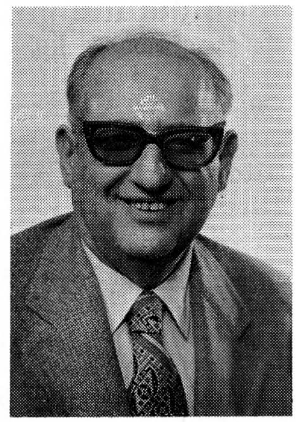
ChE views and opinions

THE INTERFACE BETWEEN INDUSTRY AND THE ACADEMIC WORLD*

EDITOR'S NOTE: Prof. Shinnar's paper was presented at an Engineering Foundation Conference on Chemical Process Control at Asilomar, Pacific Grove, CA, Jan. 18-23, 1976. We thought it worthwhile reading for students and faculty alike.



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I CAME TO THE academic profession quite late, after many years in industry, and my values and outlook were formed during my industrial career. Having worked in many fields and having had a varied career gives one the advantage of an overlook, and one often sees things that an insider cannot see. This paper is about some of these impressions on the present status of control.

Let me start with three episodes that happened to me recently and induced me to choose this topic for presentation. The first was a question asked of me by the chairman of one of the top chemical engineering departments in the United States. He asked me if process control today is still an active field of research in ChE and if it makes sense to have somebody in this field. It was an honest question, which is also asked by quite a few others, even those who have been active in control in recent years and are now leaving it. I'll try to answer it later.

The second occurrence was a letter I received from a former student of mine who obtained his Ph.D. in the U.S. in the area of control. I sent him a recent paper *(1)*, and in commenting on it he complained that our engineering profession is so far backward in the application of novel ideas in control that he has decided to go where the action is and become an applied mathematician.

The third happening was a comment by a reviewer that Vern Weekman received on a paper of his. The reviewer complained that the authors were unfairly criticizing the academic world, since he questioned how an academic could know what is and what is not implementable in industry. I don't know who here was hard on whom. I can hardly imagine a more severe condemnation of our academic engineering profession than this statement. If engineering professors have ceased to know what can and cannot be implemented, what are we teaching?

In these three episodes there is a reflection of the whole sad state of research in process control as well as an indication as to what needs to be done.

THE STATE OF PROCESS CONTROL

LET US NOT avoid the issue; the state of process control is rather sad. True, we have had

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many important theoretical and mathematical advances in recent years, and, as Professor Athans' paper [8] pointed out, quite a number of them could be very significant, and I definitely agree with him. But on the other hand, the application of these advances in industrial practice has been rather meager, and even those that are active in designing controls for completely automated complex plants complain that the publication of the academic community seem to be irrelevant to any conceivable needs. Furthermore, some of our best people are leaving the field disenchanted, and it is not attracting top students as often as previously. This is happening just as exciting applications are starting finally to appear, and, there are definite trends in industry that will require a better understanding of modern control.

But even in industry the love affair with process simulation and control is cooling. The heat is on almost all the research groups in the industry. Maybe we started too early and promised more than we could fulfill. But we could reasonably expect more understanding from industry. Let me remind you that the total expense of any major oil company on research in process control in any given year is less than for one major television commercial, and there is less evidence that commercials sell gasoline.

Somehow I feel that some of the recent adadvances in control theory offer exciting possibilities for better design, but there is very little knowledge as to what these values really are, where they can be successfully applied, and what the pitfalls are, and there is no question a lot of it is irrelevant.

Just look at the tremendous literature on Kalman filters. We listened to some top practitioners and heard that only one had ever really used one successfully. Listening to him, I realized that he used it in a different way than it is presented in the control literature, as a tool in interactive computer-aided design in which the coefficients are guessed and continuously adjusted by the results of the simulation. Now I would like you to relook at the literature on Kalman filters. How much of it really deals with the basic problem, which is to decide how to guess the structure of the covariance and, furthermore, to decide in what cases it is going to be useful.

