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SOME INFLUENCES

or

"Everything I needed to know about real analysis I learned from Andy!"

§1. As my title is meant to indicate I wanted to convey in this essay some of the many influences that Andy Bruckner has had on the real analysts of our generation, but the subtitle should acknowledge that I have settled for telling you the more personal story about some of the influences that Andy has had on me.

It was a particular pleasure to present a talk at this meeting that honors Andy's 65th birthday and to write an essay on this subject for the many admirers of Andy who were unable to attend. The occasion was indeed very special for some us. Fourteen years ago a real analysis meeting was held at Santa Barbara. There had been a special year in real analysis at UCSB hosted by Andy and Jack Ceder and Tom Boehme. That year there was in residence a remarkable collection of real analysts including Casper Goffman, Paul Humke, Miklos Laczkovich, Gyuri Petruska, and David Preiss. As well as these there were frequent distinguished visitors arriving throughout the year. For me especially it was a spectacular year and a great experience. I'm sure that at the time I remember Paul saying that this was a once in a lifetime opportunity. Well it really was! I don't expect ever to be able to repeat such a year.

Andy was then a young man of merely 50, although at the time we all thought of him as one of the old guys—a very senior mathematician. It was

*This essay is a loose approximation of an address given at the 1998 Real Analysis Symposium held at UCSB in honor of Andy Bruckner's 65th birthday.

only Casper Goffman who knew then just how young Andy really was in 1984. Now on the occasion of Andy's 65th birthday nearly all of us have returned. This is all very nostalgic for us. We are now older than Andy was during the special year, so we finally understand just how young 50 is. It's not 50 that's old: it's 65 thats old!

I can't say that I have any really clear idea of how to give a talk on such an occasion. Oddly enough, I met Andy at exactly such a talk. It was in back in 1979 that Casper Goffman celebrated his own 65th birthday and a conference was held in his honor at Purdue University that summer. Many of his former students, colleagues, collaborators and friends went and Andy was there presenting a talk on derivatives. It was at that meeting that I first met Andy and also at that meeting that I learned how a talk like this *should* be given. One of the speakers gave a most remarkable talk that was a masterful blend of mathematics and appreciation for the friendship and encouragement that Cas had offered him and others over the years. That talk was given by Daniel Waterman and I'm sure anybody who was there must remember it as well as I do.

My title is "some influences" and I would like to be able to tell you of the many influences of Andy on real analysis. Certainly there is no doubt that he has influenced us to a great degree. To take just one topic—derivatives—that is so closely tied to Andy's image: everybody's research program on derivatives has certainly been touched by Andy. The problems, the focus and the energy of the subject have been driven by the many expository articles and talks that Andy has given over the years.

When you think about it, it is really quite amazing that so much research *has* been done on derivatives just in the last 20 years. This should be placed in some context. Back at the turn of this century the Youngs, William Henry Young and Grace Chisolm Young, were also working on derivatives. At the time several famous mathematicians complained to them that they could see no possible interest in derivatives. For a while Young was simply dismissed as the guy who was beat out on the integral by Lebesgue, in spite of a continuing and important research program on real functions.

By the 1960s and 1970s it was again true that some of the then fashionable mathematicians were saying the same things about derivatives. One reviewer referred to this area as a "cul de sac"; in north america we would prefer to say "dead end street" instead of "cul de sac"—an even more unpleasant and uncharitable term. If anything kept this research topic so alive it was Andy pointing to all the interesting features and problems of the subject.

