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# With Tosio Kato at Berkeley

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I am looking back at a period of about 36 years of collaboration (and, later, friendship) with Tosio Kato, mostly, while being his collegue at U.C., Berkeley, 1962-1988, and, after his retirement, 1988-1999.

Actually, 1963 was not the begin of my acquaintance with his name or person. It so happened that we both grew up within very similar mathematical climates, but at different ends of the world: In Japan, and in Germany.

In fact, while I was a member of a well groomed community, Kato seems to have developed his environment all on his own, living in the countryside of Japan, during the second world war - according to his remarks at occasion of award of the Wiener prize, in 1980.

I am a student of Franz Rellich, at Goettingen. Rellich started the rigorous investigation of analytic perturbation of self-adjoint eigenvalue problems, in a series of 5 papers, in the 1930-s [Re] , in the light of the quantum mechanical publicity of such problems.

(Schroedinger [Schr] had given a formal discussion of the Stark effect, as perturbation of the hydrogen atom by an electric field, using such calculus.)

In the early 1950-s Rellich came back from a trip to the USA, and told me about a young physicist, Tosio Kato. He had asked Kato a question, concerning an inequality of Erhard Heinz, one of his students then. In his thesis, Heinz had closed a gap in one of Rellich's theorems, proving on his way a cluster of inequalities, all quite nontrivial. One of these inequalities contained a constant which Rellich conjectured to be 1.

Kato confirmed Rellich's conjecture - practically over night - and thus made his name public at Goettingen. [1]

For a while, in the 1940-s and early 1950-s, there must have been largely parallel developments. Rellich's theory had focused mainly on the perturbation of finite dimensional isolated eigenvalues of self-adjoint operators. We learned about resolvent techniques from Rellich and B.v.Sz.Nagy [Ng]. K.O.Friedrichs [Fr] already had results on perturbation of continuous spectra. General focuses of our attention were the spectral theory of regular

[1] The inequality (while proven for unbounded self-adjoint operators) is nontrivial and interesting already for $n \times n$ -matrices: Let $A, B$ be self-adjoint, $\geq 0$ , then $\| Qx \| \leq \| Bx \|$ , and $\| Q^*x \| \leq \| Ax \|$ , for a matrix $Q$ and all $x$ , implies $| (Qx, y) | \leq \| B^\nu x\| \|A^{1-\nu}y\|$ for $0 \leq \nu \leq 1$ and all $x, y$ (with $(.,.)$ denoting the inner product). Heinz had the factor $(1 + |2\nu - 1|)$ , at right, in this inequality.

This is closely related to the fact (first established by Loewner, essentially),that $0 \leq A \leq B$ (for self-adjoint $A, B$ ) implies $A^\nu \leq B^\nu$ , as $0 \leq \nu \leq 1$ (cf. also Kato [K56], Dixmier [Di] , HOC [Co] , ...) .

and singular eigenvalue problems of elliptic differential operators, for 2-nd order ODE after H.Weyl [We] (1910) , for 2-nd order PDE after T.Carleman [Ca] , for n-th order ODE after Kodaira [Ko], and Levinson[Le]. There was the abstract spectral theory of unbounded self-adjoint operators of J.v.Neumann [Ne] and F.Riesz [Ri] , and the complex analysis approach of Titchmarsh [Ti]. The uniqueness of selfadjoint extensions of minimal elliptic differential operators (i.e. essential self-adjointness of the operator with domain $C_0^\infty$ ) was a much discussed topic.

At Tokyo, on the other hand, Kato was working independently on very similar subjects. In 1948 there appeared his paper "Examples in which the perturbation method fails" [K4]. Then, in 1949-1950, a series of 3 papers "On the convergence of the perturbation method" [K7,9,10], where he develops his own version of rigorous perturbation theory, quite independent of efforts at Goettingen - and unknown to us. Actually, from remarks and conversations at the occasion of his Wiener prize it appears likely that his work of [K7,9,10] essentially was complete already by 1945, but its publication was delayed, perhaps due to events related to the second world war. This work on perturbation theory became Kato's doctoral thesis [K17].

