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SCIENCE

Fuel Conservation and Applied Research

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There is a consensus among professionals and laymen that applied research has a major role to play in fuel conservation, but this agreement has not developed into support by an effective constituency in the same way that there is support for space, defense, coal utilization, high-energy physics, and other technical programs. The way the American system works, such a constituency must "sell" the program before the American taxpayer—through the agency of his elected officials and civil servants—will provide the funding for it. We believe that applied research has not been understood well enough by enough people, and as a result it has neither done well nor "sold" well on the scale that is needed. One of the consequences is delay in the development of fuel conservation technology.

Although applied research directed to fuel conservation is being funded, in most cases the funds are allocated for "deliverable" products, such as an electric car, a "total energy system" for utilizing waste heat in a restaurant, industrial components such as fuel cells and turbines, and so on. For the most part each of these government-sponsored "hardware" projects independently fulfills only its own needs for applied research. A manager responsible for delivery of a specific piece of hardware rarely has the authority, and even more rarely the incentive, to fund applied research that supports hardware developments for which he is not responsible. Thus, if the scope of any applied research effort is determined solely by the needs of a single government-sponsored development, there will be little or no incentive to lay a sound base for an overall beneficial improvement in the technology resource. As a result, there is a paucity of funding for applied research that cuts across individual government-sponsored hardware projects and directly benefits technology developed outside the government [for example, see (*1*)].

In this article we discuss the importance of generic applied research with the aim of encouraging both the technical and political support of a program that would be appropriate and useful for the common good. To that end we describe a number of past, present, and future examples of applied research efforts related to fuel conservation and suggest an institutionalization of the means by which such efforts can be organized and utilized for an effective long-term fuel conservation program. Although fuel conservation in itself is ample reason for seeking the full benefits available from applied research, by using the example of fuel conservation we hope to achieve much more. The same approach can contribute to the attainment of other technologically related national and human goals such as reducing atmospheric pollution, increasing industrial productivity, conserving scarce minerals, and developing renewable sources of energy. A well-organized applied research program may well be the key to success in such endeavors, just as we show it to be a key factor in the achievement of effective fuel conservation.

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The Role of Applied Research in Conservation

Applied research is different from basic research. Even when the technical substance of the work is the same, the motivations are different. Applied research is motivated by the desire to improve technology in a specific manner, whereas basic research is motivated by the curiosity of the researcher. Basic research may have a profound effect on technology; but the magnitude, direction, and time of the effect is not predicted. Thus the justification of the budget for basic research cannot be subject to a conventional cost/benefit evaluation, whereas the budget for applied research should be.

Fuel conservation is an area in which applied research is particularly important. In contrast to the development of new energy sources or fuel extraction processes, the basic science and technology of processes for converting source energy into useful form are relatively well established. There is, however, a popular misconception that because these processes *are* well known, there is little or no need for major technical effort in improving their efficiency or their degree of utilization.

Nothing could be further from the truth.

It is, perhaps, useful to consider fuel conservation in three different stages:

1) The use of less fuel to do less work. This is the easiest stage to understand and to implement; for example, one can drive slower, turn off lights, and turn down thermostats. This is the stage most people consider to be dominant in conservation; hence the impression that little engineering effort (and commensurately little capital investment) is needed.

2) The use of less fuel to do the same work. This is just as easy to understand, but less easy to accomplish. It takes both a modicum of design and some investment; for example, one can add building

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insulation, install storm windows, use radial tires on automobiles, and change incandescent lighting to fluorescent. This stage of conservation is characterized by the intelligent manipulation of known methods, components, and information rather than the development or modification of technology.

3) The use of less fuel to do more work. This is the stage which offers by far the greatest quantitative opportunities for fuel conservation, but extensive applied research and development are needed to accomplish these major savings. Examples are topping or combined cycles, cogeneration, total-energy systems, and district heating. Although little new science or basic technical understanding are required, considerable refinement of that knowledge is needed so that it can be applied reliably, practically, and economically to the multitude of energy conversion processes used in our modern civilization. Such an endeavor will require much engineering talent and substantial capital investment.

A Case History from the Past

The development and improvement of electrical power generation provides a striking example from the past on the role of applied research. The basic laws of electromagnetism and thermodynamics were known; the added ingredient in this case was the discovery of the new methods to apply this knowledge, that is, invention. On the surface it could appear that the "project manager" (in this case the inventor) performed the necessary applied research to support his own product. The facts, however, indicate otherwise: there was no product; the goal of the inventor or discoverer was the applied research itself. Also, in many cases the initial "product" of this applied research was inferior to existing technology. The primary difference was that the inventor, through various analytical models or theorems, was convinced that the potential performance for his discovery would greatly exceed the performance of existing technology, and the efforts to improve the performance to become competitive after the initial invention were considerable. This was the area of applied research: the construction of a body of knowledge that was immediately manipulatable into the next design and test. On the other hand, the existence of so-called previous "successful" systems led to resistance to support new concepts which could (i) render the existing system obsolete or (ii) divert funds or resources from immediate problems of delivery or product improvement. This situation is similar to that which exists today.

