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APPLYING IDEALIZED SCIENTIFIC THEORIES TO ENGINEERING

ABSTRACT. The problem for the scientist created by using idealizations is to determine whether failures to achieve experimental fit are attributable to experimental error, falsity of theory, or of idealization. Even in the rare case when experimental fit within experimental error is achieved, the scientist must determine whether this is so because of a true theory and fortuitously canceling idealizations, or due to a fortuitous combination of false theory and false idealizations. For the engineer, the problem seems rather different. Experiment for the engineer reveals the closeness of predictive fit that can be achieved by theory and idealization for a particular case. If the closeness of fit is good enough for some practical purpose, the job is done. If not, or there are reasons to consider variation, then the engineer needs to know how well the experimentally determined closeness of fit will extrapolate to new cases. This paper focuses on engineering measures of closeness of fit and the projectibility of those measures to new cases.

1. IDEALIZATIONS AND APPROXIMATIONS

The simplest view of engineering is that it is no more than a straightforward deductive activity: engineers select from among the many equations provided by scientists, insert the parameter values of interest, carry out the calculation (or have a machine do it), and then apply the answer to the particular project at hand. Badly taught courses in engineering serve to foster this position. But no one with any real experience in engineering is likely to acquiesce to so simple and unsympathetic a view.

It is commonly claimed by engineers and engineering historians that engineering is not reducible to applied science, that it is more an art than a science, and that it contains an irreducible 'design' or imaginative aspect.[1] Part of the motivation for such assertions comes, I believe, from the absence of exact and complete scientific analyses of the complex systems that are of interest to engineers.[2] By implication there are such analyses of the systems of interest to scientists. If there is this difference in the availability and power of analytical methods, then we should expect experimentation to play different roles in engineering and science.[3] In the sciences experimentation will be used to test the

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truth of theories, by checking the truth of scientific predictions. But since engineering must make do with simplified analyses, there is no question of truth. In engineering, therefore, experimentation will be used only to test the practical reliability of necessarily simplified analyses.[5]

Consider though these two key episodes in the history of science: Newton's derivation of Kepler's laws, and Einstein's calculation of the bending of starlight near the sun. In both cases, in order to achieve a computable prediction, it was necessary to assume the rather extreme idealization that there is only one large massy body in the universe.[5] These cases suggest that the need to achieve real, as opposed to in principle only, computability is a constraint on both engineering and scientific practice. Nothing can even begin to happen in the way of testing or application of theory in the absence of some calculated numbers. Therefore idealizations and approximations must be used by both scientist and engineer. There is really no choice for either practitioner but to simplify. But if science is an activity constrained in this way, then our proposed distinction between science and engineering collapses. Both enterprises are characterized by the absence of complete exact solutions. Furthermore, we now are without support for the claim that scientists aim for the truth whereas engineers aim only for practical applicability. Because the idealizations required for real computability are strictly false, it appears that failed predictions can always be explained away by appeal to this falsity.

The problem created by the use of idealizations for science then is to determine whether failures to achieve experimental fit to within experimental error are due to the falsity of theory or of idealization. In other words, the problem is to determine when we can praise theories for achieving as close a fit as is achieved and blame the idealizations for the failure to achieve experimental fit to within experimental error. In rare cases where experimental fit to within experimental error is achieved, it must be determined whether this is due to the truth of theory and fortuitously canceling idealizations, or to a fortuitous combination of false theory and false idealizations.

For the engineer the problem seems rather different. Experiment from his or her point of view reveals the closeness of predictive fit that can be achieved by theory *and* idealization for the particular case examined. If this closeness of fit is good enough for some practical purpose then the engineer's job is done, assuming that he restricts his practice to the duplication of the experimental case. If predictive fit is not close enough or if there are reasons (perhaps economic) to consider variation, then the engineer needs to know how well the experimentally determined closeness of fit will extrapolate to different cases.

This paper will focus on the second of these problems, that is, on the establishment by engineers of measures of closeness of fit and the projectibility of those measures to new cases.[6]

2. APPLYING SCIENCE

Scientific theories are more easily and better tested (*ceteris paribus*) the fewer complicating idealizations there are. This is noncontroversial. Hence, good targets, classic experiments, tend to be simple and direct. Engineers, since they must satisfy complex practical requirements, do not have this sort of freedom to concentrate their efforts on the construction of simple systems. Suspension bridge design would be considerably simplified if wind and weather effects could be eliminated by the construction of huge protective barriers. But the cost of such design simplification would be prohibitive. (Depending on research priorities, the expense of simplification may or may not be a problem for a scientific experiment.) The situation is similar when we consider the desirability versus the cost of achieving very low friction, extreme rigidity, and in general extreme or null values for all the parameters that add complication and loss of easy applicability to our scientific theories. So we need to know how engineers manage unavoidable complexity.

