NORTH OF ENGLAND INSTITUTE OF MINING ENGINEERS.

TRANSACTIONS.

VOL. VII.

1858-9.

NEWCASTLE-ON-TYNE: ANDREW REID, 40 & 65, PILGRIM STREET.

1859.

ADVERTIZEMENT.

THE Institution is not, as a body, responsible for the facts and opinions advanced in the Papers read, and in the Abstracts of the Conversations which occurred at the Meetings during the Session.

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Report

It has been said that a prosperous scientific or literary institution ought, like a river, to go on increasing in volume and magnitude. The Council of the North of England Institute, therefore, have reason to congratulate the meeting on the state and prospects of the Society, as evinced by the increase of members during the past year. Amongst the papers, which may be enumerated as follows, there are many of great practical value:—1. On the Working and Ventilation of Coal Mines in Northumberland and Durham, by Mr. John Wales. 2. On Gas Drifts for drawing Goaves, by Mr. Alexander Ross. 3. On the Magnetic Ironstone in Rosedale, by the President. 4. On the Relative Importance of certain Causes in producing Changes of Density in the Atmosphere of Mines. 5. Remarks on the same subject, by Mr. Atkinson.

It will be observed that a series of important and interesting experiments in causes affecting the density of air circulating in mines, are still in course of being detailed and explained. The question is, in fact, yet unexhausted, and requires to be much further investigated.

The President has, from time to time, laid before the Society the steps taken by the Special Committee appointed to further the foundation of a College of Mining and Manufacturing Science, and the results which encouraged them to persevere in the attainment of this most desirable object.

In the governing body of the University of Durham, there exists a position to further the views of the Institute in a satisfactory manner,

as shown by the notice already laid before the Society of a proposed arrangement to be made with them, in which, it will be observed, the independent character of the College is maintained, consistently with the greatest amount of available and valuable aid to be derived from the University.

The Council have also to state that the Committee had an interview with the Secretary of State for the Home Department on the subject of the proposed Mining College. The deputation, consisting of Mr. N. Wood, President of the Institute, Mr. T. J. Taylor, Mr. I. Lowthian Bell, and the Rev. Temple Chevallier, Registrar of the University of Durham, was introduced by Mr. Mowbray, M.P. for Durham, and was accompanied by the following Members of Parliament:—Sir M. W. Ridley, Lord Adolphus Vane, the Hon. H. G. Liddell, Mr. Farrer, Mr. Henry Pease, Mr. Hugh Taylor, and Mr. George Ridley. The deputation explained to the Home Secretary the character of the proposed institution, as shown in the printed prospectus and transactions, and answered the various questions put by the right honourable gentleman, stating that, while the promoters could charge themselves with the outlay of £5,000, estimated to be required for the expenses of establishment, they expected the aid of Government, to the extent of £3,000 a-year, for the endowment. The Home Secretary received the deputation favourably, and undertook to communicate on the subject with the Privy Council.

The Council have, on a former occasion, alluded to the series of experiments, under the direction of Sir William Armstrong, Dr. Richardson, and Mr. James Longridge, on the best means of consuming the smoke of bituminous or semi-bituminous coals; and on the economic and evaporative value and power of the Hartley coals of this district, as compared with the coal of South Wales. The Reports drawn up and published by these experimenters have led to the publication of another series, drawn up under the sanction of the Honourable Board of Admiralty by their Commissioners, Messrs. Miller, Taplin, and Lynn, on the same nationally-important subject, and printed and published by order of the House of Commons. As to the results of the whole, the Council need not hesitate to say that, in their opinion, this interesting question may be considered as being really settled by the conclusions recorded in these very able Reports. The results of the series of experiments prove that, when the *gaseous* contents of the Hartley coals of the North of England are burned together with the solid carbon of the coal, the heating power of the Hartley coal is fully equal to that of any other coal.

Before concluding, the Council must be allowed to express their deep regret for the loss that the Society has experienced in the death of the late Mr. John Wales, of Hetton, which occurred soon after the publication of that lucid and practical exposition of the most approved methods of working and ventilating coal mines, of which the public, as well as the Society, is happily in possession.

To the Report of the Finance Committee the Council need only to refer. It is appended to the present Report, and will be found to be, upon the whole, satisfactory.

