

RESEARCH LANDMARKS FOR
CHEMICAL ENGINEERS*

NEAL R. AMUNDSON
University of Houston
Houston, TX 77004

MATHEMATICIANS AND PURE scientists, as contrasted with engineers, are normally much more aware of the history of their disciplines and who did what when. Engineers and, in particular, chemical engineers (except those with a special interest in the history of technology) appear to know little about the roots of their profession. This is probably not a remarkable fact since the research of the vast majority of engineers is more related to problem solving than to the elucidation of fundamental ideas and principles. There are certainly great exceptions to this notion. It is the purpose of this effort to mention papers which have been landmarks for me personally since they are things to which I have given a good deal of thought. Their authors may not have been the first who considered the problems about which they wrote, but, in my view, they are the ones who had the greatest impact in their areas of the profession. Many of us are guilty of the secondary reference, assuming that this author gave due credit to the primary investigator. Soon the name of the person who originated the idea (or even the first to exploit it) is lost. In the engineering literature the same problem frequently arises in many different guises: for example, in heat transfer, mass transport, potential theory, etc. It does cause some consternation when attempting to give credit priority. No attempt is made here to be exhaustive, and the reader will probably find many omissions that are more interesting and more important to him.

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*This paper is a part of the lecture given as one of the Phillips Petroleum Company Lecture Series on April 12, 1985, at Oklahoma State University, Stillwater, Oklahoma. Portions were also given at the Peter V. Danckwerts Memorial Lecture in London on May 12, 1986.

LANDMARK PAPERS

One of the problems I have been interested in for some time is how char or carbon burns. In 1972, in a fit of patriotism and good will, we embarked on a program of coal gasification and combustion—at least, that is what I thought we were doing. We soon discovered that we did not know much about what happened to single particles of char when exposed to an ambient atmosphere containing oxygen, carbon dioxide, carbon monoxide, and perhaps water. If one forgets about water, everyone knows that there are three main reactions among carbon, carbon monoxide, oxygen, and carbon dioxide. The simplest undergraduate problem is to suppose that the carbon is impervious, is spherical, and is surrounded by a stagnant boundary layer or film. The question then involves what happens at the carbon surface and what happens in the boundary layer itself. Stated with this degree of simplicity many researchers have thought about the problem and indeed there are three landmark publications from which all other research and engineering on the subject derive.

Nusselt [1] in 1924 considered the simplest model in which he assumed that carbon reacted with oxygen, with the product CO diffusing through the boundary layer without reacting. The heat generated was also conducted through the film, and the whole process occurred in a quasi-steady state, which is just another way of saying that the lifetime of the particle is long compared with other transient processes. This gives a mathematical model which is essentially algebraic in character. Clearly it is limited since it neglects two of the reactions. Burke and Schumann [2] considered a superficially similar model in which they assumed that the only reaction which occurs is also between carbon and oxygen but produces carbon dioxide which must diffuse out. We say superficially, for this reaction is a two-step one, the first being the production of carbon monoxide and the second the oxidation of carbon monoxide to carbon dioxide. In a later paper Burke and Schumann [3] made the significant extension to what has been called a two film model, in which the



Neal R. Amundson is the Cullen Professor of Chemical Engineering and a professor of mathematics at the University of Houston. He received his BChE (1937), his MS (1941), and his PhD (1945) from the University of Minnesota, where he also served as a faculty member from 1939-1977 and head of chemical engineering from 1949-1974. He is the author of numerous papers and six books, including *First Order Partial Differential Equations*, Vol II, still in press. He has been the recipient of many awards.

carbon monoxide formed at the surface reacts with oxygen as it diffuses to the ambient. In order to make the problem simple, they assumed that the carbon monoxide oxidation occurred at a sharp interface where the reaction stoichiometry could be met. Clearly this position is a parameter of the model depending upon the other reaction and ambient parameters, and, as these change, the position of the flame front shifts.

