ALM 39
Advanced Lectures in Mathematics

Hodge Theory and L^2-analysis

Hodge 理论和 L^2 分析

Editor: Lizhen Ji

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ADVANCED LECTURES IN MATHEMATICS

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Dedicated to Steven Zucker for his 65th Birthday

Conference on Hodge Theory and L^2-cohomology, Johns Hopkins University, November 21—23, 2014

Preface

Steven (Steve) Zucker has made major contributions to the following three areas of mathematics and the interactions among them:

1. Hodge theory in algebraic geometry (e.g., normal functions, variation of mixed Hodge structure, degeneration of Hodge structure);
2. L^2-cohomology, and also L^p-cohomology for $p \neq 2$;
3. Compactification of locally symmetric spaces (Satake, reductive Borel-Serre, toroidal, etc.).

The famous Zucker conjecture from 1980 relates items (2) and (3) above. It was resolved independently in 1987 by Saper-Stern and by Looijenga, using very different methods. For a broader, detailed description of Steve's work, see his own narrative which follows.

In November 2014, a conference titled "Hodge Theory and L^2-cohomology" was held in Steve's honor at Johns Hopkins University. This book is based on the conference. Besides contributions from most of the speakers, several other people were also invited to contribute. Since Steve's work and some of the contributed papers involve L^2-analysis, which includes L^2-cohomology, the title of the book was changed to "Hodge Theory and L^2-analysis". By consensus, the conference was of high quality. We believe that this book reflects the conference well and is also of high quality.

It perhaps helpful to emphasize the intimate connection between Hodge theory and L^2-analysis. In his thesis in 1851, Riemann tried to use the Dirichlet form and the Dirichlet principle to prove the Riemann mapping theorem. In his very influential paper on abelian functions in 1857, he also used harmonic functions and the Dirichlet principle to prove the Riemann inequality in the Riemann-Roch theorem. These results were made rigorous and systematically presented in Weyl's classical book *The Concept of a Riemann Surface* which was published in 1913. Some of the results for Riemann surfaces were later generalized to higher dimension Riemannian and Kähler manifolds by Hodge, Weyl and Kodaira, whose works gave birth to the Hodge theory.

The contributors to this book had been asked to make their papers expository to the extent possible. We hope that this book will serve as a valuable introduction to Hodge theory, L^2-analysis and the interaction between the two. All papers have been carefully refereed, and we would like to thank the referees for their help.

The conference was held at approximately the 65th birthday of Steve. We wish him many happy and productive years to come.

Lizhen Ji

September, 2016

The Research Career of Steven Zucker: An Autobiographical Account

Acknowledgments. I thank Rafe Mazzeo, Arvind Nair, and Morihiko Saito for their constructive comments on earlier versions of this narrative. I must thank in particular Benjamin Diamond, who served as the copy editor of its penultimate version.

For the reader's convenience, I divide the references to my work to date among the three areas mentioned in the Preface.

(1) Hodge theory in algebraic geometry (normal functions, variation of mixed Hodge structure, degeneration of Hodge structure, etc.): [Z01]–[Z06], [Z09], [Z12]–[Z23];

(2) L^2-cohomology, and also L^p-cohomology for $p \neq 2$: [Z03], [Z07], [Z10], [Z24], [Z28];

(3) Compactification of locally symmetric spaces (Satake, reductive Borel-Serre, toroidal, etc.): [Z08], [Z20]–[Z22], [Z24]–[Z28].

In what follows, there will be no false modesty, and I will avoid caustic remarks. In addition to a description of my work, I will acknowledge the major influences of other mathematicians, as well as including the impact of my own work on that of others. I wish to put in the records that I gave general (moral) support to two first-rate mathematicians who were getting a bit discouraged while in graduate school: Kari Vilonen and Teruyoshi Yoshida. I know now that Yoshida remembers this well and with appreciation; Vilonen is less clear, but he believes it is accurate. I mention this to remind the reader that there are ways of helping a novice beyond the imparting of mathematical ideas.

I was born in New York City on September 12, 1949. A year later, my parents (and I) moved to Queens Village, located in the Borough of Queens, NYC, where my father had set up his optometric practice. I went to public elementary, junior high school (a.k.a. middle school) and high school. All were in walking distance from our home, but we lived even closer to the city line. That precluded the idea of going to a selective high school, for the commute would have been way too long; besides, the local high school was rather good. I have one sibling, a brother, born in 1952.

