# THE ORE MINERALS AND THEIR INTERGROWTHS

SECOND EDITION

by

## PAUL RAMDOHR

Heidelberg

English Translation of the 4th Edition
(with Additions and Corrections by the Author)

IN TWO VOLUMES
VOLUME 1

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### PREFACE

### TO THE SECOND ENGLISH EDITION

The present book will, with all probability, be the last edition revised by the author. Therefore, he asks permission to give some more remarks than those which are to a certain extent technical necessities for the preface of a textbook.

My books on oremicroscopy have obviously inspired many interested scientists to investigate and to clear many questions. Certainly it turned out that not everything, which I thought to have obtained by my work and which I explained, was in all details correct. But that is a natural consequence of the progress in science. I, myself, have in such cases often taken up these questions again and reexamined, with three different results:

1. My statements were simply wrong — due to very varied reasons — or they could, many years ago, not be right due to the then imperfect methods and/or the too sparse material. The latter refers e.g. to the distinction between valleriite and mackinawite or to the complications in the system Ag-Sb between silver and dys-

crasite and many others.

2. Other cases show that nature is often much more complicated than we assumed. One e ample are the Pb-Sb- or Pb-As-sulfosalts, where not only the actual compounds turned out to be much more numerous than anybody expected, but also the number of the components was greater. In addition — or perhaps in a certain opposition — there is now often an occurrence of solid solution with a broad spectrum of different properties complicating the definitions as well as the clear descriptions. Another problem is the "formula", traditionally accepted to be stoichiometric, which they are often not at all. Many members of the niccolite group, e.g. are marked by the vacancy in "cation" sites, in the case of melonite varying between NiTe to NiTe. — here apparently continuously — while in other cases — I doubt whether it is always justified — to assume a sudden alteration of the stoichiometry with often rather unlikely complicated members. One of these extreme cases is e.g. pyrrhotite. I hope, the reader will understand when I am not willing to agree to the "trend" of some colleagues to always create new names. Tiny splittings in the X-ray powder-diagrams can have essential meaning but it is not necessarily so.

A similar problem is the "anion deficiencies", e.g. frequently occurring in minerals of the sphalerite and wurtzite structures and their relatives, even when not due to low partial pressure of S. To give a new name (or x names!) for a high-temperature chalcopyrite, which became cubic with principally the same lattice, and having lost a trace of S, and perhaps stabilized by a minimum content of Ni, is in my opinion completely unnecessary and in the consequence dangerous. Just in this group there exists a possibility of replacement or a further addition of other metal atoms as a result

of the very loosely packed ZnS lattice. Here we should not wonder, when as a result the lattice is distorted from cubic to tetragonal, to o'rhombic, to hexagonal etc. and/or it shows variations in space groups or polytypes. We should try first — even when we find it hard — to emphasize more the common properties of this group, and also many other more than the differences, since small variations in composition can go parallel with very distinct color deviations.

3. There is also a great number of new reports in literature, which apparently contradict my data or really do. The author could, of course, neither investigate all the deposits of the world, nor could he always get complete or in each case authentic material of all minerals — however in the course of his long life he worked hard in order to get at least a good general view. But it astonishes when the author is reproached for oversights, e.g. when somebody states that sphalerite stars in chalcopyrite are not a result of exsolution! I do not at all deny, that for one or the other reason these stars may occur somehow in similar form. However that exsolution is the rule can be proved by correct observations from more than 1000 deposits and laboratory experiments which can be carried out in a few hours! Some beginners put much more weight on statistics of two observations rather than on conclusions drawn from reliable statistics from 50 or 200!

Some discrepancy may derive from the overestimation of the experiment. — "Daraus schließt er messerscharf, daß nicht ist, was nicht sein darf" (From this he concludes "knife-sharp" that something cannot be which ought not to be!) But, nevertheless, it often is so! In dozens or perhaps hundreds of cases low-temperature compounds are known, which experimentally do not occur at all or only at normally intolerably long times of reaction. Vice versa, there are high-temperature minerals, which, in the experiments are absolutely instable below a certain temperature, become durable by tiny additions, and can also occur in nature. In metallurgy these are absolute trivialities. Expressions, such as "do not exist" should not be used at all, "could not be proved experimentally" only with the suggestion "in due time", or "with the experimental apparatus which was at our disposal". The author remembers a time when it was said that pyrite "could not be produced experimentally". But once you know how to make it, it is not at all difficult to make pyrite! A long list of examples could be given.