A second factor, which is also similar to the circumstances operable today, is the source of funding for the applied research which was needed to bring a concept to the point of demonstrated superior performance. Such funding rarely came from the existing organization which could benefit the most. In fact, such funding generally came from external sources in the form of risk capital.

A third factor which has some similarity to today's situation was the role of government: it ordered the first of a kind. This is important, for the embryonic utility companies were reluctant to pioneer and risk possible bankruptcy through performance failures of some new development.

An example of the project manager's reluctance to fund research is Thomas Edison's opposition to alternating-current (a-c) systems. He had earlier concluded that 110 volts was the optimum direct-current (d-c) voltage, even though a maximum transmission distance of only about 1 mile could be accommodated from one station. Edison advocated the low-voltage d-c system because of safety, in spite of accidents which included at least one fatal fire and a fire in J. P. Morgan's library (*2*). Other companies, including Westinghouse and Thomson-Houston, were already advocating a-c transmission at thousands of volts, as well as the use of a-c arc lamps for street lights. But such high voltages were not suitable for Edison's filament incandescent lamps. However, when the a-c transformer was patented by Lucien Gaulard and John D. Gibbs in England, George Westinghouse recognized the patents' extreme importance and quickly purchased them. His engineers demonstrated 4 miles of transmission in 1886 (*3*).

The important invention which facilitated the use of a-c for long-range transmission was the a-c induction motor, conceived by Nikola Tesla (*4*). By 1883 he had demonstrated his design of the induction a-c motor and had conceived the idea of polyphase systems. He tried to interest the Continental Edison Company, for whom he worked, but was rejected. He finally patented his inventions in 1887; 1 month after he presented his invention at an AIEE meeting, George Westinghouse bought the Tesla patents, proceeded with the necessary applied research and development, and successfully demonstrated a 30-kilovolt a-c system in 1893.

J. P. Morgan, who had a sizable interest in the Edison companies, urged Edison to merge with his competitors, who held patents in the field of a-c. The new General Electric Company (GE) promptly began to compete in research and development of a-c equipment, aided immensely by their purchase (in 1893) of a small firm in Yonkers which employed a draftsman named Charles Steinmetz, who had just presented his famous paper (*5*) on the analysis of a-c circuits.

The higher rotational speed of the a-c generator (1800 to 3600 rev/min) set the stage for the utilization of the steam turbine, which curiously was also developed almost simultaneously. The first practical steam turbine was designed and built by Charles Parsons in 1884 (*6*). It utilized a series of successive stages to develop the necessary high expansion ratio. His first pressure-compounded turbine was actually a pair of opposing turbines, to balance the thrust on the shaft.

After building a 6-horsepower, 18,000 rev/min unit, Parsons left to form his own company. It is interesting that the original company, which retained the patent rights to the pressure-compound turbine, did nothing to either exploit them nor improve them, possibly because the required investment would interfere with the existing product line, and the risks of improving the performance were deemed high.

The usefulness of the steam turbine was further enhanced by deLaval (*7*), whose concept (1890) was to expand the steam to low pressure through his famous convergent-divergent nozzle, and then pass the steam through an impulse turbine wheel. The resulting shaft speeds needed for efficient operation were extremely high, and therefore required reduction (through helical gears, another deLaval invention). But the alternative technique of velocity staging, whereby a large number of impulse turbines are used with each set taking only a small change in steam velocity, proved more practical for the direct drive of the a-c generator shaft. This concept, patented by Curtis in 1889, was further developed by GE (*8*) but, again, applied research was needed to improve its performance.

The government also played a key role in pioneering long-range transmission. In 1892 the city of Buffalo, New York, initiated the financing of the hydroelectric generators for Niagara Falls and the 22-mile transmission line to Buffalo, which was completed in 1896 and was the first large-scale successful demonstration.

The lesson is clear: applied research, which can change the direction of progress, cannot be funded from within the management framework of a project which in itself is trying to improve its own performance. Applied research must have its own independence, for invariably the initial invention or concept is rarely performance-competitive, but must be improved considerably prior to demonstration of superiority. The applied research must result in knowledge which is then applicable to design and development. Finally, government must in many cases be the initial demonstrator by purchasing the first prototype of a new system, since the perceived attendant risks are usually greater than those which the majority of private industry are willing to undertake.

Current Efforts

Ceramic gas-turbine components. Ceramic turbine parts offer the potential for increasing gas turbine performance inexpensively while simultaneously minimizing nitrogen oxide (NO_x) emissions. Turbine inlet temperature could be increased from the present 1150°C (*9*) (for uncooled blades) to 1400°C, offering a gain in thermal efficiency of a gas-turbine engine with regenerator that is 20 percent above that of present gasoline engines and 11 percent above that of present diesel engines (*10*). Further, this increase in turbine inlet temperature reduces the extent of the required high temperature (stoichiometric) combustor region which produces NO_x. Ceramic raw materials costs are about one-tenth of superalloy costs, and their use avoids the expense of fabricating blade-cooling passageways which would be needed to achieve comparably high gas temperatures with metallic blades. Besides the turbine rotor and blades, ceramic components could include the first-stage stator and shroud (for example, Si_3N_4), the combustor (SiC), and the regenerator (Li-Al-SiO_2). Should the use of such ceramic components prove practical for small vehicle engines, there is a potential for saving $7 billion a year nationally (*11*); in addition, the technology has potential application for stationary-power combined gas-steam cycles using low-energy gas generated from coal (*9*), offering an increase in plant efficiency from 28 percent for a gas turbine to 45 percent for the combined cycle.

