight. Too much time, money and mental effort have already been spent on binding the cuts and gashes on this homunculus, on stitching back on fallen limbs and sending it back out into the world. We must stop healing it when it returns and instead focus on coming up with genuine theories of unemployment and inflation, built from the ground up on solid theoretical grounds, and not on observed empirical trends.

**History**

If someone were to ask me, "What is the Phillips Curve?", I would be forced to answer them with a question: "In what year?" At times, the curve itself has been vertical, linear, curved, logarithmic, downward-sloping, upward-sloping, concave and convex. The labels on the axes of its graph have also varied, from change in nominal wages, change in real wages and inflation calculated in sundry ways on its y-axis, to unemployment, demand for labour and its proxies on the x-axis. It seems that so long as there is some steady relationship between a variable having to do with inflation and another having to do with unemployment, the Phillips curve is whatever it has to be in order to fit the data.

---

3 These only describes the shapes for the curve that arise from the different theories created to account for the empirical trend. Those derived from the raw data only show far more variation.
A. W. Phillips was the first to propose such a relationship in a 1958 paper on British economic history\textsuperscript{4}. In it, he suggested that for the period 1861-1957, the change in nominal wages in the United Kingdom was related to the level of unemployment and its first derivative.

Keynesians seized on the suggestion with the bestial ferocity of a pack of starving wolves. Their school lacked a theory of inflation, and the Phillips relationship appeared to fill that gap. With it (or a suitably formalized version) they felt their general model would be complete. The caveat in brackets is not a trivial one; Phillips' paper, though now a classic\textsuperscript{5}, and often described as seminal, was far from rigorous. As the author himself put in his final paragraph, his 'conclusions are of course tentative'\textsuperscript{6}. It is no wonder that many economists, particularly R.G. Lipsey in 1960, took it upon themselves to 'reconsider Phillips' work in some detail\textsuperscript{7} and 'treat the data by standard statistical methods if at all possible'\textsuperscript{8}.

In the paper from which the quotations are taken, Lipsey refined Phillips' original statistical approach and tried to build a theoretical basis for the observed curve, relating the percentage change in the wage rate to various powers and the first derivative of the unemployment rate, fitting the curve to the data by the least-squares method where needed. He found that though the exact nature of the relationship changed from year to year (and reversed itself for 1920-1950), high levels of inflation tended to be associated with low unemployment. With reservations, though he couldn't by himself provide a suitable model to explain all of the variations between the variables, he agreed that the Curve could serve as

\textsuperscript{4} (Phillips, 1958)
\textsuperscript{5} It was amusing, after going through several mint-condition volumes of bound economic journals, to find that the one containing the November 1958 \textit{Economica} was more-than-well-thumbed, and that in fact its highlight pages were nearly falling apart.
\textsuperscript{6} (Phillips, 1958), p 299
\textsuperscript{7} (Lipsey, 1960), p 1
\textsuperscript{8} \textit{Ibid}, p 1
an empirical basis for a theory of inflation by ‘suggest[ing] further hypotheses, the testing of which will in turn suggest further improvements in the theory.’

In their eagerness to take the Lipsey-Phillips suggestion as fact, the economic community at large was acting in an almost Friedmanish fashion nearly a decade before the future laureate would phrase those principles himself. The tentative results of the 1960 paper were taken as hard, solid fact. That there was no theoretical basis for the Phillips Curve didn't matter, nor did the fact that the relevant regression equations varied unpredictably over time; the only thing that mattered was that it worked.

Unfortunately, it didn't.

Attempts to apply Lipsey's model (or a host of other similar ones) were disastrous. Still the econometricians and theorists persevered, unwilling to toss out such a beautiful relationship. Regrettably, beauty is not always truth. Various attempts were made to patch up the scheme; a second derivative of the wage rate was added to the equations, as were productivity growth, proxies for the monopoly powers of unions, in fact, any variable that could possibly mend the relationship and restore the trend was introduced, but to no avail. Each of these new, improved Phillips Curves would break down when applied outside of the time and place for which they had been calculated.