Our procedure here will be to exploit the suggestive or heuristic value of a simple example: the pendulum. If one's interest were testing Newtonian mechanics, then measuring pendulum performance in media of low pressure and density would be sensible because that would minimize the effect of many of the various idealizations needed to generate computable predictions. If one wished to use pendulums, however, as reliable, low cost, and easily transportable *instruments* for determining variation in gravitational field strength (as was the case from the seventeenth century on), then it makes sense to allow them to oscillate in air under normal atmospheric pressure and then to correct or transform these oscillations "to what would have been observed had the pendulum been swung in a vacuum" (Stokes 1850, p. 1). That is, one can try to subtract the effect due to the air to get a residual due to gravity alone. And historically this is what was done. Perhaps this 'reduction to vacuum' in the interests of economical and convenient instrumentation was not per se engineering, but as a piece of applied science we can hope that it will provide some insight into the applicability of science to practical problems.

Making the hydrostatic correction for buoyancy was the first response to the problem of correcting for the presence of air. For the purposes of illustration and easy explanation, we shall ignore here the various other historically made corrections and stick with the point mass pendulum of elementary physics. Correcting for buoyancy means that the weight of the pendulum, the downward restoring force, must be changed from the simple mg to $(m-m')g$, where m is the mass of the pendulum bob, m' the mass of the displaced air, l the length of the suspension cord, and g the gravitational field strength. Making this substitution (and utilizing the $\theta = \sin\theta$ approximation of the standard elementary analysis), the differential equation of motion is

$$ml\ddot{\theta} = (m - m')g\theta.$$

Solving for the period we get

$$p = 2\pi\sqrt{lm/(m - m')g}$$

instead of the $2\pi\sqrt{l/g}$ of the uncorrected elementary (vacuum) analysis.

Since nineteenth century analyses were in terms of the number of oscillations to be expected in a particular time period (usually the mean solar day), we shall use henceforth the inverse of the period, to be denoted N. Subscripts a and v will be used to denote respectively motion in air and in vacuum. If we let d be the ratio of the specific gravities of pendulum bob and air, i.e., m/m', the above equation transforms to

$$N_a = (1/2\pi)(\sqrt{g/l})\sqrt{(1 - 1/d)}.$$

That is, the number of oscillations to be expected in air in unit time will be equal to the number of oscillations in vacuum multiplied by a *correction factor*. As already noted, historically the problem was to convert the observed number of oscillations in air, i.e., N_a, to what that number would have been had the pendulum oscillated in vacuum, i.e., N_v. Rewriting the above to reflect his aim, we get (using a standard nineteenth century series approximation)

$$N_v = N_a(1/\sqrt{1-1/d}) \cong H_a + \{1/[2(d-1)]\}N_a.$$

This form shows clearly the anticipated effect of the air, namely, a reduction in the number of oscillations to be expected, i.e., an increase in period. This equation in its approximate form conceptually determined the problem for experimentalists who took their role to be the determination of a correction factor more accurate than the hydrostatic $1/[2(d-1)]$.

Baily, one of the principals in these experiments, determined values for N_a at thirty-two inches and one inch of atmospheric pressure for a variety of spherical and cylindrical pendulums. Essentially what Baily discovered (on the assumption of linearity of effect) was that in the case of one and one-half inch spherical pendulums, if the hydrostatic correction were multiplied by 1.8, one could achieve reliable predictive accuracy. This correction factor appeared to vary inversely as the size of the pendulum bob. (It was also influenced by the thickness of the supporting cable or rod.) The cylindrical pendulums by contrast behaved badly and no simple pattern could be discerned. The correction terms here were specific to each of the cylindrical pendulums.[7]

What Baily, in effect, did was to quantify the *biasing* effect of the idealization that air acts only as a hydrostatic agent in the case of spherical pendulums. His procedure is not to be construed as a test of the underlying fundamental theory, Newtonian mechanics. That is because the interpretation of the correction factor is that it represents what is needed to overcome the bias introduced by the various idealizations used in the hydrostatically corrected derivation of the pendulum period.[8] If we were instrument designers, our interest in Baily's work would be the *projectibility* of his results to other cases of interest. We might wonder, for example, about the projectibility of scale, to pressures below one inch of mercury, to compound pendulums, and to non-spherical and non-cylindrical shapes. Assuming Baily's work as prototypical of applied science, the question that emerges is:

> How can experimentally determined measures of the bias introduced by idealizations be extrapolated or extended to other problems of interest.

