ON THE PROPERTIES TYPICAL OF ECONOMIC TIME SERIES

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This paper summarizes the results of econometric time-series analyses performed by the author and colleagues over the last seven years, using the Box-Jenkins Approach in interaction with Economic Theory. Typical univariate properties, typical data anomalies and typical relationships are described. Common practice in Econometrics is criticized and certain aspects of Economic Theory are discussed.

1. INTRODUCTION.

It was with ill-defined feelings of excitement, trepidation and curiosity that I traveled to Lancaster in November 1977 for my first meeting with Gwilym Jenkins. I had reason to think that this encounter might open up new possibilities for my own development, but I was certainly filled with anticipations anxiety. Previous experience had taught me that most famous men are, to put it lightly, disappointingly all-too-normal human beings. Fame seems often to arise from a confusion of overall ability with a rapid and specialized mind neatly connected to a fast mouth. There had, however, been a few outstanding exceptions in my experience and, as often happens if we pay attention, these anomalous cases had made all the difference in the world. These were men I could aspire to emulate and each of them had left a deep and lasting impression on me. Gwilym Jenkins, as it turned out, was to be one of these very rare exceptions, an extraordinary scientist, teacher and human being, and finally, an absorbing enigma, a man with one foot in this world and the other firmly planted on the far side of that mystery we call death.

The territory covered by my own memory of Gwilym Jenkins is, as I am sure it is for all those who knew him, vast, beautiful, apparently chaotic and essentially unfathomable. To make a map of it would be an overwhelming, if not impossible, task. To confuse such a map with the territory itself would constitute an unpardonable stupidity. This paper, therefore, can offer but a mere token to that memory.

My first meeting with Gwilym Jenkins led inevitably to a series of later meetings. Over the two years previous to it, I had worked intensively, but on my own, to learn to use the Box-Jenkins approach to time-series analysis in practical econometric model-building, but had come to the recognition that expert guidance was needed in order for me to achieve minimal competence in this endeavor. We spent the better part of three month, on different occasions in 1977-1979, in virtually daily interaction. Over that period he closely supervised almost all my model-building activity. His influence has dominated all econometric work that I have done since. The main objective of this paper is to give some account, even if fragmentary, of the resulting research, none of which has yet been published in English, much of which has not yet been - published even in Spanish.

1.1. THE BOX-JENKINS APPROACH

The Box-Jenkins (B-J) approach is, I believe,

a peculiarly modern scientific methodology.
After pioneering work in the nineteen-six-
ties, G.E.P.Box and G.M.Jenkins gave a monu
mental initial description of the approach
in Box and Jenkins /8/. This has been signi
ficantly expanded in many later works by
them and by others; for a selection of a few
key works and expositions, which will also
facilitate access to the rest of this now
immense literature, see Box and Tiao /9/, -
Jenkins /20/, Box and Tiao /10/, Jenkins -
/22/, Tiao and Box /32/, Alavi and Jenkins
/2/ and Jenkins and McLeod /23/. In my view
at least, work since 1970 has expanded the
detailed content of the methodology, but has
not modified in any essential way the syste-
matic prescriptions that make the approach a
methodology and not merely a set of new
techniques.

That the B-J approach is a methodology and
not merely a new set of techniques, that the
approach is still undergoing active detailed
development and that this methodology re-
quires that the user acquire an unusual re-
search psychology, appear to have made -
effective assimilation of the approach by -
substantive disciplines such as econometrics
a fairly difficult task.

Special emphasis on the requirement for an
unusual psychology is not, I believe, mis-
placed. For example, a central systematic
prescription of the approach is the interac
tive theory-practice iterative process; see
Box /5/, /6/ in addition to references cited
above. To intellectually accept this prin
ciple is easy enough, but to make it an ope-
rational characteristic of real model build-
ing is no mean task, because this entails
the development of a special capacity for
the conscious self-control of the model-
builder's attention. Another example arises
in diagnostic activity, where a set of tools
has been and is being developed that admits
a more or less routine application, but
where high-quality model-building requires
one to "expect the unexpected" so to speak;
this also requires special qualities of the
model-builder's attention. I suspect that it
is this need for psychological change that
makes experience under expert guidance es-
pecially necessary in the case of the B-J
approach. Of course, this should not be re-
garded as a weakness of the approach, but as

a challenge to be met.

1.2. COMMON PRACTICE IN ECONOMETRICS.

That common econometric practice has not yet
come close to assimilating the B-J approach
does not strike me as an exaggeration. A few
isolated techniques have come into more com-
mon use, but the approach itself, as an inte
grated whole, has been taken seriously only
by a small handful of notable econometricians,
a minority within the elite of that disci-
pline; foremost among this minority, Granger
and Newbold /17/ and Zellner /37/, /38/
should be cited, the specific works mention-
ed facilitating access to others by the same
authors and by other econometricians. Econo-
metric model-building on the whole continues:
(1) to impose specific a priori representa-
tions radically at odds with the properties
typical of economic time-series, (2) to be
driven almost exclusively by (often mis-
guided) notions of economic theory, (3) to
ignore data-based specification procedures,
(4) to ignore most statistical diagnostic
procedures and (5) to indulge in general me-
thodological confusions such as that which
mistakenly regards size and complexity of
models as necessary for scientifically good
description. Gwilym Jenkins commented aptly
on common practice in econometrics on at
least two recent occasions; see Jenkins /21/
and /22/, Part IV. I largely agree both with
his criticisms and his suggestions.

The only review of Jenkins /22/ that I have
seen in the econometric literature is that
by Chow /11/. Generally receptive to the B-J
approach, this author nevertheless repeats
several arguments frequently found among eco-
nometricians and, in my opinion, misguided.
First, it is suggested that B-J involves
much so-called "data mining", certainly one
of the sins in model-building that neither
Jenkins nor any other competent B-J analyst
is guilty of in anything like the degree
common in conventional econometrics. The
competent B-J analyst generally has to esti-
mate relatively few models precisely because
he: (1) takes the most obvious features of
the data into account from the outset and
(2) does not permit questionable overelabo-
rations of economic theory to introduce large
numbers of essentially irrelevant alternative

models. Second, large models (with several hundred variables at least) are claimed to be justified by the nature of general-equilibrium theory in economics and by the fact that there are paying clients who supposedly demand them. There is nothing in general-equilibrium theory asserting such a need for large numbers of variables; that is simply a misreading of economic theory. As for the presence of paying clients for such models, that hardly seems a sufficient justification for a scientific methodology, though it might be for a confidence racket. Third, credence is given to the idea that model specification should <u>start</u> from the so-called "structural" form based on economic theory when, in fact, common specifications are generally about as related to convincing economic theory as the "man in the moon" is to astrophysical theory.

