# History of the study of association schemes, a personal view

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**Misha Klin asked me to talk on the history of association schemes. So, I will try.**

**However, this is not easy at all, as I am ignorant of many aspects of the history of association schemes. Actually, I would be able to talk, only from my very personal experiences and only from my very personal viewpoint.**

**So, the scope of this talk is very much limited. I would like to ask for your permissions and understandings about this.**

**Around 1968, as a student of Tokyo University in Japan, I started my research in finite group theory. In particular, I studied on finite permutation groups and on finite simple groups, as well as on finite Chevalley groups and their representations. My advisor was Professor Nagayoshi Iwahori. In addition, Professor Micho Suzuki and Professor Noboru Ito, both of them were in USA then, influenced us very much.**

**First let me explain the correspondences between the group theoretical concepts and the combinatorial concepts.**

**(i) coherent configurations $\Longleftrightarrow$ general (not necessarily transitive) permutation groups**

**(ii) general (not necessarily commutative) association schemes $\Longleftrightarrow$ transitive permutation groups,**

**(iii) commutative association schemes $\Longleftrightarrow$ multiplicity-free permutation groups (Gelfand pairs),**

**(iv) symmetric association schemes $\Longleftrightarrow$ generously transitive permutation groups,**

**(v) distance-regular graphs $\Longleftrightarrow$ distance-transitive groups (permutation groups of maximal diameter),**

**(vi) strongly regular graphs $\Longleftrightarrow$ rank three transitive permutation groups,**

**(vii) complete graphs $K_n$ $\Longleftrightarrow$ rank 2 (i.e., 2-transitive) permutation groups.**

My interests started in the reverse order

$$\text{(vii)} \to \text{(vi)} \to \text{(v)} \to \text{(iv)} \to \text{(iii)} \to \text{(ii)} \to \text{(i)}.$$

Namely, I was first, most interested in (vii) : multiply transitive permutation groups. I was very much interested on this topic, mainly through reading the book by Wielandt: Finite Permutation Groups (1964) and from the tradition of the study of permutation groups in Japan. (N. Ito, Suzuki, Nagao, Tsuzuku and others.) At that time (of late 1960's) "no nontrivial 6-transitive groups were known, and the known 4-transitive permutation groups were Mathieu groups $M_{24}, M_{23}, M_{12}, M_{11}$." Now, using the classification of finite simple groups (CFSG), all the 2-transitive permutation groups have been classified. Hence, it is known that there is no nontrivial 6-transitive groups and that Mathieu groups $M_{24}, M_{23}, M_{12}, M_{11}$ are the only 4-transitive permutation groups.

However, a proof which does not use the CFSG is not known.

I personally think that it would be very desirable (although there is currently no real hope at all) to get a proof which does not use the CFSG.

**Then I was interested in (vi): rank three transitive permutation groups, with the hope of finding new sporadic finite simple groups. Several new sporadic finite simple groups such Hall-Janko, Higman-Sims, Suzuki, etc. were discovered in the late 1960's as rank 3 permutation groups. (But all the sporadic simple groups were already discovered, when I started to search new finite simple groups.)**

**I read the paper of Sims on strongly regular graphs as well as the paper of Hoffmann-Singleton (1960) on Moore graphs of diameters 2 and 3.**

**Then I was interested in the concept given in (v), through reading the papers,**

**D. G. Higman: Intersection matrices for finite permutation groups (1964),**

**W. Feit and G. Higman: On finite generalized polygons (1964), etc.**

I read many papers on finite permutation groups as well as papers on distance-transitive graphs, including the work of Biggs : Finite Groups of Automorphisms (1970). My main interests were mainly on permutation groups, and to study them along the line of D. G. Higman. Then naturally I knew the concept of Moore graphs of diameter $d$, as distance-regular graphs of diameter $d$.

Then jointly Tatsuro Ito, we solved the non-existence of Moore graphs of diameter $d \geq 4$, (and $k \geq 3$). (At that time Tatsuro was an undergraduate student at Tokyo University and I was supervising him as a junior faculty.)