Another paper of Kato [K15], parallel to our work, deals explicitly with essential self-adjointness of Schroedinger operators, under the potentials occuring in quantum mechanics of a general system with finitely many particles. This work appeared in 1951, but was completed already in 1944, according to Kato's remark on p.211. The same problem also had been in the focus of my own efforts, but in the narrower frame of separation of variables, in my own thesis. Also, while supervising the thesis work of Stummel [St] (replacing Rellich who already was ill), we dealt with these problems, but a couple of years later. We were aware of a much earlier article of Carleman [Ca], but not of Kato's work.

Again, unknown to all of us, there were strong efforts behind the "iron curtain", in Russia - this was at the time of Stalin, where scientific interchange between east and west was difficult, if not impossible (c.f. [Po], [Ur] , and many others.)

Rellich died in 1955, and his school, with nobody directly working on perturbation problems, disintegrated. The focus for perturbation theory shifted from Goettingen to Tokyo, where a new star had appeared, soon to take the lead, and soon assembled his own school.

The development from this point may be best described by pointing to Kato's book "Perturbation theory for linear operators" [K69], which appeared in 1966 with Springer Verlag. Also, perhaps, pointing at his lecture at the international congress 1970 at Nice

[K86]. During the years 1949 to 1966 Kato had published about 60 papers, most of them dealing with the problems described above. In his book he systematically discusses this theory, by then largely created by his own effort. Moreover, to quote from the preface: "...since the book is partly intended for physical scientists, who might lack training in functional analysis, not even the elements of that subject are presupposed. The reader is assumed to have only a basic knowledge of linear algebra and complex analysis."

Indeed, his book contains an introduction into theory of Hilbert and Banach spaces, theory of closed linear operators, self-adjoint operators, spectral theory, semi-groups, all written so well (and in the right context) that I have often used it as a source of information, even for these general subjects. Many people seem to have had the same experience. This might also be one reason that Springer Verlag decided to issue an inexpensive paperback edition, for their "best seller".

Under Kato's leadership, perturbation theory was carried into Banach spaces. We, at Goettingen, had exclusively dealt with Hilbert spaces. Of course, in Kato's own words, "there is no decent Banach space, except Hilbert space." Spectral theory, at least, looses its beauty and applicability (at least in quantum mechanics) when generalized beyond self-adjoint operators. But Banach space techniques have proven invaluable, especially for nonlinear problems, and have fascinated people by their trickiness.

There was the perturbation of continuous spectra, started by Friedrichs [Fr], but brought to a completion under Kato's leadership. In a somewhat special but very typical case, Friedrichs had studied unitary equivalence of perturbed and unperturbed operators. The unitary operators, in that respect, were identical with the *wave operators* of scattering theory. Thus appeared a close connection between perturbation theory and scattering theory. Within this setting it was found that "the continuous spectrum is rather unstable, but the absolutely continuous spectrum is rather stable - at least when perturbed by trace class operators" , to essentially quote Kato in [K86] . Other names to be mentionned, in this connection: Aronszajn [Ar], Birman [Bi], Kuroda [Ku] , Rosenblum [Ro] .....

The attention was turned to scattering theory, with *time dependent* and *stationary* approaches (cf. [K88], for a survey).

A very interesting result, in that respect is [K68] (1966), after his book had appeared: Perturbation by certain, not necessarily self-adjoint operators, bringing forth a similar operator, and a spectral operator, in the sense of Dunford.

Then there was Fredholm theory: It was overlooked for a long time that, in infinite dimensional spaces there exist Fredholm operators of index $\neq 0$ . Hilbert did not see this, he was corrected by F.Noether, in that respect. The Fredholm property, as well as the index, stay invariant under small continuous as well as arbitrary compact perturbations. This even works for unbounded closed operators in Banach space, and with perturbation small in the "gap norm". In his paper [K44] Kato even investigates perturbation properties of "nullity" and "deficiency" - that is, of the dimension of null space and codimension of range .