In 1966, I headed off for Brown University (that was *before* the "new curriculum" was enacted) and I graduated in 1970 with a degree in Mathematics. From there, I went to Princeton University, receiving a Ph.D. in Mathematics in 1974.

Working towards my dissertation was over a rocky road. Phillip Griffiths had given me the problem of fixing [BlGr], which was unfortunately dependent on strong ampleness conditions, so that it would apply to the example they really had in mind. Towards the end of my second year, when I heard that Griffiths was moving to Harvard, he recommended that I continue to work under Spencer Bloch, who was junior faculty and also of an algebraic bent (I was more analytic in nature). The problem was, given the mapping $f : Y \to P^1$ of a smooth variety Y of dimension $2n$ onto P^1 associated to a Lefschetz pencil of hyperplane sections, to show that every primitive integral Hodge class in $H^{2n}(Y)$ comes from a normal function. I became aware that the obstruction to a complete solution to the assigned problem was the inability to see the Hodge structure on $H^1(P^1, R^{2n-1}f_*\mathbb{C}) \simeq H^1(P^1, j_*\mathbb{V})$, where $j : S \hookrightarrow P^1$ is the inclusion of the Zariski-open subset over which f is smooth, and $\mathbb{V}$ is the restriction of to S of $R^{2n-1}f_*\mathbb{C}$ (underlying its variation of Hodge structure) from a construction $\underline{\text{on } P^1}$. Not only was there an essential difficulty, but it was only resolved much later by [Z03]. Fortunately, I could produce a theorem that was almost as good by technical "magic". For reason of that, I was hired as assistant professor at Rutgers University; my thesis, trimmed down a little, was published as [Z01].

I am grateful that my next publication [Z02] was remembered by Pierre Deligne when he published [D2]. One of the appendices in that article got nearly buried in the literature. That appendix came about because Fred Almgren (of geometric measure theory) had heard about a possible counterexample to the Hodge conjecture, and he wanted to hear more about it, for the use of integral currents might have been relevant. It concerned a special class of abelian varieties with a three-dimensional space of Hodge classes. I recognized that if one had a compact complex torus (not an abelian variety) with only the corresponding two-dimensional subspace of Hodge classes, it was impossible that these classes were representable by analytic subvarieties, for there were no *positive* classes. Thus, there is no formulation of the Hodge conjecture for general Kähler manifolds.

I have already presented [Z03] with due fanfare. The article was strongly influenced by my interaction with Deligne. I had gone to IHES during the summer of 1975, where both Deligne and Joseph Steenbrink were in residence. I knew of Steenbrink because we had finished our dissertations at about the same time, and we exchanged preprints (his was published as [St]). It was determined that Deligne had an unpublished manuscript that provided a proof of the theorem on normal functions when f is smooth. After I finished looking at it—it seemed so complicated to me at the time—Deligne sketched an approach to proving it when f had singular fibers. I was awestruck! He suggested using some sort of L^2-cohomology; the features of the problem led to the Poincaré metric. Various technical issues had to get resolved, and I finished work on it after about two years. The main result is roughly:

Let $\overline{f} : \overline{X} \to \overline{S}$ be a proper morphism of varieties, where $\overline{X}$ is smooth, $\overline{S}$ is a smooth complete curve, f is smooth over $S \subset \overline{S}$ and $j : S \hookrightarrow \overline{S}$ is the inclusion.

Then for any variation of Hodge structure $\mathbb{V}$ *over* S — *in particular* $\mathbb{V} = R^q f_* \mathbb{C}$ — $H^1(\overline{S}, j_*\mathbb{V})$ *has a natural Hodge structure that is compatible with the Leray spectral sequence for* $\overline{f}$.

As pointed out by Donu Arapura in [Ar], his work supersedes in a big way the argument in [Z03] for the compatibility with the Leray spectral sequence; keep in mind, though, that as much as I like [Ar], it was published more than 35 years later.

As asserted in [CGM], Deligne was wondering about the generalization of this main theorem to higher dimensional $\overline{S}$. This led to the famous iterated truncation formula for intersection cohomology in [GM], as well as the L^2-cohomology of a variation of Hodge structure for a product of punctured discs (in [CKS] and [KK]).

