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Allen's _Enclosure and the Yeoman_:

The View from Tory Fundamentalism

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Robert Allen has written a masterful and important book. It combines first-rate economics with first-rate history, and will take its place on the growing shelf of masterful historical economics--I have in mind other recent additions to the shelf such as Price Fishback's _Soft Coal, Hard Choices_ or David Mitch's _The Rise of Popular Literacy in Victorian England_. We social-scientific historians, or more narrowly we economic historians, or still more narrowly we historical agricultural economists, need give nothing up to the so-called social scientists. We do the real science; they too often remain on the blackboard. We have acquired some of our intellectual values from the History Department; the economists have, unfortunately, acquired most of their's from the Math Department. Allen's book is a splendid example of getting off the blackboard and into the library and archive. It is economic science. Twenty more books like this and our fortune is made. However one views the conclusions Allen wishes to draw about the decline of the yeoman, he has permanently raised the level of the debate.

Even the best books, though, have flaws. The more explicit the scientist has been the easier it is going to be for another, if less learned, scientist to locate them. That's what makes Allen's book serious science. Alexander Gerschenkron commended Fishlow on railroads for its "statistical appendices in which the author offers a full insight into his laboratory and without which no real appreciation of the importance of the study and the validity of its interpretative results is possible." The same can be said of Allen's book.

I am going to show in some matters on which I have some slight expertise, concentrating on Chapters 6 through 9, with 13, that Allen has gone beyond and sometimes against his evidence. From this I infer that in other matters, where I have no way of reading critically, he has probably made similar mistakes, arising I think from his political passsion. But the thoroughness of the book itself makes the critical reading possible, leaving a large increment to historical knowledge, whatever one's politics. If economics is going to become a real science we must stand thus on each other's shoulders, the better to see further. The trick is to avoid standing on each other's faces.

The basic point (not of the book, which is wider: I refer only to Chapters 6 through 9, and 13), on which Allen and I agree, is that the theory of rent is relevant to measuring productivity change from enclosure and that an application of the theory, with others, to the English evidence shows that (1.) rents were doubled by enclosure but that (2.) after all the increase in productivity was not large by national standards--a "respectable order of magnitude for a mere shift in the distribution of property rights," I said in an article in 1972 (p. 35) to which Allen makes some reference late in his book, but only 1½ percent of national income withal (this is the figure given at p. 159 in the other of the two papers of mine that Allen has read, a longer version of the 1972 paper, published in 1975b). But in 1972 I did not have much evidence, and was embarrassed by the lack: "if one had the temerity to ignore the many qualifications necessary in view of the argument of this essay . . . ." the calculation follows, but only then (1972, p. 35; and 1975b, p. 159).

Later I got the evidence, mainly in the form of what one could charitably call "time series," namely, rents on land before and after enclosure, actual, promised, pamphleteered, and journalized. Some of the evidence is reported in an obscure paper presented to the Agricultural History Society in 1983, summarized in a more accessible paper in 1989. Allen has not had a chance to read those papers; if he did he would probably agree with me that after going through repeated stories of doubling of rent, from an array of sources, one comes to believe it.

The core of the 1972 paper, so far as it has to do with enclosure, is something on which Allen and I thoroughly agree: the accounting identity, shading into an economic theory, that rent is a residual from other costs. The accounting is simple (I had used it before, incidentally, in my book on iron and steel in Britain, with industrial profit in the place of rent [McCloskey 1973, p. 125; cf. p. 20]; the fact is not irrelevant to the relationship between my work and Allen's): if rent is, say, 10 percent of costs, and if enclosure results in what is after all a quite modest increase of 10 percent in total productivity, rents--being the residual--will go up much more sharply; in fact, they will double, and greed in pursuing them will, as I argued, explain the enclosure movement. (I choose the illustrative figures with care: Allen and I agree that they are of this order.)