Moreover, while in Hilbert space the compact operators alone have the property of not disturbing the Fredholm properties, in some more general Banach spaces there exists other closed 2-sided ideals - such as the strictly singular operators, introduced by Kato - with the same property.

Finally, there is perturbation theory of semigroups. (Chapter 9 of Kato's book). Analytic perturbation of holomorphic semigroups goes rather well. More general cases are also discussed.

Kato's book really made perturbation theory accessible to everyone. He then was the accepted founder of this subject. General references almost exclusively go to his book, ever since.

Kato and I met personally in 1957, at Cal. Tech. In 1959 or early 1960 chairman Bernard Friedman told me that he was attempting to hire Kato to Berkeley's faculty (I had joined Berkeley in 1958). While this started in 1959, he is listed as a faculty member only in 1962. Personally, I believe to remember that he started work at Berkeley only in 1963.

At Berkeley, at that time, there were H. Lewy, C.B.Morrey, M.Protter, working in the field of partial differential equations. We had a joint "PDE-seminar" (originally organized by M.Protter) going continuously from the early 1960-s until 1991 - when all of us were retired or no longer living. Kato joined us, and became one of the leading contributors.

Thus begun an exciting time, for all of us. One will recall that a period of student upraisings started at Berkeley, in 1965, lasting well into the 1970-s. It spread through most of the world. In 1972 and 1976 I still experienced it in Germany, while we, at Berkeley, then had come to rest, more or less.

Parallel to this student movement - or starting earlier, perhaps with the begin of the space age, there also went a strong scientific evolution, with many young and ingenious minds coming to light. To us, working in "classical analysis" this could be felt as a turn of attention towards "Functional Analysis" and "Global Analysis". Regarding the first, we were driven from Hilbert spaces to Banach spaces, and further on to Frechet spaces and more general types of topological vector spaces. This trend was most fashionably expressed by the books of Bourbaki. At Berkeley, John Kelley (with his book on General

Topology) had captured the audiences. Regarding "Global Analysis" - our graduate course on ODE practically died (i.e., was replaced by a course of totally different structure, under the influence of differential topologists - mainly Steve Smale, with his structural stability). Likewise, topology entered the field of PDE, with the Atiyah-Singer index theorem - followed by theory of pseudodifferential operators, a part from distribution calculus conquering theory of linear equations, after the book of Hoermander [Ho] .

In these times, at Berkeley, there was comparatively small interest in our kind of work. I recall that, in 1972 we had about 8 students in the graduate classes of ODE and PDE, combined. Evident that this situation strongly affected all of us. Kato accepted (had grown up in) Banach spaces, and distribution calculus. But I saw him objecting strongly against more general analysis of topological vector spaces.

One particular development may have influenced him strongly: There was this trend of global analysts, using theorems of classical analysts as application examples for more general theories. In the 1960-s Kato had started to work on existence and uniqueness for solutions of the Euler and Navier-Stokes equations of ideal and viscous flow. In a paper [K70] of 1967 he had worked out this for the 2-dimensional Euler equation, in arbitrary bounded smooth domains, and he also had discussed higher dimensions for the domain $\mathbf{R}^n$ [K91] . In 1970, an article by Ebin and Marsden [EM] appeared, using a theory of infinite dimensional manifolds for an existence and uniqueness proof (of Euler and Navier-Stokes) for bounded smooth domains $\Omega$ of arbitrary dimension. They introduce a metric in the group of volume preserving diffeomorphisms $\Omega \to \Omega$ such that the Euler flow appears as the family of geodesics (the existence for small times then is proven). Of course, the diffeomorphism $\nu_t$ is obtained by letting the fluid particle at x flow along for the time t.