Listening to the two sides of the arguments on the usefulness of modern control reminded me of two other episodes that happened to me. You have to excuse my habit of telling stories. In my culture it is a basic belief that a short story or joke often replaces a thousand words.

During the Israel Independence War in 1948 I was engaged in the manufacture of explosives and ammunition. Once I faced the problem of designing a simple small siren intended to be put on small bombs, to increase their psychological effect. I had no idea how one designs a siren and was looking for some sketch to copy. To save time I went to a professor I knew, and I still remember him going to his shelf and giving me two volumes of "Das Handbuch der Theoretischen Physik." I was reminded of this story by the claim that modern control theory is there—just go and use it.

The second episode symbolizes for me the stand of some of our industrial assessment members. In the early 1950's a group of young engineers were sitting in a house in Haifa and reminiscing about the war. One fellow recounted his experiences in the British Corps of Engineers. The British Army instructions at that time required that prefabricated pre-stressed concrete slabs should be reinforced in all four corners. Now, every compentent engineer knows that we only need two reinforcements, in the two corners on the lower side. One guest was an old Englishman who had stayed in Israel, and he commented that we were all a little young and inexperienced and did not fully appreciate the wisdom of the British Army. The manual is intended for use by the average sergeant in the British Army, who as likely as not is a Sikh with a minimum understanding of English.

There are probably many really valuable results hidden in the literature of modern control that merit being brought to a form useful for the control engineer. But we need to extract them, test them, and bring them to a form where they are useful tools in real empirical design.

He might be the only one in the company who can read that manual. You have to imagine him standing there with his curved knife in his mouth studying the manual, and, when he takes out the knife and starts to yell, you hope he'll know where to put the slab. If you presume that he'll know which side is up, you have lost in advance.

The Ziegler-Nichols tuning method of PI controllers almost fulfills the same requirement. But modern process control is never going to have a reinforcement in each corner. This is not its objective. It will need highly educated engineers to use it for special applications where it is justified. But it is also useless to tell industry, "There are two thousand mathematical lemmas, and why don't you use them?" As almost all assessment reports agree, modern control theory is not in a state where it is easily used.

ACADEMIC-INDUSTRIAL INTERFACE

THE PROBLEM IS really at the interface. The information flow from academics to industry and back is jammed, and the question is what we can do about it.

It would be very valuable if the process industries would publish more about their successes and failures. Some of the secrecy surrounding control is really bordering on the ridiculous. But it is rather hard to hope that they'll really do it in a useful way. The aerospace industry has much less of a problem, since much of the work is government financed and therefore published, and it also employs a much larger number of theoretically educated engineers.

If we want to improve that interface, it is the engineering societies and, above all, the engineering faculties who can and should do this job.

I don't worry about algorithms or computers eliminating the engineer. Complex design algorithms need a much higher degree of intellectual input than present methods and increase the need for highly trained personnel.

As a profession, engineering is not a science but rather the knowledge of bringing scientific development into useful practice, very often making empirical advances before the scientist understands them. Even design, which is much more formalized, is only partly based on scientific calculations and relies heavily on intuition and experience. Part of it can be computerized and formalized, but in the end judgment will play a large role in the synthesis.

Now design or process development is not easy to teach and much harder to do research on. To promote good research we have more and more gone over to focus our research on hard science, picking up areas left by the physicists and chemists, and slowly we have become a professional taught by non-practitioners. Maybe we are the only profession to do so. Can you imagine a medical school where all professors are physiologists and nobody is a clinician? Now medical research is much less clean and less scientific than physiology, but the latter would have no application without the first.

I see nothing wrong in having a large part of our research devoted to clearly definable scientific problems, both theoretical and experimental, but somehow we have to make an attempt to bring engineering back to our research. Nowhere is this more felt than in theoretical engineering and especially in control.

PROCESS CONTROL DESIGN

THERE ARE SEVERAL needs in engineering design that good theoretical research can fulfill.