Aside from the fact that no theory is ever sacrosanct, economic theory is incapable of supplying convincing dynamic or stochastic specifications except in very rare cases and is barely able to offer a few fairly weak suggestions on steady-state properties of economic systems. Some prestigious economists, e.g. Gordon, Klein, Ando and others who thus express themselves in the Sims volume /29/, evidently think otherwise, but they have not been able to convince others who are not precisely novices in economic theory. For example, Gordon's attempt, in the cited volume, to describe what he regards as consensus macroeconomic theory, constitutes a pathetic example that merely lends weight to the skepticism expressed in the same volume by Sargent, Sims and others, a skepticism which I share.

One of the more mystifying features of the methodological discussion in econometrics is that it is almost exclusively devoted to rather complex issues in multivariate-stochastic (or so-called simultaneous equations) model-building, when the vast majority of published practical econometric work involves much simpler single-output relationships . Even conventional practice has, in fact, always involved going from simpler to more elaborate systems; most large-scale econometric models were formed by combining many single-equation regression models in some way. That the conventional single-equation

research is methodologically defective cannot be ignored: static regression methods extended by the hodge-podge of special techniques typical of econometric practice simply are no match for the corresponding univariate stochastic and single-output transfer-function-noise methodologies of the B-J approach. No matter how one combines simple models to get a complex model, if the simple models are bad, it is likely that the complex model will be also.

As an example, consider the econometric literature on energy demands, a politically hot issue over the last decade. See Taylor /30/, /31/ for surveys. There are two main classes of work in this literature: (1) many single-output models and (2) a few multiple-output models. The single-output work does not use B-J, but uses static regression analysis -- plus the common econometric extensions, and imposes utterly <u>ad hoc</u> specifications, supposedly based on convincing economic theory, but in fact based on famous "arm-waving" exercises such as those by Koyck /24/, Balestra and Nerlove /3/ or Houthakker and Taylor /18/. A competent B-J analyst can appreciate at a glance that these models are statistically inadequate in most cases: levels of series are taken as mean-stationary, noise and transfer structures are forced to have common autoregressive parts and residuals, when published, have trends or wander. In a recent study, García-Pardo /14/ ilustrates in this area how economically sensible and statistically adequate models can be obtained using B-J. As a consequence of extensive work on energy demands for Spain, in collaboration with Garcia-Pardo and D.Peña, I have found that scale elasticities of these demands are much lower than those of conventional econometric studies; well-defined negative price elasticities have also been found in many cases. Our data is similar to that used in conventional econometrics and we have stuck to single-output models, based on similar assumptions as to the absence of feedback, but the way we build models is very different and so are the results.

The multiple-output models mentioned above are even more hallucinatory than the single-output models. See Fuss et al./13/, Berndt and Wood /4/ and Pindyck /28/ for a representative

sample. Joint systems of energy and other de-
mand equations are postulated and much atten-
tion is lent to "testing" restrictions based
on static-optimization individual-behavior
theory: homogeneity, symmetry, adding-up, se-
parability. This is, in a sense, admirable;
such work uses almost the only theory there
is. But even this theory is extremely flimsy;
why static optimization should be plausible
is open to doubt and aggregation is known to
potentially destroy most of these properties.
But the most devastating feature of this work
is that all transfer functions are specified
a priori to involve immediate response (i.e.
the static aspect of the theory is taken li-
terally) and error processes are assumed mean-
stationary or even white, an assumption that
is very dubious given properties typical of -
economic series.

Note that the systematic errors in methodology
found in this specific literature on energy de
mands are far from irrelevant. They may well
have led to disastrous misjudgements in many
national energy policies over the last decade.

1.3. RESEARCH TO BE SURVEYED AND ORGANIZATION OF THE PAPER.

The volume of econometric analyses that I and
collaborators have carried out with the B-J
approach since 1977 and the diversity of eco-
nomic questions treated are sufficient to make
it impossible here to offer more than a very
sketchy summary. I have chosen the properties
typical of economic time-series as title for
the paper, because these properties permit a
minimal organization and selection of findings
and also because I feel that ignorance of many
of these properties is one of the things hold-
ing up progress. Of course, no amount of know-
ledge about what is typical can substitute for
the competent use of a good methodology; there
are always atypical cases lurking around at
the fringes of our knowledge and not infre
quently these are valuable invitations to
breach the limits of that knowledge, invita-
tions that should never be ignored.

The properties typical of economic series
cannot be regarded as a new theme. Granger
/16/, e.g., considered estimated power spec-
tra of such series and found a typical shape,

noting the "overpowering importance of low
frequency components". He cites other early
work on typical properties. It is also rele-
vant to recognize that some belief on the
matter of typical properties is at least im-
plicit in any model-building approach, since
the forms thought to be typical must be con-
tained in its most general representation.
Thus practice common in econometrics today,
e.g., usually assumes implicitly that the le
vels of economic series are mean-stationary,
at least after accounting for deterministic
trend components; stochastic nonstationarity
is not even entertained. The generality of
representation allowed by a methodology is
one area in which the rule is that "What is
not allowed is forbidden". The novel feature
of this paper is that the most general repre
sentation allowed, within any model class
(univariate stochastic (US), single-output
transfer-function-noise (UT), multivariate
stochastic (MS), multiple-output transfer-
function-noise (MT)), is that explicitly
allowed by the B-J approach, and this is in
general much richer than representational
forms commonly allowed in econometrics.

The need for higher reporting standards for
time-series analyses than those now common
is, I think, evident. On this occasion how-
ever, there seems to be no choice but to vio
late my own position on this: the ground to
be covered is too broad to admit detailed re
porting and therefore there is no pretense
here of presenting the evidence that backs
up the generalizations offered. However, I
will briefly list the projects involved and
cite what published reports there are; many
other unpublished reports are available from
me on request.