This paper raised much attention. Another proof, with similar geometrical aspects, was given in 1974 by Bourguignon and Brezis [BB]. Other, more conventional proofs, working with solutions in $L^2$-Sobolev spaces, were given by Temam [Te] (1975), Kato and Lai [K142] (1984), Kato and Ponce [K151] (1986) , and [K154](1987), and [K155] (1988) In Morrey spaces (for Navier-Stokes) we have Kato [K164] (1992). For Hoelder spaces we have Bardos and Frisch [BF1] and [BF2] (1975) , and finally, the proof recovered from Kato's computer, (cf. [K174])

So, this problem must have remained with Kato through the rest of his life. "His own proof" (which we found after his death) presents a remarkable contrast with Ebin and Marsden, just about typical for Kato's personality.

Coming back to a description of life at Berkeley - my personal relation to Kato developed quite slowly. Perhaps, it must be said, that I also was engaged seeking classical problems as applications for functional analysis. The general atmosphere demanded this: it just was fashionable, and students wanted to learn about the "new trends". For me it was application of $C^*$-algebras to singular integral operators. With Lewy and Morrey already on the verge of retirement, perhaps, at times, Kato's mathematical interchange with Chernoff and Marsden (at Berkeley) may have been stronger.

Perhaps even, we always kept at a distance mathematically. Indeed, I got tangled up in complicated theories, and often was not open to other problems. We both raised our students and competed for the very good ones. We had much common ground, mainly spectral theory of differential operators, but also linear hyperbolic problems (but not Euler or Navier Stokes, by the way). In spectral theory of self-adjoint operators I may have been one of his frequent discussion partners. In matters of hyperbolic semi-groups or evolution equations each of us went his own way. But our families seemed to fit together, with Japanese and German attitudes perhaps a bit similar, in some respects.

Remarkably, we never published a joint result. But at times I very strongly profited from Kato's presence. There was this time, in the early 1970-s, when I was deeply entangled in my $C^*$-algebras with scalar- or operator-valued symbols but could not force myself anymore to carefully write things down for publication. I resolved by feeding my problems and results to my students producing either joint papers or their thesis.

Kato rescued at least one paper for me (Essentially on the Calderon- Vaillancourt boundedness result for pseudodifferential operators). Indeed, just his kind interest in the result was enough for me, to overcome my inhibition. He even made some literature checks for me, for this paper. Again, I had neglected to focus on "symbols with positive $\delta$", in Hoermander's notation, while this seemed to be popular. So, he captured that result for himself and published a note on it [K106] .

Another little event, for the two of us: I had a result on essential self-adjointness of powers of second order elliptic operators $L$, such as the Laplace operator on a complete Riemannian manifold. While my proof was highly complicated, Chernoff [Ch] gave a short and elegant proof, under slightly different assumptions, using finite propagation speed of the hyperbolic equation $u_{tt} = Lu$. Kato then extended Chernoff's result. [Note, I use every occasion to point out both their results leave out some crucially important cases where only my (complicated) proof applies.]

At that time also, we had a common student(G. Childs [Chi]) . Kato suggested a Hoelder condition type extension of symbol conditions for $L^2$-boundedness as thesis topic, and asked me to supervise Child's thesis.

Among the numerous mathematical contacts we had, I especially recall the following

In 1972 we both thought about essential self-adjointness (in $C_0^\infty(\mathbf{R^n})$ ) of a second order elliptic operator of the form

$$L = \sum \partial_{x_j} a_{ij}(x) \partial_{x_k}$$

(with identically vanishing potential). Note, for $n = 1$ (i.e., $L = \partial_x a(x) \partial_x$ , $a(x) > 0$), the equation $Lu = 0$ is solved by the constant $u = c \notin L^2(\mathbf{R})$ . Hence there must be the limit point case of H.Weyl. There is a unique self-adjoint extension, i.e., $L_0$ is essentially self-adjoint, regardless of the choice of $a(x) > 0$. Could this also be true for higher dimensions?