Before attempting an answer to this question, let me first consider an objection to the way we have conceptualized the problem. We are assuming the existence of an underlying fundamental theory that is taken to be true.[9] But it is typically the case in engineering that the underlying theory used is already known itself to be only approximately true. As has been frequently noted, men were sent to the moon using Newtonian and not Relativistic mechanics. Furthermore, it is sometimes the case (e.g., in economics and systems engineering), that there is no underlying or fundamental theory. Here one deals only with systems of descriptive or phenomenological equations. Obviously these possibilities will serve to complicate any analysis of applied science. However, since the projectibility problem is difficult and interesting even given the assumption of an underlying theory, we shall in this paper ignore the objection and stick with our original conceptualization of the problem.

3. ENGINEERING RESPONSES TO THE PROJECTIBILITY PROBLEM

One engineering response to the projectibility problem is to avoid it by developing physical systems which satisfy more accurately than previous systems the idealizations used in the engineering analysis. So, for example, with respect to pendulums, suspension systems are made with lower mass and reduced friction. In the case of air resistance, this avoidance strategy would involve designing low cost and easy to use methods of operating pendulums at low pressures. The development of servomechanisms provides an actual case of this strategy since increasing the speed of operation serves to decrease the relative importance of Coulomb friction.[10] It should not be thought that this strategy of avoiding extrapolation is necessarily mindless and unimaginative. In fact it has been claimed that one measure of engineering design ability is success in finding systems or forms which satisfy practical constraints as well as those imposed by the idealizations of available analyses. Maillart's concrete bridges instantiate this sort of success. He was able to invent and develop a structural form which maximized the accuracy of the relevant idealizing assumptions of arch analysis and which had, relative to other designs, greater practical virtue.[11]

The most common response to the projectibility problem is the conservative one of minimizing in new physical systems deviations from earlier successful examples. In terms of Baily's experiments, this response would allow us to operate our pendulums at normal atmospheric pressure but would restrict them to being only minor variants of those used by Baily. The insight here is simply that since there may be lurking discontinuities, we should not be too venturesome in testing the continuity of the deviation between real performance and our idealized analysis. Because these hoped-for continuities typically are not grounded by theory, "every new engineering design is an experiment, as small departures from convention may have disproportionately disastrous results" (Pippard et al., 1953, p. 191). An interesting case where this very general approach of not overly straining continuity was violated is that of the Tacoma Narrows Bridge, 'Galloping Gertie'. This bridge was considerably more flexible and more narrow than existing long span suspension bridges, a fact noted by the board of engineers reviewing its failure.

With a depth ratio of stiffening girders of 1/350, the Tacoma Narrows Bridge with a much smaller weight went far beyond the precedents, notably the Golden Gate Bridge with a depth ratio of 1/168, and the Bronx-Whitestone Bridge with one of 1/209.... In respect to width also, the Tacoma Narrows Bridge, with a ratio of 1/72 surpassed in slenderness all others, which show a range of ratios between 1/14 (Triborough) and 1/47 (Golden Gate). (Ammann et al., 1941, p. 74)

These large variations from existing examples were also explicitly commented on *before* the disaster in an official review of Moisseiff's design proposal. In addition, general skepticism, but unfortunately only general, was expressed about Moisseiff's argument that existing structural theory justified his design as being adequately stiff.

There seems to be some question even in [Moisseiff's] mind as to whether the obtained stiffness is other than 'rather satisfactory'. The ratio of width to span is 1/72 which greatly exceeds the corresponding ratios of other long suspension bridges.... It therefore seems to me that it would be advisable to widen the superstructure to 52 ft..... This width would give a ratio of 1:54 approx. and would provide greater convenience and capacity for highway traffic. The cost would be increased considerably, but the additional cost would certainly be justified. (Condron 1938, pp. 4–5)

And here we note that the risk and consequent cost of failure is being used to justify what would otherwise not be a sound practical decision.[12] These general concerns about the large variation from existing practice were rejected, however, by another team of consultants.

It might seem to those who are not experienced in suspension bridge design that the proposed 2800-foot span with a distance between stiffening trusses of 39′ and a corresponding width to span ratio of 72, being without precedent, is somewhat excessive. In our opinion this feature of the design should give no concern.