The same result was also obtained by R. M. Damerell independently. Here, I understood that the combinatorial formulations of the problem is more transparent than the formulation in group theory. Of course there is a big advantage of being able to use stronger group theoretical tools if the problem is formulated as a group theory problem, but the result in combinatorial formation is stronger and more desirable, if the problem could be solved in the context of purely combinatorial setting. (It seems that it is not so easy to solve the Moore graph problem in the group theoretical setting as distance-transitive graphs.)

**In around 1974, I was offered a tenure track position from Ohio State University. At that time, and even now, I was not good at English, and I was very much surprised that I could get the job. I was working mainly on finite permutation groups and I think perhaps the work on Moore graphs was recognized by some people in USA. So, I went to the Ohio State University in 1974-1976. My English was very poor, and it was not possible for me to teach the undergraduate courses, and so I was allowed to teach only graduate courses in the first year. Ray-Chaudhry and R. Wilson were there as well as group theorists Koichiro Harada, Ron Solomon and Zassenhaus, among others.**

**I was in the Ohio State University, in 1974-1976 and 1978-1989. Before going to Ohio, I attended the NATO Advanced Study Institute on Combinatorics held in Nijenrode castle in the Netherlands for 4 weeks, and I met many mathematicians there, including Delsarte, Cameron, Higman, Kantor, Brouwer, and many others, (the organizers were M. Hall, Jr. and van Lint).**

**So, around 1974, I already knew the work of P. Delsarte: An algebraic approach to the theory of association schemes (1973).**

I was strongly impressed with the work of Delsarte (1973), in particular with the concept of Q-polynomial association schemes (and the duality between $A_0, A_1, \ldots, A_d$ and $E_0, E_1, \ldots, E_d$) as well his philosophy that association schemes give a good flame work to study coding theory and design theory from a unified viewpoint. I really saw a strong backbone of good theory in the philosophy of Delsarte. (I felt some similarity of it to the philosophy of the classification problem of finite simple groups.)

So, I gradually moved my interest from finite groups (finite permutation groups) to combinatorics.

Later, when we wrote our book: Algebraic Combinatorics, we stated that the work of Delsarte marked the start of algebraic combinatorics. Dijen Ray-Chaudhuri commented that he cannot agree completely with that statement. He thinks that many different streams, including purely combinatorial earlier work (with statistics origin of Bose school) as well as studies in each individual area such as "algebraic coding theory", "algebraic design theory", and "algebraic graph theory", etc. collectively led to this research direction, called "Algebraic Combinatorics". I very much agree with his opinion. Nonetheless, I think the work of Delsarte was very notable, and that the theory of association schemes became very deep by the encounter with the group theory.

**The fact that the CFSG would soon become to be completed (and it was announced to have been completed around 1981) changed the situation quite a lot. Personally, I was more interested in the problems that do not depend on the CFSG and solving the problem at the purely combinatorial level. Yet, I was interested in the problems like pursuing the classification, like the CFSG.**

**I was interested in the representation theory of finite Chevalley groups, as I already mentioned. Also, I was interested in the classification problem of distance-regular graphs in (v). Also, as I mentioned already, I was very much impressed with the important concept of Q-polynomial association scheme (defined by Delsarte). In addition, the calculations of spherical functions associated to certain homogeneous spaces by C. Dunkl, D. Stanton, and others. So, these led me to the following observations and problems.**

**(a) As far as considering the known examples (of large class number $d$) of P-polynomial association schemes and Q-polynomial association schemes, they overlap greatly.**

**(a-i) Can we show that a P-polynomial association scheme is Q-polynomial, and/or Q-polynomial association scheme is P-polynomial if $d$ is large? (No counter example is known for primitive association schemes, although some counter examples are known for the imprimitive case.)**

**(a-ii) Can we characterize P- and Q-polynomial association schemes? Can we classify P- and Q-polynomial association schemes with large $d$.?**