It's really as simple as that, although both Allen (1992 pp. 270-72) and I would like our readers to think it is somehow a more complicated point, requiring deep wisdom and a great deal of mathematics to grasp. Alas, it's not: it's trivially simple, or else I, for one, would never have been able to use it for history.

A more formal analysis of why rents and productivity are connected was given in my intermediate textbook of 1982 (2nd ed. 1985, pp. 488-89; 496 Problem 2; and Answer p. 611-12; I gather Allen did not read it). Allen gives the same analysis in Chapter 9 of his book but does not cite the earlier formulation (viz., my 1972 or 1975b) until Chapter 13, where part of it appears under the guise of a "social saving" calculation by McCloskey, described as "not quite on the mark." Allen appears to think that his calculations of what he calls "Ricardian rent" are different from my calculation of "social saving." But they are identical. Ricardo knew it, the appropriation of the fruits of progress by the landed classes being his own dismal expectation. And the Fishlow-Fogel calculations of social saving on American railways, which is where Allen's tag comes from, can be expressed indifferently as increase of income or increases of rent, as Fishlow and Fogel proved (for which see again my intermediate textbook, 1985, p. 220).

These theoretical and doctrinal matters, though, are not important. Allen and I anyway agree on the method. He acknowledges (Chp. 9, p.) that the calculation using rents is "a more encompassing test of efficiency than the comparisons of yield per acre and output per working [he] undertook" measuring difference in output (as against rent). Some confusion on the point has been generated in the minds of Allen's readers by an overemphatic expression in his earlier paper of his (published in 1982), which does not recognize that rent is "more encompassing." Happily, it is now straightened out, since Allen has shown empirically that the two sorts of calculations--rents on the one hand and physical productivity measures on the other--come in fact to the same conclusion about enclosure.

In finding out what happened in an enclosure we have first, complements of Allen, Turner, and many other students, a good deal of new evidence on physical productivity: 10 percent increases on enclosure seem typical. We have, second, McCloskey's "time series" (ho, ho) of rents, from which we gather again that productivity increases something like 10 or 15 percent when a village was enclosed.

And, third, we have now a promising supplement, the core of Allen's research idea so far as productivity is concerned: examining statistics on rents in open and enclosed villages, or on open and enclosed farms in different villages, "cross sections." Allen used before, and most elegantly, a sample of 231 large farms in the 1760s scattered through Arthur Young's tour books (Allen 1982; Allen 1986). I did a similar exercise about the same time with the mouthwatering statistics in Parkinson's <u>Rutland</u> (1808), and now Allen has exploited the same writer's work more systematically (McCloskey 1983, esp. pp. 69-71; Allen 1992, Chp. 9; Allen does not appear to be aware of my earlier work on the subject).

Here is the puzzle. From the statistics on village differentials in Rutland both Allen and I and Allen alone from his farm differentials in the Young sample arrived at a surprising low estimate of the rise in rent, much lower than the results from the "time series." In my Rutland calculations of 1983 the rents accruing to landlords in 9 open villages was 14.9 shillings per acre as against 22.2 shillings in 44 enclosed villages, a difference that looks at first not too far from the doubling in the time series. As Allen and I have stressed, however, the "rent" relevant for productivity calculations must be the full economic rent, especially when comparing rents on farms in different villages, and must therefore include the poor rates and tithe. Including rates and tithes makes the figures for Rutland 21.9 shillings as against 26.0, a difference in rents between open and enclosed villages of only 19 percent. (My crude attempts to control for land quality did not seem to alter the conclusion.)