The development of the deflection theory of suspension bridge design in recent years for both vertical and lateral deflections has proven beyond doubt that the matter of width ratio is limited not by structural stress but only by the amount of lateral deflection in wind which can be realized without discomfort or fear to the driver of an automobile over the bridge. (Andrew et al., 1938, pp. 6–7)

What we have here is an example of another engineering response to the projectibility problem: one argues on the basis of the underlying theory and an enumeration of the relevant physical entities that no discontinuities are to be expected, i.e., that the correction for idealized bias can be safely extrapolated in a continuous fashion. It is not necessary to analyze this defense of Moisseiff's design in detail in order to see that we will have to develop a distinction between idealizations which are complete, i.e., say something about all the relevant entities or factors, and those which are incomplete, i.e., which ignore relevant entities or factors.

So, for example, when we describe the sine of the angle of displacement of our pendulum as being the radian measure of the angle, we are attaching a description which is only approximately true of a relevant 'entity', in this case the angle. On the other hand, when we ignore the viscosity forces of the air, our description is incomplete since it leaves out of discussion a relevant force. Exactly how this distinction is to be made will depend on the formal representation we give to a theory and to its associated referential devices. We need not await resolution of this issue to support the development of the proposed distinction and to sketch its likely applications.

In terms of the proposed distinction, the argument for the projectibility of 'the deflection theory of suspension bridges' to cover the Tacoma bridge design can be understood as containing the following two components. First, there is the claim that this 'theory' (i.e., Newtonian mechanics *and* associated idealizations) shows that no discontinuities are to be expected. This claim must be understood as relying on an assumption of completeness. The second component, then, we would expect to be a justification for this assumption of completeness.

Since the Tacoma design called for a simple girder structure, the drag or horizontal forces generated by a heavy wind were expected to be large. Using experimental models to estimate the drag coefficient, Moisseiff and his team designed the bridge to withstand the drag created by winds considerably in excess of one hundred miles per hour. By contrast, the lift forces generated by suspension bridges (given their lack of a streamlined airfoil) "are small in comparison with the dead and live loads and are therefore generally neglected in the strength calculations"

(Ammann et al., 1941, p. 99). So it seemed safe to ignore these forces, these entities, and to accept the 'deflection theory' as complete.[13]

In some cases, analysis will predict discontinuities of system response as input or environmental parameters are varied within estimated operational limits. In such cases, engineers will be forewarned to expect discontinuities. Idealized analyses can also be used to determine the projectibility of the behavior of scale models.[14] It must be emphasized, however, that since engineering analyses will always be idealized, predictions about discontinuous or catastrophic behavior are not guaranteed to be correct.[15] In the following sections we shall examine several ways of determining the reliabiliy of the predictions of idealized engineering analyses.

4. USING IDEALIZED ANALYSES TO DETERMINE PERFORMANCE LIMITS

Idealized analyses are often used to provide operational or performance limits for physical systems.[16] As we shall see, the existence of such limits serves to simplify and make more determinate problems about the projectibility of idealized analyses. Roughly speaking, limits to operation can be derived when actual variations from the idealizations all pull in the same direction. In the pendulum case, the presence of air served to increase the period. If it could be shown that deviations from the other idealizations had similar effect, i.e., to increase and not to decrease the period, then our idealized hydrostatic analysis would serve as a natural *limit,* as it were, of pendulum efficiency. The efficiency standard set by ideal thermodynamic heat engines is, of course, the paradigm of the sort of case I have in mind. At the very least, such standards provide a convenient zero point, and sometimes a scale as well, for measuring relative efficiences of actually constructed systems. For example, Ivanoff, in a classic paper on process control, writes that the "treatment of the [control] problem proposed is, however, regarded not as a universal and inflexible theory, but rather as a standard by which one can judge the quality of a plant from the point of view of exact regulation, and which one can use to compare the various methods and systems of control" (Ivanoff 1934, p. 118).

Sometimes, depending on the details of the case, ideal limit analyses can be used to determine necessary conditions for real success. Again the ideal heat engine provides a standard example. Another example comes from a classic paper by Nyquist on control theory and amplifier design.