The trend to give "data" which deliver foolproof right determinations has been developed e.g. since Murdoch, Davy and Farnham and Short, who tried in vain to get it by systematic etching, works now especially in two directions, indeed seeming to be extremely easy to recommend to the experienced observer:

- 1. The determination of hardness, at first as scratching-hardness (Talmage) and polishing-hardness, now especially as micro-hardness because it seemed to lead to quantitative accessible values.
- 2. The reflection-behaviour, already subjectively the most obvious characteristic, seems, regarding the modern highly developed methods, to be especially useful for quantitative data.

But both disappointed! The difficulties are here not caused by the technique of measurements but by the material itself: In the chemism where tiny, often almost trace-liké components or likewise the pre-treatment can change the hardness completely. The same (in some cases at least) may happen very quickly with the reflection-behavior. In the polishing-technique e.g. already polishing in water or oil, polishing

under high or low pressure can cause varying micro-hardness but also the reflection behavior (mostly by differently strong or quick tarnishing) can be influenced considerably. The variability may also be caused by the difference between "real-crystal" and "idealcrystal", where it results in various but not at the first glance always visible properties: in the hardness getting higher or lower (compare the behavior of technically pure (99.5%) Zn with the so-called 5-nines Zn (99.999%). The reflectivity is mostly higher the nearer it comes to the ideal crystal. If e.g. out of these reasons UYTENBOGAARDT & BURKE give in their tables for a surely in hardness not strongly anisotropic and besides that in its chemism rather simple mineral, such as ranimelsbergite, a Vicker's-hardness of 368-1048, then this proves clearly that such statements are not very useful and that it is not possible to call them "quantitative". A remark "differs in surprisingly wide data limits" would express much more. The figures of the measurement values might be alright — but when from these a "mean" is taken, this is really rather "risky".

In the reflection behavior we have the same problem. The measurements can be carried out much more accurately than they are significant for the object. We do not know all reasons, why the values vary so strongly already in the same section with excellent fresh polish and exactly the same method. Chances play perhaps the same part as the natural pre-treatment (shearings, recrystallizations) or lattice deficiencies or minute admixtures etc. When it is said from standards which have been used for a long time (not any more), e.g. galena or pyrite, that they have always and everywhere the same reflectivity (of course, only as long as they were not distinctly tarnished) then this was surely a mild self-delusion. With ideal conditions, from the same deposit and with material not being zoned, differences of 4 units in an intermediate reflectivity, i.e. > 8% are not at all unusual. This seems to be small compared with the hardness-values but concerns in this medium range minerals showing strongly overlapping properties. Anyway, I think, the measurements of reflectivity may have a genuine chance in future if we compare statistical broadness (across the whole spectrum) — and if we are extremely careful! But today we are still very far from this goal.

For very uncommon minerals we should go again and again back to the powder-diagram and the microprobe. However, these too have their tricks. I mention powder-diagram on page 305, here only a few words on the microprobe. The microprobe or the microsonde, as it was first named by its inventor Castaing, has given many valuable results, sometimes it has simplified the work greatly, but in many cases also shown that the facts are far more complicated than we at first assumed. — In spite of its invaluability it should not be forgotten, that the careful visual microscopic observation is still the primary! First of all, we must observe, that there is something to be seen at one place of the section, which is worthwhile for further investigation, then we can work with the microprobe!

Cases like the discovery of wairauite, CoFe, having been found accidently during the investigation of awaruite and without any suspicion microscopically will, due to time and cost, remain rare exceptions. — Some publications of careful work with the microprobe has only proved things which were already known to Berzelius and Gustav Rose some 150 or 120 years ago. On the other hand, there still remain many things much more worthwhile to be investigated.

Now some remarks on literature. It seems to be surprising that my list of literature still mentions very ancient papers. First of all, I think, that this is a matter of gratitude

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To all these gentlemen I owe my sincere gratitude. Professor G. C. AMSTUTZ gave also some suggestions for the new edition. My friend and old pupil, Professor A. El Go-RESY was so kind as to read the proofs with me and gave some helpful advices.