Later on , in 1975, H. Kalf gave us a copy of his translation of a Russian paper by N.N. Uralceva [Ur] , holding a counterexample: $L$ needs not to have a unique self-adjoint realization, if $n > 1$.

In the early 1970-s Kato had tought about improvement of B.Simon's result on essential self-adjointness of Schroedinger operators with positive potential [Si]. The news about his new proof reached me: One day he asked me to proof read a short manuscript concerned with certain distribution inequalities:

(1) Let $v, \Delta v \in L^1_{loc}(\mathbf{R^n})$. Define $sign\ \bar{v} = \bar{v}/|v|$ as $v \neq 0$ , $= 0$ , as $v = 0$ . Then we have

$$\Delta |v| \geq Re[(sign\ \bar{v}) \Delta v], \quad \text{in the sense of distributions}$$

- i.e., $\Delta v$ and $\Delta |v|$ are taken as distribution derivatives, and the inequality means that the difference of the two sides assumes nonnegative values at every nonnegative testing function.

(2) For general $v \in L^2(\mathbf{R^n})$ , if $v \geq 0$ , and $\Delta v \geq 0$ (again in the sense of distributions), then $v$ vanishes identically.

One point of this was that (1) and (2) imply essential self-adjointness of $L = -\Delta + q$ in $C_0^\infty \subset L^2(\mathbf{R^n})$ whenever $q \geq 0$, under reasonable local assumptions on $q$ (a result just published by B.Simon [Si]). - Kato's proof was "2 lines" (modulo the proof of (1) and (2)).

Kato proves (1) and (2) first for smooth functions u, then uses Friedrichs mollifiers to obtain them in general [K94]. I found it interesting to play with (1) and (2), but must confess, I had nothing essential to add.

Without much explicit discussion among the two of us I developed a relation to Kato's paper [K44]. Fredholm operator investigations had drawn me to perturbation problems involving the ideal $\mathbf{K(H)}$ of compact operators in $\mathbf{L(H)}$ , for a Hilbert space $\mathbf{H}$ . For

Banach spaces there can exist other closed 2-sided ideals, the operators of which do not disturb the Fredholm property, nor the Fredholm index, such as the *strictly singular operators* Kato introduces in [K44] .

Clearly, the above things are relatively small events, out of Kato's life. But, perhaps, they reflect the spirit surrounding us.

Mathematically, Kato and I were independent. Perhaps none of us was willing to accept the others lead. We had much common ground, but we both did our own thing. Each of us had his own students and postdoctoral coworkers. For Kato there were Rafael Iorio, Arne Jensen, Gustavo Ponce, for example, who worked with him, during certain periods.

It was mentioned already: Kato's work on perturbation theory essentially was complete (a few years after) he came to Berkeley. He continued the general line by devising an extensive theory of linear (and later, nonlinear) evolution equations (he had defined hyperbolic, parabolic, analytic semi- groups). There is extensive work, also on specific such equations, linked to Euler, Navier-Stokes, Korteweg-de Vries, Schroedinger, Dirac, often regarded as ODE's in Banach spaces (of many kinds), on which we will not report, pointing to other such attempts (cf. the Kato-obituary by Jensen, Kuroda, Ponce, Simon, Taylor) in the AMS Notices [Ta]).

But it was obvious that Kato needed some friend, outside mathematical things. This may have been always so, but it became more pronounced during the second half of the 1990-s, when his wife became ill.

I recall a series of family gatherings: In the late 1960-s Kato visited us in Germany, after attending a conference there. At that time I was on leave, at Hamburg; we were living in a small town in the vicinity.

In 1972 we had rented a cottage near Echo Lake, in the Sierra Nevada. The Kato's visited us there, and we made some joint excursions.