Now, this fact as to the equality of gain and loss appears to be an accident connected with the non-linearity of the circuit and far from throwing light on the conditions for stability actually diverts attention from the essential facts. In the present discussion this difficulty will be avoided by the use of a strictly linear amplifier, which implies an amplifier of unlimited power capacity. The attention will then be centered on whether an initial impulse dies out or results in a runaway condition. If a runaway condition takes place in such an amplifier, it follows that a non-linear amplifier having the same gain for small current and decreasing gain with increasing current will be unstable as well. (Nyquist 1932, p. 126)

Ideal limit analyses also provide convenient attachment points for empirical theories of deviation. The advantage of having an ideal limit is that projectibility problems due to mutually contravening corrections are somewhat simplified. The study of the efficiency of 'cut-off valves' in steam engines provides an example. By mid nineteenth century it was realized that the principal cause of variation between real and ideal efficiency was the condensation of vapor in the steam engine cylinder during expansion. Furthermore, increasing expansion (a way of satisfying ideal conditions on output and efficiency) led to increasingly more serious losses by cylinder-condensation. Engineers therefore were faced with an optimization problem for which they had no theory. Extensive empirical studies were conducted, *à la* Baily, and the results as summarized by Thurston, a leading nineteenth century engineer, were,

that the cylinder-condensation . . . varied sensibly as the square-root of the ratio of expansion, and the method of variation is apparently substantially similar for other forms and proportions of engine. The amount of such condensation usually lies between one-tenth and one-fifth the square-root of that ratio, if estimated as a fraction of the quantity of steam demanded by a similar engine having a non-conducting cylinder, it being here assumed that the engine is one of fair size. The proportion of loss is some inverse function of the size of the engine – probably nearly inversely as the diameter of cylinder. (Thurston 1891, p. 276)

So here one compared the ideal way of increasing efficiency (increased expansion) with the real efficiency-robbing condensation. The value of the thermodynamic theory of ideal efficiency is that it made the experimental investigation of actual steam engine efficiency more determinate. This is because all actual features could be expected to pull away from the ideal Carnot cycle.

5. EXPLANATIONS OF CORRECTION COEFFICIENTS

We have dealt, so far, with attempts, sometimes theoretically inspired, to discover experimentally coefficients that could be used to correct the bias introduced by the use of idealizations (in conjunction with fundamental theory). These coefficients may be specific to particular cases, or they may vary in a lawlike way as the case of interest is varied. The next stage of analysis is to explain why these law-like correlations are as they are. One way to do this is by means of a *fictional* or *instrumental* theory. Such theories can also be used to generate (in lieu of experimentation) correction coefficients. Airy's explanation of Baily's data in terms of the fictitious 'weight of adhesive air' is the sort of theory I have in mind.[17] Using the analysis of the pendulum given above and letting V be the additional oscillations to be expected in vacuum (i.e., $V = N_v - N_a$), we can describe Airy's approach as follows. Think of the oscillations in air and in vacuum (for simple ideal pendulums) as being controlled by the equation

$$N_i = \frac{1}{2}\pi\sqrt{\frac{W_i}{ml}}$$

where W_i is the weight of the pendulum bob in air or in vacuum (i.e, for i respectively equal to a or v). Therefore, the weight-in-vacuum will be to the weight-in-air as "the ratio of $(N_a + V)^2$ to N_a^2". But since $N_a \gg V$, for the values under consideration, it follows that,

$$\frac{N_a{}^2}{(N_a + V)^2} \approxeq 1 - \frac{2V}{N_a}.$$

Therefore,

$$W_a \simeq W_v\left(1 - \frac{2V}{N_a}\right).$$

That is, it is "*as if* it [the pendulum bob in air] had lost the weight $W_v X\ 2V/N_a$". But the *real* loss of (effective) weight as determined by the hydrostatic calculation (given above) is $(m'/m)W_v$, where W_v of course is just mg. "Consequently the portion which is not accounted for by the mere displacement of the air [i.e., the hydrostatic effect], is"

$$W_v\left(\frac{2V}{N_a} - \frac{m'}{m}\right).$$

This extra loss could be accounted for, in the sense of generating the same effect, if the inertia of the bob were increased by the amount $(1 + 2\ V/N_a - m'/m)$. That is because,[18]

$$\frac{1}{2\pi}\sqrt{\frac{W_v\left(1-\frac{1}{d}\right)}{lm\left(1+\frac{2V}{N_a}-\frac{1}{d}\right)}} \cong \frac{1}{2\pi}\sqrt{\frac{W_v\left(1-\frac{2V}{N_a}\right)}{lm}}.$$