The low differential, I repeat, was a puzzle. True, the "cross sections" have methodological difficulties of their own. The chief one is familiar from econometric studies of production functions, namely, that a "sample" of firms having to participate in the same market will be biased towards finding no differences of efficiency. The market pushes out the unusually inefficient, with the result that the open fields that survived must have been especially suited to openness. To put it another way, the sample is self-selected and non-random: places do not become enclosed by accident.[1] It is suggestive, for instance, that all the 9 open fields surviving in the tiny county of Rutland (18 miles across at its broadest) were in the southeastern side of the county, in Wrandyke hundred. (Thomas Weiss has pointed out to me that stupid landlords would also be stupid in seizing [even small] gains from enclosure. They would be bad at farming and also bad at enclosing. So a cross section on this account would exaggerate the experimentally-controlled difference in efficiency. On this interpretation Wrandyke Hundred would be the region of stupid landlords.)

Fortunately, Allen's work here in separating the farms by soil has solved the puzzle and reestablished the doubling of rent and therefore the 10 percent figure for the increase in total productivity. Separating the Parkinson farms in Rutland into his categories of heavy arable, light arable, and pasture, he has concluded from the rents that the difference in productivity between enclosed and open farms was 12, 3, and 11 percent (Chp. 9). And as he says, "[t]hese comparisons. . . confirm the general conclusions about the efficiency effect of enclosure that [he] reached by the study of [physical] yields and labour productivity in Chapters 7 and 8."

The rise of rent on an enclosure, then, was 100 percent notionally, and probably a little lower in practice, implying a rise of productivity of perhaps 10 or 15 percent (I said 13 percent in 1972, and have used a figure of 10 percent in my writings on open fields; McCloskey 1975a, 1976, 1984, 1989, 1991, writings which have escaped Allen's attention). Allen, again, had once argued that the rise in rent did not measure a rise in productivity (he argued more strenuously in this line in the 1982 paper than he does now in the book). As noted above, he reckons from his Young sample (the selection bias that may afflict such samples has already been noted) that economic rent did <u>not</u> increase when a farm was enclosed. Why then did the rent paid increase? "Rents rose when villages were enclosed either because the efficiency of agriculture increased and hence the value of the land rose or because open field rents were less than the value of the land and rents were raised at enclosure to eliminate the disequilibrium (Allen, 1982, p. 939). Allen was arguing that open fields rented below equilibrium. (Notice that this is 1982 Allen, not 1992.)

I suggest that the problem lies somewhere in the Young sample, not in the figure of doubling of rents on enclosure. I think Allen would now agree.

There are various routes to a test. It has long been recognized in the literature that Parliamentary enclosure in the eighteenth century truncated all leases in a village, and that in a period of accelerating inflation such as the late eighteenth century it is not strange to suppose that a Parliament of landlords would enact a renegotiation of the lease (McCloskey 1972, p. 33). It would be a simple matter to calculate the gain from unexpired leases, since the county-by-county Reports to the Board of Agriculture in the 1790s and 1800s record the prevailing length of lease. If Allen was right in 1982 the counties with the longest leases should be the ones experiencing the highest percentage declines of land in open fields. Unfortunately, no one has done the calculation.

Yet Allen's argument and therefore his Young sample face the problem that the differential favoring enclosure seems to have been of long standing, not confined to the various French Wars of the eighteenth century and their accompanying inflations (shockingly high rates of price rise, upwards of 1 percent or even--goodness gracious--2 percent per year). However plausible would be a temporary disequilibrium in the 1760s, say, it would be odd for landlords to surrender land at rents below equilibrium for centuries. A landlord doing so would be spurning a doubling of his income, in view of the doubling of annual rents to be had by being a little bit greedy (the same point applies to Victorian "failure" in iron and steel). Such a man is not at any rate the grasping landlord of Ricardian theory or of Restoration comedy or of medieval poetry and preaching.