During a certain period, in the 1970-s, we adopted the habit of driving out, on Sundays, having a picknik, somewhere, at interesting places, such as a winery, at Sonoma, or, Mount Diablo. Always we took along a single guest, often German or Japanese.

In the 1990-s our excursions became more conservative. Often we just met in one of our homes. Eventually, we gathered mainly at our place. The Kato's were nearly 10 years older than us. But then, they always "rewarded" us, by taking us to a concert or show. In one case even Tosio did the preparation and cooking for a joint meal at their home.

The early 1980-s also was the time of *personal computer invasion* into Mathematics. Our principal interest then was the acquisition of a math-text-editor. We both did not wait for the emergence of *TEX* - perhaps this was a disadvantage, later on. Tosio acquired an editor from a computer science collegue at Stanford. I spent considerable time, developing my own editor (I am still using). By the mid 1990-s it turned out that our papers had to be typed twice - the second time by a secretary, using TEX. Both of us had gotten used to typing our ideas directly into our computers, at home. In matters of PC-s I was able to advise him.

In 1996 the Kato's had moved into an appartment of St. Paul's Tower, a retirement community, at Oakland. There life was easier there, although not free from problems. Still they managed an independent living. Only on very few occasions they needed help.

On Saturday, October 2, 1999, near midnight, I received a phone call from the nursing supervisor of the tower, telling me that Kato had died.

I want to terminate this lecture with a report about Kato's Nachlass - the unfinished work, he left behind.

As mentioned, Kato and I were not close coworkers, but, perhaps, he was close enough for me to understand what he was doing, and to develop an opinion.

I knew that Kato had been active, during 1998/99, working on the existence and uniqueness problem for solutions of the Euler equations of ideal flow. In our conversations, enough shone through to make me see that one of his main concerns was a more careful analysis of a direct decomposition of the space of vector fields (and, more generally, 2-forms) discussed in chapter 7 of Morrey's book [Mo]. This is the well known Helmholtz decomposition (see also, below). It is orthogonal in $L^2$, but here the emphasis was on the space $C^{1+\alpha}$, of differentiable vectors with Hoelder continuous first derivatives, over some (nice) domain $\Omega$ of $\mathbf{R}^n$ . Kato was in correspondence with D.Gilbarg, regarding the question about the decomposition also being *direct* in the Banach space $C^{1+\alpha}$ . The question finally was answered positively. Yes, that decomposition induces bounded projection operators, also in $C^{1+\alpha}$.

On Kato's desk an (almost) complete manuscript was found; I suspected, it had already been submitted for publication. Indeed, this now is the paper [K172] .

There seemed nothing else. Yet, of course, I knew that he kept his recent work in his computer. With Mrs. Kato's kind permission I examined his 2 computers. A Pentium 75 (he had bought with my assistance, a few years ago) contained only general (recreational) things. But in an old 386-PC, I had built for him in 1987, I first found his math-editor, and then a collection of files he had created. I managed to print out some of them, then selected about 25, all concerned with the Euler equation, and sent them to S.T.Kuroda. With the cooperation also of H. Fujita and H. Okamoto, we started examining these files.

It became clear, after a while, that, among the 25 fragments, the last 5 , called eulern1a,...,eulern.4a, and euler67.b seemed to be significant. It probably may be said that these represent two papers, near completion. They were written in September 1999, the last one on October 1, the day before Kato died.

These files were translated into TEX by Kuroda. We are attaching the TEX printouts to this volume, organized as 2 continuous manuscripts (cf.[K174], [K175]).

I mentioned earlier Kato's strong interest in these general problems, manifested in a series of existence proofs (his own, and those of others) under various assumptions, in a variety of Banach spaces.

Actually, these efforts are quite old. There is work of Lichtenstein [Li] (1925) (assuming compactly supported vorticity), and of Wolibner [Wo] (1933) (2-dimensional, under the restrictive assumption, that the circulation of the flow should be zero on each inner component of the boundary of $\Omega$) , and of Judovic [Ju] (1963) (for weak solutions), to mention some very early efforts.