Therefore,

It appears that the phenomena . . . may generally be explained by supposing a quantity of air, depending on the figure of the body, to adhere to it whilst it is moving, and to add to its inertia without altering its gravitation. (Baily 1832, p. 440)

This analysis is clearly fictional (or instrumental) since it attributes properties to the surrounding air that it cannot have (given current fundamental law). But it should be noted that the analysis, while counterfactual, is in terms of the basic concepts (e.g., mass and weight) of the underlying fundamental theory.[19] The practical advantage of adopting this fictional-as-if theory was that it generated predictions for compound pendulums which were as accurate (or nearly so) as those obtainable for simple pendulums by using Baily's correction coefficients. That is, one used Baily's coefficient to calculate the weight of the adhesive air and then utilized these weights in standard calculations for the periods of compound pendulums.[20]

Constructing such fictional theories is a common practice in engineering.[21] What can we say about their explanatory value? Such cases pose severe tests for current philosophical theories of explanation. I think in the Airy case, there were at least two features that led him and his contemporaries to regard the 'adhesive air' as explanatory. First, there was the systematic predictive success of the theory for compound pendulums. Second, it was at least an open possibility that the viscosity or stickiness of air could be used to explain why air acted as if it possessed Airy's set of counterfactual properties. And, in fact, this latter explanation was ultimately given by Stokes as part of his 'theory' of viscosity.

Stokes's theory, since it was non-fictional, represents a further escalation in the treatment of variations between ideal analyses and real performance. Such 'theories' will consist of more realistic (but still idealized) descriptions of previously considered entities or factors, along with idealized descriptions of entities or factors previously not considered.[22] If computable, such improvements typically lead to a narrowing of the gap between predicted and experimental values. Stokes's analysis in terms of viscosity, because it was more realistic and complete, provided a better understanding of performance differences between ideal and real pendulums. In particular, it revealed hitherto unsuspected (and empirically undiscovered) dependencies such as that between period and size of the arc of oscillation. (See Stokes, 1850, pp. 253–326.)

Since Stokes's analysis is still idealized, though not *as* idealized as the simple hydrostatic account, the nature of the problem of application remains essentially as we first presented it, namely, experimentally to determine the bias due to the remaining idealizations and to justify extrapolation to new cases. It also should be noted that the benefits of more realistic accounts carry the cost of computational or analytical complexity: Stokes's equations could be solved only for simple shapes.

The engineering analogy is clear: if the improved analysis is not computable in the case of interest for the engineer, he or she will have to make do with some piecemeal combination of older analysis and (risky) extrapolation of new analysis from computable to non-computable cases. One such combination strategy is to use simpler analyses to 'correct' or supplement more realistic analyses when the latter are not computable or solvable for all elements or aspects of the engineering system. Stokes, for example, used his viscosity equations to compute the motion of pendulum bobs. Given analytical difficulties, however, he used the 'ordinary' equations of hydrodynamics, i.e., viscous free, to calculate the effect of the suspension wire. (See Stokes, 1850, pp. 67, 83–84.)[23]

6. PROJECTIBILITY RECONSIDERED

Our problem has been to justify the extrapolation of experimentally determined correction factors to new cases. The basic mode of argumentation that underlies all of the engineering responses discussed above is that of analogy. One argues that new cases are sufficiently similar to experimental cases so as to justify application of the correction factor. Viewed this way, engineering responses to the extrapolation problem can be seen as generating refinements of the conditions for being a *relevant instance* of the combination of idealized analysis and correction factor. That is, I am suggesting (for the time being) that we conceive of the combination of underlying fundamental theory and idealization set as itself a theory, but one which suffers from vague criteria of application.[24]

Given this conception, the engineering methodology discussed above can be seen to divide into two basic strategies: (1) developing ways to live with this vagueness; (2) developing firmer criteria of application. So, for example, making a system more closely approximate the idealizations used is an example of the first strategy since such systems prima facie will be instances, and furthermore, instances where the correction factors, if anything, will be smaller than experimentally determined values.[25] Constructing systems which vary only by a small amount from existing systems is also an example of the first strategy, since it is just the conservative strategy of minimizing possibilities for having a non-instance.

Things get more interesting when attempts are made to explain or give theories about the form of the correction factor. Such explanations may reveal instances where previously it was thought none existed. Airy's adhesive air explanation, for example, showed compound spherical pendulums to be instances where Baily's correction coefficients could be applied. Since the criteria for application of the Baily coefficients were clarified and developed by Airy's analysis, this counts as an example of the second of the above engineering strategies. Refinement of instance criteria may also reveal that apparent instances are not so. As Stokes showed, pendulum period depends on amplitude. Therefore, Baily's correction coefficients were amplitude specific. These ways of refining the concept of an instance (and others discussed above) make for more reliable application of the combination of idealized analysis and correction factor because we improve our reasons for believing we have the analogies and disanalogies right. But since idealizations will always be required to insure real computability, this process of refinement of the concept of an instance is never ending.