Now these three models are extremely simple, but their depth is deceptively obscure until more complicated problems are investigated. Suppose these three simple models are computed by varying only the ambient temperature but holding all other parameters fixed. Then the particle temperature is determined by the ambient temperature, and a locus in the plane of particle temperature versus ambient temperature is obtained. The result of the computation is that these three loci confine and define a finite area with asymptotic regions at very high temperatures and at very low temperatures. What makes these three papers important is the following: Suppose that one considers the more rational model by assuming all three reactions occur and that the carbon monoxide reaction is distributed through the boundary layer—then the system is a set of four non-linear differential equations with non-linear boundary conditions for transport of energy and mass. If these equations are solved holding all parameters fixed except the ambient temperature, one can show with ease that the solution for a fixed ambient must lie inside the region defined above by the three simple models and the whole locus of solutions for variable ambient must lie inside the re-

gion, entering the region at the low temperature asymptote and leaving at the high temperature asymptote. Thus the three simple models are limiting solutions in the real sense and define a feasible region of solutions giving important bounds on the burning behavior.

In the field of reactor engineering probably no object has received as much attention as the continuous stirred tank reactor. The reason for its popularity in undergraduate courses is its simplicity. In the steady state it is described by a simple algebraic equation or equations, while its transient is described by an ordinary differential equation or a system of equations. Thus one can illustrate its behavior with relatively simple machinery, and its pedagogical importance cannot be overestimated. There are not many problems involving reactors under varying temperatures that can be studied without a good deal of pain.

Mathematicians have discovered the stirred pot, and the pathology of the system has now been studied in excruciating detail, and the methods of modern differential topology, in particular, singularity theory, have been employed to study the structure of the

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steady state solution space. In very recent times the problem for general reaction systems and reactor configurations has been considered, and problems have been solved which I never thought possible just a few years ago. How did all of this come about?

In 1935 there appeared a paper by MacMullin and Weber [4] in the old *Transactions of the AIChE* with a long title but which started out as "The Theory of Short Circuiting in. . . ." This reference to short circuiting is the well-known fact that there is always a non-zero probability that a molecule which enters the reactor will leave without reacting unless the reactions are instantaneous. This is a deficiency of the reactor and is a result of the mathematical by-passing because of the well-mixedness assumption. This was the first instance, as far as I know, where the C^* was considered, and it is a strangely out-of-place paper for the times. It is an extremely lucidly written theoretical paper which considers single continuous stirred pots and staged pots for reaction systems of various orders as well as for dissolution of solids. While the content is certainly not 1986, the style and presentation would certainly be acceptable today as a theoretical paper. This material is now standard undergraduate fare, but I suggest that it was an eyebrow

raiser in 1935. Recall that chemical engineering did not become mathematically oriented until the 1960's.

This paper lay fallow for almost ten years when K. G. Denbigh [8] (1944), while involved with Imperial Chemicals Industries during the war, became interested in chemical reactors, and in particular with the comparison of yields in batch, stirred pots and tubular systems. Denbigh was the first to my knowl-

Our aim here is not to review the literature but to point out that it was K.G. Denbigh who started it all by a casual remark at the Campus Club lunch. There should be more Campus Clubs and Denbighs to visit them.

edge to discuss in a rational way what became a favorite pastime some years later and is now a part of almost every textbook on applied kinetics and reactor engineering. This paper was followed by one in 1947 which extended the first but considered several different kinds of polymerization mechanisms using various polymer statistics. While the MacMullin and Weber paper predated Denbigh's by ten years I think that the world was ready for Denbigh, and his had substantially more impact.

We should not leave Denbigh here, for it was through his influence while a visiting professor at the University of Minnesota in 1953-54 that he introduced me to a certain optimization problem. At lunch one day, Denbigh mentioned the possibility of applying a temperature gradient along a tubular reactor in order to improve the yield. There is no difficulty with a single endothermic reaction, for then one should run it at the highest temperature consistent with other constraints. With an exothermic reaction, however, the front end can be run hot to increase the forward rate while the rear end must be run cold to suppress the reverse rate. It is not difficult for this case to compute, for a given length of reactor, what the temperature profile should be in order to maximize the yield of the product. This is almost a trivial problem in the calculus of variations. However, for consecutive and simultaneous complex reaction systems the problem is far from trivial and has been examined subsequently by a host of researchers. Our aim here is not to review the literature but to point out that it was K. G. Denbigh who started it all by a casual remark at the Campus Club lunch. There should be more Campus Clubs and Denbighs to visit them.