- **The first is a need for straightforward algorithms, as, for example, the measurement of kinetic parameters in complex systems.**
- **The second is a need to better understand design decisions. Theoretical work can contribute to that by solving clearly defined cases, illuminating to the engineer what the potential problems could be. A good example of this is the theoretical work in reactor design, an area in which I also contributed. Now, in very few industrial cases would one expect an engineer to solve the type of complex models that have been solved or discussed in the literature. Hopefully, my own students do not interpret their work this way. However, from such theoretical modeling and related work we delivered rather well-working principles for reactor design: how to identify kinetic parameters in a simple way, how to structure the experiments needed for scale-up, how to identify reactors, and, most importantly, how to distinguish between simple problems and those which require more advanced methods. This is the most fruitful area for theoretical engineering research. But in order for it to be really useful the results have to be explained to the practicing engineer in a form he can understand.**

There are other types of theoretical research that I took part in. Some of the most difficult problems solved often only confirm that methods used by the engineer have a sound basis, but they do not lead to new insights.

Years ago when I worked in rheology, everybody was busy for years trying to understand the complex work of Coleman and Noll on constitutive equations. I don't want to belittle the eloquence and relevance of that work to continuum mechanics as a theoretical science. But the insight that we got from that to real rheology, and especially

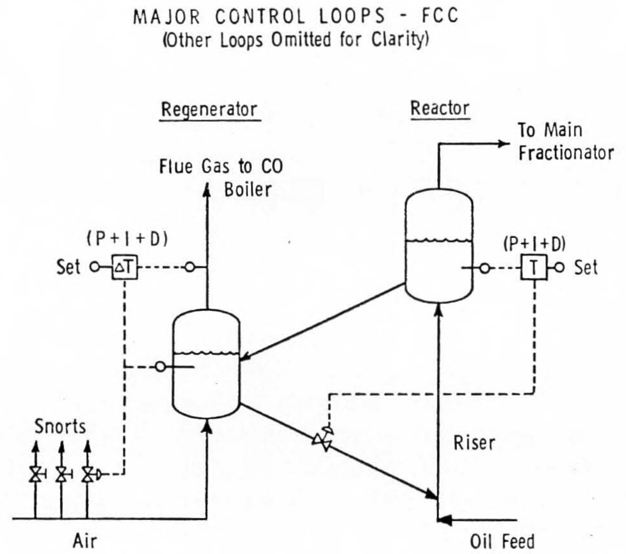

FIGURE 1. Schematic of conventional control scheme.

to problems of interest to the engineer, was rather small. We learned that a capillary rheometer measures the same parameters as a cone and plate viscosimeter and that it is impossible from such measurements to predict the behavior of the liquid in accelerating flows. We knew that long before. But we learned little about how to treat those more interesting cases and had to go back to simpler and more *ad hoc* theories. I admit of having done similar things myself. It did not start out that way.

The best way of describing such work from an engineering point of view is maybe the expression of Moliere's hero in the *Bourgeois Gentilhomme,* "I never knew I speak prose." There is some importance in knowing that one speaks prose, and from a purely scientific point of view this is often very interesting. But the importance that we give to such mathematical rigor in our engineering profession has little relation to its real value to the profession.

The fourth type of theory is the one that leads nowhere. I remember a good example from the time I was a graduate student. At that time a fashionable pastime was to write down equations of mass transfer in multicomponent systems. Some of these equations were tensors of the sixth or eighth order. There was no way that anybody could ever measure that many coefficients or even design a hypothetical experiment to measure them. The only thing we learned is that too much rigor will lead to unsolvable problems.

Now in engineering we start to give the highest ranking to the "I know prose" research and much less to that which leads to real insights in design. Nor do we insist that our results be presented in such a way that such insights to dirty problems are made clear. We have to learn to appreciate both types of research.

Consider for example the study of FCC control by Kurihara [2]. It is a very useful piece of work, and let me therefore discuss it in more detail.