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Finance Report.

The Treasurer's Accounts for the past year have been examined and are now submitted to the attention of the Institute.

The balance of cash in hand shows a slight increase over the last year's statement, which, with the interest upon the deposit with the District Bank, makes a reliable balance in cash of £667 18s. 9d.

The Library and a collection of good useful Maps of Geology merit the further outlay and attention of the members. Amongst others, the Maps, so far as published, of the Geological Survey of Great Britain and Ireland, together with such authentic Geological Maps as have been issued by foreign governments.

The Committee again press upon the attention of the Institute the getting up a proper catalogue of the books, instruments, models, &c, of the Society, to be published regularly, as they increase, with the "Transactions."

A*ugust*, 1859.

P. S. REID

EDW. F. BOYD.

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The TREASURER IN ACCOUNT WITH NORTH OF

For the Year from August, 1858,

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ENGLAND INSTITUTE OF MINING ENGINEERS

to and with August, 1859

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General Statement, August, 1859

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Patrons

His Grace the Duke of Northumberland.

The Right Honourable the Earl of Lonsdale.

The Right Honourable the Earl Grey.

The Right Honourable the Earl of Durham.

The Right Honourable Lord Wharncliffe.

The Right Honourable Lord Ravensworth.

The Right Reverend the Lord Bishop of Durham.

The Very Reverend the Dean and Chapter of Durham.

The Venerable Archdeacon Thorpe, the Warden of Durham University.

Wentworth B. Beaumont, Esq., M.P.

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OFFICERS, 1859-60.

President

NICHOLAS WOOD, Hetton Hall, Fence Houses.

Vice-Presidents

WILLIAM ANDERSON, Cleadon Cottage, South Shields.

THOMAS JOHN TAYLOR, Earsdon, Newcastle.

R. STEPHENSON, M.P., 24, Great George St., Westminster, S.W.

EDWARD POTTER, Cramlington, Newcastle.

Council

WILLIAM ARMSTRONG, Jun., Wingate Grange, Ferry Hill.

WILLIAM BARKUS, Low Fell, Gateshead.

CUTHBERT BERKLEY, Marley Hill Colliery, Gateshead.

CHARLES CARR, Cramlington, Newcastle.

GEORGE B. FORSTER, Cowpen Lodge, Blyth, Northumberland.

THOMAS G. HURST, Backworth Colliery, Newcastle.

THOMAS W. JOBLING, Point Pleasant, Wallsend, Newcastle.

JOHN R. LIDDELL, Netherton Colliery, Morpeth.

MATTHEW LIDDELL, Benton Grange, Newcastle.

RICHARD MATTHEWS, South Hetton, Fence Houses.

JOHN MARLEY, Mining Offices, Darlington.

P. S. REID, Pelton Colliery, Chester-le-Street, Fence Houses.

Treasurer

EDWARD F. BOYD, Moor House, Durham.

Secretary

THOMAS DOUBLEDAY, Newcastle.

Honorary Members

John Alexander, Esq., Mining Inspector, Glasgow.

John J. Atkinson, Esq., Mining Inspector, Bowburn, Durham.

Lionel Brough, Esq., Mining Inspector.

Joseph Dickinson, Esq., Mining Inspector, Manchester, Lancashire.

Matthias Dunn, Esq., Mining Inspector, 5, St. Thomas' Place, Newcastle-on-Tyne.

Thomas Evans, Esq., Mining Inspector, South Wales.

Goldsworthy Gurney, Esq., Bude Castle, Cornwall.

John Hedley, Esq., Mining Inspector, Derby, Derbyshire.

Peter Higson, Esq., Mining Inspector, Ridgefield, Lancashire.

H. G. Longridge, Esq., Mining Inspector, Elm House, Erdington, Birmingham.

Charles Morton, Esq., Mining Inspector, Wakefield, Yorkshire.

Edward Shipperdson, Esq., South Bailey, Durham.

Robert Williams, Esq., Mining Inspector, 37, Queen's Street, Edinburgh.

Thomas Wynne, Esq., Mining Inspector, Longton, North Staffordshire.

De Von Decken, Berghauphnan, Bonn, Prussia.

Mons. de Boureille, Paris, France.