Applied research on ceramic gas-turbine components was initiated by the Department of Defense's Applied Research Projects Agency in 1971. A combination of industrial development laboratories was used, coordinated by the Army Materials and Mechanics Research Center at Watertown, Massachusetts. Initially, 300 ceramics were screened, then fabrication procedures such as hot pressing, slip casting, and injection molding were developed, including the process control required to arrive at the desired properties. This applied research led initially to satisfactory strengths and design techniques appropriate for the material, and culminated in a successful test. Work is proceeding (*12*) to develop materials with improved strength, to prevent crack formation and corrosion decomposition (for example, by doping), and to devise nondestructive testing methods.

Magnetohydrodynamic electric power generation. As in ceramic turbine development, the rationale for magnetohydrodynamic (MHD) concepts is that the maximum conversion efficiency for a gas cycle increases with increasing combustion temperature. In MHD systems, the temperatures are sufficiently high to ionize potassium, which is added to combustion-generated gas to make it a good electric conductor. The seeded combustion gas is expanded to high speed and flows through a channel in a magnetic field. Electrodes at the sides of the channel act as "brushes" for this d-c generator.

Twenty years of research, primarily at one industrial research laboratory and a few universities, have proved the scientific feasibility of operation with coal and air. The Department of Energy has initiated a major project aimed at demonstration of the commercial feasibility of an MHD-stream system in 1984 (*13*), at about 1000 thermal megawatts. The initial phase of the project is directed at a Component Development and Integration Facility (CDIF), to facilitate the development of other components under operational test conditions. Typical technical problems to be addressed include materials to withstand the high temperatures, ash, and potassium environment; coal feed and combustion; channel and electrode configuration; air preheaters; slag removal; and superconducting magnets. Although the CDIF program is projected to ensure successful integrated system operation, applied research will continue to support the overall program objectives on such topics as coal-fired generator technology, seed recovery systems, MHD channel materials and configurations, multistage coal combustion, and limited testing of electrodes (on the Soviet U-2 clean-fuel facility). Small-scale testing and gathering of basic physical data are planned in the areas of seed recovery and slag rejection, corrosion resistance of components, and NO_x suppression and sulfur removal.

Fluidized-bed combustion. Major contributions to fuel conservation may come from applied research on the use of fluidized-bed combustion for coal-burning furnaces. The basic principle of fluidized-bed combustion is that the burning coal particles are supported and burned in a rising stream of air. Although this process was demonstrated back in 1921, it was applied research which brought it to the stage of incipient wide-scale commercialization.

First it was necessary to control the combustion temperature (so as not to generate deleterious NO_x) and to prevent agglomeration of fused coal particles, which would destroy the fluidization of the bed. These were accomplished by (i) introducing a large (20 : 1) fraction of "ballast" particles, for example, ash, inert stone, or, for high-sulfur coal, limestone, to reduce the probability that two coal particles would collide and fuse together and (ii) immersing the relatively cool boiler tubes right in the combustion bed, rather than using the conventional large, open furnace where only the walls absorb heat. The heat generated by the burning coal particles is transferred very efficiently by both direct contact and radiation to the ballast particles—such processes have an effective conductivity far superior to that of copper—and the ballast particles in turn deliver their heat to the boiling water flowing in the boiler tubes. This heat-transfer process turned out to be so efficient that in the first test model, built in 1966, more than half of the tubes had to be removed to keep the fire from going out! Important consequences of the use of ballast particles in the bed were (i) considerable reduction of fly ash, since the coal particles could be much larger than in conventional pulverized-coal burners, and (ii) use of limestone to react with the coal's sulfur, forming nonvolatile sulfur carbonates which remain in the bed, preventing the emission of the gaseous sulfur oxides which constitute one of the most serious health pollutants in coal utilization.