Kurihara took a fluidized bed cracker and developed a simple lumped parameter model for it. He then took the standard industrial control scheme which is given in Figure 1, taken from Lee and Weekman [3], and looked at the connections between measured and manipulated variables. He then formulated an optimization problem in the following way. The system is assumed to be at a state X_i, different from the desired steady state, and has to be brought back to the desired steady state. At this desired steady state, all manipulated inputs have a known value. The feedback law is then written to minimize a performance index using some values for costs of control action and for profits based on reducing the deviation from the desired steady state. It is shown that a linearized analysis gives a very similar solution to the full non-linear optimization and furthermore, the control scheme given in Figure 2 gives almost the same result.

Now, there is much more in the thesis than I

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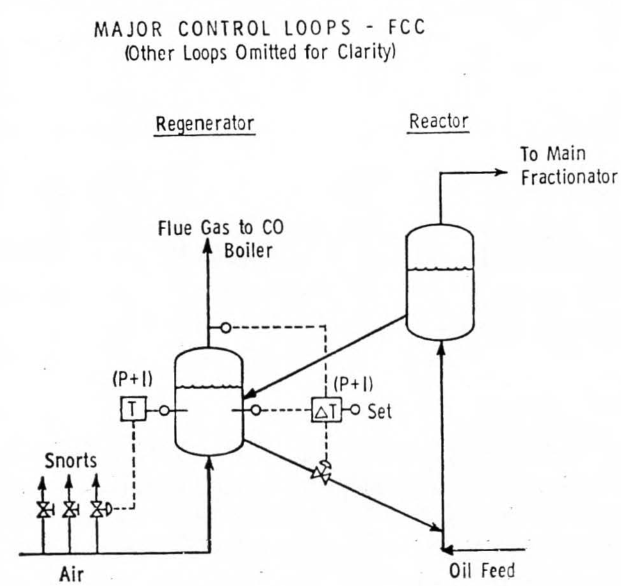

FIGURE 2. Schematic of Kurihara scheme.

SHINNAR: Interface Between Industry and the Academic World

Continued from page 153.

just said. Kurihara also analyzes the information flow in the unit and diagnoses the main difficulty of control. The main parameter controlling the performance is the level of coke on the catalyst particle. This again depends both on the reactor performances as well as on the regenerator condition. The time scale of the coke build-up is large, on the order of an hour, whereas the residence time of both oil and air flow in the unit is measured in seconds. This long time lag leads to difficult control.

What the scheme in Figure 2 really does is minimize this interaction by keeping the regenerator conditions more constant. To do this we need an additional measured variable on the regenerator to be kept constant.

But if one looks at the control scheme in Figure 2 from the viewpoint of an operator, an immediate deficiency is apparent. The reactor, which is the main part, has no control, and the operator has no direct way to change the level of conversion in the unit. Lee and Weekman [3] discuss this in detail and show that this can be corrected by a cascaded feedback loop, given in Figure 3.

The control scheme in Figure 3 is much smoother and faster than the controller in Figure 1, which is a significant improvement. It has, however, some of the same deficiencies, namely, that it does not have sufficient manipulated variables to allow the operator to really achieve what he needs to do, which is to be able to adjust the steady state of the unit to meet varying process requirements and varying constants. In the refinery we don't make money by reducing the level of the control input needed. This is fixed when we choose the manipulated variable. We make money by being able to work close to a constraint, and both our goal and the nature of the constraint change with time.

In reality the operator does this or tries to do this by using additional manipulated variables, which don't appear in any scheme. He changes the feed allocation between different units. Furthermore, he can change catalyst activity by adding and withdrawing more or less catalyst or ordering a different catalyst.