It made me both angry and anguished that [Z03] (available as a preprint) was judged insufficient to warrant my promotion to tenure at Rutgers in the Spring Semester of 1979. My supporters in the department were able to arrange, however, that I be granted *two* terminal years and that proved to be very useful. Before getting into that, let me say that I had told David DeGeorge (an assistant professor working in representation theory) what I had been working on, and he pointed out that [MM] seemed similar. The difference was that in [MM] Hodge type was taken only from the base, whereas Deligne's construction mixed in the Hodge filtration of the variation of Hodge structure (i.e., of the fibers). It didn't take long to see a 4-fold decomposition of the cohomology emerging (in [Z06]). Robert Langlands expressed an early interest in [Z03] that I didn't quite understand at the time; in retrospect he was looking towards [Z07] and [Z10] and their generalization. Little did I know that I was venturing close to the terrain of Shimura varieties [D1].

Virtually immediately after we started graduate school together in Princeton, it became apparent that David Cox and I *had* to write a paper together, for reason of the juxtaposition of names. When he was appointed assistant professor at Rutgers the year after I was, we were together at the same place again. Our article [Z04] is, however, a serious piece of mathematics that was put together to address a question posed by our senior colleague William Hoyt. An application of [Z03] appears in the last section.

The article [Z05] was written almost immediately after [Ft] appeared. It was viewed by me to be an exercise in limit Hodge theory. At first it didn't occur to me that I should publish it, but once I did, it had a significant impact on subsequent work in algebraic geometry (see [Fn:§3]).

I want to describe the benefits of having had that second terminal year. Princeton is only a half-hour's drive from Rutgers, so it is relatively easy to attend talks and seminars at Princeton University and the Institute for Advanced Study. Given my emerging interests (in the direction of [Z07]), both DeGeorge and our senior colleague Nolan Wallach urged me to attend Armand Borel's seminar at IAS. It was extremely useful for getting oriented in the subject, and after [Z07] was available as a preprint, for unearthing a notorious error in [BS].[1] It also highlighted two techni-

[1] There was a gap in an important proof in [Z07:§4]. Borel was helpful in suggesting how it might get fixed. Though his manner was often quite gruff, he had a robust sense of humor and was at bottom warm-hearted.

cal points: to use sheaf theory (local to global methods) to study L^2-cohomology on a Riemannian manifold, one had to invoke a compactification so as to "store" the global L^2 condition at the boundary; moreover, the fineness of sheaves had to be specially verified at the boundary, where cut-off functions needed to have bounded differentials. The former article is best-known for the so-called Zucker conjecture, which asserts a natural isomorphism between the L^2-cohomology of a locally symmetric variety X and the (middle) intersection cohomology of its Baily-Borel Satake compactification X^{BB} (from [BB]). Because X is understood to have its canonical class of quasi-isometric complete, locally-invariant Kähler metrics, this imparts a Hodge structure to the intersection cohomology $IH(X^{BB})$. It had been reported that Langlands was excited by the conjecture, for (if true) he thought it might allow for one to manage without the complicated toroidal resolution(s) of singularities $X^{tor} \to X^{BB}$ from [AMRT] (no such luck!)[2].

Moreover, I could apply selectively in 1980 for my next position, as there was always the following year. One of those applications became an offer, an *untenured* associate professorship at Indiana University, and that had to begin in January of 1981. With an advance offer of membership in the IAS 1981–1982 special year in Algebraic Geometry, and a promise of a vote on tenure while I would be away on leave, Daniel Gorenstein, then Chair at Rutgers, encouraged me to view it as a very good deal. Besides, by the end of the Fall Semester of 1980, I had finished my business with Borel; I was ready to move onward.

I was on the faculty of Indiana University for a total of three and a half years, and in residence for one and a half. During the Spring Semester of 1981, I wrote almost all of [Z08], a work so technical that I find it hard to believe I was the author! I was later told by C. Skinner that it was regarded as the definitive treatment of Satake compactifications. I had entered the arena in order to show the presumably useful fact that the Borel-Serre compactification X^{BS} (the manifold with corners constructed in [BS]) mapped onto X^{BB} as a morphism of compactifications of the locally symmetric variety; indeed, it does so for every Satake compactification, even in the absence of complex structure, and these are always finite in number.

Much later, it was reported to me by R. Mazzeo that [Z07:§2] had applications beyond arithmetic groups. It showed how sheaf theory could be used on a manifold with corners. Also, I learned directly from [ChSh] of its relevance on product domains. Indeed, that Section of [Z07] was written with the hope of general application.

Two very different proofs of the Zucker conjecture were offered at about the same time, in 1987; they appeared in print as [Lo] and [SS]. I wrote an expository article [Z11] that presented both proofs. For each, the goal consisted of showing that truncations by degree coincided with truncations by weight, and that led to the notion of weighted cohomology in [GHM]. Both proofs take off from [Z10] and diverge from there. In the case of [SS], the authors did hard analytic calculations; [Lo] used softer methods. With the conjecture proved, it followed that the spaces of L^2 harmonic forms were finite-dimensional in all degrees, and that d had closed

[2]Recent work points to ways around the use of the toroidal compactifications after all. See, e.g., [V].

range. These facts[3] could also be deduced by $(\mathfrak{g}, K)$-cohomological methods from representation theory (see [BG] and [BC]), but the geometric interpretation was missing. The isomorphism allows one to compare structures on the two sides, e.g., the nature of the action of the Hecke algebra.

I went on leave at IAS as planned for 1981–1982. I remember that when the Fall Semester began, there was a superabundance of talks, something that my constitution could not quite bear. I recall a case where I could not decide whether to attend a certain lecture. In vacillating over that, I passed the beginning time of the talk, and my decision was made de facto; I was happy! I must concede, though, that I had acquired a good sense of the state of Algebraic Geometry at the time. In the Spring, I heard rumors that Johns Hopkins University wanted to hire me as an associate professor. This came rather late, and I returned to Indiana University for AY 1982–1983. The situation was uncomfortable because tenure had been granted at IU as expected, whereas the position of associate professor at JHU never carried tenure in those days; moreover, the Mathematics Department was not given the authority to hire at the Full Professor level. To remain at all competitive, it was a departmental decision that hiring at, or promoting to, the level of Associate Professor was a virtual promise to promote to Full Professor. I spent AY 1983–1984 on leave at Johns Hopkins.

Writing up the article [Z09] with El Zein was a bit tricky because of our different styles (American versus French), so we patiently worked it out line by line. One of the noteworthy features of this article was the use of Deligne-Beilinson cohomology to give a simple criterion under which the normal function of an algebraic cycle (over a curve) extends across singularities.

The collaboration with Steenbrink was a model of efficiency. The content of [Z11] was carried out in his office in the mathematics institute of the University of Leiden, over a pair of four-week periods in the late spring of 1981 and 1982. He had a long table in his office. We would sit at opposite ends, scribbling out our thoughts. Whenever one of us had something useful to say, he would go up to the blackboard. I recall that, as it happened, one of us would lead the progress on one day, the other the next day, and so on. I kept a strict set of notes, which made for a quick answer to questions like "Do you remember how we ...?" The thrust of our discussions in 1981 was the mixed analogue of [St] (where $\overline{f}$ was no longer proper), and it seemed as though by 1982 "everyone under the sun" had studied this problem. In 1982 we shifted to finding the naive linear algebra behind the existence of relative weights (from [D3]). Since they do not exist in general, it was saying that local monodromy in algebraic geometry satisfied some non-trivial equations, and we proceeded to determine them. After that, we could prove the mixed Hodge version of Hodge theory with degenerating coefficients. There was one item that Steenbrink was supposed to finish, but he conceded defeat; the collaboration was finished. Somewhat later on, I found I could take care of it myself, and that became the seed for [Z13].

The source of the collaboration in [Z16] is a bit humorous. Morihiko Saito had sent me a preprint under the same title and told me it had been submitted for publication. I pointed out to him that the first non-trivial case was contained

[3] as well as a formula for the L^2-cohomology

in [Z13], and he proceeded to offer me coauthorship! I protested that it was unnecessary, but if he insisted I would want to rewrite the paper. That became our understanding, but I was soon contacted by the editor about signing the copyright agreement. I told the editor about the deal with Saito, and he contacted Saito to confirm it. Well, how long would it take? I was asked. A couple of months, and I set out to perform my end of the bargain. After two false attempts I got it right, and that is what appears in [Z16].