The Fundación Ramon Areces financed a study
of monthly series of foreign exchange re-
serves in relation to the exchange rate; see
Treadway et.al./36/ and this is described
briefly in Jenkins and McLeod /23/. The Fon-
do para la Investigación Económica y Social
of the Confederación Española de Cajas de
Ahorros financed a study of Spanish inflation,
involving analyses of monthly, quarterly and
annual time series of the Consumer Price Index, a
nominal wage index and the M3-type money
stock; the relationship between the price in-
dex and the money stock is analyzed in Tread-

way /35/ and a book-length report was prepar-
ed but not published. This same line of work
has been greatly expanded under a new grant
from the Fundación Ramón Areces and a book
will be published on it in the near future:
quarterly analyses of the Consumer Price In-
dex, Industrial Production Index, registered
unemployment, a nominal wage index, the M3-
type money stock and several other macroeco-
nomic series were carried out. The full
range of model types, from US to MT, comes
into play.

The Ministerio de Economía financed US and
intervention analyses of nearly 100 monthly
and quarterly disaggretate series relating to
real activity levels: industrial outputs, com-
ponents of exports and imports, labor market
series and miscellaneous business indicators.
Growing out of this work, a doctoral thesis
is nearing completion on the methods appro-
priate for interpolating missing values,dis-
tributing semester aggregates over quarters
(at several times in an otherwise quarterly
series when the sample survey involved was
made only for the semester) and evaluating
changes in definition while respecting under
lying stochastic structure in a series; this
is an important practical problem in Spain,
because the main source of data on labor is
affected by these inhomogeneities.

The Ministerio de Hacienda financed a study
of several monthly series of tax receipts.

Besides the monthly analyses of demands for
industrial fuel-oil, fuel-oil for thermal-
electric plants and aggregate electricity,
related to the energy prices, the Industrial
Production Index, a rainfall index and the
stock of water available for hydroelectric
generation, found in García-Pardo /14/, D.
Peña and I have done monthly analyses of
gasoline sales and price, and several other
monthly energy demand analyses have been
carried out. The Ministerio de la Presiden-
cia financed a study of 18 energy demands in
relation to prices and different scale varia
bles, based on short annual series.

Recently the Ministerio de Economía y Hacien
da financed a study of short annual series
of employment, production and real wage.

A doctoral thesis by D. Grandal is nearing
completion involving monthly and quarterly
analyses of quantity, price and other varia-
bles in agricultural markets. This is related
to expectations formation and involves multi-
variate stochastic analyses.

Besides the work mentioned so far, there have
been studies of monthly sales of a large de-
partment store chain, monthly movements in
the deposits of a private bank and others
involving data of individual firms.

The experience on which I generalize below
is limited to monthly, quarterly and (usual-
ly short) annual time-series. I do not pre-
tend to have anything to say on series with
sampling intervals of less than a month or
more than a year, or on series with more than
one seasonal period. In most cases, record
length is 10 years or more, in a few cases
monthly series of seven or eight years are
involved. By short annual series I refer to
14-18 years of data in practice.

In this paper I will make use of conventions
in notation and terminology of the B-J ap-
proach without repeating definitions. Jenkins
/22/, pp. 95-123 and pp. 127-146, should be
consulted in case of doubt.

The paper consists of four further sections.
Section 2 discusses typical univariate proper
ties, section 3 typical data anomalies and
section 4 typical relationship properties.
Section 4 concludes with some comments on
economic theory.

2. TYPICAL UNIVARIATE PROPERTIES.

Univariate stochastic models are useful in
forecasting and are also built for each time-
series potentially to be included in more com
plex models, because they often help uncover
data anomalies, are useful in prewhitening
for certain relationship-specification opera-
tions, can be used to initially specify error
structures in certain relationship models and
often lead to an early and direct increase in
understanding of the system under study.
Starting with Nelson /26/, chap. 8, US models
have been frequently shown to outperform com-
plex large-scale econometric models in fore-

casting. It is not infrequently the case in
P-J analyses that one finds it extremely dif
ficult to find other time-series the explicit
consideration of which actually improves the
forecasting performance of a good US model.

2.1. NONLINEAR INSTANTANEOUS TRANSFORMATION

Economic time-series virtually always require
the logarithmic transformation when the Box
and Cox /7/ family of transformations is con-
sidered and when sufficient data with suffi-
cient variation in level is available to stu-
dy the matter carefully. The only exceptions
met with in my experience are interest rates,
which probably are best modeled in the loga-
rithm of the interest rate plus one, and cer-
tain indicator series based on surveys of
business opinion. The latter constitute an
open issue.

2.2. DIFFERENCING AND DETERMINISTIC COMPONENTS

All economic series that I have met need at least
one difference to achieve mean-stationarity.
Except in the case of short annual series,
most need two differences; interest rates and
price quotations on (apparently efficient) ex
changes (e.g., London metals market) are an
exception and usually need but one difference.
In the case of monthly or quarterly data of
aggregate price indices or stocks (i.e. varia
bles with no time unit in their dimensions),
three differences are often needed to obtain
robust forecasting models though two may do
for other purposes. One of the differences
almost always needed for monthly and quarter-
ly series is the seasonal difference. Except
for short annual series I have never found
a non-zero mean parameter μ to be needed nor
have I detected deterministic seasonal struc
tures.

Short annual series form a very special case,
because the data is so scanty that one may
well be forced into expedient solutions that
do not reflect typical properties. It is
often necessary in such cases to use but one
difference and to include $\mu \neq 0$; that is,
one must accept a deterministic slope compo-
nent along with stochastic level and slope -
components. It is sometimes possible to use

two differences and have a zero mean parame-
ter and I regard this as desirable whenever
feasible because it matches up with proper-
ties known to be typical of monthly and
quarterly series. Modeling short annual se-
ries is, however, a very "artistic" activity
and not central to this paper.

The general properties given in the first pa
ragraph are somewhat more extreme even than
those common in the B-J literature; many may
question if I am not often actually overdif-
ferencing. This is a fairly subtle but, I
think, highly relevant matter that needs
some qualifying comments.

Let it be clear that I am not talking about
differencing decisions based merely on ini-
tial identification, but on efficiently es-
timated US models statistically adequate
within the limits of B-J diagnostic tools
available. Initial identification not infre
quently leaves the exact degree of differenc
ing needed in some doubt, usually as between
two options such as ∇ versus $\nabla\nabla_s$ or $\nabla\nabla_s$ ver
sus $\nabla^2\nabla_s$. The potentially overdifferenced --
case will usually carry with it a partially
compensating MA(1); on estimation, near non-
invertibility of this may signal a need for
one difference less. The potentially under-
differenced version will usually carry with
it some AR structure; on estimation, near
non-stationarity can signal a need for an-
other difference.