Geheimerbergrath Von Carnell, Berlin.

Baron Von Humboldt, Potsdam, Prussia.

Mons. Gonot, Mons, Belgium.

Mons. de Vaux, Inspector-General of Mines, Brussels.

List of Members.

1 Adams, W., Ebbw Vale Works, Newport, Monmouthshire.

2 Anderson, W., Cleadon Cottage, South Shields, County of Durham.

3 Anderson, C. W., St. Hilda's Colliery, South Shields, County of Durham.

4 Arkless, B., Tantoby, Gateshead, County of Durham.

5 Arkley, G. W., Harton Colliery, South Shields, County of Durham.

6 Armstrong, Jun., W., Wingate Grange, Ferry Hill, County of Durham.

7 Arthur, John Selby, West Hetton Colliery Office, Ferry Hill.

8 Ashworth, Thomas, care of J. Hadwen, Esq., The Towers, Poynton Colliery, Cheshire.

9 Atkinson, J., Coleford, Gloucestershire.

10 Attwood, Charles, Towlaw, Darlington, County of Durham.

11 Bailes, Jun., Thos., Thistleflat Colliery, Crook, Darlington.

12 Bailey, W. W., Kilburn, near Derby, Derbyshire.

13 Bailey, Samuel, Ward Collieries, Walsal, Staffordshire; or Wood Green Houses, Wednesbury.

14 Barkus, W., Low Fell, Gateshead, County of Durham.

15 Barkus, Jun., Wm., Broom Hill, Warkworth.

16 Barras, T., Great Burdon, Darlington, County of Durham.

17 Bartholomew, C, Rotherham, Yorkshire.

18 Bassett, A., Tredegar Mineral Estate Office, Cardiff, Glamorganshire.

19 Beacher, E., Thorncliffe and Chapeltown Collieries, Sheffield, Yorkshire.

20 Bell, John, Normanby Mines, Middlesbro'-on-Tees.

21 Bell, I. L., Washington, Gateshead, County of Durham.

22 Bell, T., Shincliffe Colliery, Ferry Hill, County of Durham.

23 Berkley, C., Marley Hill Colliery, Gateshead, County of Durham.

24 Bewick, J., Grosmont, Whitby, Yorkshire.

25 Bigland, J. Bowdon Close Colliery, Bishop Auckland, County of Durham.

26 Binns, C., Claycross, Derbyshire.

27 Bolckow, H. W. F., Middlesbro'-on-Tees, Yorkshire.

28 Bourne, P., Whitehaven, Cumberland.

29 Bourne, S., Shelton Colliery and Iron Works, Stoke-on-Trent, Staffordshire.

30 Boyd, Edward F., Moorhouse, Durham.

31 Braithwaite, Thos., Eglinton Iron Works, Kilwinning, Ayrshire.

32 Brown, J., Bank Top, Darlington, County of Durham.

33 Brown, J., Barnsley, Yorkshire.

34 Brown, J., Whitwell Colliery, County of Durham.

35 Burn, D., Busy Cottage Iron Works, Newcastle-on-Tyne.

36 Byram, B. Wentworth, Rotherham, Yorkshire.

37 Cadwallader, R., Ruabon Colliery, Wrexham, Denbighshire.

38 Carr, Chas., Cramlington, Newcastle-on-Tyne, Northumberland.

39 Carr, Wm. Cochrane, Blaydon, Newcastle.

40 Chapman, G., Bridge Street, Crook, Darlington, County of Durham.

41 Charlton, G., Little Town Colliery, County of Durham.

42 Clark, W. S., Aberdare, Glamorganshire.

43 Cochrane, A. B., 31, West Parade, Newcastle.

44 Cochrane, C, Ormsby Iron Works, Middlesbro'-on-Tees.

45 Cockburn, Wm., Hutton Mines, Guisbro', Yorkshire.

46 Coke, Richard George, Ankerboold, Chesterfield, Derbyshire.

47 Cole, W. R., Bebside Colliery, Morpeth, Northumberland.

48 Cooper, Phillip, Grange, Durham.

49 Cope, J., Kingswinford, Dudley, Worcestershire.

50 Cossham, R., Shortwood Lodge, Bristol, Somersetshire.

51 Coulson, W., Crossgate Foundry, Durham