Enter Milton Friedman. Phillips, Lipsey and others had already been emphatic in stating that unemployment was not necessarily a measure of excess supply of labour. They had realized that equality in the demand and supply of labour meant only that the number of job vacancies was equal to the number of workers available. Thus, even at the labour market equilibrium, there would be some frictional unemployment. Friedman refined this concept, distilling it into what he called the 'natural rate of unemployment'\textsuperscript{10}, which is now defined

\textsuperscript{9} Ibid, p 31
\textsuperscript{10} This has been further metamorphosed into the NAIRU, the 'Non-Accelerating Inflation Rate of Unemployment'.
simultaneously as the 'rate of unemployment consistent with an unchanging inflation rate', and 'the rate of unemployment that exists when the economy's output is at its full-employment [read: labour-market clearing] level'. (It is interesting to note that Stiglitz's definition assumes the validity of the Phillips relationship). The reason, Friedman stated, that it was impossible to fit a stable relationship between inflation and unemployment was that there wasn't one - it was constantly shifting. So far, so good. That is what the data had shown all along. Instead, he proposed proportionality between unexpected inflation and the difference between unemployment and the natural rate of unemployment. In his own words, 'there is no stable trade-off between inflation and unemployment; there is a 'natural rate of unemployment' ... unemployment can be kept below that level only by an accelerating inflation; or above it, only by an accelerating deflation'. Or, put algebraically (after Abel et al):

$$\pi - \pi^e = -h(u - U)$$

where $\pi$ = inflation, $\pi^e$ = expected inflation, $h$ = a positive number, $u$ = the unemployment rate and $U$ = the natural rate of unemployment.

Not only that, but in the long run, the Phillips effect will disappear, and the curve will be reduced to a vertical line at the natural rate of unemployment.

One can only imagine the applause that must have greeted the conclusion of that fateful address in 1968. Since then, the 'expectations-augmented' Phillips curve has been regarded with a quasi-religious awe, and volumes have been filled with attempts to estimate the natural rate of unemployment, and to use said estimates to forecast future economic activity.

---

11 (Stiglitz, 1997) p 1
12 (Abel et al., 1995) p 633
13 (Friedman, 1977) p 15
14 This phrasing of the relationship, found in Abel et al, p 507, is admittedly over-simplified, and is included merely for the purpose of illustrating the concept.
This model was seen to fit the data much better than its predecessor, and why wouldn't it? Its creator had failed to give the empirical definition of two of the key variables. Without a precise guideline on the calculation of expected inflation and the natural rate, it was inevitable that the resulting leeway in estimates of the two variables would lead to a better match with observations - this fallacy and many others will be dealt with in detail in the following section.

Objections to the Model

I. The Long Run

The Frankenstein analogy used earlier seems particularly apt in light of what Friedman's long run implies - a world of accommodating zombies which go about their business in a fully predictable fashion, never causing any shock that hasn't been already accounted for. "In the long run," Keynes once said, "we will all be dead," and indeed this seems the only explanation for this world devoid of any surprise!

I can already hear murmurs from the audience. "You are treating the model unfairly!" they cry. "Supply shocks are more than taken into account; no one ever said that the natural rate of unemployment was static! Full-output employment moves when it must; all that is being said is that in the long run, there is no misinformation, and expectations coincide with actuality. The natural rate becomes the reigning unemployment rate, but it is free to shift under the appropriate pressures\(^{15}\)."

To this I reply: "Yes, it is true that the long-run Phillips curve can be seen as that attained when \(\Pi\) is equal to \(\Pi^e\), but when with this be reached? Economists, who you will undoubtedly agree spend more time thinking about such matters than other people, have been working on the prediction of inflation for over a century. The very fact that this paper is being written is ample evidence that we are far from being able to forecast it with infinite precision. If a

\(^{15}\) More on the natural rate later.
hundred years of labour by a community of professionals has failed to culminate in a vocational consensus on the matter, how long will it be before the public at large is perfectly informed? A long run, indeed!"