All US models involved in generalizations -
presented here were estimated with the Gwi-
lym Jenkins and Partners, Ltd. USES program,
which uses the approximation to maximum like
lihood based on minimum sum-of-squares with
backforecasting. Bias in estimated MA(1) pa
rameters should then be toward the non-inver
tibility boundaries and not away from them;
see Alavi and Jenkins /2/, section 5.1.3, on
this.

It should be emphasized that the B-J approach
consciously admits deterministic trend and
seasonal structures. See Abraham and Box /1/
for a useful discussion. Even the equation
regressing the level of a variable on the
time index is admitted, this being signalled
by a non-invertible IMA(1,1) form with $\mu \neq 0$;
however, this has never arisen as an adequate

description for economic series in my experience.

Though the B-J approach admits deterministic forms, cases do arise in which, even after the complete model-building process, two different options on differencing continue to be acceptable, that is, neither the MA(1) in the potentially overdifferenced model appears non-invertible nor does the AR in the potentially underdifferenced model appear non-stationary. In most such cases, neither option involves a deterministic component either, e.g. when the options are $\nabla\nabla_s$ and $\nabla^2\nabla_s$ and the two models are very similar; in such cases one can use the more differenced version for forecasting, because it is more adaptive, and either of the two will usually do for other purposes. In a very few cases, i.e. when the options are ∇ and $\nabla\nabla_s$, a deterministic seasonal is implied by the less differenced form; detailed examination has never led me to use the less-differenced -- form in such cases: often I have found some previously overlooked data anomaly which, when treated explicitly, led to a clear preference for the more differenced form.

In general my work does not involve officially deseasonalized series. In a few cases, we have been asked to analyze such series and then we have sometimes needed to use a single difference and a non-zero mean parameter. I suspect that inappropriate deseasonalization is behind this.

2.3. STATIONARY ARMA STRUCTURES.

More variety arises in the ARMA structures, but there are certain forms that occur frequently and in certain contexts and others that almost never occur.

Certainly the most frequent forms are MA(1) with positive parameter. Such forms appear in partial compensation of differences and lead to exponentially-weighted-moving-average forecast functions. The IMA(1,1) structure is indeed quite frequent, both in regular and seasonal parts of models.

The AR(2) form with complex roots and period of around three arises in certain contexts with high frequency. The annual operator

where the period is in years is very typical of aggregate series, especially real or nominal production series for the whole economy or large sectors of it like the industrial sector. When this operator appears in the analysis of a very disaggregate series, e.g. gasoline sales, this can suggest dependence on some measure of the state of the aggregate economy. The monthly AR(2) with complex roots and period of three months often signals the presence of effective inhomogeneity due to the variations in number of working days per month, an hypothesis that can then be explicitly checked. This same operator also often signals the relevance of some kind of quarterly payments process in bank deposit data and tax-receipt data, e.g.; one can usually find reasons for this such as laws requiring quarterly clearance of firms' income-tax or social-security liabilities.

Higher than first-order MA terms seem to be very rare. Higher than second-order AR terms also arise infrequently. Real AR terms do occur sometimes, but are usually of low order; they seem to be substantially less frequent than complex AR terms.

3. TYPICAL ANOMALIES.

One of the areas in which practices common in econometrics are clearly very deficient is in the detection and treatment of anomalous data. A competent B-J US analysis very frequently reveals data abnormalities and paying attention to them often yields high pay-offs in terms of: (1) reducing errors in data, (2) improving forecast precision and effectiveness in monitoring, (3) detecting relationships and (4) reducing distortions in identification tools and estimated model parameters. Most such anomalies are not obvious when one looks at the levels of economic series, but become more obvious as the series is differenced and modeled. Sometimes they are so large that they even foul the US analysis itself by distorting analysis tools.

The analyst must keep a sharp eye out for anomalous data from the start. Once detected, he can then ask the data what form of anomaly appears to be involved and this can help in his later search for explanations –

27

from outside the sample. Since there are four
very common kinds of anomalies in economic
series, these are treated here. Of course,
other kinds can certainly arise, too. For
this section, we are not considering annual
or quarterly data in most cases; detection
of anomalies is easiest in monthly data.

3.1. STEPS IN LEVEL.

A step in the level of a series can arise from
a change in definition or in methods of cons-
tructing the series. For example, peseta
foreign reserve stocks in Spain were revalued
at the time of the November 1967 devaluation
of the peseta; this was not very well known
to economists using the series until it was
uncovered by a US analysis; see Treadway et
al./36/, chap.3, where data graphs are pre
sented. A step in level can also arise when
some relevant input variable takes a step
change; in this case the anomaly can signal
a relationship. For example, in Spain gaso--
line price is administered by the state and
is changed infrequently; when it is raised,
especially when the rise is large, the reduc-
tion in gasoline sales can be detected and
it of course involves a step-in-level type
of anomaly. Changes in law also generate step
changes in level in series affected by them.

When a series with a step change is subjected
to a regular differencing operation, the ano-
maly appears as an impulse. Two regular dif-
ferences will leave a sequence of two im
pulses of opposite sign and of about the same
size. A seasonal difference operation applied
to either pattern will merely repeat the im-
pulse configuration one year later with signs
changed. Thus detection can often be achieved
at the initial US identification stage, if
the analyst is awake to the possibility and
if the step is large enough.

The step-on-level anomaly will distort identi
fication and diagnosis instruments in diffe--
rent ways and may also distort estimated US
parameters. For example, if one regular and
one seasonal difference are being used, then
the sample autocorrelation function (acf of
$\nabla\nabla_{12}\ln Z_t$) will involve a negatively distorted
value at lag 12, an estimated $MA(1)_{12}$ para-
meter will be distorted upward; if two regu-
lar and one seasonal difference are involved,

then the acf will have considerable negative
distortion at lags 1 and 12, a somewhat smal-
ler positive distortion at lags 11 and 13 and
$MA(1) \times MA(1)_{12}$ parameters will be distorted up
ward at estimation.

When this kind of anomaly is detected and
when it is clearly due to a change in defini-
tion or is not understood, the analyst can
consult statisticians responsible for cons-
tructing the series. Sometimes they can sup-
ply a quantitative value to be used in
correcting the data; not infrequently, on
receipt of such information, the analyst
can ask them if similar operations were per-
formed at other dates in the series. This
can often lead to a whole set of data correc
tions even when other definitional changes
had not been detected in the US analysis.
This, in fact, occurred in the foreign-ex
change-reserves case. There are other cases
in which all one can learn is that there was
a change in definition and perhaps the expect
ed sign of the effect; in such cases, an in-
tervention analysis can be performed to esti-
mate the effect and use it to correct the -
data: the deterministic input is a unit step
at the date in question and the transfer
function is but a constant (s=0, r=0, b=0).