One of the popular topics in chemical reactor engineering is that of the study of intraparticle effects in catalyst particles. Since the internal surface area per unit of volume is so great, most of the reaction takes place on the internal porous surface. This in a

sense slows things up since the internal surface is not as readily accessible unless all of the surface can be made available. Every chemical engineering student knows that if he wants to determine how effective a catalyst particle is, he must know the Thiele modulus. From relatively simple plots resulting from not too difficult equations he can determine the effectiveness factor of the particle, which is the ratio of the actual conversion to that obtained if all of the surface area were available for reaction at the ambient condition. This problem has probably generated more research papers than any other single topic in the last twenty years. Generalizations to complex reaction systems, non-isothermal particles, effects of poisoning, optimum catalyst profiles in particles, etc., have occupied many, many researchers. This was all started by E. W. Thiele [9] a long-time employee of Standard Oil Company of Indiana, professor at Notre Dame, retired, and very much alive at age 90. His birthday anniversary was celebrated at the AIChE meeting in November, 1985, in Chicago. Thiele published a paper in *Industrial and Engineering Chemistry*, 1939, on the "Relation between Catalytic Activity and Size of Particle." This rather short paper had a remarkable effect in the United States, although it must be said that Damkohler [10] and Wagner [11] in Germany and Zeldovich [12] in Russia worked on almost exactly the same problem with similar results at almost exactly the same time. Thiele not only considered the slab catalyst but also the sphere and did the slab for a second order reaction that leads to the use of elliptic integrals. To his great prescience he also considered the case in which the volume change on reaction is large, still not an easy problem for most students although this material is standard in course work.

The name of Thiele should be familiar to all undergraduate chemical engineering students from their introduction to the McCabe-Thiele [13] graphical method for computing binary distillation in plate columns. McCabe and Thiele developed this method while they were graduate students! Thiele's name is also associated with that of Geddes [14] in connection with a method of calculation for multicomponent distillation. Not many chemical engineers have their names associated with three significant problems.

Of primary interest to chemical engineers is the packed bed reactor, that is, a tube or large cylinder packed with a solid packing material consisting of an active catalyst on an inert carrier. Its widespread use results from its simplicity of construction and operation, although for highly exothermic reactions the dissipation of the heat generated may be troublesome. The packed bed reactor is particularly interesting from an educational point of view since there is a vast

hierarchy of models which may describe it, depending upon the parameters of the system and the desires of the engineer. The simplest model is the equilibrium model in which it is assumed that the bed is in local equilibrium at each point. This is not very realistic but does give some valuable information about the structure of the transient solution, although it has been little considered in the literature.

The next simplest model is the one analogous to heat transfer in an adiabatic bed neglecting everything except the heat transfer resistance at the particle surface. This would conform to the reaction case in which a first order reaction takes place inside a porous particle with rapid intraparticle diffusion and with a rate limiting mass transfer resistance at the particle surface, certainly not a model with great applicability, but non-trivial none the less. The analogous heat transfer problem was solved by Schumann [15], the same of single particle combustion fame, and was hailed at the time as being of inestimable value in the iron and steel industry. Schumann's solution has been rediscovered and republished in other contexts several times for adsorption, ion exchange, isotope exchange, and even in heat transfer.

The landmark for reactions in tubes, the tubular reaction case, was a series of papers by Gerhard Damkohler [16-20] which appeared in the *Zeitschrift für Elektrochemie* in 1936. There are five in all, and these should have laid the foundation for the continuous distributed models for non-isothermal tubular and packed bed chemical reactors. These papers were all but ignored in the United States except by a few and are still referenced only but slightly. They investigated the various dimensionless groups involved and the problems of obtaining similarity conditions in scale up. Extensive computations and comparison with experiments were made long before most in the U.S. knew that there was such a field as chemical reaction engineering. These are truly exceptional papers and show, I think, the power of the German system pre-W.W.II. Damkohler was in the Physikalisches Chemisches Institute at the University of Goettingen, the center of German and world scientific inquiry and activity at the time.