When [S1] and [S2] appeared, I felt that I had been dethroned as "King of Hodge Theory". True, Y. Kawamata's work in higher dimensional geometry had already given a vast generalization of [Z05] (see [Fn]), but I meant in that title the development of Hodge theory, not its applications. Saito's work gave a non-L^2 Hodge structure for intersection cohomology (with coefficients in a variation of Hodge structure); that is the "jewel" in the crown. The method uses a complicated, recursive construction using $\mathcal{D}$-modules. However, it was [Z03] that defined the ground level, viz., over a curve (see [Sb]), and that provided some consolation. The outcome is realized algebro-geometrically in [dCM] and [dC] for geometric variations, using perverse cohomology sheaves, because of the role of the decomposition theorem in both formulations. This I consider to be state of the art.[4]

I had a nice collaboration with another Saito, namely Masa-Hiko. The objective was to produce [Z19], an interesting result about deformations and the infinitesimal Torelli problem for fiber spaces. While working on it, he asked me to explain Faltings' classification of non-rigid families of principally polarized abelian varieties, with its finiteness theorem of Arakelov type. It became a model for [Z18]. The question arose as to whether [Z18] should be considered joint work. It was decided that it was, despite the fact that I did not involve myself with K3 surfaces. (This should be contrasted with the situation concerning [Z13].)

The way by which [Z14] came about is also somewhat interesting. Both Richard Hain and I wanted to try to produce a solo proof of the theorem that classified unipotent variations of mixed Hodge structure, and we set out on our own. Eventually, each of us realized that he was stuck on one main point. But these main points were different! We settled on publishing it as a joint article.

A recurring theme will be deciding that a candidate for the Hodge structure for the intersection cohomology (middle perversity throughout) of a variety Y is "the right one". When Y has only isolated singularities, the answer is clear, because this intersection cohomology can be expressed in terms of the (ordinary) cohomology of Y and its regular locus. By this method, I could verify the Hodge theoretic Zucker conjecture, viz., that the isomorphism in the Zucker conjecture (from [Z7]) respected the Hodge structures in such cases [Z15]. In general, this problem is wide open. The people working in representation theory and automorphic forms would like the L^2 version to be the right one.

The three publications [Z20] through [Z22] were the output of a major collaboration. It began when Michael Harris had started working on [Z20]. He then sought a collaborator, and I became a natural choice. In contrast to [Z12], the

[4] See, however, [S3].

communication between the coauthors was almost entirely by e-mail. That was probably the best way to proceed, for our styles of doing mathematics are so different. On the other hand, our outputs were combined without difficulty. The main issue was to understand the boundary of the smooth toroidal compactifications X^{tor}. These are constructed over X^{BB}, stratum by stratum, so as to form a resolution of singularities of the latter (they also depend on a combinatorial parameter, but that does not affect the nature of what follows). We also had to compare this with the boundary of the Borel-Serre compactification X^{BS}, despite the non-existence of a morphism of compactifications between X^{tor} and X^{BS}. This took a lot of effort, but it ultimately led to the Hodge-theoretic isomorphism of the spectral sequences for the deleted neighborhood cohomology of the two. Of course, on a manifold with corners, the deleted neighborhood cohomology coincides, parabolic by parabolic subgroup, with that of the boundary itself. This led to a workable formula for the E_1-term. We had hoped that this would allow us to rule out the existence of so-called ghost classes (see [Z22: (4.6)] for the definition) for Hodge-theoretic reasons, but alas they exist in abundance [KR].

[Z23] was my only publication with more than one coauthor. Indeed, I knew of Johan Dupont only through Hain as a hub. My contributions may or may not be obvious, but I will confess that I forced myself to write a clear exposition of Dupont's proof of the second main theorem, which expresses the Borel regulator as two times the reduction of the universal Cheeger-Simons class modulo $\mathbb{R}$.