When the effects of an input variable have
been detected in a data anomaly, the analyst
is then best advised to seek out the data on
the input variable and build a single-output
transfer-function-noise (UT) model, perhaps
of one of the decomposed forms; see Jenkins
and McLeod /23/, chap. 7.

3.2. IMPULSES IN LEVEL

Impulses in level are particularly frequent
and important, because this is the form typi
cal of human errors in data transcription.
These often arise in official publications
amd also in keying data into a computer. Im-
pulses in level can also signal the rele-
vance of anomalous external events, e.g. la-
bor conflicts in industrial production se--
ries or an abnormally cold month in heating-
fuel demands; they can also suggest an input
variable if one knows of a matching impulse
in some candidate for input.

When an impulse-in-level is regularly dif-

ferenced once, the resulting series has a
sequence of two impulses of opposite sign
and of about the same size starting at the
same date. Two regular differences will ge-
nerate a sequence of three impulses, the -
first and last of the same sign and of about
the same size, the second of opposite sign
and of about twice the size of the others.
A seasonal difference applied to either of
these configurations merely duplicates it
one year later with the signs changed. The
distorting effects of one such anomaly can
therefore be quite relevant in practice.For
example, if two regular and one seasonal
difference are being used, the sample acf
will have powerful negative distortions at
lags 1 and 12, large positive distortions
at lags 2, 11 and 13 and weaker negative --
distortions at lags 10 and 14. An analyst
who only looks at the acf, but ignores the
data graphs, can easily fall for the pre-
sence of a regular AR(1) with negative para
meter in such cases as well as detecting
MA(1) and MA(1)$_{12}$ terms. On estimation, the
moving-average parameters will be distorted
upward and this AR(1) may well seem signi-
ficant, though one bad data value is all
that is involved.

When impulse-in-level anomalies are detected,
the first action is to check for errors in
transcription, one's own first and then
those of the source. If none is found, then
one has to contemplate the question as to
what extreme external event is being reflect
ed. This is a point at which wise recourse to
economic theory, common sense or hard work
on publications relating to institutions and
their crises can pay off.

3.3. COMPENSATING IMPULSES IN LEVEL.

By compensating impulses I mean a sequence
of two impulses of opposite sign; often they
are of similar magnitude. These are impor-
tant, because they can reveal a frequent
kind of heterogeneity in the sampling inter
val. For example, in December 1976 Spanish
customs Registers were closed on the 31st
day rather than on the 25th day, which was
the standard. This produced a compensating
impulse effect, positive in 12/76 and
negative in 1/77, in every foreign trade

series, disaggregate and aggregate, measured
in money terms or real terms. Another example
involved the monthly register of gasoline
sales, which was closed several days early
in July 1973, the remaining sales being re-
gistered in the following month's account;
this was of special relevance, because there
was a price change in July 1973 also and it
was important to get this accounting error
out of the data before trying to measure the
price effect.

When one differences a series with compen-
sating impulses in level, distortions build
up even more rapidly than in the previous
cases discussed.

Sometimes those responsible for constructing
the time-series can correct an error of this
kind; this occurred, for example, in the
case of gasoline sales, because daily sales
records could be accessed and the sums recom
puted. When this is not possible, as in the
case of the customs data, recourse to in-
tervention analysis is indicated: if the va-
riable is being analyzed in logarithms, then
the input can be set to $z_{t_1}^{-1}$ in the first --
month (t_1) and to $-z_{t_2}^{-1}$ in the second month
(t_2), with zeros at all other dates, and the
transfer function is merely a constant,
which will be in units of Z.

3.4. EASTER EFFECTS.

Many real production, consumption or sales
and tourism-related series show Easter ef-
fects and approriate modeling of them can
help clean up seasonal structure as well as
reduce unexplained variance. The critical
point is that Easter is a holiday that usual
ly falls in April, but sometimes falls in
March. Detection of this effect is easiest
in the US residuals, though sometimes it is
seen at the initial identification stage of
US analysis. Easter effects often do not
show extreme values; the question is if --
Easter months show a consistent sign. Once
detected, these can be modeled with interven
tion analysis: the input has unit impulses
in Easter months and zeros elsewhere, the
transfer function is merely a constant.

29

3.5. GENERAL COMMENTS.

When data anomalies are found, explanations from outside the sample should be sought diligently. Data clipping, that is intervention analysis without such outside information, may sometimes be necessary, but it can easily be misused. Outside explanations are often available, though finding them may require imagination and hard work.

In the analysis of 156 monthly observations of gasoline sales, nine different data errors were uncovered and corrected, Easter effects were prominent, gasoline price rises generated quantifiable step changes in level and also negative impulse effects on level. This latter effect is interesting, because it has an unusual but economically sensible interpretation: gasoline price rises lower average consumption rates of gasoline, but they also therefore lower average desired stocks of gasoline in auto-users' tanks and this latter adjustment can only be achieved by temporarily reduced purchases. Most of the effects described in this case were detected in anomalous data values or residuals of models.

In an analysis of 157 monthly observations of an industrial production index for output of the mining sector, besides Easter effects, seven coal miners' strikes, one lasting two months and another lasting three months, plus two major mining disasters, were detected and modeled. Residual variance as compared with the best US model was reduced by more than 50% as a result of these intervention analyses and the resulting model allowed an evaluation of the real effects on production of these different incidents. It was no mean task to discover the presence of the strikes in the years before the death of Franco; censureship was tight on labor conflicts, though we were able to find brief newspaper references to support the analysis. This model was painfully built on the basis of time-consuming scanning of newspapers, work that was oriented by looking for explanations of extreme values that came to light only iteratively.

Sometimes work on anomalies can turn up information one might almost prefer not to know. On one occasion, we spotted anomalies in the same month and of opposite signs in two series of sales of the same energy product to two different classes of industrial purchasers; the government-controlled price had also been raised in the same month. One group of industrial purchasers had apparently had inside information about the forthcoming price-rise and had acted on it with profit.

In looking for causes of extreme values in economic aggregates, it is often useful to study components to determine if the extreme behavior is general or due to but one component; in either case, this can help uncover the cause. This can be done at successive levels of disaggregation until the source of the extreme value is isolated. The Spanish Consumer Price Index, for example, shows a large step-in-level increase in January 1977. By looking successively at components, it was shown to be due to a more than 200% increase in the state-fixed price of coffee in that month, though coffee does have a very small weight in that index. This permitted the decision to model the index without coffee for purposes of studying inflation, which monetary economists know is not to be confused with movements in the price of but one narrowly-defined good like coffee.