It seems that Kato's final efforts - in the two manuscripts of his Nachlass - are devoted to his ultimate and own proof, of existence of classical solutions, in Hoelder spaces. Moreover, it seems to us, that he just achieved this task, with a really simple proof (a 3-line argument, modulo some detailed discussions regarding the above points and some standard calculations or estimates). At least he seems to have regarded his n-dimensional achievement so significant, that he spent the last day of his life, to also translate his 1967 paper [K70] on the 2-dimensional case into this new form (This is the manuscript [K175]).

Here are some comments on closer details: First, we believe that the introduction of [K174] really was written only for Kato's own orientation. It should *not* be regarded as *complete*, in the sense that all necessary connections are quoted. We closely followed the generation of the paper [K172], from a set of files in his computer to the final version, and noticed how carefully he worked over his final introduction.

Similarly, in his proof of Theorem I in [K174], he focuses only on existence while he also states uniqueness. However, please note that the steps of his existence proof, we describe below, are all reversible, and the fixed point implied by the contraction mapping theorem is unique. In that respect we also note his result in [K172], where he shows a smoothness for the solution, even under very general (distribution- like) assumptions on the initial values.

Furthermore, the manuscript [K175] is quite sketchy, leaves out some detail.

Also, there are no "References", but "[K]" (or "[KK]") might mean [K172] , or [K70], preferably, or another paper of himself. "[M]" , Morrey's book [Mo] , "[T]" should mean

To comment on [K174]: A *flow* is (defined as) an n-vector field over $\Omega$, tangential at $\partial\Omega$, and with vanishing divergence.

An arbitrary (smooth) n-vector field $u$ on $\Omega$ allows a unique decomposition $u = v + w$ into a *flow* $v$ and a *gradient* $w$, with $v, w$ orthogonal in$L^2(\Omega)$:

Given $u(x)$, we want to find a scalar $\phi(x)$ such that $u - grad\ \phi = v$ satisfies $div\ v = 0$ and $\nu.v = 0$ at $\partial\Omega$. Clearly this amounts to solving the *Neumann problem* $\Delta\phi = div\ u$, in $\Omega$ , $\partial_\nu \phi = \nu.u$ on $\partial\Omega$. That solution $\phi$ exists and is unique (up to an additive constant, not influencing the gradient). If $u \in C^{1+\alpha}$ then $div\ u \in C^\alpha$ , and $\nu.u \in C^{1+\alpha}$. This implies $\phi \in C^{2+\alpha}$ -i.e., $w = grad\ \phi \in C^{1+\alpha}$ - using *general elliptic theory* . [That is, we use a (well known) Schauder-type estimate, for the solution of the Neumann problem.] So, indeed, there is a pair of projections $1 = P + Q$ , projecting onto the flow and the gradients, respectively, and $P, Q$ are bounded in $C^{1+\alpha}$.

Instead of using that *Schauder-type estimate* one may link this result to a variational type argument, using theorem 7.7.4 of [Mo]. For both discussions we refer to the fragment *euler2.1*, proposition 1, recovered from Kato's computer. [Actually, the $C^{1+\lambda}$ boundedness cannot be derived under Morrey's assumptions, but follows from an argument of [K172] (lemma 1.2 there). [2] (See [K173], where similar considerations are made for Sobolev spaces.)]