Now of course there is plenty of evidence for Good Landlords. In the Agrarian History of England and Wales, Christopher Clay noted for instance the third earl of Clare, who declared in his will of 1689 that he was "not willing [that his tenants] should be harassed for what they are unable to pay" (Clay 1985, esp. p. 242; Clay calls his section, "Different Landlords, Different Approachs," pp. 241-245). Clay and his co-author for Wales, David W. Howell, spend some pages giving examples of the harsh and the lenient landlords. The nuance and shading is certainly useful: it is useful to be reminded that a "good" landlord can ignore the dictates of the market as long as his money lasts. One is reminded of the farming joke in bad times: "You can make a lot of money in farming--if you start with even more money." But such a method, giving examples of good landlords and bad, cannot resolve the issue, as Clay and Howell understood. We need to know how much, overall, the good landlords subsidized the bad. More particularly, we need to know if there was a change in the attitude of landlords, occurring happily at the same time as enclosure. It would be strange. The puzzle for future research is to bring the strangeness of the Young sample into agreement with the more ample evidence, from many centuries, and now including evidence from Allen as well, that landlords got higher rents from enclosures mainly on account of the (modestly) higher productivity.

However all this turns out, one would like to know what explained these (modest) productivity increases. A regression would be nice, if there were statistics on the agricultural output of villages before and after enclosure. But in 1972 there were not. "The refinements require more information," said I somewhat pompously in 1972, in the way of assistant professors, "particularly a usable sample of the history of enclosure in a good number of villages, complete with the villages' topography and soil types, their size, their tenurial arrangements, and estimates of the costs and benefits of their enclosures" (1972, p. 35). Although yields per acre did appear to have increased during the eighteenth century, it was difficult then, before Allen, Turner, et al., to decide by how much, and still more difficult to allocate the increase in any detail to specific regions or times. There was wide disagreement on the magnitude of the increase, if "disagreement" is quite the right word to use for a difference of emphasis on an issue that all writers confessed was much in doubt. For wheat yields alone, Chambers and Mingay (34ff) followed Phyllis Deane and W. A. Cole (1964, 62-75) and the earlier work by Fussell (1929), in putting the increase at only 10 percent for the entire century. On the other hand, Ashton (1955, p. 51) had followed the suggestion of Bennett (1935) that the increase was a third in the second half of the century alone.

Then Robert Allen spoke out loud and bold. From the agricultural writers of the eighteenth century he extracted first the Young sample and later more samples. He has been able to carry out the program adumbrated by me in 1975, running regressions of yields on varying conditions of soil, crops, and tenurial arrangements to see where enclosure happens and why.

But again the conclusion of Allen's work has been misunderstood, partly because most people have relied again on his earliest article (1982), which depended on the Young sample, and have not followed his later work. Allen wrote boldly in 1982 that "enclosure did not raise efficiency. . . . [I]t was possible to accept the statistical hypothesis that open and enclosed fields were equally efficient" (p. 950). Stefano Fenoaltea, for example, thinks that Allen's "sophisticated cliometric investigation reach inductively the very conclusion I had reached with the simpler tools at my disposal" (1988, p. 196, citing Allen 1982 alone; by "simpler tools" Fenoaltea means speculation undisciplined by fact). Now after further study, challenged in the meantime by Turner's findings from the Crop Returns of 1801 (1982, 1986) and my use of the Reports to the Board of Agriculture, Allen has come to conclusions, *pace* Fenoaltea, close to the rest of the literature.

Allen makes some useful distinctions between types of land: "In the light arable district, yields in open and enclosed villages were identical. In the pasture district, enclosed yields were perhaps a tenth higher than in open villages. In the heavy arable district, enclosure boosted yields about a quarter" (1991, Chp. 1, p. 27). Though such figures do not startle a reader of his book, or a student of the existing literature, Allen does not make entirely clear which of his many investigations give them. The slight fault of exposition arises again from the book's virtues: Allen's evidence is rich and he is perfectly candid in describing it. Candidly described evidence in this veil of tears will from time to time contradict itself. The figures just quoted--0, 10, and 25 percent on the three types of land--do not agree with what seem to be his most comprehensive estimates, on the basis of a sample of his South Midlands district c. 1800 (Table 7-2), where they are 13.7 percent on heavy arable, 8.1 percent on pasture, and 5.6 percent on heavy arable. The 25 percent figure for heavy arable in Allen's summary seems to come instead from a much smaller Cambridgeshire sample (Table 7-3 and text discussion). But whichever of these estimates one takes, the overall conclusion is the same: they are not zero; and they are not 50 percent; they are the moderate productivity gain that would result in a doubling of rent (recall the accounting), just as the other students of the matter have been saying. Allen affirms that "in most places the gain [in <u>labor</u> productivity on an enclosure] was . . . on the order of a tenth" (Chp. 1, p. 28), which together with his estimates of output per acre, taking one sort of land with another, gives overall productivity gains of 10 or 15 percent.