**As you know, (a-i) is still open, although there are many recent work on Q-polynomial association schemes which are not P-polynomial. Some small $d$ cases are studied extensively and some such examples are obtained (Penttila, Moorehouse, Williford, etc.).**

**(a-ii) was first successfully solved by D. Leonard (1982, 1984) in the sense that their spherical functions (essentially equivalent to the character table of the association scheme) are expressed by Askey-Wilson polynomials or their relatives (some special cases or some limiting cases). I think this work of Leonard opened a very close connection between theory of orthogonal polynomials and that of association schemes.**

**Tatsuro Ito and I wrote a book: Algebraic Combinatorics, I (Association Schemes) which was published in 1984 by Benjamin/Cummings. We proposed the classification of P-and Q-polynomial association schemes as the main target in this research direction. We used the terminology "Algebraic Combinatorics" intensionally, to indicate this new direction of research.**

**Classification problems of P-and Q-polynomial association schemes with the given spherical functions (equivalently with the given parameter set $p^k_{i,j}$) were studied more extensively than before, (in addition to some earlier work by Bose and others). Then the book "Distance Regular Graphs" by Brouwer-Cohen-Neumaier (1989), which gives an encyclopedic treatment on these and related topics, was published, and gave a very good guide on these subjects.**

**Around 1990, Paul Terwilliger introduced the concept of "Subconstituent algebra (Terwilliger algebra)" and used it aiming at toward the classification of P-and Q-polynomial association schemes (with large $d$).**

**At the beginning, the situation looked very promising, but it turned out that the situation is far more delicate. Terwilliger further introduced the concept of "Tri-diagonal pairs" that is an extension of the concept of Leonard pair, and worked with Tatsuro Ito and others extensively. Note that the concept of Leonard pair describes the spherical functions (the character table) of the P-and Q-polynomial association scheme.**

**Very recently, Ito and Terwilliger succeeded in the determination of the irreducible representations of Tri-diadonal pairs. I expect that an up-to-dated explanation will be explained tomorrow in the talk of Tatsuro Ito. It is still an interesting open problem how these results will actually be applied to the classification of P-and Q-polynomial association schemes (with large $d$).**

# Further works on association schemes

Although we proposed the classification of P-and Q-polynomial association schemes (with large $d$) as the main target, there are many many different directions for the study of association schemes and their generalizations.

(1) The general study of commutative association schemes (Cf. Problem (iii)).

The study of commutative association schemes (equivalently the study of Gelfand pairs or the study of multiplicity-free permutation groups in the group theory terminology).

**It would be very important to grasp what are the spherical functions of general commutative (or symmetric) association schemes. Namely, we want to know the possible character tables of these association schemes. (This is equivalent to know the spherical functions.)**
**The concept of compact symmetric spaces were defined and studied and classified by E. Cartan in 1920's. We want to study (commutative) association schemes as a discrete analogy of them. Note that the concept of P- and Q-polynomial association scheme is an analogy of so called rank** 1 **symmetric spaces. It seems that there are general classes of P-and Q-polynomial association schemes of arbitrarily rank (although it is not carefully studied yet) whose spherical functions should be described by multi-variable polynomials, which should be regarded as multi-variable version of Askey-Wilson polynomials.**

**I think it would be the most important problem to understand what is the most natural concept of "multi-variable" Askey-Wilson orthogonal polynomials. (Several multi-variable versions of Askey-Wilson polynomials are already proposed and studied. But I think the right definition is not yet successfully given.)**

**(2) Generalizing the concept of association schemes.**
**Again, there are many directions.**

**(2-a) Drop the combinatorial property.**

**Namely, consider in a more algebraic level. (Say, "table algebras", or "hyper groups")**

**Earlier work can be seen in the work of "character algebra" by Kawada (1942) and Hoheisel (1939). Some work of M. Krein (1949-1950) in functional analysis may be relevant. Cf. the work of L. Scott: Some product of character products (1977) concerning the non-negativity of Krein parameters $q_{i.j}^{k}$ of an association scheme.**