There will always be surprises. Because Economics deals with willful, quirky, unpredictable people, it will never be reduced to a science of the sort that physics or biology have become; there will always be factors exogenous to the model which will nonetheless influence it. Oligopolies will form in secret, unions will rise and fight to their members' disadvantage, people will behave irrationally - even if Man were to suppress all his individuality, were the members of society as invariant as the atoms in a crystal lattice, still there would be unexpected change. Nature is well-known to supply floods and earthquakes when least expected. Because of the chaos inherent in humanity and nature, and because of the human incapacity for omniscience, the long run is not a useful concept. The world is best seen as a series of short-runs, each shock that arrives having another at its heels. There is never enough of a lull for anything close to Friedman's construct to be achieved; the best we can hope for is a sort of 'medium-run', a calm in between major shocks during which inflation changes at a more or less constant rate.

II. The Short Run

Once we have chosen to focus on the short run, we must decide: will it be u(Pi) or Pi(u)? The Phillips relationship has been used both to predict changes in inflation based on unemployment levels, and changes in unemployment levels based on inflation\textsuperscript{16}. Clearly, the variables are not that closely related - it must be one or the other. If both relationships held, then a change in u would produce a corresponding change in Pi, which would induce a further change in u and so on, a trend which is not seen in the data. Interchangeability of the two functions in

\textsuperscript{16} For instance, R.J. Gordon in the Journal of Economic Perspectives, Vol. 11, no 1, p 11, has Pi(u), whereas Friedman used u(Pi).
the standard Phillips curve (high Pi associated with low u) would result in a self-stabilizing spiral\textsuperscript{17}.

This leaves us with a true dilemma: which came first, the chicken or the egg? Without sound theoretical foundations, it is hard to tell from empirical trends alone (no matter how prettily formulated) why it is that high inflation seems to accompany low unemployment. Is it truly a primary relationship? Does a change in our independent variable (whichever we may choose it to be) de facto induce a change in the dependent one, or could it be a secondary effect at work? For instance, could it be argued that the programs used to lower unemployment in North America and Western Europe\textsuperscript{18} tend to create inflation, or that the disinflationary programs adopted by the same regions tend to raise unemployment? If so, why? What part, what mechanism in these policies causes the observed effects?

All the Phillips-based theory, from 1958 onwards, has been overwhelmingly based on trends found in the date from the developed countries. Even granting for the moment the validity of the Curve, there is great danger in trying to extract general principles from such a biased collection of observations. Might there not be something in the 'developed' mindset, in the first world's way of looking at economic problems, which causes the relationship? This would explain some of the more puzzling variations in the observed trends. Even in North America, the Phillips curve has infrequently shifted more than can be explained by changes in the natural rate of unemployment, and at times even reversed itself. Friedman himself spent the second half of his 1976 Nobel address attempting to tackle the problem of a disturbing trend he'd noticed, that of an upwards-sloping Phillips

\textsuperscript{17} With due consideration given to a proportionality of influence that is not one to one (the proportionality constant between Pi and u being 'h' in Abel et al's formulation).

\textsuperscript{18} It is no accident that I have singled out two geographic areas. In surveying the literature, I have been hard-pressed to find data for third-world countries, and so unable to back with fact my strong suspicion that the Phillips relationship does not hold even vaguely for developing countries. As a case in point, when I recently spoke on this subject with a friend of mine who lives in Mexico, he was genuinely startled at the thought that anyone would think of such a trend. "Why," he exclaimed, "over here we have always had high inflation and high unemployment at once!"
Curve. At the same time, this would make the empirical model useless where it was most needed - the emerging economies in the third world. The economic personality of its governments, vastly different from that of its G7 counterparts, would doubtless result in an equally distinct unemployment/inflation correlation.

Lacking a perfectly accurate and complete global database, it would be more satisfying to first create a theory and then check its predictions in the data than to eyeball a trend in imperfect data and go on to tailor a theory to it, without so much as knowledge of all the variables which it should contain.

III. The Variables

I have often heard it said that there are two things which one does not want to see being made: econometric estimates, and sausages. Few times has this proven truer than in the empirical determination of the expectations-augmented Phillips Curve and its components.