Allen's main theme is that much of the other gains in productivity, a virtual doubling since the Middle Ages, was achieved by what he calls "the yeoman's agricultural revolution" of the sixteenth and seventeenth centuries, not the landlord's of the eighteenth. The theme is unobjectionable, the reorientation important, and his arguments for it often persuasive. Allen does erect on it an anti-capitalist and populist tale that is not so persuasive as his agricultural history. He has no time for the view that Marx may have been mistaken in placing the "rise" of capitalism in the 16th century. Like Marx, Allen ignores the evidence of a capitalist, individualist mentality in England much earlier: he dismisses Macfarlane in a line, but is not aware that this puts him at cross purposes with some medievalists. And his yearning for distinction leads him here as elsewhere to denigrate scholars who have anticipated his findings--he joins for example in the fashionable and political animus among English agricultural historians against Eric

Kerridge, who was the first of many, now joined by Allen, to speak against the fixation on the eighteenth century. Allen carries political grudges quite far, which undermines his ethos, the faith in the implied author on which all scientific persuasion depends. But I agree with his theme, and in fact myself articulated it earlier.

So Allen has come to agree with my speculations about enclosure published in the 1970s and my evidence published in the 1980s. How, then, has Allen "gone beyond and sometimes against his evidence," as I claim?

Consider first a case in which my hypothesis, sneeringly (and inaccurately) called throughout "Tory fundamentalism," is being tested. Allen, speaking of his own sample from Young (1768-70; Allen and O Grada 1988, p. 98) and from the crop returns of 1801 as analyzed by Turner (1982), says: "In every case the table shows a higher yield for enclosed than for open villages. However, the differences are never statistically significant and the percentage differences in the means are quite small" (Chp. 7, p. 134, referring to Table 7-1). He then says that they are "very small" relative to the doubling of yield from medieval to modern times, which last is certainly true and important, as we have agreed. Allen and I are historical economists; we are always finding things to be smaller.

But the issue is, small relative to what? As I will argue in a moment, the statistical significance he appeals to (notice that the statement is not as sharp as in 1982) is irrelevant. It is hard to see what Allen means by saying that the differences by eighteenth-century standards are "quite small." Unlike the McCloskey/Allen standard of contribution to post-medieval improvement, no eighteenth-century standard of comparison is offered. The yield differences between open and enclosed villages even in the Young/Allen sample c. 1770 were in fact "large," and in the sample from 1801 Crop Return, as Michael Turner has pointed out, are "very large" (Turner 1982, p. 98 [*my citation here is in error: corrigendum est*]). By what standard? By the standard of rent differences implied. To put the matter again in terms of rough and ready facts on which we all agree, Allen and I reckon that gross incomes from an acre of land were about 100 shillings, with land before enclosure renting at about 10 shillings an acre. That is to say, the share of rent was about 10 percent of gross farm income. The 9.2 percent productivity difference in the Young sample (crop weighted by Allen, Table 7-1) would therefore double rents. Allen in fact criticizes the sample he collected from Young on similar grounds: "Thus, [Young's] sample is dominated by villages like those in the light arable and pasture districts, where the differential between open and enclosed yields was small" (Chp. 7, p. 11). As we just saw, Turner's work has established that productivity differences by 1801 were still higher, about 25 percent. Allen's further work for 1801 gives as I have noted crop-weighted differentials of 5.6 percent for the light arable district of his South Midlands region, 8.1 percent for the pasture district, and 13.7 (the text gives 14.7, but seems to be in error) for the heavy (that is, clay) arable district. In his ungenerous way he criticizes Turner for comparing inherently better land that had been enclosed with inherently worse land that had not been enclosed (Chp. 7, p. 137), which may be a fair point, although he should have conceded that Turner's evidence covers all of England, as against his tiny samples. Still, productivity differences from 5.6 to 13.7 percent are enough to double rents.

