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Algebraic Invariant Theory in Systems Research

Lecture 1

Historical and Personal Reflections on the Problem of Passive Network Realization

Rudolf Kalman[1]

ABSTRACT

The birth of theoretical electrical engineering could be dated to Heaviside's brilliant idea (early 1900's) of replacing linear differential equations by an algebraic symbolism. This allowed Ohm's Law to be applied not only to resistors but also to the reactive elements (capacitors, inductors) thereby creating a general notion of impedance. The problem of realization (this was not the terminology used a hundred years ago) arose immediately: "Given an impedance, is there a passive RCL network which has that impedance?" (The easy question, "Given a resistance, is there a resistor with that resistance?" has already given rise to an industry.)

Surprisingly quickly, a nice answer to such questions was given in 1924 by a young "mathematician", Ronald M. Foster (1896-1998), (B.S. in Mathematics 1917 from Harvard), whom Bell Laboratories was converting into an "engineer". His answer was YES, with reservations. In his work surroundings, Foster was concerned mainly with lossless (CL) networks, and, for that case, the answer was unequivocably "yes", with classical mathematics (polynomials and rational functions) providing precise, necesssary and sufficient, conditions. Foster created a family of thitherto unknown networks for the purpose and gave explicit formulas for the capacitors and inductors of those networks to realize the desired impedance. When (and only when) the computed component values turned out to be positive, the impedance was realizable.

The very next year, 1925, a brilliant young student at the famous Charlottenburg Technische Hochschule in Berlin, Wilhelm Cauer (1900-1945), who was just getting his Diploma, noticed that using the same reasoning as Foster but a different mathematical tool (continued fractions) easily solved the realization problem also for the RC and RL cases including Foster's CL results. In doing so, Cauer created another set of new families of networks, with new realizability conditions—which turned out to be equivalent to those originally discovered by Foster.

But the general case, passive RCL synthesis, proved to be extremely recalcitrant.

Unfortunately (??) neither the engineers nor mathematicians really understood Heaviside's genius idea: use Algebra. For the next 25 or 30 years, perhaps even today, the field of theoretical electrical engineering, even at the very best institutions, was mesmerized by the mirage (or delusion?) of the Laplace transform (used only in working with rational fractions) as the tool-for-everything. This created a wide stream of second-rate doctoral theses. Progress in creative research came to a halt.

But the problem was never forgotten. Curiously, a strongly held local dogma gained acceptance at MIT: "To solve any kind of difficult problem in theoretical (electrical) engineering, you must first find the right mathematics!" — Today any creative mathematician would strongly disagree: the research task of mathematics, equally well from the pure as from the applied bias, is to invent new frameworks (or deepen old ones) to tackle problems known to be important but previously inaccessible.

There was a spectacular exception to this uninspiring state of affairs. Otto Brune (1901-1982), a South African engineer, informally supervised by Wilhelm Cauer, had "solved" the RCL synthesis problem in 1931 in a justly celebrated doctoral thesis at MIT. He did that by adopting a nonalgebraic condition (positive realness) on the impedance which forced him, in order to prove sufficiency, to allow ideal transformers (equivalently, closely coupled inductances) in his realization algorithm, making it de facto useless for most practical purposes, but maintaining intellectual interest in the problem. Today, Brune's thesis is viewed as the first effective formulation of the concept of "passivity" in linear system theory.

When I arrived at MIT as a naive but eager junior in 1951, like my near environment, I was already under the magic of the Laplace transform. I soon found a mentor in Professor E. A. Guillemin (1898-1970), a wonderful person, creative as a teacher but not as a researcher. Guillemin's big tome on "Passive Network Synthesis" (1957), to which I was exposed in its formative stages as a student in his graduate courses 1952/1953, was written under the baneful influence of Brune's great contribution. Sadly, he extolled Brune as the "founder of passive network synthesis", veering away from the algebraic inspirations of Heaviside, Foster, and Cauer, and heading into a dead-end street.

Intrinsic uneasiness soon made me lose enthusiasm for mainstream network synthesis research. I turned in a different direction. I retained some nostalgic memories, however, and did not throw away my large reprint collection.