Starting from the Euler equations

$$(E) \qquad \partial_t u + (u.\partial)u = grad\ p\ ,\ div\ u = 0\ ,\ \nu.u = 0\ ,\ as\ t \geq 0\ ,$$

with initial conditions

$$(E_0) \qquad u = u^0\ ,\ as\ t = 0,$$

the projection $P$ is applied - note, we have $P(grad\ p) = 0$ , $Pu = u$ :

$$(E') \qquad \partial_t u + P(u.\partial)u = 0\ ,\ t \geq 0\ ,\ u = u^0\ ,\ as\ t = 0.$$

Now $div\ u = 0$, and $\nu.u = 0$ are built into $(E')$, if satisfied initially. For a solution $u$ of $(E')$ we may define a pressure $p$ by solving $grad\ p = Q(u.\partial)u$ - this is solvable, by definition of $Q$. Since $P = 1-Q$, $(E')$ takes the form $\partial_t u+(u.\partial)u = (1-P)(u.\partial)u = Q(u.\partial)u = grad\ p$, showing that $(E), (E_0)$ and $(E')$ are equivalent.

[2] Looking at various fragments from Kato's computer, it seems that he worked hard to get his own boundedness proof, for $P, Q$, avoiding elliptic theory.

Next, $(E')$ is *linearized* - i.e., replaced by

$$(E'') \qquad \partial_t u + P(v.\partial)u = 0 \ , \ t \geq 0 \ , \ u = u^0 \ , \ t = 0 \ ,$$

where $v$ is a given fixed flow, depending on $t$, and with values in $\mathbf{Y}^{\mathbf{n}} = C^{1+\lambda}(\Omega, \mathbf{R}^{\mathbf{n}})$.

The aim, of course, is to prove existence of a unique solution $u = u_v$ of $(E'')$, under suitable restrictions on the flow $v$, and then show that the map $v \to u_v$ has a fixed point. Such fixed point will solve $(E')$, hence give a solution of $(E), (E_0)$.

To anticipate the point, Kato uses the contraction mapping theorem, in a certain Banach space, to get a *unique* such fixed point.

First $(E'')$ is converted into the form

$$(E^3) \qquad \partial_t u + (v.\partial)u = Q(v.\partial)Pu \ , \ t \geq 0 \ , \ u = u^0 \ , \ t = 0 \ ,$$

This is equivalent to $(E'')$, by a trick of C.Y.Lai.

Now, $(E^3)$ is regarded as a first order linear PDE $\partial_t u + (v.\partial)u = f$ , with given $f = Q(v.\partial)Pu$ . Of course, the differential operator $\partial_t + (v, \partial)$ is a formal scalar. Therefore solving the initial value problem just amounts to solving a first order ODE along the characteristic curves. The solution contains $f$ only as an integrand of an integral from 0 to $t$, along the characteristics. An estimate on the map $(u, v) \to f = Q(v.\partial)Pu$ , essentially due to R.Temam, showing, this is a bounded operator $\mathbf{Z}^{\mathbf{n}} \times \mathbf{Z}^{\mathbf{n}} \to \mathbf{Z}^{\mathbf{n}}$ , suitable $\mathbf{Z}^{\mathbf{n}}$, the influence of that term may be made small by choosing a small $t$-interval $[0, t_0] \subset [0, T]$. Thus we get existence of $v$ in such small interval $I_{t0}$. Moreover, the difference of two such $u_v$, for two different $v$ involves only the difference of these integrals on $f$, no longer the initial value $u^0$. That, indeed, is responsible for the fact that we have a contraction map.

The space $(\mathbf{Z}^{\mathbf{n}})$, by the way, is a ball of radius $R$, in the space of all flows within $C(I_{t0}, C^1) \cap B(I_{t0}, C^{1+\lambda})$ , with $C^{1+\lambda} = C^{1+\lambda}(\Omega, \mathbf{R}^{\mathbf{n}})$ , including $\lambda = 0$ , where "C" and "B" indicate the continuous and the bounded functions on $I_{t0}$, respectively.

*Remark:* The use of intersections $C(.,.) \cap B(.,.)$ seems to be a device to avoid having to "shrink" the Hoelder exponent $\lambda$.

This seems not the place of discussing a relation to other or earlier proofs . However, we hope to have made the point that the above is a first rate "Kato-proof", comparable with some of his earlier ingenious inventions.

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