So perhaps it is not surprising to find Allen in one of his many emphatic and ungenerous moods saying in contradiction to his own evidence that rising rents from enclosure "are difficult to reconcile with the evidence directly bearing on the relative efffiency of open and enclosed agriculture" (Chp. 9, p. 173). He is mistaken, as we have seen: his figures of the rise in productivity can be reconciled closely with the rise in rents; they both imply a productivity differential of something a little over 10 percent. After a splendid effort over many years Allen has confirmed a back-of-the-envelop calculation offered most diffidently in 1972.

So Allen and I agree on the magnitude of improvement from enclosure: we agree it was no great shakes in explaining the improvement of agriculture from the Middle Ages to modern times, but not a gain that a landlord would spurn, considering that he was the residual claimant. We agree on many of the details of how the improvement was achieved. But Allen, the reader can see from the example, in an admirable attempt to make his argument vividly clear, sometimes offers simple, memorable conclusions that contradict his own carefully handled evidence, as in this matter of productivity. It is perhaps the main and repeated fault of presentation in what is otherwise a finely written book.

The reader may wonder whether I myself am being overvivid for effect, in order to sustain my own well-known opinions on productivity. After all, it seems astonishing that the chief critic of my early calculations turns out on moderately close inspection to agree with them.[2] Let me therefore give an example in detail of a reading of

Allen's work on a minor subject in which I have no interest a priori in disagreeing with his conclusions.

Allen quite reasonably emphasizes the importance of underdrainage as a factor in the superior productivity of enclosed villages. Judging from the incessant complaint about excess water in English agriculture early and late, and Allen's revealing findings about underdrainage before the coming of clay pipes (Chp. 7), he must be correct to emphasize it. I have, too: the "control of water" as I put it (1976) is what English agriculture is all about, as anyone who has ventured there without wellies can testify. But to emphasize factor X does not mean that one must set factor Y at zero, unless factor Y has a small effect by the standard of the question at issue.

Statistically speaking, to return to an earlier point, in exploring the importance of underdrainage Allen openly, with no sense that he is making a mistake, misuses statistical significance to drop variables. The practice is bad history, though it needs to be emphasized that he is here following the usual practice among economists. His econometrics is not sophisticated--as we have seen, for example, it takes no account of sample selection bias; and one could add that it takes no account of simultaneity and other misspecification errors or of errors in variables, both of which are important in the present case. It reaches, however, the standard set by most applied economics. But the practice of dropping insignificant variables, however popular it is among economists, is statistically indefensible and leads to substantive error (see Goldberger 1991, p. 240; Arrow 1959; Ohta and Griliches 1976; Kruskal 1978; Leamer 1983; Mayer 1975; Denton 1988; Guttman 1985; Tukey 1986; McCloskey 1985).

In particular: Allen's Table 7-3 shows for Cambridgeshire (40 observations in one county, it should be noted, a slender basis for the bold assertions he then makes) a regression of yields for wheat, barley, oats, and beans on three variables expressed as present or absent: enclosure, "moderate or a little drainage," and "an extensive system of hollow and surface drains" (pp. 138-139). He says, "What is striking . . . is that it is only the coefficients [on the presence or absence of hollow drains] that are _significantly_ different from zero . . . Whether a parish was open or enclosed, in itself, had _no_ impact on yields" (Chp. 7, p. 139, italics supplied).