Needed greatly in the use of models for fixed bed reactors are the many parameters. We assume that in catalytic reactors, packed (say) with spheres, that each sphere is in a smooth homogeneous field—that is, that the sphere sees the same homogeneous field

in all directions. This allows us to treat the sphere as being radially symmetric. We treat the homogeneous flow field as a continuum with constant average velocity, superimposed on which is a radial and axial mixing process. Experiments have amply illustrated that these mixing processes occur and they seem plausible intuitively. The question which arises is, what is a convenient mathematical formulation for the mechanism of these dispersions. R. H. Wilhelm and his graduate students, although there were some others, essentially solved this problem in the early 1950's in a series of classic papers [21-23]. One considers for the packed bed that the spheres form a three dimensional array. The intracellular spaces among the spheres are considered as mixing cells connected by narrower passages between and among spheres. Thus the interstitial regions in a packed bed may be considered for high Reynolds' numbers as an array of well-mixed vessels connected by conduits. A molecule entering one cell must move laterally in order to move forward to new cells as it moves through the bed since a particle will always block its straight-through passage. Note here that we are considering cells connected only axially, not directly radially. But since the particle arrays are staggered there will be a net radial movement as axial movement proceeds. Thus the movement through the bed is a random walk, and the probability that a molecule is in a given cell at a given time can be computed. It is well known that in the limit a random walk is a quasi-diffusion and so one assumes that the radial dispersion is a Fickian mechanism, although the dispersion itself is not concentration gradient driven. It is driven by the convective flow. Thus one should be able to define a radial dispersion coefficient (one hesitates to say a diffusion coefficient) for a Fickian model which depends only upon the local geometry, particle size, and interstitial velocity. Wilhelm carried this through in an elegant way and showed theoretically as well as experimentally that the radial Peclet number (ud/D) is about eleven.

The analysis for axial dispersion can also be done by probability arguments and the Poisson distribution obtained becomes in the limit by the central limit theorem a normal probability distribution superimposed on a mean convective flow. The normal probability distribution is similar to the fundamental solution for the diffusion equation, so we are back to a Fickian formulation. This time the Fickian mechanism

is not so clear since experiments done by Hiby [24] show that there is no back mixing—clearly a flaw in the logic, although the probability argument precludes back mixing. Nonetheless, the axial Peclet number can be shown to be about two for high Reynolds' numbers both experimentally and theoretically, certainly a remarkable and, in my view, a fortuitous development. This analysis is now a standard treatment in reactor engineering courses, but the axial dispersion part has these theoretical deficiencies which have been refractory to improvement thus far.

Those of us who have taught chemical reaction engineering or elementary partial differential equations always get into a little trouble when discussing the appropriate boundary conditions for the tubular reactor, empty or packed. The flux condition at $x = 0$ and the zero gradient at the exit are more or less force-fed to the student. The appropriate formulation is difficult, and most of the time the formulation, discussion, and solution are erroneous and not consistent with the actual engineering geometry of the inlet and outlet. Papers appear periodically and often but the resolution is non-trivial for a rigorous treatment. Nevertheless we use the conditions above, the so-called Danckwerts boundary conditions introduced to chemical engineers by Peter Danckwerts [25]. The parochiality of the chemical engineering profession is probably no better illustrated than by the fact that Irving Langmuir [26] in 1908 wrote a remarkably lucid paper which appeared in the *Journal of the American Chemical Society* (JACS to all) in which he considered the empty tubular reactor with axial dispersion and reactions of arbitrary order using the Danckwerts boundary conditions, ten years or more before Danckwerts was born!