We now ask whether the presumed truth of the underlying fundamental theory plays any real role in engineering methodology. Since the idealizations needed to generate a prediction are false, all inferences to predictions will be unsound. Therefore, an answer to our question cannot be based on any truth preserving role played by fundamental theory when used as a premise. Instrumentalists will be inclined to take the offensive here and assert that since it is only the combination of theory and idealizations that matters for the engineer, and since that combination is false, it follows that empirical adequacy and not truth is what is at stake.[26] Continuing in this instrumentalist vein, we note that the falsity of idealizations shields fundamental theories from both falsification and confirmation. Therefore, there can be no theory testing. But if this is true, then our proposed engineering methodology is also the methodology of science.[27]

This sort of instrumentalist line overlooks the crucial role played by claims that one idealized account is *more realistic* than another. (For example, Stokes's viscosity account is more realistic than the simple hydrostatic account.) Basically, more realistic accounts provide (in ways noted above) better insight as to what counts as a relevant instance. But judgments about relative realism typically depend on the assumption that the underlying fundamental theory is true. So we now begin to see a role for truth. Furthermore, there is also the presumption that true theories will produce *more accurate* predictions when fed *more realistic* descriptions of the system of interest. (This was for the engineer the practical payoff of Stokes's viscosity theory.) Failure to produce better predictions in such circumstances is a prima facie disconfirmation of theory. All of this provides, I believe, promising material for use against instrumentalists and a topic for further research.[28]

NOTES

[1] For a good review of the attitudes of American Engineers see Layton (1976). For Layton's own views on design and art see p. 698. (Cf. Billington 1979, pp. 107–21.)

[2] For clear and thoughtful expressions of this view see (Pippard et al., 1952), which is a series of letters by engineers about the role of experimentation. For example: "There is a ... point which requires emphasis: engineering is still to a not inconsiderable extent an art. There is a disturbing tendency ... to assume that all engineering problems are capable of exact analytical solution, and that not only are the results of such analysis valid without physical checks, but that any problems not so soluble are not engineering .... Why should it be thought almost immoral, verging on cheating, to avoid difficult analysis by well-planned experiment to use dial gauges rather than differential operators? Personally, I would sooner trust the results of the trial and would be unwilling to pin too much faith in the analysis until checked by experiment on model or prototype" (p. 466).

[3] By experimentation in engineering we mean to include the empirical analysis of built

operational systems, such as bridges, as well as prototypes, scale models, and other simplified versions of what is actually to be built.

[4] The Popperians also arrive at this position, but by a different path. Agassi (1967, p. 362), for example, asserts: "As Popper has stressed, in pure science we may try to refute the most corroborated view, and in applied science truth is of little importance. In applied science, that is, the question of the truth of the theories to be applied hardly ever matters, though the question of the applicability of results from it is crucial and is answered by simple tests".

[5] For more details on the Newton case see Glymour (1980, pp. 203–26), and Laymon (1983). For the light-bending experiment see von Kluber (1960), Earman and Glymour (1980), and Laymon (1982).

[6] For a discussion of the first problem see Laymon (1985).

[7] The cylindrical pendulums were suspended on pivots built into the cylinders; the spherical pendulums consisted of bobs suspended by rods or wires. For details see Baily, (1832). Stokes gives a review of both experimental and theoretical work in his (1850) work.

[8] The use of the concept of bias in this context is due to Wimsatt (1981), who develops notions similar to those presented here.

[9] In addition to Newton's laws, I have in mind the Relativistic Field Equations, Maxwell's Laws, and the Schrödinger Equation. These laws are fundamental in the sense that (when taken to be true) they contain no *ceteris paribus* clauses.

[10] See, e.g., Hazen, (1934), p. 304.

[11] See Billington (1979), p. 92.

[12] Ammann et al., (1941), p. 40, report that: "The capacity of the bridge, a two-lane roadway and two footwalks, appeared from the prospects of traffic development to be ample for many years to come, and as much as could by justified economically". What Condron has done implicitly is to propose a decision matrix where the cost of a more certain avoidance of failure is entered as a factor. See the interesting discussion of this sort of consideration in Pippard et al., (1953), pp. 465–466.