There is an amusing illustration of how pervasive Andy's influence really is. In Andy's 1978 monograph [3] one finds the following result

Suppose $f : \mathbb{R} \to \mathbb{R}$ is Baire 1. Then a necessary and sufficient *condition for f to be also Darboux is that at each point x there are sequences $x_n \nearrow x$ and $y_n \searrow x$ so that $f(x_n) \to f(x)$ and $f(x_n) \to f(x)$.*

The reference supplied (it appears as [214] in the list of references) is to J. Young, *A theorem in the theory of functions of a real variable,* (1907). Well there is no J. Young. There is a W. H. Young who is in fact the author of the paper. There is a G. C. Young, his wife, who edited all of his papers and co-wrote many (although her name did not necessarily then appear on the paper). There were four little Youngs, two of whom went on to establish mathematical careers of their own, L. C. Young and R. C. Young. But there is no J. Young. It was just a typo. But in spite of that you will find that in the last 20 years this guy J. Young has a better citation record than most of the rest of us do. Clearly everyone who writes in this area has a copy of Andy's book open at all times and they just lift the references from there. Even Andy's typos have had a pervading influence on our subject. Unfortunately the new edition of Andy's book that came out in 1994 not only adds new material but corrects this wonderful typo. It's a shame. I have always enjoyed seeing J. Young getting this credit.

§2. How should I track down these many influences of Andy? I started to think about this problem in the modern way: I went to the World Wide Web. A search for "Andrew Bruckner" will give you lots of hits. First of all there happens to be a minor american composer named Andrew Bruckner. And you might find an undergraduate student whose activities on the web are reported. But there is only one famous Andrew Bruckner apparently and the bulk of what you will find shows Andy's various activities—talks, seminars, publications, etc.. One site that is worth visiting clearly has the genuine article:

http://www.stolaf.edu/people/analysis/UCSB98/all-andy.gif

I also went to the AMS Web Site and searched there for Andy. There you will find 121 refereed publications in their database and twice that many Math Reviews that Andy has himself written. I was overwhelmed with material and no closer to my task of conveying, in my limited fashion, a sense of Andy's influences. As you look through these you find, of course, articles on derivatives and dynamical systems that many of us are following. But there are also papers on topology, differential equations, statistics and many other areas that have interested Andy over the years.

So how can I convey a sense of Andy's influence in the restrictions of a 50 minute talk and a short essay such as this? Fortunately for me there was one item from among all this material that did catch my eye as I was searching around and which brought back many memories. I have read many of Andy's papers over the years; some I have studied very, very intensely. I confess I haven't read them all—or even read all of those that I have copies of. But this one paper had a huge impact on the direction that my research would take.

While it was the 1978 monograph [3] that had perhaps the greatest influence on me and everybody else I'm sure, and that still continues to have an influence—there was a much earlier publication that impacted on my own research directions. This is Andy's 1971 monograph [2] on the differentiation of integrals, a 50 page report on an area I had only been dimly aware of. In 1971 I was very much in need of some fresh ideas and searching about for an interesting research direction. So this article came just at the right time.

I want to trace the effect that this article had on me and tell you just some of the ideas in that paper. So this will necessarily tell you a very narrow story of Andy's influence. Many of us could tell a similar story—some paper, some talk, some problem, some discussion with Andy, some collaboration with Andy, some chapter in one of his books, all of these things that might have had a profound influence on the course of your thinking. Indeed, during the course of the meeting, I did hear the same story repeated in various versions. It was seldom the same paper or the same event, but my story is quite typical of a common tale told by many.

§3 I'd like to give you a brief summary of this remarkable paper. The date 1971 may seem old but in fact I think there is only one problem that is posed in the paper that was later solved. Everything else in the paper is up to date and interesting.

Andy starts with the Lebesgue differentiation theorem that we all learned as students.

$$\frac{d}{dx}\int_a^x f(t)\,dt = f(x) \quad a.e. \tag{1}$$

Perhaps we should pause here and pay some respect to Lebesgue. We take this theorem so much for granted that I'm not sure even when we present it to our students we show the proper degree of reverence. Remember that before this the differentiation of the integral would have been established at points of continuity of the integrand. You need only the simplest properties of the integral to prove that! For the first time, Lebesgue was integrating functions that might be everywhere discontinuous and so it would have been quite exciting for him to discover that the true version of the Fundamental Theorem of the Calculus was this, with almost everywhere derivatives and requiring a proof that is significantly deeper than the old calculus treatment.