But statistical significance is not the same as scientific significance. Allen is not looking at the size of the coefficients and assessing their importance for some historical question (his analysis surrounding labor demand after enclosure in Table 8-4 is a more serious case of letting significance tests make the scientific decisions, though not relevant to the subject here). The sample size is 40 and history has not performed a perfectly controlled experiment involving the variables of interest, and so the coefficients do not all have _statistically_ significant values. But that means merely that the sample was not large enough (mathematically speaking, sigma divided by the square root of N is therefore not small enough) and history not cooperative enough to solve the sampling problem at conventional levels of confidence. The sampling problem is not normally, and is not here, the main scientific problem. The main scientific problem, to repeat, is how large an effect is, such as the effect of Enclosure itself on yields. To use a sampling criterion (statistical significance in the conventional sense) to answer a scientific question (the significance of the effect) is to look for one's keys under the lamp post, having lost them in the dark, because the light under the lamp post is better.

In other words, Allen is mistaken when he says in one of his many conclusions beyond the evidence that "[w]hether a parish was open or enclosed, in itself, had no impact on yields" No impact. His regression for wheat is Yield = 18.8 + 0.89 [Enclosure present] - 1.34 [Moderate drainage present] + 3.04 [Hollow drainage present]. In this equation the 0.89 coefficient on Enclosure is "insignificant" at conventional levels (though of course "significant" at the << [t = 0.91698] level) and the 3.04 coefficient on Hollow is "significant." Allen then runs the yields on Hollow drainage alone, dropping Enclosure, and gets Yield = 18.6 + 3.69 [Hollow drainage present].

But to repeat there is no justification in statistical theory for dropping insignificant variables. Variables should be dropped if they are scientifically insignificant, not if they are merely statistically so. The scientific significance of Enclosure is substantial. It "in itself" results in a 0.89 / 18.76 = 4.7 percent increase of wheat yields over what they would be without enclosure or "moderate" drainage (apparently deleterious or signalling bad practice, because it reduces yields) or hollow drains. True, according to the Cambridgeshire sample the variable Hollow results in a larger increase, namely, 3.04 / 18.8 = 16 percent. That is Allen's valid and useful insight. At any rate in 40 villages in Cambridgeshire at the end of the eighteenth century enclosure had much of its effect through hollow drainage (Allen establishes earlier that villages without enclosure could install underdrains; true, something is missing, namely, three centuries of enclosure history *without* hollow drain, an oversight attributable to Allen's lack of interest in the history of open fields themselves). But for wheat yields the best estimate with his data is not that enclosure had "no" effect otherwise but that it had a 4.7 percent effect, *about a quarter of the total*. For barley the presence of enclosure in itself had a 8.5 percent effect as against a 12.5 percent effect from hollow drains, 40 percent of the total; for oats, 5.5 percent as against fully a 24 percent effect for drains; and for beans a peculiar negative effect (minus 8.5 percent) as against a positive 26 percent for drains. For barley, therefore, his statement of "no" effect from enclosure "itself" is quite misleading; for wheat somewhat so; and for the other crops an acceptable exaggeration for clarity of expression. It has nothing to do with statistical significance; it has to do with the size of the coefficients.

And of course, as Allen stresses, drainage was itself an effect of enclosure (see his Table 6-6; which is confirmed in the tendency for his Hollow-only regressions to have higher coefficients on Hollow than they do when he runs the full regression: the Hollow variable in the Hollow-only regressions is picking up the effect of the Enclosure variable, suggesting that Enclosure goes with Hollow drainage). If one adds together the coefficients on Enclosure and Hollow, which is the correct calculation, the effects of enclosure on yields are slightly above Allen's calculations using the (mildly) misspecified Hollow-only regression. The wheat, barley, oats coefficients sum to 21, 21, and 30 percent for the effect of enclosure (leave aside the beans, or put them in at 4.12 minus [odd, this] 1.36 = 2.76, giving a 17 percent increase attributable to enclosure). These calculations agree roughly with his new results for the heavy clay lands in his larger sample (Table 7-2) from the South Midlands for barley and oats (and beans if you wish; Allen does not explain why the wheat coefficient for the sample in Cambridgeshire is 21 percent as against his estimate of 2.5 percent in Table 7-2).