### ABBREVIATIONS

Abbreviations were avoided wherever possible, even despite the possibility of criticism. For physical and a few crystallographic data the conventional symbols are, of course, used.

$n_{\omega}$ or $n_{O}$ , $n_{\varepsilon}$ or $n_{E}$	<ul> <li>main indices of refraction in uniaxial crystals (ordinary and extraordinary directions).</li> </ul>
$n_{\alpha}, n_{\beta}, n_{\gamma}$	- main indices of refraction in biaxial crystals.
$R_{o}, R_{E}, R_{\omega}, R_{s}$	- reflectivity of uniaxial crystals.
$R_g, R_m, R_p$	- reflectivity for biaxial crystals ("grand, moyen, petit"!).
$\varkappa$ , or $\varkappa_0$ , $\varkappa_E$	<ul> <li>absorption index kappa.</li> </ul>
# '	- cleavage, or cleavage after
<	- cleavage, or cleavage after smaller than
>	- larger than
~	- approximately or similar
>	- similar, but somewhat larger
Ø THE STATE OF THE	on the average (or diameter)
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# TABLE OF CONTENTS

### Volume 1

A-133			rage
108	-	addworpnotei (sales (**)	VII
WRITER	s's preface to the second English edition	serphical distribution X	\ TT
TRANSL	ATORS		XX
ABBREY	VIATIONS	XX Continue of aggregative	XII
401		The second of th	1
INTROD	UCTION TO THE GENERAL SECTION: INTERGRO	WTHS OF THE ORE MINERALS	1
GENETI	IC SYSTEMATICS OF ORE DEPOSITS		3
A. Met	teorites	San	4
B. Ma	gmatic sequence		4
78 I.	Plutonic Rock series	eri i era il all'il de la	5
WEI.	(a) Intra-magmatic stage	The states year, and states with the series	6
611	1. Magmatic differentiation through unmi	xing of fluide	6
	2. Magmatic differentiation through crysta	illization	7
	3. Main crystallization of silicates	in) Parapiornia	9
	4. Deposits formed by filter pressing	netal atomal (4)	12
54	(b) Pegmatitic-pneumatolytic stage	equitourile mail and forest (1)	13
KP F	(c) Plutonic-hydrothermal deposits	algorit the amenda of	15
815	(veins, replacements, impregnations)	THE STREET STREET, STR	10
II.	Subvolcanic Series	tion at part on " above to	19
III.	Extrusive sequence		20
C. Sed	imentary sequence	ingégi i eren ar aptoasais n ≥ 74.1 P	23
eri.	Concentration of mechanically weathered me	aterials de la mollemana	23
II.			-24
	. Precipitation of dissolved substances on the		24
	Precipitation of dissolved substances in the		27
- S. S. C.	Deposits of coal and petroleum and of the mar		31
V.	The zone of oxidation and cementation	collais generating related to the	31
VI.	The zone of oxidation and dementation		O L
D. Me	tamorphic sequence		37
The	e metamorphism of ore minerals		37
	General Aspects		.37
II.	Metamorphism through change in temperat	ure and confining pressure	39
	Metamorphism at high and highest pressure		47
1117	Metamorphism at high and highest pressure.  Metamorphism under the influence of directions and the state of t	acted programs with or without	_ •
TV.	Metamorphism under the influence of directions of the second state of the second secon		47