Andy asks us to rewrite (1) according to the definition

$$\lim_{h\to 0} \frac{1}{h} \int_{[x,x+h]} f(t)\,dt = f(x) \quad a.e. \tag{2}$$

and then use a different suggestive notation.

$$\lim_{I\Rightarrow x} \frac{1}{|I|} \int_I f(t)\,dt = f(x) \quad a.e. \tag{3}$$

where we think of I as an interval containing x and $I \Rightarrow x$ means that the interval is shrinking to x.

This expresses this as an averaging process, which is of course what differentiation is, but the first expression (1) masks this behind the familiar notation of derivatives. Expressed in the form (3) we can ask for what the Lebesgue differentiation theorem must look like in higher dimensions or in an abstract space. In a general setting this looks like:

$$\lim_{I\Rightarrow x} \frac{1}{\mu(I)} \int_I f(t)\,d\mu(t) = f(x) \quad \mu - a.e. \tag{4}$$

Under various interpretations of $I \Rightarrow x$ is this valid? For example just in $\mathbb{R}^2$ with μ as two dimensional Lebesgue measure, what happens?
(a) If by $I \Rightarrow x$ we mean shrinking disks or squares then (4) is valid for all Lebesgue integrable functions [ordinary differentiation].
(b) If by $I \Rightarrow x$ we mean shrinking intervals then (4) is valid for all *bounded* Lebesgue integrable functions but not true in general [strong differentiation].
(c) If by $I \Rightarrow x$ we mean shrinking rectangles then (4) is not valid even if f is merely the characteristic function of some open set.

How do we make sense of this? Perhaps the right answer is "Study the proofs and the counterexamples". To be sure. But there is a *method* that can be used to clarify proofs and counterexamples. That method involves generalizing the situation. Find some more or less abstract way of restating and re-studying the problem. And it is this that I re-learned from Andy. All young mathematicians are told such things but it sometimes takes the right setting for the lesson to have an effect.

There is a structure called an abstract differentiation basis which can be used to clarify what is going on here. Here are the ingredients of such a theory.

- a measure space X.
- a pair of measures ν and μ.
- a family of sets that play the roles of the intervals I.

- a meaning attached to the expression

$$\lim_{I \Rightarrow x} \frac{\nu(I)}{\mu(I)}$$

Mainly in a general measure space you have to assign some kind of abstract meaning to the notion of these generalized intervals shrinking to a point. Some authors use nets and some use filterbases. Generally you don't have to be too fussy here to develop an adequate theory.

I quickly went out and got copies of all the papers I could find on the subject. Unfortunately there were many papers of considerable obscurity and as I looked back at Andy's paper I could see that there were some gentle hints that this was so in this area. One reference, commended by Andy for its clarity, is De Possell [7]. I don't know much about de Possell, but he was evidently a wonderfully clear thinker and writer. It's much like Lebesgue's very lucid style. In fact it is a delightful paper and one I never would have discovered without this pointer from Andy. Some of the other papers (e.g., [5] for a notorious example) are rather more murky. If it hadn't been for Andy's paper I expect I might then have abandoned the whole area. When you are young and see such impenetrable stuff in an area you assume everyone else finds it all trivial and its only you that think these guys are obscure.

How does the general theory develop? The beginning point is the Vitali theorem. This is usually a good way of starting to develop an abstract theory. Study the techniques that you have already used and find an abstract way of expressing those techniques. You have to define what you would mean by a Vitali cover in this setting—much in the way you'd expect on the real line—but now taken relative to the convergence in the differentiation basis. The same tool—the Vitali theorem—that you would use on the real line has an abstract expression, but now as a property, the *strong Vitali property*, rather than as a theorem.

But you don't always have a Vitali property that is this strong and so weaker versions emerge, for example the *weak Vitali property*.

