卡尔曼对我科研工作的影响
——一些追忆和思考

安德斯•林德奎斯特　瑞典皇家理工学院

1959年，还不到17岁的我得到奖学金支持作为外国交流生来美国读高中。那时艾森豪威尔还是美国总统。我到达的时候恰逢苏联发射了“伴侣号”（Sputnik）人造卫星，在冷战的太空竞赛中领先一步。这在美国引起了对人造卫星的恐慌，美国人甚至在思考是不是他们的教育体系衰退导致了落后。尽管只是个中学生，我也被邀请到扶轮社（Rotary clubs）活动和学校董事会议去做演讲，有两次我还被当地电视台采访来比较美国和瑞典的教育体系。

当时我并不知道后来会认识一些和太空竞赛至少间接相关的重要人物。1969年，美国已经成功登月，在1972年，鲁迪•卡尔曼告诉我没有卡尔曼滤波登月是不可能的。后来在20世纪80年代，我成了莫斯科控制科学学院的常客，时常来见这里的自动控制先驱，而在20世纪90年代苏联解体后，我与圣彼得堡的弗拉基米尔•雅库波维奇开展了紧密合作。这篇随笔源自我在IEEE控制系统杂志上的一篇文章（vol. 37, no 2, April 2017），是关于卡尔曼的，但后面还会提及雅库波维奇。

1972年春季，我在奥地利维也纳第一次见到了卡尔曼。当时我刚完成瑞典皇家理工学院的博士论文，受邀在一个小型论坛上讲我当时在随机控制理论方面的成果，而卡尔曼也参加了那个论坛。卡尔曼显然是对我的报告印象深刻，以至于当晚带我去吃晚饭并邀请我下一个学年到佛罗里达。当时他刚离开斯坦福到佛罗里达大学就职。就这样，我成了他1972年新成立的数学系统理论中心的第一个博士后。

我第一次见到卡尔曼正是在他科研产出达到顶峰的时候。他完成了数学系统理论中代数理论方面的相当漂亮的工作，详见参考文献[1]。此前十年，他就已经完成了我们今天称之为“卡尔曼滤波”的完整的理论。当时有人对卡尔曼的结果存疑，还有的说有人在他之前就已经推导出了类似的滤波方程。我始终认为这对卡尔曼是极其不公正的。卡尔曼滤波不仅仅是一组滤波方程。它是系统理论的重要组成部分，包括矩阵Riccati方程，并且推广到诸如最优控制反问题这样的其他许多问题。实际上，卡尔曼对问题有非常独

到的眼光，而且知道怎么用有趣、美妙的语言来阐述。尽管他受的是工程师教育，但他总是希望别人当他是数学家，这不是没有原因的。他看待问题时的确非常重视数学家特有的美和清晰的原则。他可以抛开工程文献中经常遇到的次要考虑因素，直接抓住根本的数学问题。他另一个值得书写的长处，是确定标准，规定记号、整体风格和严格的语言，正是这些使得我们这个研究群体区别于其他应用数学方向和工程群体。

作为一个科学家，我从卡尔曼身上受益良多。他关于实现理论的工作，启发了我在Szego多项式中用系统矩阵替代变量，正是这个小技巧，促成了一篇提交于1972年、介绍在卡尔曼滤波中用一种快速算法替代Riccati方程的文章[2]。此外，卡尔曼关于卡尔曼–雅库波维奇–波波夫（KYP）引理的早期工作，是我和乔治奥•皮齐（Giorgio Picci）在随机实现理论方面工作的重要组成部分。更重要的是，在表示和看待问题的方法上，他是我的榜样。

关于这一点，一个例子是合理协方差扩展问题，如卡尔曼在文献[6]中所表示。他对这个问题非常着迷。他几乎就踏上了正确的方向，但方向正确与否不是最重要的。他追求简洁和对称，并为部分随机实现极小度寻找矩阵秩条件，就像确定性部分实现理论中的Hankel条件那样。现在我们知道这是不可能的，详见文献[7]中的定理2.2。此外，卡尔曼深信代数是系统理论的终极工具，他认为解集可能由Schur参数在代数约束下参数化，很不幸这也是条死胡同。特里丰•乔吉欧（Tryphon Georgiou），卡尔曼最聪颖的学生之一，于1983年第一个攻克了这个难题[8]，使用的是分析和拓扑，而不是代数。受卡尔曼和乔吉欧部分结果（我当时并不知道乔吉欧做出这样的结果）启发，我最终和克里斯•伯恩斯（Chris Byrnes）合作，尝试解决这个问题。这是项时间跨度很长的研究，在此期间我们通过乔吉欧的一篇文章[9]了解到了他的工作。我们在和古谢夫（Gusev）及马特维耶夫（Matveev）合作的文章[10]中最终解决了乔吉欧这篇文章遗留的问题，继而提出一种解决该问题的凸优化方法，之后我们联合乔吉欧发了一系列文章，把相同的原则用在一些应用问题和纯数学问题上，比如推广萨拉森（Sarason）一个关于广义插值的结果。所有这些努力最终推动了一个关于带复杂性约束的矩问题的统一数学理论产生，并催生了一个可用于将解平滑参数化、比较和形塑为规范的范例。这个理论可以应用到系统与控制的许多重要问题领域中，包括信号处理、语音处理、鲁棒控制、模型降阶、图像处理、系统辨识和信息融合。

在数学系统理论中心，我也有幸见到了KYP引理的作者之一V.M.波波夫，他也被邀请到了卡尔曼的这个中心。不过，因为不在本文讨论范围内的原因，波波夫很快离开了中心，在1972年秋季转到了数学系，此后不久我也转到了数学系。我在卡尔曼中心任职总共只持续了四个月，而不是原计划的一年，我和波波夫的讨论在在数学系得以继续。而KYP引论的另一位作者，V.A.雅库波维奇，后来成了我的紧密合作伙伴和好朋友。他经常访问我在瑞典皇家理工学院的研究组，我们合作了5篇文章，大部分关于受迫振荡的追踪和阻尼优化的通用调节器。不幸的是雅库波维奇几年前疾病缠身，继而去世，一个重要的、时常给我启发的合作伙伴，一个非常好的人，离去了。

卡尔曼曾不止一次跟我讲起他的艰难历程。他关于卡尔曼滤波的开创性文章，最开始竟然被拒稿。结果他决定在一个非顶级的机械工程期刊上发表这项开拓性工作。这样的拒稿在之后几年里一再上演，加上一些类似的其他事件，使他对

科研群体和他本人在其中的位置有了一些看法。外界把卡尔曼滤波的概率表示过程假设成高斯的，他对此很反感。实际上他认为卡尔曼滤波完全是确定性的问题。这一点我恰好认同[12]。

某种程度上，卡尔曼是他早期成功的受害者，和许多早年就获得巨大成就的数学家一样。而他本人在做出了一系列划时代的贡献后，开始寻求解决一些后来证明是他无法逾越的待解决难题。就这样，卡尔曼后半生的很多努力并没有产出非常重要的成果，大多是悬而未决，但提出了一些很有启发性的问题。如果能够更好地与人合作并支持年轻研究人员，卡尔曼本来可以在控制与系统学界扮演更重要的角色。那样的话他的洞察力和眼光将会是合作中的珍贵宝藏。

该中译稿由本刊编辑贺瑞君翻译提供。

Kalman's influence on my scientific work

Some recollections and reflections

Anders Lindquist

At a time when I had not yet turned 17, I got a scholarship to spend my senior year in high school in the United States as a foreign exchange student. This was in 1959, when Eisenhowerwas still the President of the United States. I arrived at a time when the Soviet Union had just launched its Sputnik satellites, thus achieving a lead in the cold–war race for space. This led tothe ”Sputik scare”
in the United States, and Americans were considering the possibility that the decline of their school system was causing them to get behind. Despite being a young kid, I wasinvited to give lectures on education in Europe at Rotary clubs and school board meetings, and at two occasions I was interviewed on local television comparing the quality of the Americanschool system to that in Sweden.

What I did not know at this time, was that I would get to know some key players connected,at least indirectly, to this space race later in life. By 1969 the United States had been able to put a man on the moon, and later in 1972 Rudy Kalman would tell me that this could not havebeen achieved without Kalman filtering. Later in the 1980s I would be a frequent visitor at the Academy Institute of Control Sciences in Moscow to meet pioneers in automatic control, andlater in the 1990s, after the fall of the Soviet Union, I would have close collaboration with Vladimir Yakubovich in Saint Petersburg. The focus of this essay, which is based on a articlein IEEE Control Systems Magazine (vol. 37, no 2, April 2017), will be on Kalman, but I shall briefly return to my relation to Yakubovich later.

I first met Kalman in Vienna, Austria, in the spring of 1972. I had recently finished my PhD at the Royal Institute of Technology (KTH), Stockholm, Sweden, and I was invited to givea talk on my recent results in stochastic control theory at a small workshop that Kalman also attended. Apparently Kalman was favorably impressed with my talk, for he took me out for
dinner the same evening and immediately invited me to come over to Florida for the coming academic year. Kalman had just moved from Stanford to the University of Florida, and thisis how I became his first postdoctoral associate in his new Center for Mathematical Systems Theory in the fall of 1972.

When I first met Kalman, his impressive scientific output had just culminated. He had finished his work on a beautiful algebraic theory of mathematical systems theory; see, e.g.,[1]. A decade earlier he had completed a comprehensive theory on what we today call Kalman filtering. At the time, some of Kalman's critics claimed that the results were in the air and that
September 5, 2017 DRAFT1others had derived similar filtering equations before him. I have always claimed that this position is grossly 2 unfair. Kalman filtering is not just a set of filter equations. It is an important part of
systems theory that includes a theory for the matrix Riccati equation and provides extensions to many other problems like the inverse problem of optimal control. In fact, Kalman had a very good taste for problems and knew how to

formulate them in an interesting and beautiful way. He always wanted to be regarded as a mathematician rather than an engineer to which
he had been educated, and not without reason. Indeed, his look at problems was that of a mathematician for which beauty and clarity of principle was paramount. He would identify the underlying mathematical problems and remove secondary considerations often encountered in the engineering literature. He should also be credited with fixing standards, canonizing notations and prescribing an overall style and a rigorous language which distinguished the writing of our community from other applied mathematics and engineering communities.

As a scientist I owe a lot to Kalman. His work on realization theory inspired me to replace the variable in the Szego ¨ polynomial by the system matrix, a trick that eventually led to the paper [2], submitted in fall of 1972, introducing a fast algorithm for Kalman filtering in lieu of the Riccati equation. Moreover, Kalman’s early results [3] on the Kalman–Yakubovich–Popov(KYP) Lemma became an important building block in my work with Giorgio Picci on stochastic realization theory [4]; also see [5]. More importantly, he has been a role model for me in his way of formulating and looking at problems.

A case in point is the rational covariance extension problem, formulated by Kalman in [6]. He was obsessed by this problem. He was hardly on the right path, but that matters less. He wanted simplicity and symmetry and was looking for a matrix–rank condition for the minimal degree of a partial stochastic realization, akin to the Hankel condition in deterministic partial realization theory. Today we understand that this cannot be done; see, e.g., [7, Theorem 2.2]. Moreover, at this time being a firm believer in algebra as the ultimate tool of systems theory, Kalman thought that the solution set could be parameterized by the Schur parameters subject to algebraic constraints, which also turned out to be a dead end. Tryphon Georgiou, one of Kalman’s most brilliant students, made the first crack on this problem in his thesis [8] in 1983 using analysis and topology instead of algebra. Inspired by Kalman and initially oblivious of Georgiou’s partial results, I eventually got together with Chris Byrnes to try to solve this problem. This led to a long stretch of research during which we became aware of Georgiou’s results via his paper [9]. We finally solved the part missing in [9] in a paper together with Gusev and Matveev [10] and subsequently proposed a convex–optimization approach to the problem [11], after which we joined with Georgiou for a long series of papers applying the same principles on a number of applied problems as well as problems in pure mathematics, e.g., generalizing a result of Sarason on generalized interpolation. All this eventually resulted in a unified mathematical theory for moment problems with complexity constraints, leading to a powerful paradigm for September 5, 2017 DRAFT 2 smoothly parameterizing, comparing, and shaping solutions to specifications. This theory could be applied to many important problem areas in systems and control, including signal processing, speech processing, robust control, model reduction, image processing, system identification and information fusion.

In the Center for Mathematical Systems Theory I also had the pleasure of meeting V.M. Popov from the KYP Lemma, who had also been invited to Kalman’s center. However, for reasons that are beyond the scope of the present account, Popov soon left the center and moved to the mathematics department during the fall of 1972, and so did I shortly thereafter. Altogether my affiliation with Kalman’s center lasted only four months, rather than the full year originally planned, and my discussions with Popov continued in the mathematics department. The third person in the KYP Lemma, V.A. Yakubovich, later became my collaborator and dear friend. He became a frequent visitor in my group at the Royal Institute of Technology (KTH), and we coauthored five papers, mostly on universal regulators for optimal tracking and damping of forced oscillations. Unfortunately his illness and subsequent death a few of years ago put and end to my collaboration with a valued and inspiring colleague and a wonderful person.

Kalman many times told me about his uphill battles. His seminal paper on Kalman filtering was initially rejected. As a result, he decided to publish this ground–breaking work in a less prestigious mechanical engineering journal. This initial rejection, to which he repeatedly came back in latter years, and a few other similar events colored his view of the scientific community and his own place in it. He disliked probabilistic presentations of the Kalman filter where the processes were assumed to be Gaussian. In fact, he considered Kalman filtering a completely deterministic problem. On this point I happen to agree with him [12].

In a sense, Kalman became the victim of his own early success. Like many mathematicians with remarkable success in their early years, and in his case, a series of truly transformative contributions, he came to look for open problems that proved to be beyond his reach. In this way, much of his efforts during the second part in his life did not lead to substantial scientific results, but mostly loose ends and insightful problem formulations. Kalman could have become a much more important asset to the systems and control community had he been a better collaborator and supporter of young researchers. In fact, his insights and good taste for problems could have been a gold mine in such collaborations.

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