4. TYPICAL RELATIONSHIPS BETWEEN SERIES.

The evidence on relationships so far accumulated with the B-J approach is not sufficient to say very much about what is typical. Not many years ago, the "lack of relationships" almost seemed typical; see Feige and Pearce /12/. Since then, however, more relationships have been found, though I do not know of many genuinely bidirectional multivariate stochastic cases. More experience is undoubtedly needed. Nevertheless, a few generalizations are available and these call for revisions and perhaps some extensions of economic theory.

4.1. VERY SLOW-ACTING RESPONSES.

Many of the most hallowed relationships supposedly firmly established by conventional econometric research not only arose from a deficient methodology, but involved such

slow-acting responses that it is doubtful they could ever be supported by lengths of sample record in existence. When analyzed with the B-J approach, they either do not appear to exist or are found to act much more rapidly than imagined. This leads to the generalization that <u>detectable responses are generally faster-acting than thought on the basis of conventional econometrics</u>.

How did many of these slow relationships arise in econometrics? Consider the following virtually paradigmatic research sequence often found in the history of econometrics. Suppose that Y_t is expected to be influenced by X_t. Often some economic theory suggests a steady-state relationship:

$$Y_t^* = \alpha + \beta X_t \qquad (1)$$

where there may be a sign-restriction on β, and where the asterisk indicates a steady-state value. The economic theory in question is often based on some static-optimization theory for individual behavior and there may be little reason to expect aggregates to behave as individuals do, but this is ignored here as it usually is in the literature.

Early on in econometric work, researchers often considered a simple regression model based on the assumptions $Y_t = Y_t^*$ and the presence of a white-noise error a_t added to (1). Such simple models failed to describe the data to the satisfaction of researchers and hence generalizations were sought. One of the most frequently used appends an "adjustment process" to the previous model. Here I will use the simplest of these "adjustment processes", but the issues are the same in all cases. The appended dynamic assumption is:

$$\nabla Y_t = \gamma(Y_t^* - Y_{t-1}) \quad \text{for } 0 < \gamma < 1. \qquad (2)$$

Note incidentally that there is no pretense of any economic theory for (2). Some of my economist colleagues may want to remind me that years ago I spent considerable efforts developing economic rationalizations for relations like (2), in the belief that econometrics showed them to be typical of the -- data; see Treadway /34/ where other such work is cited. Needless to say, today I regard that belief as mistaken. Furthermore,

I do not regard my earlier work as justifying single-variable processes like (2); what is worse, I now feel, there is nothing in that work to support the utterly arbitrary way error structure is specified.

The usual procedure is to add a white-noise, or at least mean-stationary, error to (2) and then combination yields:

$$Y_t = \alpha\gamma + \gamma\beta X_t + (1 - \gamma)Y_{t-1} + a_t \qquad (3)$$

which is then fitted. Of course, (3) can be written:

$$Y_t = \alpha + \left[\frac{\gamma\beta}{1 - (1 - \gamma)B} \right] X_t +$$

$$+ \frac{a_t}{1 - (1 - \gamma)B} \qquad (4)$$

which is but a highly restricted form of the B-J transfer-function-noise model:

$$Y_t = \nu(B)X_t + N_t \qquad (5)$$

where $\nu(B)$ is a (s,r,b) transfer function and N_t follows an ARIMA (p,d,q) process. The key restrictions of (5) found in (4) are that: (1) the error process is stationary $(d = 0)$ and (2) the same AR(1) factor $\{1 - (1 - \gamma)B\}$ is forced to be present in both the transfer function and the error.

The first restriction is not innocuous; it is disastrously at odds with the data. The error N_t is destined to represent variables omitted from the model and many of these are presumably economic, thus typically they are far from being mean-stationary, as observed in Section 2. When $\nu(B) = 0$, it is obvious that γ will be estimated to be very small so that the AR(1) approaches a difference. When $\nu(B) \neq 0$, this will also tend to arise, though the second restriction will confuse matters slightly. In fact, in the majority of cases published, estimated values of γ are very low. However, given the second restriction, this kind of result leads to the conclusion that the relationship is very slow-acting.

The B-J analyst considers (5) directly without restrictions and chooses (s,r,b) and (p,d,q) to describe the data. He is not ig-

noring the relevant economic theory because he can interpret $v(1) = \beta$, thus the steady-state interpretation is available. What he does ignore are the arbitrary dynamic and stochastic assumptions underlying (4). If he submits estimated forms of (4) to diagnosis, he often finds them glaringly inadequate in statistical terms. But this is not the central point of this subsection.

Suppose we relax the first restriction in (4), use an adequate error structure, but estimate a $(s = 0, r = 1, b = 0)$ transfer function. Often the estimated transfer-function parameters will not be significant; when they are, the estimated response will be much faster than that (erroneously) calculated by estimating (4). This is part of my point: the response is faster than imagined, if it is detectable. But still this is not the central point.

Suppose we take the estimated transfer function from (4), consider the variances of Y_t and X_t under appropriate differencing and consider the length of record available. It is very often possible to show that, even if that particular transfer function were correct in form and parameter values, it could not be detected or well-estimated under the conditions given, i.e. the length of record and variances. This means that claims by conventional econometricians to have tested specifications like (4) may not only be doubtful, but impossible in practice.

4.2. FAST-ACTING RESPONSES.

Aside from its critical relevance to commonly accepted slow responses, implicit in the previous section is the idea that existing lengths of record support modeling of faster-acting relationships. Do they in fact arise in research?

It is often the case that record length is essentially fixed, but that the researcher has some range of options for selecting sample interval, e.g. when monthly series are available, which can be analyzed as such or aggregated to quarterly or annual. The choice of sample interval in such cases is relevant; faster reactions are best modeled with shorter interval, slower reactions with longer.

Econometrics has not traditionally paid much attention to monthly time-series, but it is now clear that these offer a rich field for the B-J approach. Incidentally, for all but the most developed economies, monthly series tend to be the most plentiful form of data and most more-aggregated series are merely created from them. We have already seen that monthly analyses are very useful for detecting data anomalies and treating them. It is also possible to uncover useful relationships between monthly time-series.