These Vitali conditions don't tell the whole story. If you study Banach's proof of the Vitali theorem you can extract some interesting geometric ideas that explain what is going on. This appears in the abstract theory as a *halo condition*, that just pulls out the main geometric idea of Banach's proof. Again that's a pretty standard trick when you are developing abstract theories: study the most famous proofs and look for general principles. Thus we have abstract versions of the Vitali theorem (strong Vitali properties, weak Vitali properties) and then geometric versions of Banach's proof (Halo properties, weak Halo property, Halo evanescence property, etc.). While this sounds technical and certainly has its technical aspects the development does a lot to clear up what is happening in the situations of different differentiation bases.

Thus, for example, in $\mathbb{R}^2$

- ordinary derivatives [i.e., disks or squares] have the strong Vitali property, and that in general allows for the Lebesgue differentiation theorem for all integrable functions.
- strong derivatives [i.e., intervals] have the weak Vitali property and that in general allows for the Lebesgue differentiation theorem for bounded functions.
- rectangular derivatives [i.e., rectangles] has neither property, and the Lebesgue differentiation theorem fails badly.

This then can be considered an answer to our question. Why do these different derivatives behave so differently. The answer (*an* answer really) is expressed in terms of these Vitali properties. This is the *geometry* (if you like) behind these different types of derivatives.

§4. Are there any benefits to this abstract approach? Well, of course, there is greater generality, but I am a bit reluctant to mention that. I don't see that the greater generality is really the goal. It's more a mathematical *method* than a goal. You don't generalize to get stronger results, you generalize because it's in your nature as a mathematician to generalize. You generalize to gain insight. You generalize to find a new perspective, a new way of looking at old problems and old solutions. By generalizing you organize your methods, separate hypotheses. Often you find that old methods solve new problems this way. You can find new methods that wouldn't have occurred to you until you re-expressed the problem.

This process can be unpleasant and unrewarding at times. L. C. Young (he was a small child when he last appeared in this essay) wrote in his autobiography of this process as "lemon squeezing". As I first read his comments I assumed he was speaking of the fact that so many mathematical publications seem to take an earlier result and squeeze just a slightly stronger or more general statement from it, often in an uninspiring way. But Young goes on to point out how this very necessary process, every now and then, does indeed elevate mathematical ideas to a new level. My favorite is the Weierstrass approximation theorem. There were countless generalizations and extensions over the years, now forgotten. But at some point in this process appeared Stone's generalized version that carried the theorem to a new elegant and powerful setting. Without the years of generalizations and the many other efforts such new ideas might never appear.

Now if in 1971 I had merely started off reading the literature of abstract differentiation theory without Andy as my guide I think it all really would have seemed some strange, empty generalization and I might have dropped the subject pretty fast. Certainly there are plenty of squeezed lemons littered about in the subject. But Andy helps explain not just the mathematics, but why the mathematics is interesting. Its odd that so few mathematicians can do that!

For example let's follow one theme in Andy's paper: the Radon-Nikodym theorem. Loosely stated this asserts that, under appropriate hypotheses on two measures ν and μ,

$$\nu(B) = \int_B f(x)\, d\mu(x)$$

for some function f called the Radon-Nikodym derivative,

$$f = \frac{d\nu}{d\mu}.$$

But as Andy points out this is a fraud. It is an *existence* theorem but it is not a differentiation theorem. There is no *process* of differentiation, just the announcement that such a function exists but no method of finding it. I remember as an undergraduate being quite entertained by the so-called Radon-Nikodym derivative. It has all the properties of a derivative—sum and product rules. There is even a chain rule for this derivative. But I think students can easily lose perspective. Its not a derivative. Some students might think—"hey, why study derivatives, we've got Radon-Nikodym. It always does the job doesn't it?". In fact in some graduate courses so little attention is paid to the real line and to derivatives that students somehow get the impression that modern measure and integration theories don't need that stuff. Absolute continuity survives as a measure-theoretic idea and derivatives just disappear. In [4] we do motivate the proof of the Radon-Nikodym theorem by thinking of it as a real derivative theorem and using that to guide the construction, but only as a heuristic device.