Articles [Z24] through [Z26] comprised an in-depth examination of "my favorite space", the reductive Borel-Serre compactification X^{red} of an arithmetic quotient of a symmetric space of non-compact type (these are the locally symmetric varieties when the symmetric space is Hermitian). As with the Borel-Serre manifold with corners, the construction makes sense for the non-Hermitian case. The (open) boundary faces of X^{BS} are nilpotent fibrations over a lower-rank symmetric space, and one simply collapses each fiber to a point. With this operation effected, one gets a stratified space X^{red}, whose closed boundary strata are the reductive Borel-Serre compactifications of the aforementioned lower-rank symmetric spaces. Making this construction was a useful technical trick in [Z07], where it was done without ado. The name "reductive Borel-Serre" was attached a few years later. In [Al], Albin considered X^{BS} from the point of view of manifolds with corners having iterated fibration structure à la R. Melrose. If one first modifies X^{BS} as a manifold with corners, one gets a Melrose manifold with corners, which blows down to yield a resolution of the Thom-Mather stratified space X^{red}.

Item [Z24] was my first solo research article to be written in ten years, and it reestablished my status as a research mathematician. The main theorem was something that was waiting to be plucked some twenty years after the Zucker conjecture was made, though it was much easier to prove. For contrast, the Zucker conjecture was about L^2-cohomology, and it was a difficult matter to prove it, even after it was posed (see [Z11]); the main theorem in [Z24] admitted a direct proof nearly immediately after it was asserted: $H_{(p)}(X)$ is isomorphic, when p is finite and sufficiently large, to the (ordinary) cohomology of the compactification X^{red} of X. Though determining the L^∞-cohomology would have been more natural, that had to be pushed down into $H_{(p)}(X)$ to gain a topological interpretation (one can do this because the locally invariant volume of X is finite).

For [Z25], I introduced the notions of an excentric quotient of the Borel-Serre compactification and of the toroidal compactification(s) (constructed in [AMRT] in the Hermitian case). These involve taking the quotient of their boundary strata by a real torus (product of circles), yielding the compactifications $X^{BS,exc} \to X^{red}$ and $X^{tor,exc}$ of X. Though they are not homeomorphic (when X is Hermitian), they have a striking resemblance, and one finds that the two are homotopy equivalent. On the other hand, the greatest common quotient of X^{red} and X^{tor} is X^{BB} [Ji]. This was carelessly conjectured for X^{BS} instead of X^{red} in [Z21]; there was a counterexample known to the authors that passed our notice when the conjecture was made. In that instance, X^{tor} happens to be homeomorphic to $X^{BS,exc}$.

Article [Z26] started a venture into "outer space". In that article, a mixed Hodge structure is constructed on the cohomology of X^{red} (when X is Hermitian), satisfying all obvious compatibilities, e.g., with the mixed Hodge structure on the cohomology of X^{BB}. The construction uses an inelegant recursion that is based on the description of its closed boundary strata given three paragraphs back. It made sense to attempt to find an underlying motive. With the suggestion of Marc Levine, Joseph Ayoub and I met in 2007. I presented Ayoub with the task of motivizing [Z26], in the sense of V. Voevodsky. He preferred and needed a more canonical construction, one that involved motives over a diagram of schemes, and the like. Despite the length of the resulting article [Z27], he wanted to submit it to a top journal. I thought that would be unreasonable, but he insisted. I had no strong objection, though, because he was the junior author, interested in promoting his contributions to mathematics. Besides, the result was *that* remarkable, showing that something that was not an algebraic variety could have motivic cohomology groups (over $\mathbb{Q}$). As I like to say, it justifies the occurrence of X^{red} in the literature; as M. Goresky put it, X^{red} thinks it is an algebraic variety. Because of the heavy use of motives, most of the writing was done by Ayoub, and it was important that the article be written in English — *sorry, Joseph; he was more comfortable in French* — and he really did an excellent job.

Article [Z28] was the only collaboration involving the organizers of the conference. Lizhen has told me that he considers himself, to a certain extent, to be a student of mine. What I make of this is that his involvement in the vast ocean [BJ] of compactifications of symmetric and locally symmetric spaces, and the relations among them, stems in part from his work for [Ji].

Last but not least, I mention the collaboration with Jean-Luc Brylinski [Z17], which provided an account of advances in Hodge theory that occurred roughly in the years 1975–1990, the fifteen years after the expository article [GS] came out.

References

I am listing as [Z**] *those publications of which I was an author. Coauthors (if any) are noted thereafter as:* "(with +++)".

[Al] P. Albin, On the Hodge theory of stratified spaces. In this volume.