**(2-b) Drop the symmetricity or commutativity, and consider association schemes in general. Problem (iii), (ii) (Say, study of distance-regular digraphs.)**

**I think it would be important to study when they are Schurian, namely when they are coming from groups, as many works have been done by many people here. (Cf. many many talks given here.)**

**(2-c) Drop the transitivity (when it comes from a group), namely to consider "coherent configurations". Problem (i).**

**Note that the concept of "coherent configurations" was introduced by Higman (around 1970) and very much studied. I understand that the essentially the same concept was introduced in Russia (by Weisfeiler and his school ?) around the same time, as we will see in the title "W-L" of this conference. The concept of "coherent configuration" is certainly the most general and most natural concept generalizing the concept of association schemes. Coherent configurations are composed of association schemes in a sense. The composing is very involved, and it is very similar to "how general finite groups are composed of simple groups", or "how imprimitive association schemes are composed of primitive association schemes".**

**(2-d) Association schemes on triples (or on $t$-tuples).**

**Association scheme was defined as a pair of a set $X$ and the set of relations $R_i$ ($i = 1, 2, \ldots, d$) on $X$. Namely, $R_i$ are subsets of $X \times X$. How about if we take the $R_i$s as the subset of $X \times X \times X$ ?**
**This is an interesting problem, and there are some works already done on this problem, e.g., Mesner-Bhattacharya (1990). On the other hand, it seems that the study in this direction is not very successful so far, although I do not know why this research is difficult. Further study would be very desirable.**

**(2-e) Association schemes on infinitely many points.**

**Generally Riemannian symmetric spaces (or homogeneous spaces for compact Lie groups) are natural analogy of association schemes (for the continuous case). Beyond this, there are some attempts, see e.g., P.-H. Zieschang, Barg-Skriganov (2015), etc., but I am not familiar on these works to be able to make any useful comments.**

## The origin of association schemes

The concept of "association schemes", as well as "strongly regular graphs", is due to Bose and his school, including Nair, Shimamoto, Mesner and others (around 1940-1955 ?)

It seems that the origin is from partially balanced incomplete block designs (PBIBD). These were studied in statistics, and particularly in the area of experimental designs, as the name "Fisher's inequality" shows.

Some classification problems of strongly regular graphs and association schemes were studied starting in 1950's. For example, the characterizations of Johnson association schemes $J(v, d)$ had been started in late 1950's by Bose and their school, and studied by Chang (1959), Bose (1963), Bose-Laskar (1967), Dowling (1969), Liebler (1977), Moon (1983), and finally by Terwilliger (1986) and Neumaier (1985).

**An interesting fact, we knew rather recently, was that Mesner knew the strongly regular graph $(v, k, \lambda, \mu) = (100, 22, 0, 6)$ corresponding to the Higman-Sims rank 3 permutation group, as early as 1956 (Ph.D thesis) and 1964 (mimeographed note). However, it was not known to many researchers, including myself, until we (Bannai-Griess-Praeger-Scott) edited the memorial issue of D. G. Higman in 2009 in Michigan J. Math.. This was a real surprise for us, but also very understandable.**

**In my very personal opinion, the theory of strongly regular graphs and association schemes, became very rich, after the encounter with finite group theory, say the discovery of finite sporadic simple groups as rank 3 permutation groups. Although the concepts of strongly regular graphs and association schemes are the best formulation of the problems (compared with the permutation group theoretical formulation of distance transitive graphs), its importance may not had fully been appreciated even among the researchers themselves working on these topics at that time. Maybe, this explains why the work of Mesner was not widely known. Although the strongly regular graph was there, it was not well recognized that its automorphism group is a new finite simple group (discovered by Higman-Sims in 1968)! Please see the articles by Jajcayová-Jajcay-Krammer (2010) and Klin-Woldar (2011, 2017) on this particular topic.**

**I think there are other sources of the ideas of association schemes and related concepts.**

**The book of Wielandt (1964) explains some of them: Schur rings, transitive permutation groups of degree $2p$. Later, the last topic was generalized by an unpublished paper of Peter Neumann on the study of transitive permutation group of degree $3p$.)**

**Certainly some of the ideas go back to Schur, as well as older representation of finite (permutation) groups, such as by Burnside, Frobenius, Maillet, Dedekind, etc., some of them go back to 19th century.**

# Interaction with Russian mathematicians

(1) J. J. Seidel was a kind of ambassador of mathematics (combinatorics) and visited many places and gave many news and information. Actually, he visited us to Columbus Ohio (I think around 1978–79.) He gave me many new informations on “spherical designs and codes”, and related topics. I had started my research on spherical designs around 1976–1977. Besides that I also remember that he explained to me about the book: “On Construction and Identification of Graphs” (1976), edited by Weisfeiler. At that time, I could not understand the details, but I was impressed with the originality of the mathematics in the book. It seems to be deeply based on the Russian school of mathematics. It may be strange to say, but I was also very impressed with the fact that the English was not so good. Nevertheless, it was developing their own mathematics very forcefully. (At that time, it was usually said, among mathematicians in USA that the English of Japanese and Russians are usually not good. So, I felt some sense of closeness.) Seidel said that he arranged so that the book was published from the Springer lecture notes series of mathematics.

**Incidentally, I got a request from Weisfeiler to send an offprint of our paper: Eiichi Bannai and Etsuko Bannai, On finite subgroups of $GL(n, \mathbb{Q})$ (1973, J. Fac. Sci. Univ. Tokyo). At that time, it was common to ask offprints to the author, as in order to get a paper, there was no internet. (So we usually got papers either from the library or directly from the author.) That paper of us was a paper trying to generalize the concept of reflection groups by considering the groups generated by elements whose non 1 eigenvalues are at most two. The result obtained there was very partial and not strong at all. But it was clear that he understood our intension very well, and so I was very encouraged. Although I did not have a chance to meet him, we had some exchanges of communications. What I remember in the correspondence is that he had some good Japanese mathematician friend in California(?) (If I am correct that is Hidehiko Yamabe who is famous on his work on Hilbert's 5th problem but he died young in USA) and his wife's name was also Etsuko.**

**Shortly after that, I learned that Weisfeiler had disappeared in Chili, so I was very surprised and felt very sorry. I feel that he had a very broad mathematical interests, and if he should had lived longer, we could have more interactions.**

**(2) I do not remember how our correspondences with Russian mathematicians has started. I remember that I was very much impressed with the paper of A. Ivanov "Bounding the diameter of a distance-regular graph" (1983).**
**Soon after we published our book of Bannai-Ito: Algebraic Combinatorics I, we got a request from A. Ivanov and his group that they would like to translate it into Russian. The Russian translation was actually published by "Mir Publishing House" in 1987.**
**Around that time, it became possible for Russian mathematicians to travel to the western countries more easily. I remember that I first met some of them in Montreal conference in 1986, and then met A. Ivanov in Calcutta India (in Bose Memorial Conference) in Dec 1988.**

We started more communications with Russian mathematicians, and we could invite some of them (A. Ivanov, Faradjev, A. V. Ivanov, Shpectorov, Klin) to Japan at the time of ICM1990 in Kyoto. I also had chance to visit Moscow in Russia (Soviet Union then) before ICM1990 at System Institute and met Klin and Muzychuk there. In 1991. A. Ivanov and others organized a workshop in Vladimir near Moscow, and many of us from Japan participated in it. Since then, we have many more chances to meet at various places in the world.

I could continue more, but I think I have almost explained what I wanted to say, and I almost used up my assigned time. So, let me stop here.

Thank you very much again, for letting me allow to present my very personal experiences and personal views, in this conference WL 2018 in Pilsen, which celebrates the 50 years of the work of Weisfeiler-Lehman.

**Thank You**