The fact that Kurihara's work did not lead to a useful controller design does not detract from the usefulness of his work. In fact, the complexity

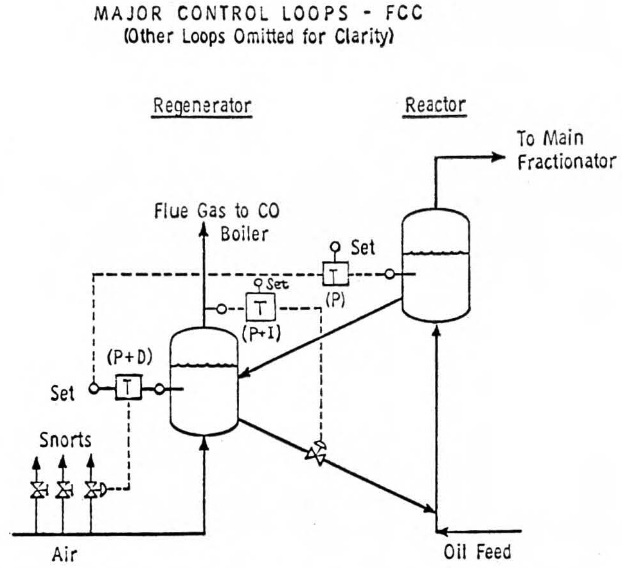

FIGURE 3. Schematic of modified control scheme.

of the problem is such that one cannot expect *academia* to do that, unless there is a real integration with an industrial project. But that is not necessarily what we want from *academia* here. It is sufficient that we understand in what way the modern control theory used in the example could be helpful in designing industrial controllers. And the negative results of Kurihara's work are far more illuminating and important than the positive ones.

OPTIMAL CONTROL

I LEARNED FROM THIS example some of the basic shortcomings of optimal control as well as some of its advantages. For example, it makes clear that the standard formulation of costs and profits in optimal control, both deterministic and stochastic, have very little to do with real costs and profits and are only indirectly relatable. Furthermore, complex chemical systems are often not controllable in the full sense, and controllability in the mathematical sense is not the same as in the operational sense. I realized that those decisions which are made before one writes the algorithm, namely, which variables can be measured and which should be manipulated, are more important than the choice of the algorithm itself or the profit function. The main result of the algorithm is in determining the dominant roots and in decoupling the reactor and the regenerator. This is rather insensitive to the profit function used.

We could have obtained some of the same results using the methods proposed by Rosenbrock [4] for multivariable controller design. This illuminates one of the main paradoxes of optimal control in process control application.

On the one hand it is clear that the term *optimum* is highly misleading. It is not a real optimum in any sense and can give rather unusable controllers, as pointed out by Rosenbrock [4] and myself [1]. It is also in no way a straightforward design algorithm but depends on the skill and understanding of the designer much more than the Ziegler-Nichols method does.

On the other hand optimal control can provide very useful information to the designer. But this information must be integrated into a design procedure which checks the stability and sensitivity of the total system and its overall performance. The test of the algorithm is outside its formulation and needs a good understanding of the system.

The properties of the algorithm are often less important than the quality of the clues it can provide and the way it integrates with the designer's knowledge, experience, and intuition.

But modern control literature is not written this way. The unsuspecting reader gets the impression that he really deals with a straightforward design algorithm. Even as great an expert as Rosenbrock attacks optimal control on philosophical grounds; that is, he heads in a direction that minimizes the intellectual contribution of the engineer. On the other hand we heard a repeated claim at this conference that successful use of optimal control requires too much of a theoretical knowledge.

Personally I don't worry about algorithms or computers eliminating the engineer. Complex design algorithms need a much higher degree of intellectual input than present methods and increase the need for highly trained personnel. I feel Rosenbrock attacks an image that modern control literature projects more than a reality. The real problem is that in the present state modern control theory is not easily integrated with the way an experienced engineer designs a control system. We have mathematically become so complex that even professors have stopped understanding each other. What we need is to translate the results of modern control theory into the language of the practicing engineer and to present the insights obtainable in a simple form. When results and insight are presented in a simple form, they often look obvious, but this does not detract from their value. It simplifies them.