	(a) General	Page 47
	(b) Application of the concept of "depth zones" of rock metamorphism to ores and ore deposits	77
	(c) Minerals formed during serpentinization of ultrabasic rocks	79
THE OR	E TEXTURES	
Order o	f presentation	81
PRINCI	PLES OF THE CLASSIFICATION OF THE ORE INTERGROWTHS	82
A. The	a fabric properties considered from a purely geometric point of view	84
, I.		84
,	(a) Internal nature	84
	(b) External grain properties	93
II.	Intergrowths of several minerals	104
400	(a) Oriented intergrowths	108
H/X	(b) "Emulsion" textures	110
10/10/	(c) Penetration textures	110
Asido.	(d) Myrmekitic intergrowths	110
ш	The forms of aggregates	123
	(a) Arrangement in space (b) Contact rims	124 129
1 .	(c) Mineral inclusions in ore minerals	134
T37	Schneiderhöhn's systematic classification of the structures	102
IV.	and textures of the ores	137
B. Gen	etic fabric types	139
I.	Texture of primary precipitation	139
ö .	(a) Growth fabric (crystallization from melts and solutions).	139
	(b) Colloidal textures	144
0	(c) Sedimentary textures	157
II.	"Transformation Textures"	162
D E	(a) Paramorphs	163
3.4	(b) Exsolutions	165
31	(c) Decomposition structures	194
	(d) "Verdrängung" — "Replacement" — "Metasomatism" (e) Thermal transformations	194 218
	(f) Oxidation textures, and	218
	(g) Cementation zone	218
III.	Radioactive haloes, lattice destructions, blasting	231
	Recognition of the genetic position of ore deposits	238
ı.	Typomorphic minerals, mineral assemblages, paragenetic	
	sequences and fabric types	238
78 II.	Ore minerals and ore associations as "geologic thermome-	
	ters" Particular of April 1980 and A	242
III.	Relicts	250
IV.	Further possibilities of genetic interpretation of textural	200
	characteristics approximation of the characteristics	, 258
C. The	relationship of ore textures to industrial minerals and benefication problems	263
DESCRI	PTIVE SECTION	
Anne	otation concerning the arrangement of material in the descriptive section	283
I.		284
31	General Data	005

		CONTEN	TS	V	П
.III Page	Deflection behaviour				age
IV.	Reflection behaviour Etching				290
v.	Physico Chemistry			Z	96
vi.	Fabric			1 (* (* (* (* (* (* (* (* (* (* (* (* (*	99
	Special fabrics				02
VIII	Diagnostic features				03
IX.	Paragenetic position				03
	Investigated occurrences				04
XI.	Literature				05
XII.	Powder Diagram	(mi	(+ Allargenu		05
				" Tradica to a marchaga in	
ELEMEN	TS AND INTERMETALLIC COMPO	TINDO		office blagoring	00
Tanamen	IS AND INTERMETABLIC COMP	OUNDS		Int. a padotra	08
Copper	r (+ Whitneyite)			3	08
Silver				3	13
	Electrum)			3	21
	pride (AuCu, Goldcuprid)			3	34
Maldon	nite			TO THE TAX PROPERTY OF THE TAX	36
Lead					37
	ry (+ Kongsbergite and "Mo	schellandsb	ergite")	11 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1	37
The state of the s	nerite and Paraschachnerite			in Principles of the International Control of	38
Palladi				Table 1. The rest of the state of the second section of the section of the section of th	38
Iridiun	lladium, cf. Stibiopalladinite				39
	ım and Ferroplatinum				39
	dium and Iridosmium				40
	nskite and Sysserskite				50 50
	-iron) + Taenite				53
100 C	ite (Josephinite, Souesite, Bol	rovakite)			56
Wairau		olo v BRIDO)			57
	te — Cementite of Metallurgy				58
Schreib	ersite and Rhabdite			and the last the standard 30	61
Native				metil stimp - laft in a lig	62
Potarit	e A TABLE		an Heribard In	30	62
Tin				30 Per 201 All Call 1 - 15 - 5	62
Nigglii	te			30 A THE RESIDENCE OF STREET	63
Sviagin				30	63
	opalladinite			36	64
Polarit					64
	palladinite			assentations something	
Zinc	direct and who have a con-	"Variotiests		- [ - [ - [ - [ - [ - [ - [ - [ - [ - [	65
	Arsenic		19 2 1	나를 되는 지하는 이번에 가장 전 경험하면 하는 그 때문에 다른 그게 없었다.	65
Arseno	lamprite ontite"-Stibarsen		2		70
THE RES LAND 11 YOUR PROPERTY AND ADDRESS OF THE PERSON AND ADDRESS OF					71
Antimo					73
Sulphu					74 81
Seleniu				0.1311 (1892) 10.514 (1892)	82
Telluri					83
The second secon	te (+ Paragraphite)				84
Carlsbe	ergite and Osbornite			30	92
\$95 Lat.	0			allight again	- 48
10211				ters and other	
ALLOY-LI	KE COMPOUNDS and TELLURI	DES		38	93
Whitne	yite, Algodonite, Domeykite			39	92
Cupros	tibite				98
Kutina				39	
				24 W. 120	- 0