Return once more to the example of gasoline sales and consider the reaction to price. Before our work, the generally accepted idea was, and still is, that gasoline demand is influenced by price only over the rather long periods of time needed to amortize and renew the auto stock. It was imagined that patterns of use of that stock were not much influenced by price. However, in monthly analyses we have found a substantial and significant response within one month, indicating that patterns of use do respond significantly. We also find an additional effect distributed geometrically in time; it is difficult to estimate the profile of this additional effect with much precision, but it is clear that the reaction involved is much more rapid than reactions found in most econometric studies of gasoline demand; most of the response takes place in the first 12 months.

Many other studies have revealed similarly rapid reactions. Most energy demands studied with monthly data involve important responses to price in the first month. The response of industrial fuel-oil demand to variations in industrial output occurs in the first month. The industrial demand for imported equipment responds to industrial output mostly within one to three months.

Responses to variations in the rate of monetary expansion of different aggregate variables like the rate of inflation in consumer prices, the rate of industrial wage inflation, the level of industrial output or the level of unemplyment have not been found to be as rapid as the more microeconomic responses mentioned above, but they are usually faster than those found in conventional econometric studies. Consumer price inflation,

e.g., responds with a mean lag of about 1.5 years. This work was done with quarterly data, though this was aggregated from monthly data first corrected on the basis of intervention analyses.

The generalization here is that rapid responses are quite typical, especially in microeconomic situations. Econometrics could probably make major progress in elucidating the way prices regulate economic activity by paying more attention to monthly data and such rapid reactions.

4.3. THE ANNUAL SAMPLE INTERVAL

There are two features of the annual sample interval that suggest it should, when possible, be avoided. First, it quite often requires us to analyze such a small number of observations that all statistical inferences are doubtful. This may not occur for more developed economies with a longer statistical tradition, but the majority of economies in the world are not in such a position. Even for many of the economies of the European Common Market, annual employment series, for example, are rather short.

Second, even when records are long enough in principle to support statistical work with the annual interval, most economic relationships appear to involve speeds of response that do not favor it. Very rapid responses may not be detected at all. So-called simultaneity, that is, difficulties in interpreting contemporaneous correlation, is much more pronounced in the annual interval than in shorter intervals. We have found some evidence to suggest that one-way relationships found in quarterly analyses can appear to be two-way in annual analyses. The theoretical basis for this was given by Tiao and Wei /33/.

4.4. LACK OF RELATIONSHIPS

The kinds of results published by Feige and Pearce /12/ and Pierce /27/ seem to me more suggestive of how we should not proceed than of any lack of relationships. The reports published do not give enough information on the analyses to evaluate the importance of

each comment offered below, but there are several critical points worth making. I have obtained similar results on several occasions and have sought ways to better understand them.

One of the things that I find unconvincing in both of the papers cited is the quality of the US analyses. Many of the models presented by Feige and Pearce strike me as extremely unlikely. Those of Pierce are more difficult to evaluate, especially as I have insufficient experience with weekly data.

Both papers analyze many series at once and their procedures could well fail to reveal data anomalies important to the process of identifying relationships. Experience suggests that a few key extreme values can often "kill" a sample correlation function, leaving one with the impression of no relationship when in fact all that is involved is a few data errors or special events that need to be attended to before relating series.

These authors do not estimate parsimoniously parameterized models, but draw conclusions merely on the basis of formal significance tests on prewhitened cross-correlation functions. This is not a good way to proceed. Identification itself is not essentially a formal process of hypothesis testing and should not, I think, be forced into that mold; very often a configuration can be seen in a ccf and used to suggest a parametric model that can be successfully estimated -- even when no single cross-correlation coefficient is formally significant and even when a portmanteau test statistic fails to signal the presence of a relationship.

Finally these authors do indeed ignore economic theory entirely. Since they estimate no models, their procedure reduces to a poorly designed test of a rather irrelevant hypothesis in most cases. For example, Feige and Pearce make no effort to test the hypothesis that the money stock influences the general price level with a unit steady-state gain, an hypothesis of great significance.

In work on the relationships between money stock and price level, I have found that an approach similar to that followed by these

authors generates similar results for the Spanish economy. However, that is not the place to stop. In cross-correlation functions, it was possible to detect an apparently weak influence of money stock on general price level. A simple parametric model was chosen to be consistent with this weak configuration and estimated. It turned out that the above hypothesis was easily accepted, though the irrelevant hypothesis of no relationship was also acceptable in statistical terms. But statistical adequacy is not the only relevant criterion; results must make economic sense too. To perform tests of hypotheses in which one does not believe is gratuitous. Or do Feige and Pearce actually believe that the general price level and the money stock can be unrelated? Once the neutrality of money hypothesis was tested and found statistically acceptable, it was imposed and this led to economically interesting and sensible results.

It seems relevant to insist that model-builders: (1) use the whole B-J iterative process and not just a few fragments of it and (2) have clear initial conceptual models in mind from the start so that economic theory can be effectively brought into interaction with all the other elements of that process.

Finally I should say that the sampling intervals and length of record employed in the cited papers do not seem reasonable. Especially in the Pierce paper, it seems surprising that anyone should expect to detect more relationships than he did with weekly observations for but six or seven years.

5. CONCLUDING COMMENTS.

It is sometimes suggested that the B-J approach ignores economic theory. This simply constitutes a misunderstanding of the approach. In this connection, I cannot forget how Gwylym Jenkins used to pester me mercilessly to clarify my economic theoretic notions before doing any relationship modeling. There is nothing naively empirical about the approach; the critical thing is to make economic theory really interact with the model-building process. This theory must help state the problem at the outset, must

be considered in interpreting US models and in developing intervention analyses, must be used to state an initial conceptual model before relationship modeling, must be tested where possible, can certainly be used to impose restrictions on models when it is sufficiently convincing and must be used to interpret all results. Models must be economically sensible and statistically adequate; neither alone is enough.

What is wrong with common practice in econometrics is that: (1) it far too often imposes restrictions on models that are neither tested nor really convincingly based on economic theory and that are very often deeply at odds with the data, (2) it effectively ignores statistical adequacy as an objective and (3) it hardly follows an iterative learning process in which theory and practice interact.

The increased effectiveness in learning from the data that the B-J approach offers necessarily requires us to re-evaluate economic theory and to discriminate much more precisely than heretofore between those parts of theory that can reasonably be expected to hold in many cases and those that should be junked. I believe that, in broad terms, economic theory does not convincingly specify dynamic and stochastic structure, but does offer convincing notions of steady-state properties in many cases, though not all by any means. I will close with some examples.