For many purposes this is definitely possible. The work that Prof. MacFarlane talked about at Pacific Grove, California is a prime example of what can be done to translate the work done in one method to other mathematical languages familiar to the engineer. Morton Denn showed that a PID controller can be obtained from an optimal formulation. Our own work at present deals with this problem, and I'll mention here just two items.

Consider, for example, the case of a simple single-loop controller for an overdamped system, with no inverse response. In most cases it is sufficient to model this by a first order or second order system with a delay in series.

$$G_p(s) = \frac{e^{-\theta s}}{1 + \tau s} \tag{1}$$

or

$$G_p(s) = \frac{e^{-\theta s}}{1 + 2\tau s + \tau^2 s^2} \tag{2}$$

If we design an unconstrained deterministic optimum controller for Eq. (1) we will get a controller of the form

$$G_c(s) = \frac{e^{-\theta}(1 + \tau s)}{1 - e^{-\theta(1+s)}} \tag{3}$$

which is really a proportional controller with a dead time compensator very similar to the Smith dead time compensator. The system is in practice unstable as a small change in $G_p(s)$ will lead to instability. We can make it stable by constraining the control effort, but any experienced engineer will reject the controller because his experience tells him he does not want a proportional controller with a small gain and a dead time compensator.

Somehow we have to make an attempt to bring engineering back to our research. Nowhere is this more felt than in theoretical engineering and especially in control.

Using Eq. 2 for the model will add derivative control action. There are several ways in which we can force the algorithm to give us integral action. One given by O'Connor and Denn [6] uses constraint on the derivative of the control.

Denn also showed that by using a Pade approximation for the delay we will get a simple PID controller and that a suitable constraint will even lead to controller settings very similar to that obtained using the Ziegler-Nichols method.

Unless we use very complex stochastic formulation for the structure of the inputs, optimal algorithms will always end up in controllers similar and equivalent to those already is use, a combination of P, I, and D control with a dead time compensator and a smoothing filter. In that sense optimal control has neither led to any surprises nor to a design algorithm. In all cases we have to evaluate the results in terms of stability, sensitivity, and overall performance, and adding more criteria is only doing the same thing in an inverse way.

This does not mean the results are not very interesting. The fact that we know our empirical controller is very close to some clearly defined unconstrained optimum is very useful. Furthermore, we can get clues on proper design and tuning of dead time compensators.

On the other hand optimal control made some very significant contributions to the design of sample data controllers for the same case. I am referring here to the work of Box and Jenkins on control strategies suitable for human operators.

Take for example the above case. A simple suitable discrete model for the same process could be

$$G_p(B) = \frac{W_0 - W_1 B}{1 - \delta B} B^{k+1} \tag{4}$$

In their notation the output of the process Y_t can be written

$$Y_t = G_p(B) u_t + N_t$$

where N_t is the disturbance (or noise).

Box and Jenkins have an elaborate procedure to identify the input using nonstationary models for the noise. For most cases they recommend a noise of the form

$$N_t = \frac{1 - \lambda B}{1 - B} \alpha_t \tag{5}$$

Actually as McGregor [9] has shown this system is equivalent in the state space description to the following system

$$\begin{bmatrix} X_{1,t+1} \\ X_{2,t+1} \end{bmatrix} = \begin{bmatrix} 1+\delta & 1 \\ \delta & 0 \end{bmatrix} \begin{bmatrix} X_{1,t} \\ X_{2,t} \end{bmatrix} + \begin{bmatrix} W_0 \\ W_1 \end{bmatrix} \Delta u_{t-k-1} + \begin{bmatrix} 1-\lambda \\ \delta(1+\lambda) \end{bmatrix} \alpha_t \tag{6}$$

$$Y_t = [1\ 0] \begin{bmatrix} X_{1,t} \\ X_{2,t} \end{bmatrix} + \alpha_t$$

For an example, we will choose $\tau = 1$ and $\theta = 0.5$, and the sampling time T equal to 0.25. An unconstrained optimization will give us the following results ($\lambda = .5$)

$$u_t = -.5\ (\Delta u_{t-1} + \Delta u_{t-2}) + 2.26\,(\epsilon_t - 0.78\epsilon_{t-1}) \tag{7}$$

where u_t is the control action. Δu_t is the adjustment in control action and ϵ is the diviation of the measurement from the desired value.

This is a simple controller which uses just two measurements and two previous control actions. However, it can be rewritten in a different form.

$$u_t = -(1-\lambda)(u_{t-1} + u_{t-2}) + \frac{1-\lambda}{W_0} \left[\epsilon_t + (1-\delta) \sum_{i=1}^{t} \epsilon_{t-i} \right]$$

which shows that this is really a PI controller with a simple dead time compensator. The real value of this work is that, with a very simple strategy which an operator can easily handle, we can approximate a sophisticated controller. Furthermore, by adjusting the coefficients of these four numbers we can even include a filter or a lead compensator. The approximation is very good and even has some advantages as it avoids, for example, integral saturation.

But it is not straightforward. We note that the gain, as well as the coefficient of the compensator, depends on λ. Theoretically, the noise parameter λ can vary between -1 and $+1$. But only values between 0.5 and 1.0 will give controllers with acceptable stability margins for the gain. For others we will again have to constrain the control action to achieve stability, and if we look at the constrained controllers they are not sufficiently different from each other to justify any strong efforts to differentiate between them.

Evaluating the designs for different λ and even for more complex structures of noise gives very interesting and illuminating results, but the final design must take into account the proper stability margin, which is not part of the algorithm. In many cases stability will be the overriding final constraint; in others the structure of the noise might be more important. As this is not a lecture on controller design, I will refer you to our original paper [7].

It is true that in some sense the results of Box and Jenkins can be obtained both from classical theory or from the state space formulation. But this is hindsight. It is hard to guess that a noise structure such as in Eq. 6 is really one of the few that gives a good industrial controller. Nor did anyone else come up with such simple effective controllers for operators. But once we have them there is an advantage to translate them to a more familiar language.

This as an example of a really unforeseen result of optimal control that can be translated to the language most control engineers are familiar with. People with a background in quality control will prefer the original formulation. People with a long experience in classical process control will prefer to talk about dead time compensators, PI controllers, phase lag and phase lead compensators, and filters.

SUMMARY

THERE ARE PROBABLY many really valuable results hidden in the literature of modern control that merit being brought to a form useful for the control engineer. But we need to extract them, test them, and bring them to a form where they are useful tools in real empirical design.

The academic world is probably the only one that could do it and publish it, but we need not only people who are ready to do it but also some change in emphasis and value judgment in the academic community, especially in the U.S.

A thesis like Kurihara's is not exactly the prime example of what we value. It contains no rigor, no experiments, and no new theory. If he had spent five years and built a small FCC unit and put a trivial controller around it, at least part of our academic community would have admired it. It would have been rather useless, since it is very hard to build a small FCC with the same dynamic behavior. In real design we would use simulation anyway, and rigor would not help us since this is not our problem. What would have helped us if we would have pointed out what was wrong with his results. Very few students would today dare to do it.

This is sad. The value of theoretical work in industry as well as in scientific work is much greater in the failure mode than in the positive case. If a good sensible theory fits the data or vice versa, we learn rather little, especially if the theory is known. An experienced theoretician can guess the form of the result even without solving it. But when a reasonable theory leads to strong contradiction with experiments or our experience we learn something.

I learned this the hard way. When I started, one of my first students studied non-Newtonian liquid-into-liquid jets. We solved the equations for the power law fluid and were quite proud and tried to confirm them. Our first experiments showed some very strange effects, totally in contradiction of what theory predicts. We dutifully recorded them and finally found a set of narrow conditions where the experiments agree with theory. If I had had the sense to concentrate on the strange effects, I would have had a first rate pioneering paper instead of a rather standard one. But I learned my lesson. When we studied atomization of non-Newtonian fluids, we had a very solid linearized stability analysis for any fluid and were able to show that there are fluids for which the linearized theory does not apply.