As we now know, I was not alone with my doubts—but no one knew it at the time (1953). Foster had long been unhappy about the direction of network synthesis after Brune. But Foster was not publishing, except, at the point of retiring

[1] kalman@math.ethz.ch

in 1962, he had a cryptic survey article in the Proceedings of the IRE, which found its way into my decaying reprint collection. I read (or re-read) it in 2004 and found, to my immodest amazement, that forty years later I could not reproduce any of the major results claimed on network realization. Defeat told me that Foster had been (must have been) thinking "outside the box". This is the reason why I am here today.

Foster had another major interest stemming from circuit theory. It was graph theory. It is the natural tool for studying networks composed of two-terminal (one-port) elements, like R, C, L. (Transformers are ruled out; they are passive but have at least two ports.) Foster then inverted the classical problem: "Ask not if a fixed impedance can be realized by some network; ask what impedances can be realized by a fixed transformerless (!!) network." This reformulation placed network synthesis outside the subworld ruled by Brune's criterion.

Foster must have been devoting a huge intellectual effort to this task from about 1940 on—easy for networks with few elements, increasingly difficult as the networks grow in size. Finally, the results of this labor were abstracted in a catalog masquerading as a Master's Thesis, May 1948, at the (then) Polytechnic Institute of Brooklyn, by Foster's student, Edward L. Ladenheim. Foster called this a "publication", but it was not. It became known, much later, only to careful readers of Foster's survey paper of 1962: to me in 2006 when I got a copy of the thesis by personal contact, to Professor Malcolm Smith and Jason Jiang in 2008.

Well, what do we find?

An impressive collection of data (generally correct!) in the form of graphs, circuit diagrams and formulas. Nothing else, practically no explanations. The catalog lists for 108 "basic graphs" and the corresponding networks (I call these "generic networks") the following information:

1) Circuit diagram of the generic network;
2) Its impedance;
3) Realization formulas from impedance to network.

The catalog is very carefully constructed: the list of 108 generic networks (up to and including all networks with two (2) reactive elements) is complete, the formulas for the impedance are correct (of course, these calculations are quite easy), and the simpler realization formulas are probably all correct, but there can be doubts about the complex cases. There are some one thousand (1K) formulas in the catalog, no explanations, a really big, intriguing mess.

The big puzzle is: Foster left practically no explanations, details, examples as to how he arrived at these results. Some of these results are still unchecked; at this conference we will be talking about many things that are now clear and can be generalized to higher-order cases.

To me, Foster's most fruitful discovery—he never mentions it, he did not seem to be aware of it—was that his "no transformers" network realization problem falls squarely into the framework of algebraic invariant theory of the second half of the 19th Century, the special case of projective invariants for two polynomials (so called "Grundformen"). The catalog of "basic networks" was created exactly 50 years after Hilbert's groundbreaking investigation on invariants shifted the interest of algebraists away from applications, like Foster's. This conclusion is obvious from looking at just a few of the thousand formulas, and becomes undebatable after gaining some understanding of the results of the catalog.

So, fifteen years after Foster died, we state the utterly obvious:

Algebraic invariant theory is the natural and effective tool for the network synthesis problem. Many currently unsolved research problems in the area of system theory can be fruitfully attacked now. Adding algebraic invariant theory to the highly developed mathematical theory of linear systems opens new horizons.

Vladimir Popov (Steklov Institute, Moskva), the leading mathematical specialist in Algebraic Invariant Theory, mentioned to me recently that "He who is used to 'mathematical thinking' in some well-defined subject area finds it very hard to reorient himself into a different area because of concepts, culture, traditions specific to each field of research." This observation will help me explain why followers of Brune and Guillemin got lost in a desert, and perhaps why Foster stopped, so short of his goal. And also how I arrived by "mathematical thinking" at the remarkable result summing up Algebraic Network Realization Theory:

MAIN THEOREM

Every generic network is uniquely characterized by its own family of invariants and covariants. A network realizes an impedance if and only if they both have the same invariants. Two different networks are globally isomorphic if and only if they have the same invariants and covariants. From the knowledge of the invariants and covariants of a network it is a straightforward task to compute all possible realizations (there may be more than one, they are all isomorphic).