Koutekite, Novakite, Horsfordite		Pag 39
Dienerite, Orcelite		9 1 - 1124 11 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1 1
Oregonite		40
Mauchereite (Temiskamite, artific	ial "Nickelspeise"	40
Hauchecornite		40
Parkerite		40
Shandite		40
Heazlewoodite		40
Dyscrasite and relatives (+ Allarg	entum)	40
"Dyscrasite of Cobalt"		41:
Stibiopalladinite		41
Allopalladium		41
Rickardite		410
Weissite		419
Vulcanite		419
Melonite		420
Kitkaite		42
Imgreite		42
Hessite		Language Manager 42
Petzite		424
Fischesserite		428
Stützite and Empressite	1 m	428
Sylvanite (,,Schrifterz")		426
Volinskyite	e.	429
Kostovite		430
Krennerite (= Müllerine, Bunsenin	ie)	430
Muthmannite		43
Calaverite	(mittletter/mill	Lastranos, salo (desert) attenes 431
Nagyagite		433
Montbrayite		438
Tetradymite and Tellurobismutite	(Tellurwismut)	436
Other Tellurium — Bismuth compo	ounds	438
Pilsenite (= Wehrlite) and Hedley	ite	438
Joseite, Grünlingite, Oruetite		439
Temagamite		440
	Volume 2	PhaebogstladigitA
06		Polarito
OMMON SULPHIDES AND "SULPHOSAL	TS'	No bollermee 441
Chalcocite with Digenite, "rhomb	ic chalcocite" "	oubic shalacaita" Diuglaita
Anilite, "rose-gray chalcocite"	ic charcoche,	
Berzelianite		441
Bellidoite	- 4	468
Oosterboschite		468
Umangite		469
Argentite-Acanthite		
Naumannite (with Aguilarite)		471 478
Cacheutaite		178 478
Argyrodite and Canfieldite	3 1	
Crookesite		478
Stromeyerite	2	481
McKinstryite		481
Eucairite		484
Betechtinite		40.4
		484
Larosite		486
Larosite Jalpaite		

#### X

A COMMENTO STATEMENT STATEMENT				T T	Page
Pyrrhotite-group					592
Smythite				Salandario II.	612
Eskebornite				mer grennider. E. A. S. serraladas	612
Jaipurite				C. CO. STATE STATE	614
Freboldite					614
Niccolite				Section - contract to	615
Langisite					623
Breithauptite				1 1 2 2 2 1 1	623
Westerveldite				4.30	625
Sederholmite				1777 17,71 24,33	625
Modderite				an indiavally a	625
Millerite				WAR SEE SEE	626
Mäkinenite					630
Cubanite					630
Sternbergite-Argentopyrite-Group					639
Sternbergite					641
Argentopyrite			1 y 60 c. 1	The state of the s	641
Niningerite					642
Oldhamite				医抗性性性 医克利二甲酸	642
Alabandite				If officers	642
Iron-Alabandite					646
Galena	33				646
Clausthalite	7				659
Altaite			en block i		661
# 10 PM					662
Miargyrite					2
Aramayoite					664
Schapbachite-Matildite					665
Herzenbergite and "Montesite"					669
Teallite		4		2.18.5	670
"Beegerite"					672
Cinnabar					673
Platynite					675
Covellite					676
"Permanent blue covellite"					679
Klockmannite	* * * * * * * * * * * * * * * * * * * *	6			681
An introduction to Valleriite — Mac	ekinawite				683
Valleriite					683
Mackinawite					683
Idaite				The second secon	692
Braggite - Vysotskyite	3				695
Roseite					696
Cooperite					696
Linnaeite Group			·		697
Bornhardtite				2007	702
Trüstedtite and Tyrrellite					703
Wilkmanite					703
Indite				tigra comerció	703
Daubseelite				addlesonthe/	
Brezinaite		to high dead	H. TREES	mer Vinnennier.	704
Getchellite					704
Ottemannite				SOL DELLE	704
Antimonite, Stibnite					705
Metastibnite					709
Bismuthinite					710
					714
Guanajuatite and Paraguanajuatite	ne rakin.				
Kermesite					715
Pavonite (Alaskaite)					716