But can Radon-Nikodym be salvaged? Is there a way of finding a genuine differentiation process that yields the Radon-Nikodym derivative? As Andy puts it

> *"In what sense and under what circumstances is it a derivative in the customary pointwise sense?"*

There is an answer! There is a pretty elementary and natural structure that is usually available. The actual requirement here is that the measure space be *separable.* For measure spaces this means roughly that the space can be carved up into a finite or infinite sequence of small pieces; it is also the same as saying that $L_2(X)$, the square integrable functions on the space, is a separable Hilbert space.

Under this hypothesis the space allows the construction of an appropriate net structure:

- For each k there is a disjoint family (finite or countable) $\mathcal{N}_k$ of sets of finite μ-measure covering the space.
- Each set in $\mathcal{N}_{k+1}$ is a subset of a set from $\mathcal{N}_k$.
- $I \Rightarrow x$ means that I contains x, $I \in \mathcal{N}_k$ and $k \to \infty$.

Any "separable" σ-finite measure space allows a net structure for which the strong Vitali property holds for μ.

It follows, then that the Radon-Nikodym theorem has this version:

Under these hypotheses there is a net structure so that if ν is absolutely continuous with respect to μ then

$$\nu(B) = \int_B f(x)\, d\mu(x)$$

where

$$f(x) = \lim_{I \Rightarrow x} \frac{\nu(I)}{\mu(I)}$$

for μ–a.e. point x (i.e., the Radon-Nikodym derivative is a derivative.)

These net structures I likely would never have learned without Andy. They are in Saks' book [8] but I have to confess that in 1971 I had skipped over them on my many readings of Saks. Later when I saw how Besicovitch had used them in studying Hausdorff measures I was all prepared; I'd learned from Andy. I knew they were important in differentiation theory and so wasn't at all surprised to see that they could also be used in the study of Hausdorff measures. Since reading Andy I had a better understanding of how many different ideas were really interrelated.

Let us continue with the Radon-Nikodym story in a measure space, but ask what happens if the space is not separable so that a net structure would not be available.

In this case the lifting theorem can be used. A lifting is just an operation on measurable sets that picks out one for every family of sets with the same μ–measure and does it in a special algebraic way, preserving the features you care about. If L is a lifting on the measurable sets (roughly $L(M)$ picks out a measurable set with the same measure as M in a way that preserves intersections and unions. Then interpret $I \Rightarrow x$ to mean that $x \in I$ and I is lifted (i.e. $L(I) = I$) and "shrinking" is just meant as set inclusion shrinking. If there is a lifting this process will have the strong Vitali property for μ.

Kölzow [6] in 1968 showed that there is a lifting exactly when the Radon-Nikodym theorem is available. Then, because of this fact, if the Radon-Nikodym theorem holds for the measure space there must be a differentiation basis for the space that has the strong Vitali property and so that the Radon-Nikodym derivative is a genuine a.e.-derivative for that structure.

I doubt I would ever have had any genuine appreciation of the lifting theorem if Andy had not taught me this wonderful connection. I remember that I had seen texts on the subject and heard talks. But I hadn't felt any compelling reason to learn more about liftings since I thought it was mainly functional analysts and measure theorists who would care.

It also opened up a realization that the process of differentiation has many, many interconnections throughout mathematics. As an object of study it is still of great interest.

§5. There are many other interconnections that can be found in Andy's paper. I'll just list a few: multiple Fourier series and differentiation of integrals, functional differentiation systems and summability of Fourier series, complex analysis, boundary behavior of harmonic functions, surface area, and potential theory.

Note what we have learned by abstracting some of the ideas of derivatives. This same underlying process can be viewed in many different ways and applied to many different problems.

As I said, this paper had a considerable influence on the course of mathematics that I was to pursue. I gained many insights and more than that I developed much of the same passion that Andy had for this wonderfully rich process called differentiation. Especially this process as viewed from so many different perspectives.

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