Another important consequence of the applied research was the ability to divide the combustion bed into "cells." The bed materials—both burning coal and inert particles—are common to all cells, and can readily flow between them. But the air flow to each cell is controlled independently, therefore controlling both the cell temperature and the amount of

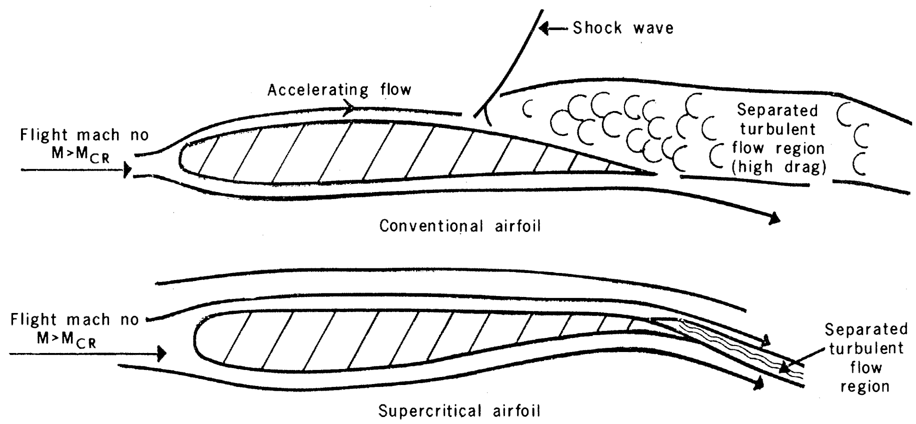

Fig. 1. Illustration of the basic principle of the supercritical airfoil.

steam generated. This type of independent temperature and flow control permits one cell, with limited heat-transfer surface, to serve as the "igniter" cell; another can be used to burn off excess carbon to minimize fly ash; a third as a lime regenerator, and so on.

Recent applied research on pressurized fluidized beds (as well as the original atmospheric-pressure versions), which involved extensive testing of small kilowatt-to-megawatt level test beds, have indicated that fluidized-bed boilers can be constructed which eliminate virtually all sulfur oxides, even with high-sulfur eastern coals, and (by reducing peak combustion temperatures) can meet current NO_x emission regulations with boiler weights and costs a third to a half those of conventional coal-fired boilers.

A 250-MW demonstration project is now being constructed with Department of Energy funds. Most important, though, these devices appear to be applicable in boiler sizes ranging from only a few to over 500 MW. Hence broad applicability to industrial cogeneration and on-site total energy systems appears possible, as well as to central-station power plants. The promise of this technology, made possible only by extensive long-term applied research, is so evident that fluidized beds have been cited by Lovins for early implementation in small decentralized power generation systems (*14*).

Supercritical wing. The supercritical airfoil, which is having significant impact on aircraft fuel conservation, is one of the more recent examples of the classic utilization of applied research. This example is especially instructive because it is a case of generic applied research carried out by the government with the results applied by industry for use by non-government customers. This constitutes the "ideal" system, in which industry supplies the creative design talent for individual specific products, whereas government places the generic applied research into the public domain. It may well become the showcase model for research applied to energy conservation, because the fruits of the research are reaped when private industry uses its results in products sold to customers.

The basic aerodynamic analysis technique was Theodorsen's method, developed about 1930, for calculating the inviscid fluid flow about an arbitrary airfoil shape (*15*). Theodorsen's application of basic Joukowsky airfoil theory allowed researchers to maximize the low-drag laminar-flow portion of an airfoil by mathematical analysis (*16*). Although extensive refinement and detailed testing ensued, from which came the bulk of all airfoil shapes used in the world's airplanes today, little new airfoil technology improvement occurred until the basic concept of the "supercritical" airfoil was published by Whitcomb in 1965 (*17*).

The so-called supercritical wing is simply a wing whose surface is contoured to maintain a pressure distribution that postpones the flight velocity at which the major drag increases associated with shock-wave formation on the airfoil's upper surface begin to occur. The (subsonic) flight speed at which the accelerated airflow over the airfoil first forms a shock wave is called the "critical speed" or "critical Mach number (M_{CR}) hence the name "supercritical wing" (Fig. 1). Whitcomb and Clark's original paper (*17*) suggested the supercritical wing as a method for achieving higher flight speeds with no loss in lift/drag ratio; it is currently considered as a mechanism which sharply reduces drag (and hence fuel consumption) at no change in flight speed.

During the period when Whitcomb and his co-workers at the National Aeronautics and Space Administration's (NASA) Langley Research Center were seeking airfoil improvements, there were no known solutions to the basic transonic flow equations which govern the motion of gases near the critical speed. Hence the first stage of Whitcomb's applied research process consisted of using the wind tunnel as an analog computer, employing the test data to gain a physical understanding of the effects of airfoil shape changes over a broad range of supercritical flow conditions. This research permitted the construction of a rudimentary set of equations defining a limited range of shockless supercritical airfoils. But the most significant consequence of this first step was its evolution into the establishment of a complete (and now well-understood) set of design criteria for such airfoils (*18*). These criteria, integrated with current transonic computer codes, now permit the analytical design of supercritical airfoils which perform as predicted at their design point (*19*).

Current efforts (by NASA) are directed at the generation of a complete set of design procedures, based on both the theory and the test data accumulated during the applied research program, which will permit the a priori design of whole families of supercritical airfoils with predictability of off-design performance comparable to that of the 4-digit, 5-digit, and NACA-1 (National Advisory Committee for Aeronautics) airfoils developed during the 1930's and 1940's. These supercritical wings are expected to reduce fuel consumption and improve safety and/or performance on a wide range of aircraft. They have already been used on general aviation light planes, civil jet transports, and transonic military aircraft with considerable success. The most significant element in their development from the original 1965 concept has been the exhaustive process of applied research, both analytical and experimental, which has now brought them to the verge of operational status.