[13] What in fact happened was that these forces, despite their relative smallness, were able to generate torsional oscillations in the extremely flexible roadway. And it was these oscillations that did in the Tacoma Narrows Bridge. The story is, of course, more complicated than this abbreviated version. There was some concern about the possibility of dangerous vertical oscillations along the length of the bridge, and some study and experimentation were done both in the design stage and after the bridge was constructed. Torsional oscillations, however, seem not to have been considered. (See Ammann et al., (1941), pp. 16–17, 19–28, appendix VII.) When examined in more detail, the Tacoma Bridge design illustrates a relatively ad hoc collection of different idealizations used in different domains of bridge performance. What this means is that engineers must be concerned about the overall coherence and projectibility of such approaches. Cartwright (1983, pp. 78–82), gives "the quantum theory of the laser" as an example of this sort of tailored use of different idealizations for different parts of a performance or behavior domain.

[14] Zeeman's simple catastrophe machine (designed to introduce some basic concepts of catastrophe theory) is an elegant example of discontinuous behavior. For details see Zeeman (1972) and Poston and Woodcock (1973). See Laymon (1989) for a discussion of dimensional analysis and the use of scale models.

15 In the case of the Zeeman catastrophe machine, one questions whether the predicted discontinuities of behavior are threatened by the idealizations used. In this case the analysis really only covers the statics of the machine. It works for dynamics only if the system is moved 'slowly' ('quasi-statics'). But from experience with similar devices, we know that this means that the system must be heavily damped and have little rotational inertia.

16 Having such limits is especially useful in the design of complex systems consisting of subsystems whose operation is relatively independent of the operation of the total system.[17]

17 Airy suggested this account in correspondence with Baily, (reprinted in Baily, 1832, pp. 431–32, 439–40). All quotations in the text are from Airy. Italics have been added, however, and the notation modified somewhat to be consistent with current treatments.

18 This depends on the approximation, $1-(2V/H_a) \cong (1-1/d)(1+2V/N_a-1/d)^{-1}$, which is correct to within one part in one hundred thousand for the values used by Airy. We note also that in the absence of a principled means of theory representation, there obviously will not be a sharp distinction between fictional-as-if analyses, such as Airy's, and those which are realistic but idealized.

19 In the case of late nineteenth century aether theories, the Newtonian counterfactuality required of the aether became so severe and persistent that Poincare (1905), for example, suggested the need for a new mechanics.

20 See Airy's account in Baily (1832, p. 440). Cf. the similar methodology employed in amplifier design discussed in Cartwright (1983, pp. 107-110).

21 Sometimes it can be shown (but using an idealized analysis) that fictional approaches will work (within certain performance ranges) as well as more realistic analyses. An especially striking example is the practice of treating spoked wheels *as if* they were solid discs possessing counterfactual properties. (See Pippard, 1952, pp. 76–95.)

22 As the examples of Stokes's viscosity 'theory' and (earlier in this paper) 'the deflection theory of suspension bridge design' show, ordinary usage sometimes will countenance a new set of idealizations as a theory.

23 Another sort of combination strategy can be employed when experimental study reveals that a process of interest becomes segmented, temporally and/or spatially, into sub-processes, some of which can be treated with idealized analyses, although not the same analysis for each sub-process. For example, masonry arches under increasing loads show arch element (voussoir) separation that in effect makes the span a three pin arch. As the load increases, the middle 'pin' moves toward the load. Within certain load ranges, therefore, a fixed-pin analysis could be used. The transition from one pin location to another, however, cannot be accounted for with such an approach. (See, e.g., Pippard, 1952, pp. 276–320.) For an interesting and detailed analysis of some of the combinations of theory and idealization used to analyze the interaction between a complex system and its environment, see Wimsatt (1981). (Cf. Cartwright 1983, p. 150.)

24 Philosophical theories of confirmation ordinarily take it for granted that instances of intended application can be determined unproblematically.

25 This is prima facie because the particulars of the case may be overriding.

26 And here we should remember that Newtonian mechanics is at best only approximately (whatever that might mean) true.

27 Cartwright (1983) constructs an argument of this sort but directs her instrumentalism only at the laws of physics. She argues that causal considerations justify our being realists

with respect to scientific entities. For a possible way of relating causal claims to the use of idealizations, see Laymon (1985, pp. 165-66).

[28] For development of some of these ideas see Laymon (1985, 1987).

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