52 Cowen, Jos., Blaydon Burn, Newcastle.

53 Coxon, F., Shelton Colliery Iron Works, Stoke, Staffordshire

54 Coxon S. B., Usworth Colliery, Gateshead.

55 Crawford, T., South Road, Durham.

56 Crawford, Jun., T., Little Town Colliery, Durham.

57 Crawhall, G. E., 24, North Bailey, Durham

58 Creswick, Theophilus, Merthyr Tydfil, South Wales

59 Crone, S. C., Killingworth Colliery, Newcastle.

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60 Croudace, J., Washington, Colliery, Gateshead.

61 Curry, Thomas, Cassop Colliery, Ferry Hill.

62 Daglish, J., Seaton Colliery, Fence Houses.

63 Davidson, A., Hastings House, Seaton Delaval, Newcastle.

64 Day, J. W., Pelaw House, Chester-le-Street, Fence Houses.

65 Deane, John, Tranent, North Britain.

66 Dees, J., Whitehaven, Cumberland.

67 Dixon, Geo., Whitehaven, Cumberland.

68 Dobson, S., Cardiff, South Wales.

69 Dodd, Benj. Blaenavon, Newport, Monmouthshire.

70 Douglas, T., Pease's West Collieries, Darlington.

71 Dumolo, J., Danton House, Coleshill, Warwickshire.

72 Dunn, T., Richmond Hill, Sheffield, Yorkshire.

73 Easton, J., Hebburn Colliery, Gateshead.

74 Elliot, G., Houghton-le-Spring, Fence Houses.

75 Elliott, W., Etherley Colliery, Darlington.

76 Embleton, T. W., Middleton Hall, Leeds, Yorkshire.

77 Errington, C.E., Westminster, London, S.W.

78 Fletcher, Jos., C.E., Dawson Place, Whitehaven, Cumberland.

79 Foord, J. B., General Mining Association Secretary, 52, Broad Street, London.

80 Forster, J. H., Old Elvet, Durham.

81 Forster, G. B., Cowpen Lodge, Blyth.

82 Fryar, Mark, Eighton Moor Colliery, Gateshead.

83 Gilroy, G., Orrell Colliery, Wigan, Lancashire.

84 Gooch, G., Lintz Colliery, Gateshead.

85 Green, G., Rainton Colliery, Fence Houses.

86 Greene, Jun., Wm., Framwellgate Colliery, Durham.

87 Greener, W., Pendleton, Wigan, Lancashire.

88 Greenwell, G. C., Radstock Colliery, Bath, Somersetshire.

89 Haggie, P., West Street, Gateshead.

90 Hall, T. Y., Eldon Square, Newcastle.

91 Hall, William, Page Bank Colliery, Crook, Darlington.

92 Hall, Thomas, West Hetton Colliery, Ferry Hill.

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93 Hann W., Hetton, Fence Houses.

94 Hardy, Benj., Woodhouse Close Colliery, Durham.

95 Harris, C.E., Jno., Woodside, Darlington.

96 Harrison, C.E., T. E., Central Station, Newcastle.

97 Hawthorn, R., Engineer, Newcastle.

98 Hawthorn, W., Engineer, Newcastle.

99 Heckels, R., Bunker's Hill, Fence Houses.

100 Hedley, Edward, Dodsworth Road, Barnsley, Yorkshire.

101 Hodgson, R., Engineer, Whitburn, Monkwearmouth, Sunderland.

102 Horsley, Jun., W., Seaton Cottage, Hartley, Newcastle.

103 Hunter, Wm., Spital Tongues, Newcastle.

104 Hurst, T. G., Backworth Colliery, Newcastle.

105 James, Christopher, Llwynellyn Colliery, Merthyr Tydvil, Glamorganshire.

106 Jeffcock, P., 3, Stuart Terrace, Green Hill, Derby, Derbyshire.

107 Jobling, T. W., Point Pleasant, Wallsend, Newcastle.

108 Johnson, G., Talk'oth Hill, Lawton, Cheshire.

109 Johnson, J., Willington, Newcastle.

110 Johnson, R. S., West Hetton, Ferry Hill.

111 Joicey, James, Quay, