There has been a revival of interest in traffic flow theory in the late 1990s, mostly because vehicle detectors have been installed at many highway locations throughout the world to record the passing of vehicles. The purpose of these detectors is to monitor congestion and provide information for possible control or to advise motorists of possible alternative routing. To understand the consequences of some strategy, however, one must have some theories or models of how traffic will respond to various actions.

Students who review the literature on traffic flow theory in an attempt to develop such models observe that most of the references on this subject are dated in the 1950s and 1960s. There is very little useful literature in the 1970s, 1980s, or early 1990s. Students wonder how this started, who were these people who wrote the early papers, and why did the subject die in the 1970s?

There are not many people left who were involved in the early developments of traffic flow theory, so it is difficult to research the history of it. Many of these people were involved for only a short time and then moved on to other things. Most of them had grown up during the Depression era and had lived through World War II. Any one of them could have told stories about what the world was like then and what motivated them, but perhaps the stories would be rather similar. The 1950s was a time to try to put civilization back together and to create a society people could live with.

I can tell the story only from my own perspective and from my encounters with others.

It is appropriate that traffic flow theory be included as part of the anniversary celebration of Operations Research, because many of the early papers on traffic flow theory were published in this publication, and the national meetings of ORSA were one of the main forums for exchange of ideas. A special issue of Operations Research devoted to transportation was published in 1964, and the journal Transportation Science (sponsored by ORSA) was created in 1966.

My first exposure to problems in transportation occurred in 1954. Professor William Prager, the distinguished professor of applied mathematics at Brown University, gave a lecture on a “fluid theory” of highway traffic. The theory he described was essentially what was later published in the famous papers by Lighthill and Whitham (1955). I will never know if Prager arrived at this himself or if he had learned about it from Lighthill. Prager never published a paper on this topic.

I thought this was intriguing. After the lecture, I suggested to Prager (whose office was very close to mine) that one should model vehicles like a gas, as in the kinetic theory of gases, rather than as a fluid as in fluid dynamics—after all, cars are discrete objects like molecules that run around and occasionally interact. This is a “one-dimensional gas.” Prager’s primary field of interest was continuum mechanics; mine (at that time) was physics, particularly statistical mechanics.

Prager said he had assembled a bibliography on traffic flow theory. He showed me his folder, but the only paper in it that dealt with any real theory was a survey paper by Wardrop (1952). He also had a copy of the Highway Capacity Manual by Norman and Walker (1950), which describes most of what was known then from measurements of traffic behavior. Actually, it turns out that this was not a complete bibliography at the time. There were some earlier papers in various journals, but not many. Most of what Prager had was British literature, which leads me to suspect that he had learned about this from Lighthill, who knew the British literature.

My first reaction to this literature was shock or disbelief. In nearly every branch of physics or any other well-developed science, researchers stumble over each other looking for new things to do and then race to see who can do them first. The paper by Wardrop (1951) involved only rather obvious things, rather crudely developed. I thought it was quite naïve at first, but later I could appreciate that this was a monumental work because, before Wardrop, there was essentially nothing. He was starting “from scratch.” How was it that the traffic engineering profession knew almost nothing about how to model things they could see with their own eyes?

This Wardrop paper is best known for its description of the distinction between the “user optimal” and “system optimal” route choice and the formulation of what is now called “Wardrop’s principles.” This is, in essence,
the statement that, for a user optimal each driver chooses the “cheapest” (fastest) route, but for a system optimal he should choose the route of minimum “marginal cost.”

But these statements were near the end of the paper. Most of the paper deals with a collection of simple facts such as the difference between the “space mean” speed and the “time mean” speed, and some discussion of delays at junctions. One can see from the published discussion following the paper that to model traffic by writing formulas was something quite new to most of the traffic engineers.

It is interesting that Wardrop, who apparently was trained as an economist, did not yet grasp the notion of “capacity,” a concept from “physics.”

The similarities of traffic with statistical mechanics were obvious. At very low densities, a molecule travels at a constant velocity most of the time until it collides with another molecule. At low densities, a car would travel at nearly constant velocity until it interacts with another car. In both systems, two objects interact only when they are within some characteristic distance; for molecules it is the range of the force field, and for cars it is something like a “safe driving distance.” In physics, the transition from a gas to a liquid or solid occurs when the average distance between nearest neighbor molecules is comparable with the range of the force. For cars, the distinction between light traffic or heavy traffic is related to how the average distance between cars compares with the safe driving distance. If light traffic is like a gas, maybe heavy traffic is like a liquid, a one-dimensional liquid. This much one can say without knowing anything about the nature of the interaction between molecules or cars.

I immediately started to construct a theory of low-density traffic. In the first approximation, one neglects the interaction between vehicles; each vehicle follows a motion independent of any other. This already implies certain stochastic properties of vehicle counts. In a second approximation, vehicles interact at most two at a time (as in the kinetic theory of gases) if they are close together. In the kinetic theory of gases, there are all sorts of phenomena associated with interactions between molecules two at a time. When two molecules collide, they exchange energy and momentum, and this leads to heat conduction, diffusion, viscosity, etc. When two cars interact two at a time, one car passes the other possibly after a slight delay, but then the two cars regain (nearly) the same velocity they had before. The consequences of this interaction is very small. It was a bit disappointing at first. It was too simple.

In later years, indeed even to the present time, some researchers try to associate with vehicular traffic all sorts of phantom phenomena analogous to the effects in gases. They don’t exist.

I also started to work on evaluating the delays at a fixed-cycle traffic signal with random arrivals. This problem was introduced in Wardrop’s (1951) paper, but he couldn’t do much with it. It seemed like a straightforward problem, but it actually was quite difficult to solve by the queueing theory methods in vogue at that time. This gave me an excuse to interact with Prager and to demonstrate that I could contribute something of interest to him.

Prager was the illustrious senior professor of applied mathematics at Brown University. He was a rather pompous European-style-king professor, but he had a son about my age, recently appointed as an assistant professor of physical chemistry at the University of Minnesota, and Prager liked to act as a father figure for several of the junior faculty in the department at Brown.

Prager was born in Germany in 1903. By the time he was 30 years old, he had published more than 30 papers and was offered a chair at a German university (Karlsruhe). He never got to sit in the chair, however, because Hitler came to power in 1933 and blocked all Jewish appointments. Prager fled to Turkey but came to Brown University during World War II to head a wartime “think tank,” which became the Division of Applied Mathematics after the war.

I wrote a little paper about low density traffic and submitted it to Operations Research (Newell 1955). I believe this was the first paper on traffic published in Operations Research, although Leslie Edie (1954) had published one a year earlier on delays at toll booths.

Before the paper was actually published, I decided to attend the annual meeting of the American Physical Society in Washington, D.C., not so much for the meeting itself but as an excuse to meet with Elliott Montroll and attend his annual party.

I had spent two and a half years first as a post-doc and then a research associate with Montroll from 1951 to 1953. In January 1951, Montroll was appointed a research professor at the University of Maryland, and I was the first of a long sequence of post-docs whom Montroll hired. But I was the only one for most of the first year, so I had a lot of opportunities to interact with him. It was a wonderful experience because Montroll had lots of ideas and enthusiasm and would propose all sorts of things to investigate.

According to Montroll’s explanation, the annual parties started because Montroll thought the tickets for the physical society banquet were too expensive, so instead of going to the banquet, he would invite some friends to his house. But Montroll had a lot of friends. After a few years, the parties grew to about 100 people (obviously rather expensive).

I went to Montroll’s house an hour or so before the party and started to chat a bit before the mob came. I brought a copy of my paper on traffic as sort of a joke and showed it to Montroll: “Look, this is what I have been doing.”

Montroll said he had written some equations describing how a driver might control his car while following another (the high-density situation). He had for some time been interested in “control theory” (mostly for electrical circuits), and this was an application of these techniques, but he said he had just put his notes in a drawer. He would take it out. Because there was an audience now, there was an incentive to follow through.

He put the copy of my paper on a side table for the party in case anyone was interested. I don’t know if anyone saw it.
Predictably, Montroll never sent me any of his notes, but the following year, a friend of Montroll’s, Robert Herman, was to take a job in the research laboratory of General Motors Corporation. Herman had been working at the Johns Hopkins Applied Physics Laboratory near Washington, DC. I had met Herman briefly when I was working with Montroll in Maryland.

I don’t know the details of how this came about, but the prevailing view at that time was that IBM and AT&T had achieved huge profits by supporting “basic research” in the physical sciences (mostly related to semiconductors, computers, etc.), but that the automobile industry was doing very little except in “development.” A physicist had been made a vice president of General Motors and apparently decided that GM should do some research in physics. Herman was hired to head a new department of “theoretical physics” and given the freedom to pursue whatever he wanted, but hopefully, in some direction that would eventually be profitable for GM.

Herman engaged Montroll as a consultant to GM and asked Montroll what sort of research he should do. Montroll suggested that research on traffic flow theory would be an appropriate thing for GM to do (even though Herman’s department was theoretical physics). Not much happened for a couple of years but they did follow up on the work Montroll had put in his drawer. The first results did not appear until 1958 in an often quoted paper by Chandler et al. (1958), published in *Operations Research*.

Meanwhile, the aforementioned paper by Lighthill and Whitham appeared. This was followed by a similar theory proposed by Richards (1956), published in *Operations Research*. Communication was a problem then. Authors published papers in whatever journals they were familiar with but journals with which other authors were not familiar. The various players had not met anywhere yet, except maybe in small, disjoint groups. A theory very similar to that of Chandler et al. (1958) was proposed independently by Kometani and Sasaki (1958), published in Japan, and several statisticians were proposing stochastic models for light traffic.

In another major development, a group of young mathematical economists, under the direction of Tjalling Koopmans at the Cowles Commission for Research in Economics at Yale University, was studying various models of transportation, which resulted in a book by Beckmann et al. (1955). This was in part an extension of the type of analysis started by Wardrop (1952), but with more emphasis on pricing policies. It also had a rather detailed analysis of railroad classification yards and another only partially successful attempt to evaluate delays for a fixed-cycle traffic signal.

In December 2000, while I was preparing these notes, Martin Beckmann and his wife spent most of a day visiting my wife and me in Berkeley. Martin and I had a lot of fun trying to remember things from the 1950s and comparing recollections. Beckmann said although his book contains a reference to Wardrop’s (1952) paper, he didn’t know about it until the book was nearly complete. Thus, economists in different countries had independently approached the subject in very similar ways.

Faculty in applied mathematics at Brown University were expected to obtain outside funding to cover half their regular salary, plus summer support, plus some extra to support graduate students. This was quite easy to obtain then for most worthwhile projects, but I submitted a proposal to do modeling of traffic and it was rejected (“Is this science?”). (Around 1964, however, I submitted a proposal on “Application of Mathematics to Transportation” to the mathematics branch of NSF, which was funded and renewed every year for about 15 years, first at Brown and later, after 1965, at U.C. Berkeley.)

Fortunately about this time (1956), the Alfred P. Sloan Foundation started a program to support young scientists. It still exists. One does not apply for this directly, but one is nominated. Someone, presumably Prager, nominated me and I was awarded a fellowship that covered half my regular salary plus summer salary for three years, and I could work on whatever I pleased. Maybe the nomination proposed that I would work on traffic problems, which would seem like a worthwhile thing for the foundation to support because Alfred P. Sloan was an ex-CEO of General Motors.

By 1956, some people in the Highway Research Board (HRB), now called the Transportation Research Board (TRB), became aware of activities going on at various places. Daniel Gerlough, who had written a paper on the use of the Poisson distribution to describe traffic (Gerlough 1955), proposed that HRB create a committee on traffic flow theory (Committee #9). An organization meeting of interested individuals was held in Detroit (Beckmann thought it was New York City). This was the first time that many of the participants had met.

This was where I first met Beckmann (he confirmed this) who would later, in 1959, join the economics department at Brown University; Adolf May, who would become in 1965 a colleague at the University of California, Berkeley; and Leslie Edie, who became president of ORSA in 1972. Robert Herman was there, possibly acting as host.

Around 1994, I tried to assemble some memoirs including more details of this meeting. I asked Robert Herman and Adolf May what they remember about this meeting, but neither remembered it at all (Herman died a few years later), but Beckmann remembered it.

This may also have been the first time that Herman and Edie had met. This initiated a series of meetings over the next few years, usually at the Port Authority building in New York City. I would take a train to NYC from Providence and Herman, with one or more colleagues, would fly in from Detroit, and we would try to interpret some of the observations of traffic in the Holland and Lincoln tunnels.

Edie had a bachelor’s degree in electrical engineering but no graduate education. He did, however, have considerable experience in “problem solving.” The problem he had been assigned was to see if it was possible to increase the throughput of the Holland and Lincoln tunnels. During the
peak period, these tunnels carried only about 2,000 vehicles per hour in two lanes (a very low number by present standards), whereas the Queen’s Midtown Tunnel carried about 2,700 vehicles per hour in two lanes (also rather low). The tunnels had comparable widths and grades, so why were the capacities of the former so low? Edie thought that if one could develop some models of traffic flow, maybe one could find the answer.

The result of this was that Edie became heavily involved in traffic flow theory activities for the next 10 years or so.

The practical consequences of his work, however, had little to do with any “theory.” In the tunnels, there was a bottleneck at the foot of the upgrade because drivers were slow to react to the change in grade. Behind this bottleneck stoppage, waves would propagate back to the tunnel entrance about every four minutes. We still do not understand how or why this happens. There is no theory that describes these phenomena, although similar behavior is observed in other places, the familiar “stop and go driving.”

By “metering” the flow entering the tunnel, restricting the maximum number of vehicles to enter the tunnel in each one-minute interval, it was possible to smooth the flow and eliminate most of the stoppages. This also led to a very modest increase in capacity. The main benefit, however, was not the increase in capacity. By restricting the flow, one also restricted the density of vehicles, i.e., the number of vehicles in the tunnel at any time. This, in turn, reduced the rate of vehicle exhaust emission and the demands on the ventilation system. The financial savings in electricity to run the ventilation system were enough to pay for the study (although the strategy had little to do with the main emphasis of the study).

As a follow-up to this, Adolf May would later introduce metering of freeway ramps in Chicago, and Karl Moskowitz, a legendary traffic engineer for the California Division of Highways (now Cal Trans), would install meters on the Oakland-Bay Bridge in California. How one should do this, however, is still controversial. “Theory” is still incomplete.

The third and final year of my Sloan fellowship was a good opportunity to take a sabbatical leave. The usual arrangement was to take a sabbatical after six years, but Brown University agreed to let me take it a year or two early.

I had no idea of where I should go. Neither my wife nor I had been out of the country before except to Canada. I planned to do some research on modeling transportation systems but there weren’t any academic centers dealing with such things. Somehow we decided to go to Stockholm, Sweden, for no good reason, where my host was to be the Institute of Statistics at the University of Stockholm.

In the spring of 1959, we took a trip to England, mostly as a vacation but also with a professional angle. I had arranged to visit the Road Research Laboratory (RRL), now called the Transportation and Road Research Laboratory (TRRL), which was located about 30 miles from London.

The main building of RRL was an estate house which had been converted into an office building, but there were also some subsidiary one-story office buildings and laboratories. The atmosphere was very much like a university. I should have spent my sabbatical there instead of just one day.

The British, particularly a group of people at RRL under the direction of Reuben Smeed, were way ahead of everyone else in modeling of transportation systems.

Reuben Smeed was originally a statistician and worked on some operations research projects during World War II. At RRL, he was primarily involved in traffic safety issues, but he apparently was authorized to assemble a team to address a variety of operations and planning issues.

None of the people he hired had a background in traffic or transportation. Wardrop joined his group in the late 1940s. F. V. Webster, who had a background in physics, tried to evaluate delays for a fixed-cycle traffic signal. Failing to obtain an “analytic” solution, he resorted to numerical simulation and curve fitting to produce a formula that is still widely used today. He wrote a short monograph on this (Webster 1958). (Despite attempts by many people, it wasn’t until 1965 that an accurate analytic formula was derived—Newell 1965.) John Tanner, a mathematical statistician, did some work on queueing for gaps in a traffic stream when he wasn’t involved in safety statistics. E. M. Holroyd was involved with routing of trips on idealized networks.

Smeed seemed to be curious and enthusiastic about everything. I remember his showing me a toy truck. The truck would roll down an inclined plane past a switch which would suddenly cause the rear wheels of the truck to lock. The truck would quickly spin through 180° and proceed down the plane backwards. It seemed rather counterintuitive at first.

The next stop on my trip was Manchester, England, also a business stop. I had arranged to meet M. S. Bartlett and M. J. Lighthill, both of whom were then professors at the University of Manchester. When I arrived in the morning, Bartlett said that we would have lunch together, but that until then one of his students, Alan Miller, would show me what he was doing. Miller, at that time, was doing a master’s thesis in which he was trying to model traffic as a stochastic collection of traveling queues behind slow vehicles. He would later also do a Ph.D. thesis on the same subject and would remain active in the transportation theory field for at least the next 10 years.

Several other people also tried to develop models of traveling queues and others would later try again, but this led nowhere. It resulted in a mass of equations that had little relevance to any practical questions. It was an interesting mathematical exercise, however, to apply some of the latest techniques of the theory of stochastic processes to highway traffic.

Bartlett himself wrote only one paper that I know of dealing with highway traffic (Bartlett 1957), but it was
rather theorem oriented and meant to appeal to mathematical statisticians rather than being a serious contribution to traffic theory. Bartlett was already one of the leading mathematical statisticians in the world at that time and would later become even more so. He wrote one of the first books on the theory of stochastic processes (Bartlett 1955).

Lighthill was a world authority on fluid mechanics, aerodynamics, etc. He died in 1999 while trying to swim around some island, a distance of nine miles, at the age of 74. I was surprised to read from his obituary, which appeared in all the leading newspapers, that he was only one year older than I. He wrote his first scientific paper at the age of 19, however, and was already a distinguished professor when I met him (I was still a novice).

Neither Bartlett nor Lighthill continued to contribute to traffic flow theory after their brief encounters.

When I returned to the USA from my sabbatical, Robert Herman was busy organizing an international symposium on “The Theory of Traffic Flow” to be held in Detroit in December 1959, sponsored by General Motors. This was the first attempt to bring together people from around the world. I do not have a list of those who attended (about 100), but the proceedings were published (Herman 1961). There were only 14 papers presented during two days, many by the aforementioned people and others prominent in the OR community. Among those presenting papers: D. L. Gerlough, J. G. Wardrop, A. Charnes and W. W. Cooper (well known in the LP field), W. Prager, E. Kometani and T. Sasaki, R. Iterman and R. B. Potts, E. W. Montroll, A. J. Miller, L. C. Edie and R. S. Foote, and yours truly.

There was also a paper by I. Prigogine, well known in the field of thermodynamics, who tried to relate traffic to the kinetic theory of gases, and by W. Helly, a student of Philip M. Morse at MIT, the legendary physicist and first president of ORSA.

Reuben Smeed was presumably present because he immediately offered to host a second symposium in London in 1963, at which 34 papers were presented. A third symposium was hosted by L. Edie and the recently formed Transportation Science section of ORSA in 1965, at which 45 papers were presented. This series has continued to meet every three years under the title “International Symposium on Transportation and Traffic Theory” (ISTTT). The 15th symposium is to meet in Adelaide, Australia in July 2002. (I have the distinction of being the only person who has attended all the previous 14 meetings.)

The Transportation Science section of ORSA was formed in 1962. The journal Transportation Science, sponsored by ORSA, and the journal Transportation Research, did not start until 1966.

The ISTTT continues, there is still a transportation science section of ORSA (now INFORMS), and the above two journals dealing with transportation theory still exist, along with a few others. On the surface, it would seem that the subject has continued to grow and develop but actually, in my view, progress peaked in the 1960s and took a sudden dive in the 1970s. What has survived is not quite the same. Why so?

As noted above, many of the people who contributed to the modeling of transportation systems in the 1950s were internationally known and brilliant scholars in their own fields, but each was trying to apply to transportation problems the techniques he already knew from his specialty. To be sure, these techniques had an application to special situations, but it was a little like a solution looking for a problem (a common complaint about OR generally). After the techniques were exploited, these people returned to what they were doing before. They did not try to develop new techniques needed to analyze problems peculiar to transportation itself.

True, traffic behaves like a “stochastic process” (actually a moving stochastic process), but, except for very light traffic, it is not like any of the idealized models that the mathematical statisticians theorize about. It is messy and can be analyzed only by crude approximations. Delays in traffic are almost always because of queues, but they seldom behave like “equilibrium” queues. Conventional queuing analysis has been virtually useless as applied to practical problems in transportation. We had to develop our own techniques. Drivers follow each other using some type of control scheme, but it is not like that in the standard literature on control theory. They simply follow at a “safe driving distance.” Certain network problems are analogous to those in electrical circuit theory, but drivers, unlike electrons, have different destinations and their reaction to change is often difficult to predict. (If drivers follow an “optimal” path, we don’t know what the objective is.)

Economic theory is seriously flawed as applied to transportation because most economists treat transportation like a consumer good that can be sold to the highest bidder, but they don’t ask: “What does society want?” (I don’t know either.)

The journal Transportation Science has degenerated into a journal of computer algorithms and “optimization” relative to ad hoc objectives. There is seldom a paper dealing with some transportation issue or the answer to some question.

Transportation has its own special issues, unlike those in other fields, and these require special techniques of analysis. Progress is slow when new ideas and techniques are needed. As in many other branches of OR, most researchers just continue to rework and refine old procedures. Will we do better in the next 50 years?

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