As mentioned above, when it was found that a constant natural rate of unemployment was not consistent with the data, the explanation seemed obvious - the NAIRU shifts with every incoming supply shock. Econometricians scrambled to derive the natural rate in many ways; I cannot be more specific, since each used his own method and inserted whatever variables provided a better match to the data and didn't fly in the face of common sense. These estimates would seem to work for a time, though with decreasing reliability. When it was clear that they no longer fit the model, it would be concluded that the NAIRU had shifted - a calculation would be made of a new, improved natural rate, incorporating some previously-ignored variable. This too, would work for a time, until another 'shift' was observed, when the hunt for an explanatory factor would be renewed.

As a group of disappointed researchers pointed out in 1975, '...the Phillips curve literature has been marked with problems in obtaining consistent, empirical
relationships, often reducing writers to appeals to "shift factors" outside the theory when the theory does not work.\textsuperscript{19}

It is chiefly on this twenty-twenty hindsight that the reputation of Friedman's Phillips Curve rests. Given that the natural rate is impossible to measure directly, and that no consensus has been reached on how to measure it indirectly, it is all too easy to find exogenous factors to account for the discrepancy between 'theory' and reality. The NAIRU works because it is made to work, and is of itself unusable in forecasting.

J.K. Galbraith\textsuperscript{20} puts it succinctly (Galbraith, p 101):

As in the past, the present estimates and reestimates [of the natural rate] seem largely a response to predictive failure ... we still have no theory, and no external evidence, governing the fall of the estimated NAIRU. We simply observe that inflation hasn't occurred, and so the previous estimate must have been too high. ... Differences of specification are also normal in the early stages of scientific inquiry. But to hold a concept in the face of 20 years of unexplained variation and failure of the profession to coalesce on procedural issues is quite another matter. This record has become an embarrassment to the reputation of the profession.

The overly ephemeral nature of the NAIRU makes it unsuitable for policy-setting. Without a sound theoretical backbone, we are forever unsure of what its exact location is, and since we do not agree on what complete set of factors go into its making, we can seldom measure its shifts until after the fact, through observation of data and an assumption of the validity of Friedman's curve. That in itself is alarming - the inflation and unemployment rates are being used to predict the NAIRU, which is in turn being used to predict the inflation and unemployment rates. Circular logic has seldom led to useful results.

\textsuperscript{19} (Stokes et al, 1975) p 16
\textsuperscript{20} The problems associated with the calculation of the NAIRU are too numerous and complicated to be dealt with at any length in this short essay. Instead, readers are encouraged to peruse the excellent critique of the concept by J. K. Galbraith, "Time to Ditch the NAIRU," listed in the references. In it he deals carefully with the theoretical and empirical issues involved, and evaluates the usefulness of the NAIRU concept as a whole.
My final point is one which I am surprised has not been seized upon with more vigour. The left-hand-side of Friedman's model depends fully upon unexpected inflation, defined as expected minus actual inflation, and yet all work so far has seemed to ignore the fact that expected inflation is not empirically measurable. A good example is found in the work of Stokes's group. Though they acknowledged early on the existence of the expectations-augmented Phillips curve, they then proceeded to completely ignore it in their analysis of regional labour markets, presumably because of their inability to measure the crucial quantities.

To model the expected inflation for one person alone is difficult enough; to try to do so for an entire economy is daunting. Each individual will have her own thoughts on the economy, many of them perversely illogical, and these, coupled with a varying interest in monetary matters, will lead to a wide range of values for $\Pi^e$. The straightforward approach to obtaining an aggregate expected inflation would be to poll the populace - but who should be polled? Heads of firms? Managers? Labourers? Once that had been done, would the resulting number be a true measure of Friedman's $\Pi^e$? If so, would its structure change with time? Would publishing or implementing in policy the results of the polls affect the result of the next polls? Each of these questions leads to a dozen others, all worthy of proper research.