5.1. DEFLATING PRICE SERIES.

Economic theory contains a convincing proposition that says that, in the steady state, all real economic behavior is zero-degree homogeneous in the set of variables measured in money units. This is often used to impose the restriction that only relative prices enter relationships, e.g. the demand for gasoline is supposed to depend only on the price of gasoline relative to other goods and not on the money price of gasoline by itself. In practice, the money price of gasoline, say, is divided by some index of the general price level.

However, the proposition in economic theory is limited to the steady state. If, e.g., gasoline demand is thought of as depending on

two input variables, the money price of gasoline and the index of the general price level, all that is specified by that theory is that steady-state gains on these two variables are of equal magnitude and of opposite sign, assuming a log-log relation; the transfer functions themselves need not differ merely by sign. Furthermore, if it is recognized that other variables measured in money units may also be involved, but are being represented by the error term, then even the steady-state restriction need not be valid. Thus the widespread use of deflated prices is not, in practice, justified by the theory. In our monthly analyses of gasoline we have used the money price of gasoline and there is nothing in economic theory violated by this formulation.

5.2. NEUTRALITY AND SUPER-NEUTRALITY OF MONEY

In studying the relationship between the consumer price index P_t and the nominal money stock M_t, a difficult-to-estimate dependence of $\ln P_t$ on $\ln M_t$ was found without using economic theory for any purpose except for the choice of problem and data.

The same theory mentioned in the preceding subsection suggests that the transfer function relating $\ln P_t$ to $\ln M_t$ should have a unit steady-state gain (long-run elasticity), at least if the series used actually measure the macroeconomic concepts of general price level and aggregate nominal money stock. This hypothesis was tested and accepted. The restriction that this value is unity was then imposed, since it is both supported by economic theory and found acceptable empirically. This restriction then permitted a much cleaner representation of the dynamic response of price level to money stock.

The model using the "neutrality" restriction indicated that the rate of monetary expansion influences the level of real-cash-balances (M_t divided by P_t) positively, a result that is inconsistent with an economic theory basing this relationship on the opportunity costs of holding real-money in the presence of expected inflation. But there is another economic theory, more Keynesian in origen perhaps, that argues that a higher rate of monetary expansion can increase the level of real-cash-balances and may effect other real variables.

To check on this, the industrial production index Y_t was studied in relation to M_t. The corresponding neutrality hypothesis was tested and also accepted. Furthermore, the corresponding "super-neutrality" hypothesis (rate of change of money stock influences no real variable) was tested and rejected; the rate of monetary expansion was found to positively influence the level of Y_t, this with a very interesting response function incidentally.

These experiences then suggested a broad look at these matters in several different price indices and real series. In all cases, the "neutrality" hypothesis was found acceptable, but the "super-neutrality" hypothesis was not. The results led to an interesting characterization of which real variables are influenced positively and which negatively by the rate of monetary expansion. Then attempts were made to integrate these single-output transfer-function-noise models in one multi-output model. Incidentally, monetary economists will wonder about possible feedback to the money stock; I have looked for this with different MS model-building methods, at the simple bivariate level and at more integrated levels and on the basis of several different hypotheses as to the nature of feedback; I have so far detected none.

There are thus many different levels at which theory and practice must interact. However, as in most other learning processes, one must begin with the simplest level. Gwilym Jenkins used to say in explaining the role of US analysis, "One must learn to grovel, before one can learn to walk".

5.3. EXPECTATIONS FORMATION IN CHICKEN FARMERS.

The so-called rational-expectations hypothesis first suggested by Muth /25/ was introduced in macroeconomics some ten years ago and has generated much interest and controversy in that context ever since. However, macroeconomics is not necessarily a very good place for researching hypotheses about expectations formation; too many other lit-

tle-understood factors may come into play. Several years ago, D. Grandal and I began research on agricultural markets in this connection, this being the oldest testing ground for expectations hypotheses. In fact, in recent years chicken producers have been studied by Huntzinger /19/ and Goodwin and Sheffrin /15/ in attempts to model expectations formation.

This is another area in which the fact that the error terms corresponding to the levels of variables in economic systems are not likely to be mean-stationary, even when deterministic components have been accounted for, does not seem to have been recognized. This is leading to absurd results. For example, many authors seem to believe that univariate price-expectations functions are not rational in Muth's sense, but it is easy to show that all of the famous univariate mecha nisms (those associated with the Cobweb theorem, Goodwin's extrapolative and Nerlove's adaptive, to use the terms of this li terature) are rational in Muth's sense for certain non-stationary error assumptions in his simple demand-supply model for a market with fixed production lag. The empirical ana lyses of Huntzinger /19/ and Goodwin and Sheffrin /15/ cited come up with expectations formulae in which the expected price depends on the past history of almost everything but the kitchen sink and yet does not depend on the past history of the price itself. This is conceptually aberrant and is entirely due to ignoring the non-stationarity of error processes. This is a case in which I am sure that chicken farmers are wiser than econome tricians.

We began our research in this area with economic theoretical work to get a clear and ge neral theoretical set-up at the outset. This consists of a demand relation along with a supply relation in which expected price trig gers production decisions; both relations may contain third variables exogenous to the market in question and both have error terms of general ARIMA form. There may be dynamic res ponse to price in the demand relation as long as the contemporaneous elasticity is not zero. A key feature is that the dynamics of the pro duction technology are fixed and known; for example, it takes about three months to turn an egg into a marketable broiler chicken, six

months to raise a laying hen, etc. A hierarchy of expectations-formation hypotheses is designed, involving: (1) what information is used by chicken farmers, since costs of obtaining it are not likely to be irrelevant and (2) how information is used, since we are also aware that the costs of model-build ing are not irrelevant either. This conceptual model of a market results in a bivariate stochastic price and quantity model or, if exogenous variables are introduced, a two-output transfer-function-noise model. These models are overidentified, in econometric terminology, under most sets of hypotheses, but we use them without restrictions at the beginning of empirical model-building to assure that we achieve statistical adequacy be fore testing or imposing hypotheses.

We do not yet have this project completed, but I mention it, because here we find a case where (1) existing economic theory is largely ignoring an important feature of the data and (2) model-building requires a de tailed initial development of economic theory itself to frame the data analysis. The B-J approach is being used in this work, but, far from being ignored, economic theory is being considered very intensively. That is, I am sure, exactly how Gwilym Jenkins would have wanted it.

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