CONT	ENTS
Chalcostibite (Wolfsbergite, Guejarite)	
Cuprobismutite	
Emplectite	
Junoite	n wakesil piog
Wittichenite	မြို့အလုပ်(Lage <sup>27)</sup> ကို အသင် မောင် (X 🚈 က သွေးလုပ်)
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Berthierite	ន់ នៅ ប្រាស់ ម៉ែង ម៉ែង ម៉ែង ម៉ែង ម៉ែង ម៉ែង ម៉ែង ម៉ែង
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Bournonite	
Berthonite	
Aikinite — Patrinite	7.5
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Hutchinsonite	
Andorite, Ramdohrite, Fizelyite	
Marrite	and the second of the second o
Freieslebenite	· Part (ACC) approxima
Brongniardite	. Adequat
Diaphorite	egger elle
Owyheeite	
Ultrabasite	
Schirmerite	भ्याचार अस्तर और वि
Benjaminite	
Franckeite	-Maire var
Cylindrite	7716e 1-10 <b>7</b>
Preliminary remarks to the Pb-As-Sulfosalt	g agnosially to the speciation T
bach, Binnental (Wallis)	
Sartorite (Scleroclase)	
Ustarasite	7 min - 1 min - 2
Baumhauerite	
Liveingite ("Rathite II")	7
Rathite (Rathite I)	*** or or ** ***
Dufrénoysite	7
	7
Lengenbachite	7
Jordanite	7
"Guitermanite"	7
Gratonite	7
Pierrotite	7
Amorphous Hardened Sulfide Colloids ("Su	alphide glasses")
Preliminary remarks to Pb-Sb-sulfosalts	7
Zinkenite	
Füllöpite	t to the parameter
Plagionite	egg et dann ag at 7
Heteromorphite	as well to 17
Robinsonite	न मंद्रकर विक्रिके
Semseyite	augmed for exercise 2
	(注句:於70
Jamesonite, "Federerz", in part, Heteromor	phite, in part

	Page
Boulangerite and Falkmanite	to emperatively selly a spid become 770
Meneghinite	na ken Padorana 772
Geokronite (Kilbrickenite)	no '991 ( n. <b>774</b>
Preliminary remarks to the Pb-Bi-sulfosalts	776
Galenobismutite	10 ft de la 1777
Bonchevite	2778
Cannizzarite	778
Weibullite	778
Cosalite (Bjelkite)	778
Bursaite	780
Kobellite	780
Heyrovskyite	781
Lillianite	781
Gustavite	782
Goongarrite (= Warthaite)	783 - 10 mg/a 16 mg 16 mg 17 m
Lafittite	783
Routhierite	783
Proustite (Light Ruby Silver)	783
Pyrargyrite (Dark Ruby Silver)	785
Rittingerite and Pyrostilpnite	**Hilosopol789
Samsonite	alimethrol790
Pyrite	stable for stable 791
Melnikovite-Pyrite ("Colloform pyrite", crys	stallized FeS, gel) 806
Bravoite with Vaesite and Cattierite (Nickel	pyrite, Hengleinite) 809
Villamaninite	and the second confidence of the second second 816
Blockite ("Penroseite")	818
Trogtalite	819
Krutaite	91draimnor 820
Bambollaite	strodgsiC820
Laurite	820
Erlichmannite	**************************************
Sperrylite	921
Aurostibite	824
Geversite	825
Michanarita	825
Insizwaite	
Froodite	(ailia7/) istoengiti das 826
Hauerite	(sarber local shrows 826
Cobaltite	**************************************
Gersdorffite	-11 7-11 RELIEF 1833
Ullmannite	836
Hollingworthite	2, 17 11 2 12 13 14 2 838
Irarsite	PRATE 2019 (*838
Merenskyite, Moncheite, Kotulskite	838
Bukovite	839
Marcasite	839
Hastite	945
Ferroselite	845
Kuneruaite	a share tribute to a superconfidence \$46
Safflorite-Löllingite-Rammelsbergite Group	the other maddless assumed granteniard 846
Safflorite (Spatiopyrite)	817 19 ATO 3 64 1
Rammelsbergite	852
Löllingite	854
Frohbergite	. tal plante staf 858
Pararammelsbergite	, namasado/1860
Costibite	n tr. 1600 201
Paracostibite	Telescondistrated in the second 1861