In short, Allen habitually outruns his evidence, leaping at the end of paragraphs or chapters to conclusions that do not follow from his always interesting and scholarly arguments in the main text. He does not do it, I am sure, on purpose, and I am not accusing him of any fault greater than excessive enthusiasm for his conclusions. If people were shot for such enthusiasm the ranks of agricultural historians would be thin indeed. But it is enthusiasm for his *conclusions*, not his *findings*. His findings often contradict his wish--showing again how serious and good the book is as scholarship.

My one larger objection to the book is its harshly political tone throughout, from page 1 right through to page 311. The reader is never in doubt for more than a page or two that Allen does not like capitalism or British landlords or the constitution of the realm, not one bit. I know I am supposed to believe that Politics is Everything, and that neat categories like The Marxists and The Tories work brilliantly well, sufficing to make all decisions worth making about what or whom to take seriously. I know that I am supposed to disdain, ignore, misread, and paraphrase without citation people who do not meet with my political approval, Left or Right. And I know from an acquaintance with the late George Stigler that it is by no means the Left alone that recommends such a course. But it is a shame. We should be able to do agricultural history without stepping on each other's faces, even for the most noble of Political purposes. Look at Price

Fishback's book mentioned earlier: its findings will infuriate the reflexive Left, but the thoughtful Left will learn a great deal, improving their own arguments even if they do not believe all of Price's; Price does not sneer at those who think the trade union was the life of labor, even when showing that the Left historians are on this or that important point mistaken. Would that Allen had taken such a liberal view.

Allen's book with the Politics left out would be a better and more believable one. On the other hand, books are devilishly hard to write and people must be indulged for the duration, allowed to write them out of whatever passion they can muster, especially if the result is so fine as Allen's. We have waited a long time for Allen's masterpiece and are glad to have it, warts, statistical significance, political correctness, and all.

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See James Heckman's recent paper on the rhetoric of experimentation, "Randomization and Social Policy Evaluation" (unpublished manuscript, Department of Economics, Yale University, Mar., 1990), in which he remarks (pp. 2-3) that "Plots of ground do not respond to anticipated treatments of fertilizer nor can they excuse themselves from being treated with fertilizer." To put it another way, he thinks that the analogy of agronomical treatment has been run into the ground.

2

It may make the assertion in the text more believable if I note that this is the second important occasion on which Allen's work has confirmed mine with methods that I proposed and used, while declaring itself to be "difficult to reconcile" with my conclusions. Since readers are otherwise liable to take away the impression from his emphatic rhetoric that he has substantially revised my picture (I know readers did on the earlier occasion), the analogy is worth a few lines. Before Allen worked on enclosures he worked on iron and steel in Britain, as I had done earlier. I had said that British productivity was very roughly the same as American (McCloskey 1973). He said, after much further work of the high quality that he now has shown us in his book, that, no, it was a tiny bit lower. In that case as in the present one the impression was given that Allen and I disagree when in fact our separate calculations agree closely, considering their inevitable crudeness. The issue is the same here, twenty years later: Allen and I agreed there was not an enormous difference between British and American productivities in iron and steel; now we agree also that there was not an enormous difference between open and enclosed productivities in agriculture. It must be our framing stories that disagree (cf. McCloskey 1990, Chps. 3 and 4), since it is not our statistics. I favor a story of capitalist success, in which matters of class were not important; Allen does not.

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