We therefore have to create an interface between the industrial practitioner and the rigorous researcher, and the only way I can see it is to start working on the fundamentals of our profession—trying to obtain an understanding of the design process itself, which never really is algorithmic but rather interactive and intuitive and strongly relying on informed judgment.

We have boxed ourselves in so much with preconceived notions about how a good paper or thesis should look that real engineering research becomes rather hard. This is strange. Even the hard sciences or mathematics feels less constrained as to what a paper should look like than we do. And there is no part of engineering where people are as ferociously prejudiced and constrained as in the academic control field in the United States.

I admit the problem is not easy. A thesis like Kurihara's or Kestenbaum's [5] is much harder to judge and evaluate. The same applies to any work dealing with dirty problems and with ill-defined notions such as design. Furthermore, when complex results are translated into simple language, they often sound obvious and, to those without experience, sometimes trivial. But we are engineers with all the advantages and disadvantages, and fleeing into sterile mathematics does not solve anything. The relevance of such work is just as hard to judge. Nor does such work necessarily make the best preparation for a student's career.

We therefore have to create a climate in which such work can flourish. We also need to create a basis of financial support for it. Research on servomechanisms is supported by NASA and DOT, but real process control, just as most research on process design, has no home either at NSF or any other agency and very meager industrial support. This is again purely a question of the intellectual climate. The needs and potential for significant improvements in process control are at least as big as those in many areas which have ample support.

Let me make one thing clear. I do not want to imply that what I outlined is the only research or even the main research control engineers should do. In process control we suffer already far too much from preconceived notions of what the only present thing to do is, and I do not want to add to this. Sound rigorous theoretical work and well-conceived experimentation can make significant contributions to modern control. But the nature of the problem is such that, unless we obtain a better understanding of the design process itself, many of the most valuable units of our work will remain useless, and some of our theoretical work will go into directions where no real need exists. We therefore have to create an interface between the industrial practitioner and the rigorous researcher, and the only way I can see it is to start working on the fundamentals of our profession—trying to obtain an understanding of the design process itself, which never really is algorithmic but rather interactive and intuitive and strongly relying on informed judgment. It will be a difficult but interesting and gratifying task.

Let me finish with another story relevant to the present state of research in the engineering profession. I read once a strategic analysis of the Maccabean War, an important event of Jewish history. The analyst showed that Judah, the Maccabean, was a military genius, the inventor of guerilla warfare, the first to be able to handle the Greek phalanx. But having beaten the Greeks in a historic battle, he forgot his lesson. He really dreamed of becoming a Greek general leading his army in a phalanx. Doing that he was sadly beaten. His brothers followed his first lessons, which led to final victory. I do not want to elaborate on this example. □

NOTATION

α_t = white noise variable
B = backward shift operator
$G_p(B)$ = plant discrete transfer function
$G_p(s)$ = plant continuous transfer function
k = defined by $\theta = k \cdot T + c \cdot T$ (k is an integer)
δ = defined by $e^{-T}{}_t$
λ = noise parameter
X_t = state vector
Y_t = output
τ = filter time constant [Eq. (1)]
θ = time delay [Eq. (1)]
ϵ_t = deviation of output from setpoint
u_t = control action at time t
T = sampling period
$W_0 = 1 - \delta^{1-\lambda c}$
$W_1 = \delta - \delta^{1-\lambda c}$
$c = \theta/T - k$

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TWENTY LECTURES ON THERMODYNAMICS

By H. A. Buchdahl, Pergamon Press, 1975

These twenty lectures present a coherent, bird's eye view of phenomenological and statistical thermodynamics. According to the author they are largely elementary in character, pedagogic in purpose and proceed in a way, which here and there, "allows physical intuition to take precedence over mathematical niceties". Nevertheless the text is abstract and mathematical. Some readers may prefer other approaches. □