	CONTENTS	XI
	Nisbite	Page 863
	Arsenopyrite (Mispickel)	863
	Glaucodot	
į	Gudmundite	871
	Irarsite and Osarsite	872
		874
	Molybdenite	874
	Berndtite	880
	Tungstenite	880
	Skutterudite (with "Speiskobalt"-smaltite and chloanthite)	881
	Patronite	887
	Realgar	. 889
	Dimorphite	890
	Orpiment	890
	Wakabayshilite	890
	Duranusite enlered energy and and and	891
	Voltzite	892
		000
	Oxidic ore minerals	893
	Cuprite **Moords	893
	Zincite	896
	Manganosite	897
	Wüstite	898
1.13	Tenorite (Melaconite)	899
	Paramelaconite	903
	Delafossite	903
	Crednerite	906
	Murdochite	906
	Spinel ' Indian to the state of	906
	Galaxite	909
	Preliminary remarks on the Ferrite-Spinel Family	910
	Magnesioferrite	911
	Magnetite	911
	Ulvöspinel — Ulvite	923
	Franklinite the shire the state of the shire t	940
	Jakobsite and Vredenburgite	943
	Chromite	946
	Hausmannite	955
	Hydrohausmannite, Hetairolite, Hydrohetairolite	958
	Marokite	959
	Bixbyite — Sitaparite	959
	Braunite	962
	Magnetoplumbite	966
	Plumboferrite	967
	Quenselite	967
	Eskolaite Karelinite	968
	Hematite, Specularite, Oligiste	969
		980
	Ilmenite (with Geikielite and Pyrophanite) Pseudorutile	997
	Högbomite	998
	Makedonite	1000
	Maghemite	1000
	Perovskite	1004
	Davidite	1004
	Rutile	1002
	Ilmenorutile, "Strüverite"	1009
	Anatona	1010

Cassiterite	1012
	1013
Plattnerite N IV O : 1	1021
Preliminary remarks on Mn <sup>IV</sup> Oxides	1021
Polianite and Pyrolusite	1022
Polianite (sensu stricto)	1022
Pyrolusite (sensu stricto)	1025
Ramsdellite	1028
Nsutite ("Nsuta"-MnO <sub>2</sub> )	1029
Psilomelane and related minerals (Psilomelane in sensu stric	cto, cryptomelane,
coronadite, hollandite)	1030
Hollandite	1035
Coronadite	1036
Lithiophorite	1038
Nolandite	1038
	1039
Columbite, Niobite-Tantalite	1039
Tapiolite	
Brannerite	1043
Pyrochlore Group	1046
Pseudobrookite	1046
"Armaleolite"	1047
Anosovite	theoretical 1047
Kennedyite	1049
Baddeleyite	1049
Uraninite (Pitchblende, Nasturán, Uranpecherz)	1050
Thorianite	1070
Hydrous Iron Oxides	1070
"Nadeleisenerz (= Needle Iron Ore"), Goethite in American	
Akaganéite = $\beta$ -FeOOH	1076
Ferrihydrite	1076
Lepidocrocite (Rubinglimmer, Germ.)	
	office design of the state of t
Manganite	
Groutite	1081 and 1081
Montroseite	1081
Heterogenite-Stainierite	1081
Woodruffite	1084
Todorokite	1084
Chalcophanite	egin commend 1085
Wolframite allowed ployby II and some	vield inflansmementaries 1087
Scheelite	B 1 807 1093
Ludwigite - Vonsenite	1094
Bonaccordite	1094
Hulsite-Paigeite	1095
Lievrite (Ilvaite)	1095
Coffinite	1093
W.A.	1037
	1 - 049.04
GANGUE MINERALS AND NON-OPAQUE OXIDE ORE MINERALS	1101
Quartz	1102
Calcite	1103
Dolomite	1105
	1106
Siderite, Chalybite	
Rhodochrosite	1107
Smithsonite	1107
Cerussite	1108
Malachite and Azurite	1108
Barite (Heavy spar)	1109
Anglesite	1109
Fluorite (Fluorspar)	1110