We should not leave the name of Danckwerts on that note for he was one of the greats in England from 1945 until his recent death. Most of his professional career was spent at Cambridge and his papers [27-34] are notable for their inventiveness. He was among the first to consider problems in reaction and absorption, and these appeared in the *Transactions of the Faraday Society*. In addition, a monograph which is a classic on absorption and reaction appeared in 1970 and was called *Gas-Liquid Reactions* [34]. During his whole career he was involved with the film and penetration theories of absorption.

The paper mentioned above [25] contained one of three ideas presented by Danckwerts in his celebrated paper in 1953. In that paper he introduced the idea of a residence time distribution for chemical reactors. The use of the experimental residence time distribution when compared with the theoretical distribution has become a standard diagnostic tool in industry

when a mal distribution of fluid flow is suspected as a cause of poor yield.

The third idea in the paper was to compute the residence time distribution in a packed bed reactor using the standard axial dispersion model for the bed. Results of these computations were compared with experiments, and, using his numbers for the parameters, one can show that the axial Peclet number, which he does not mention, is equal to 2.4! This work is referenced in a passing way by McHenry and Wilhelm but it is clear that Danckwerts had the clue to the idea that for high Reynolds number flows the axial Peclet number is a constant.

One of the earliest papers in chemical engineering in the old *Transactions of the AIChE* which caught my fancy was that of T. B. Drew [35]. This is one of a series of three papers on convective heat transfer. The first appears under the name of Drew alone and has the title "Mathematical Attacks on Forced Convective Problems: A Review." This treats for some 55 pages, as the title indicates, mathematical solutions of heat transfer problems in streamline flow for various geometries and is as out of place there as was the one previously mentioned of MacMullin and Weber. It is followed by two other papers with other authors in addition to Drew, and the total extends through 132 pages—probably a record. The first is required reading for anyone interested in applied mathematics and convective heat transfer in streamline flow.

Most of us would not think of G. I. Taylor [36,37] as a chemical engineer, but in his unique style he published two magnificent papers that many people wish they had written. Sir Geoffrey became interested, in the late forties and early fifties, in how solute in a tube dispersed and, as was his manner, made the typical simple analysis guided by a superb intuition and validated by just the proper experiments. He was concerned with finding a simple formalism for radial dispersion in a fluid flowing in a circular tube in both streamline and turbulent flow. The key result, as everyone knows, was to find an equivalent axial dispersion which would account for the radial dispersion. With the kind of insight only he possessed, he did this giving a simple formula for the dispersion coefficient which depended only on known fluid parameters. This paper was followed by a second in the *Proceedings of the Royal Society*, on the same problem for turbulent flow. The young researcher should read these as an aid in learning the craft of model building and the value of the simple experiment.

Every student in chemical engineering now takes it as well known that there are analogies between fluid friction in pipe flow, heat transfer to the wall, and mass transfer. That this is a relatively modern idea

would come as a shock. A. P. Colburn [38], in a classic paper in 1933, titled "A Method of Correlating Forced Convectional Heat Transfer Data and a Comparison with Fluid Friction," was the first to point out that for high Reynolds' numbers a plot of the friction factor versus Reynolds' numbers looked a lot like a plot of the heat transfer coefficient versus the Reynolds' number. The analogy between heat transfer and momentum transfer was born in this paper in the form now standard in all transport courses through the j factors. In 1934 Colburn collaborated with T. H. Chilton [39] while both were employed by the DuPont Company and published a seminal paper on the analogies among heat transfer, mass transfer, and momentum transfer, again through the j factors. It is impossible to overemphasize the importance of these two papers and their effectiveness in aiding the design engineer. Colburn was another one of the truly great chaps of our profession, and after a long and successful career at the DuPont Company, he initiated and organized the very successful chemical engineering program at the University of Delaware.

Those of us who have had more than a casual interest in the application of advanced mathematical ideas to chemical engineering problems have always decried the fact that while there is a plethora of books on applied mathematics, none is really suitable for courses for chemical engineering graduate students. I mean this in no parochial sense, but any unbiased examination of the extant books makes this abundantly clear. What is not known is that the beginnings of a beautiful text were made in 1947 by Robert W. Marshall [40] (a PhD student of Olaf Hougen) and Robert L. Pigford (whose mentor was A. P. Colburn), based on an extension course given at the University of Delaware in 1945 while both were employees of the DuPont Company. The preface of *The Application of Differential Equations to Chemical Engineering Problems* says the book covered fifteen lectures, and a very respectable course based on this book could be offered today, almost exactly forty years later, since the topics were well chosen and are still relevant.

EPILOGUE

This paper is sort of a quasi-history of chemical engineering research and it might be useful to place it in proper context with the chemical industry and chemical engineering education. The latter really began in the early nineteen twenties when most of the better known departments were founded. At that time there was little petroleum refining and the chemical industry was Germanic in character. The bulk chemicals were the inorganic acids, salts and alkalis,

dyes, explosives and the simple organic molecules mostly derived from coal byproducts. There were no plastics or polymers to speak of, no antibiotics, and no synthetic rubber. There was little reason to do much engineering as we know it now.

When the automobile exploded onto the scene and catalytic processes were developed, it was essential to do more engineering, and the continuous process rather than the batch process became the *sine qua non* of chemical engineering (and still is). Now it was necessary to do engineering. Heat exchangers had to be designed, so chemical engineers took up heat transfer since the design engineer required a heat transfer coefficient. He certainly consulted McAdams' book on *Heat Transmission* [41]. The need for separations in the petroleum industry and the accompanying need for vapor-liquid equilibria and thermodynamics of light hydrocarbons were important and commanded the attention of many in academic research. As mentioned earlier, up to W.W. II there were almost no catalytic processes—contact sulfuric acid, yes, and ammonia synthesis and a few others. But these were not designed—they were borrowed from the Germans. The explosion in catalysis and polymer chemistry resulting in synthetic fibers post-W.W. II changed the whole outlook of the chemical and petroleum industry and gave academic chemical engineering a quick fix, and the problems requiring solution were interesting and fun to work on. It was not difficult to be enthusiastic about chemical engineering. The new fields in the fifties excited young people.

Catalysis, as well as polymer processing, in a sense have almost been taken over by chemical engineers. Reactor engineering is a mature subject now. Heat transfer research has been appropriated by mechanical engineers as a discipline. Little is done now in conventional separation processes although it is necessary that this field be rejuvenated and expanded, for the new biological processes will require different techniques. We are now at a new point when chemical engineering as a discipline is faced with manifold problems instigated largely by the uncertain fate of the petrochemical industry in the U.S. and Western Europe and by the slow-down in the chemical industry. The kinds of things which occupied us in the past—transport, reaction engineering, separations and the like—had direct and easy application in the conventional chemical industry. The loss of control of raw materials in the U.S. will undoubtedly force us to shift our emphasis to other endeavors such as materials, biotechnology, exotic or specialty chemicals, etc. The principles we have been brought up on are still the ones which will be needed, although we will need

Continued on page 192.

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RESEARCH: Artificial Intelligence

Continued from page 185.

information. Nevertheless, it can be upgraded and used within the context of specific problems. Thus

- Accumulated data from past process designs (implemented in real life or not) can be upgraded to reveal the underlining patterns present in all similar flowsheets, as well as the sources of difference among different flowsheets.
- Analogous pattern recognition could reveal rules aiding the synthesis of control structures for complete plants.
- Extensive data on vapor-liquid, vapor-liquid-liquid equilibria (e.g., DECHEMA series of experimental data) could be used to identify patterns between molecular structure and infinite dilution activity coefficients, etc.

Therefore, the new prototype of an intelligent system should contain rudimentary capabilities of "learning" through a pattern recognition facility among large sets of accumulated data.

EPILOG

Artificial intelligence is expanding the scope of our problems and is enriching our capabilities to deliver viable solutions to otherwise hard and resistant problems. At the same time it is introducing new educational challenges that the research program at the MIT-LISPE is attempting to address and which are related to the computer-aided character of chemical process engineering, the rationalization of the man-machine interaction, and the role of fundamental science in engineering. Our research so far has produced more questions than it has answers, but the intellectual excitement and practical relevance have just started to permeate the programs of graduate research in chemical engineering.

ACKNOWLEDGEMENT

The research program described above is the work of, and rests completely on the efforts of, my graduate students: Kevin Joback, Charles Siletti, Michael Mavrovouniotis, Jim Johnston, Rama Lakshmanan, Theodore Kritikos, Nikos Valkanas, Jarvis Cheung and John Calandranis, whose dedication is making "things" possible. □

RESEARCH LANDMARKS

Continued from page 173.

to become more expert in surface physics, continuum mechanics, microbiology, biochemistry, large molecule chemistry, and other things I wish I could predict. We must change our educational perspective to include new things. For years, ever since I was a freshman in 1933, we have trained students as if they all were going to work for the DuPont Company. This was appropriate. The principles are no different for the future than they were for the past but we must find a new way to talk about chemical engineering if students are to be re-excited. We in chemical engineering have a marked advantage over all other engineers—we are the only ones who know anything about chemistry—an advantage we should work on diligently to parlay into future success.

When I was a young chap at Minnesota there were almost as large a number of students in the metallurgy department as there were undergraduates in chemical engineering. Not long after that, metallurgy disappeared as an undergraduate discipline and it took almost twenty-five years for materials science to emerge from the metallurgy grave. We must be sure that we do not allow a similar fate to befall chemical engineers.

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REVIEW: Grad Education

Continued from page 181.

Engineering Education (Goals Study). The present study summarizes the data from these reports and updates this information to 1983 with many informative tables and graphs. For example, information in this survey includes the BS, MS, and PhD degrees awarded in engineering since 1950, the most recent engineering degrees by field and level, the PhD employment of engineers since 1960, the number of foreign born awarded advanced degrees in engineering since 1970, the changes in the student-to-faculty ratios over the last decade, the average monthly salaries offered to new engineering graduates by field since 1965, the women and minorities obtaining degrees in engineering since 1978, the average research investment per PhD degree for the top thirty ad-

vanced degree granting institutions, and a comparison of the weekly professional activity of engineering research faculty with those of other disciplines. The gathering of these data in one place makes the monograph a valuable reference for all educational scholars and policy makers.

The data in this study are used to predict the number of PhDs that will be awarded in engineering during the 1983-88 time period. The conclusion from such a prediction is that on the average an additional 100 engineering PhDs will be awarded annually during this period and that this will be insufficient to fill the present faculty vacancies in engineering as well as restore the student-to-faculty ratio that existed back in 1976. The study argues that the latter is necessary if the United States is to meet the increasing competition from those foreign nations where the productivity growth has surpassed that of this nation during the past decade. Additionally, the study notes that each engineering discipline is facing many new challenges, some of which will be difficult to meet with the present number of overloaded faculty and deteriorating facilities, particularly when interdisciplinary aspects are involved.

Based on this premise, the study makes several recommendations. Not surprisingly, these recommendations are similar to ones voiced by many concerned engineering educators for close to a decade. Many of these individuals would agree that the number of U.S. citizens pursuing doctorate work needs to be increased (the study suggests 1000 additional students per year), the graduate stipends need to be increased to make graduate study more attractive, the facilities and equipment for research need to be upgraded, more minorities and women are needed in the graduate engineering program, stronger ties need to be developed between industry and engineering education, and a stronger MS program needs to be available for part-time industrial students to aid in maintaining their engineering competency. The study suggests that the Federal government, universities, and industry provide the necessary assistance where most appropriate.

Sadly, no new mechanisms or strategies are offered to make the needed inroads on these long-standing problems. There is little evidence provided to convince policy makers that the solution of these problems will once again make the United States competitive with other nations and reverse the present staggering trade deficits. In short, the study is a good summary of what has happened in engineering education over the past three decades, but it presents very few innovative ideas as to how the situation can be improved and the required investments justified. □