Future Opportunities

Among the better examples of fuel conservation through applied research are those in which there exists an effective collaboration between the producers and the users of such research. The most significant opportunities for future successes appear to be in the automotive technologies, specifically aerodynamic drag and tribology, or the science of friction, lubrication, and wear (*20*).

Ground vehicle drag reduction. Typically about 25 percent of the mechanical work of a passenger-car engine is required to overcome aerodynamic drag. The drag coefficients of current (typical) U.S.-built automobiles range from 0.45

to 0.55. Some automobiles have claimed measured coefficients as low as 0.3 (Citroen XX), and knowledgeable practitioners believe that 0.25 to 0.3 is a practical and achievable goal. And since drag, thrust, and total energy consumed are directly related, there is a potential for reducing the fuel needed to overcome drag by 50 percent—more than 10 percent of the total automotive fuel consumed in the United States.

Drag, however, is related to a number of other factors, not the least significant of which is styling—probably the dominant factor in passenger-car sales to date. Other key factors are vehicle weight (directly related to cost), safety, handling, and convenience in loading and unloading. Typical Indianapolis racing cars, for example, have drag coefficients near unity—twice those of typical commercial autos—in order to generate the negative lift needed for traction.

Since drag reduction will impinge on all these sales-related factors, the engineer must be able to evaluate the effect of vehicle configuration changes at an early stage in the development of vehicle design. The present state-of-the-art does not permit this; hence, considerable attention to consistent applied research procedures is needed.

Further, there are many gaps in the available engineering information related directly to ground vehicle drag; for example, ground effect, the influence of wheels and wheel wells, effects of intakes and cooling ducts, effects of flow-control devices such as dams, vanes, flaps, spoilers, vents, and slots, and even basic airflow over auto bodies. While it is possible from the literature to understand and predict the aerodynamic flow over many two-dimensional bluff bodies, there is little scientific understanding of the flow over a three-dimensional body in close proximity to the ground. The flow separates on the forebody and again on the afterbody. Increased understanding of these separated three-dimensional flows would provide design guidance for achieving desired aerodynamic performance consistent with other objectives of vehicle shape.

Even the testing of full-scale and part-scale vehicles in wind tunnels has many unsolved problems. There are uncertainties about simulation of the groundplane, the internal flow of the model, fine construction details (such as the effects of cracks and rain gutters on flow separation), and effects of wind-tunnel boundaries. Since the aerodynamic drag of the vehicle is often determined by flow separation and reattachment phenomena that are strongly nonlinear, even full-scale wall effects may cause an effect on measured drag which is directly opposite to the actual operational effect. Complications in model-scale measurements are even greater, particularly those related to turbulence and influences of natural obstacles and other vehicles.

Clearly, an organized, consistent applied research effort can resolve many of these uncertainties, and the relatively small investment needed for such a program, whose success potential is measured on so large a scale as a 10 percent reduction in automotive fuel consumption, would appear to be warranted.

Tribology. Tribology enters into fuel conservation in road vehicles in several ways. One obvious near-term impact is reduced friction, but in the long term, advances in tribology make possible the development of radically new engines and transmissions that are now beyond the state-of-the-art because of excessive wear at the higher, more efficient operating temperatures and pressures. At present, the problem is not so much in discovering more durable surfaces and lubricants, but in characterizing what physical properties are required to achieve the desired results.

Claims made for low-viscosity lubricants now on the market state that if they were used generally, they would improve fuel consumption of the fleet about 5 percent overall. However, not only are these lubricants expensive, but they exhibit behavioral anomalies. For example, there are data that indicate that two cars of the same make and model—presumably identical—can show opposite effects of these lubricants on fuel consumption. Such unanswered questions inhibit the widespread use of low-viscosity lubricants.

Properly conducted laboratory measurements can give considerable insight into the problem. Experiments to validate theoretical models can identify and measure the specific locations where frictional losses and wear occur in an engine that is operating under its own power. Some of these measurements have been made by "motoring" the engine, but the strong effects of loads and deflections on the rings and other components of the engine are not measured by this means. Much theoretical work also remains to be done, including the mapping of displacements, temperatures, and forces, and rheological modeling of the lubricant under operating conditions.

Laboratory measurements of friction and wear on an operating engine in coordination with theoretical analysis can also provide information that is useful in improving the detail design of the piston ring–cylinder interface, as well as in the specification of the lubricants. Some 12 percent of the mechanical work of the engine is lost in this region because of friction and blow-by at this seal. The estimated potential for savings amounts to about 70,000 barrels of oil per day for the nation's automobile fleet, while the laboratory and theoretical research effort required to achieve these gains would cost only a few million dollars—the cost of just a few days of operation by a good fraction of that fleet. It should be noted, incidentally, that the savings resulting from detailed design changes in the engine do not call for any capital investment by the consumer and relatively small capital investments by the manufacturer outside of the R & D and engineering costs.

Another example of a long-term payoff for tribology research would be in providing the information required to produce a traction drive for a practical and economical continuously variable transmission with the use of a flywheel for regenerative braking. Savings in fuel on the order of 30 or 40 percent are not unreasonable to expect.

To realize this payoff the efficiency of the transmission has to approach that of a conventional gear train (95 percent). It has to be durable, and it must not be too costly to produce and maintain. The underlying problem in the traction drive is to achieve high efficiency and compactness while maintaining durability. One rotating element has to propel another through an intervening lubricant; hence, if the normal force between them at the point of contact is low there will be slip and inefficiency, whereas if the force is high there will be wear and large stresses on the components. The lubricant is therefore a key element, and its behavior under conditions of traction is only imperfectly understood. Because of the transient load at the point of contact, the lubricant behaves as a non-Newtonian fluid. That is, the fluid shear stress does not depend simply on the velocity gradient and the viscosity of the lubricant. Thus to make a rational design it is necessary to characterize the "fluid" properties that are of interest and to devise practical means for measuring them. A model is also needed that yields an estimate of the deflections and stresses of the solid elements reacting on the lubricant as it reacts on them. The term "elastohydrodynamic" analysis does not do justice to the complexity of the phenomena. It is not surprising that the occurrences of unexpected and unexplained failures have ended development tests of promising concepts even before the question of long-term wear could be addressed.

To put the potential benefits of applied aerodynamic and tribology research into their proper context, it is useful to examine the overall prospects for automotive fuel savings. Applied research can contribute by making possible the use of better engines, better transmissions, better tires, better body aerodynamics, and other improvements. Figure 2 is an estimate of present energy losses in automobiles, Table 1 summarizes estimates of potential reductions in those losses, and Table 2 shows the resultant composite improvement achieved by realistic combinations of the specific improvements in Table 1.

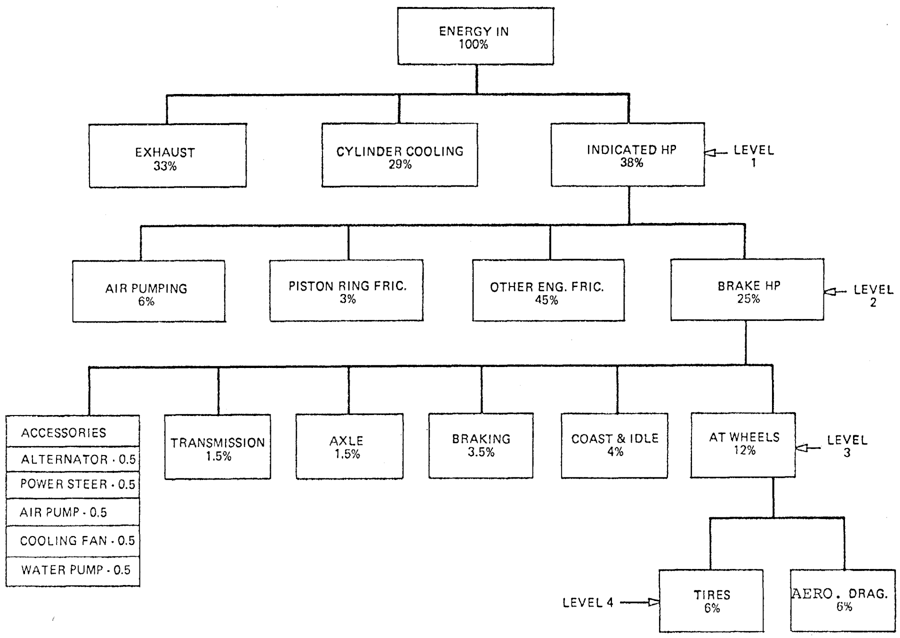

Fig. 2. Energy distribution in passenger car during EPA cycle (*21*).

Figure 3 shows the projected petroleum consumption for the 1975 auto fleet, which has a mileage of 15.6 miles per gallon, if that mileage is projected to the number of vehicle miles traveled in the year 2000. The figure shows the expected fuel consumption if the improvements discussed are utilized in the entire highway vehicle fleet. It should be noted that the autos for which the improvement is projected in the year 2000 (bottom curve) are as heavy as the vehicles in 1974 and have the same payload and speed performance. Additional savings would accrue if autos were made lighter and were designed with less acceleration capability. It is probably optimistic to assume that the research will be successful in all cases, but the bottom curve in Fig. 3 does show that at least the *potential* for savings is of major importance.

Table 1. Effect on energy consumption of specific improvements in automobiles (*21*). Total automotive fuel consumption equals 19 percent of national energy consumption. CVT, continuously variable transmission.

Improvement	Specific improvement	Improvement taken alone: Auto energy consumption (%)	Improvement taken alone: National energy consumption (%)
	Aerodynamics		
1 Low air drag design	Drag reduction of 33 percent	8	1
	Tribology		
2 Piston ring–cylinder friction reduction	Reduction of 25 percent	2.4	0.4
3 Adiabatic diesel engine	Efficiency increase of 60 percent	40	8
4 CVT-conventional engine	Operation of engine at its most efficient range	20	4
5 CVT-regenerative braking and energy storage	Operation of engine at its most efficient range	35	7
6 Improved engine and driveline lubricants	Reduction of 25 percent in engine friction	5	1
	Rolling resistance		
7 Low drag tires	Reduction of 50 percent in tire rolling resistance	13	2.5
	Traffic management		
8 Reducing idling operation in delays, reducing accelerations and decelerations	Up to 30 percent increase in urban gas mileage; 10 percent in highway	20	4
	Combustion research		
9 Diesel engines, Stirling cycle, gas turbine	Efficiency improvement of 30 percent	30	6
10 Improved Otto cycle	Efficiency improvement of 25 percent	25	5
	Advanced system research		
11 Valve resizing	Reduction of 60 percent in pumping loss	13	2.5
12 Xylan coating	Halves engine friction	15	3
13 Idle-off system	Cuts idle loss	12	2.4

Institutionalization of Applied Research

An inadequate level of applied research results can have a disastrous effect on a project. As gaps in technical knowledge are uncovered they have to be filled, and this requires additional cost and time. Because the original schedule and cost estimate are thus also affected, the project manager may try to decrease the technical performance requirements of the project, and this also may require additional funds and time if calculations and designs have to be repeated. As the project gets further behind, the technical performance requirements may be further decreased. The final result, being little more than a rearrangement of components, may hardly represent any improvement in functional performance.

To establish the proper scale at which to conduct any applied research, it is necessary to perform a benefit/cost analysis. In conservation, this means that one must make some estimate of the net gain—not only in reducing fuel consumption but also in obtaining possible ancillary benefits such as environmental impact improvement and reduction in required capital investment—for a given applied research program cost. However, there are few, if any, examples of successful a priori quantitative analyses of applied research efforts, despite the increasing use of the process called "technology assessment." This weakness of currently available cost/benefit analysis methodology has researchers actively seeking a more consistent approach, even in so prosaic an area as the proper and prompt dissemination of useful technical data. It is much easier to make a qualitative judgment based on a consensus of experience within the research community. Reynolds (*1*) has reviewed research expenditures for a number of government agencies and concluded, "These numbers support two opinions that I have heard from many colleagues. The first is that too much emphasis is being placed on development. . . . The second is that there is a 'gap' between the basic research programs and the development programs, that applied research is not receiving sufficient emphasis." Since the analysis is based on fiscal year 1976 figures these remarks do not necessarily apply in the current fiscal year 1978, but they do indicate that the research community is willing to make judgments and that these judgments may favor applied research as well as basic research.

We now can attempt to define the institutionalization of such an approach. What is needed primarily is a mechanism for generating appropriate applied research topics. This could probably be best accomplished through the existing professional societies, since they are already organized into technical disciplines. However, a government-agency or a government-funded cooperative committee may be useful to coordinate the mechanism, so that the societies can generate coherent topical applied research plans rather than simply collections of "good ideas." There is a minimum federal role for applied research in any technology affecting the national interest which is to determine whether there are gaps and to see to it that those gaps are filled, either through public or private means. Ideally the gaps should be identified by engineering societies or other professional groups which are in the best position to identify them.

The classical function of federally supported R & D is to develop technologies whose eventual payoff is potentially high but which entail too much risk for prudent private-sector investment. The role of federally supported applied research is to provide the technical information that private industry needs to reduce its technical risks. In this way private industry can be encouraged to undertake innovative hardware development on its own, giving the federally sponsored applied research a much greater leverage on technology development than does the budget for federally supported hardware projects.

The chief mechanism for ensuring that the responsibilities of a national program for applied research are properly discharged is a national forum for program planning and review. This forum should include the users of applied research—the technical managers responsible for hardware projects in government and industry—and those responsible for doing applied research: representatives of universities and research institutes.

The recognized engineering societies provide effective institutional umbrellas for such a national forum. These organizations usually include both the users and the producers of R & D in their membership, and they therefore have to consider all of their interests. The societies are chartered to promote the interchange of technical information and to promote technology development, and are therefore well motivated to do this. They also provide a constituency which will support budgets for applied research and give industry a chance to take the initiative in guiding national programs. Since industry will be the ultimate user of the results it is important to promote such initiative.

The engineering societies have additional advantages in that they can provide continuity without unduly burdening their committee members, who are rotated at regular intervals. This has a decided advantage over ad hoc committees which are sometimes organized at a national level, because such committees are usually disbanded after making their recommendations, and can neither provide a measure of the effectiveness of the recommendations after they are implemented, nor change the

Table 2. Effect of combinations of improvements in automobiles on energy consumption (*21*).

Improvements*	Auto consumption improvement (%)	National energy consumption reduction: Percentage	National energy consumption reduction: Quads in 1974 usage
1 + 7	20	4	3
9 + 5	54	10.8	8
9 + 5 + 1 + 7	67	13.4	9.9
3 + 1 + 7	55	11.4	8.1
9 + 1 + 7 + 12	53	10.7	7.7
10 + 1 + 7 + 12	49.5	9.4	7
10 + 1 + 7 + 8	55	10.4	7.7
10 + 4 + 1 + 7	52	9.9	7.3
11 + 12 + 1 + 7	37	7	5.2
2 + 1 + 7 + 11 + 12	39.4	7.5	5.6
1 + 2 + 7 + 6 + 11 + 12	41.2	7.8	5.8
1 + 7 + 9 + 11 + 12 + 13	43	8.2	6.1

*See improvement numbers in Table 1.

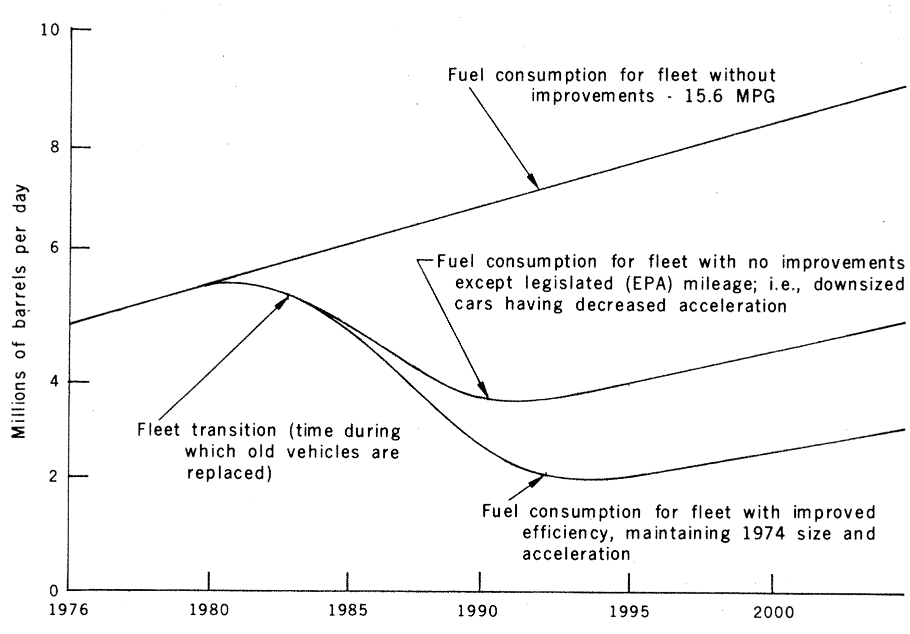

Fig. 3. Auto fleet oil consumption (*21*). Abbreviation: *MPG*, miles per gallon.

strategies or goals of a program in progress. The engineering societies also have an advantage over permanent organizations in that they are not responsible for maintaining the employment level or "sales" of such a permanent organization; hence they can recommend program termination when a program is no longer useful.

Engineering societies have responded to requests for help in assembling forums for planning and review; the American Society of Mechanical Engineers, for example, is producing excellent results in developing a plan for R & D in tribology, and the Society of Automotive Engineers is proceeding effectively to plan R & D for road vehicle aerodynamics and tire and suspension rolling losses. Further, effective applied research in any of the technical disciplines requires a community with "critical mass." Planning and review by the engineering societies, as well as interagency coordination, help to assemble such a community, although the effort to make it most effective in furthering applied research should probably be supported by providing support in larger blocks, in one place, over relatively long periods of time.

Conclusions

The critically important role of applied research has been identified in a number of specific past, present, and future activities related to fuel conservation. The importance of applied research generally tends to go unrecognized as compared with basic research and product development processes, not only in fuel conservation activities but in virtually every area of technology implementation. There are well-defined roles for both government and industry in the effective utilization of applied research, and the engineering societies offer a potentially effective existing framework for government and industry to implement these roles.

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U.S. Energy Demand: Some Low Energy Futures

Demand and Conservation Panel of the Committee on Nuclear and Alternative Energy Systems

This article presents several plausible energy demand scenarios for the United States through the year 2010, each one derived from analytic efforts conducted by the Demand and Conservation Panel (*1*) of the National Research Council's Committee on Nuclear and Alternative Energy Systems (CONAES). While the CONAES study (*2*) covers a range of plausible energy futures—from continued rapid growth in demand to actual reductions in demand—we focus here on futures in which demands are lower, in order to provide insight into how energy demand growth can be reduced, and on the consequences of low energy growth. As a further effort to explore low energy growth, we assume future economic growth to be smaller than it has been in the past or than many think it likely to be in the future. This analysis is not intended to show that low demand futures are the most likely or the most desirable; instead, it is meant to illustrate the opportunities for lower energy demand growth and the public policies required to realize these opportunities.

Low energy futures could result from constraints on supplies that appear as higher prices, import restraints, rationing, taxes, or other public policies (for instance, in response to limitations on oil and gas imports, SO_x and CO_2 production, and nuclear power). They could also result from a national decision to use energy resources more efficiently or from shifts in social priorities (such as less pollution of air and water). Adjustments to these changed conditions, whether in developing new energy supplies, expanding old ones, devising a more efficient utilization system, or changing the mix of goods and services demanded, will require decades of effort. To devise and implement a reasonable

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