Most economists have bypassed these complications, choosing instead some non-controversial proxy for expected inflation. The most popular seems to be the use of $\Pi$ itself as the unexpected inflation, which assumes a constant expected inflation of $2^*\Pi$. (Interestingly enough, if a stable relationship were indeed found between $\Pi$ and $(u - U)$, it would fly in the face of Friedman's assertion that 'only surprises matter'.) Others have gone a step further, and tried to model the expectations of the populace as a whole. In figures one an two I have illustrated two such attempts, using data from Statistics Canada's CANSIM database. I have taken what I believe to be the two simplest, most commonplace expectation proxies, and plotted the difference between them and actual inflation.
versus the seasonally adjusted unemployment rate (SAUR)\textsuperscript{21}. In figure one, I have taken the expected inflation to be the average of this month's and last month's inflation, while in figure two I take each month's $\Pi^e$ to be the average of the inflation over the previous twelve months. A sixth-degree polynomial has been fitted to the inflation plots merely to help guide the eye.

[Figures missing]

It is obvious at a glance that the relationships are quite different in both magnitude and (often) direction. The freedom in simulation of the variable is large enough that a suitable model of expected inflation can in the absence of any other correcting factors be used as a wild card to make the Phillips Curve fit any empirical data.

**Conclusions**

The Phillips relationship should never have been taken as anything more than a rule of thumb. Lipsey's early failure to come up with a plausible theoretical basis for it\textsuperscript{22} should have served as a warning sign, as should have the repeated failures of the model, in any incarnation, to conform consistently to data which varied temporally or geographically. A chaotic correlation is no correlation, and useless for forecasting or application to policy. Friedman himself acknowledged that the Phillips Curve was unsatisfactory, but rather than calling for its dismissal, he added to it 'unexpected inflation' and the natural rate of unemployment. These patches, compelling when first presented, proved nearly impossible to measure empirically, or to determine their exact composition. As a result, hindsight in remembering a forgotten variable could make this expectations-augmented curve fit almost any data (as could different estimates of the expectations in question), both bettering its track record and decreasing its applicability in the real world.

\textsuperscript{21} There seems to be some justification for using $u$ as a proxy for $(u - U)$. R. J. Gordon, on page 38 of the Winter 1997 issue of the Journal of Economic Perspectives, comments on how a NAIRU arbitrarily set at 6\% worked remarkably well for the entire post-1978 period.

\textsuperscript{22} 'Thus, although it might be held with a high degree of confidence that a significant and very interesting relationship had been discovered, a very low degree of confidence might be attached
As it stands, Friedman's curve cannot be disproved, but neither can it be proven. It can be shown to work for some sets of data, and the remaining discrepancies explained away with variables which are not yet recognised as relevant, but until we have a complete theoretical basis for the relationship between unemployment and inflation, we will not be able to truly comment on its validity.

It was once written of Friedman\textsuperscript{23} that

\textit{While granting that the assumptions of economic theory are false, he denied any significant resulting handicap. In his view, assumptions must simply work, that is, yield reliable results. Whether or not assumptions correspond with the facts is - with a few reservations - without interest.}

It is true that if we are to be merely human calculators, it makes no difference. For theorists, however, this approach is problematic. A theory that is based on unreasonable assumptions may work in its own bailiwick, but it cannot in general be used to build upon for the creation of a larger scheme - a logical system is only as good as its axioms.

Simply put, the Phillips Curve has been a false lead, a 'siren song of the gods\textsuperscript{24} that with its deadly melody has drawn us away from genuine progress on valid theories of unemployment and inflation. Each time that we've been on the brink of tossing it out, it has returned to woo us, tempting us with the easy option of statistical analysis. If price and labour theory are to evolve past their present state, we must stop trying to cram this guideline into a framework which is obviously an imperfect fit. We must drown the cat.

\begin{flushright}
\textit{The author would like to thank L. N. Willmore and Professor G. W. Smith for their support and help in suggesting source materials for this essay.}
\end{flushright}

\textit{at this stage to a particular estimate of parameters}' (Lipsey, 1960 - italics in the original) was his own euphemistic assessment of his results.


\textsuperscript{24} (Galbraith, 1997) p107
References


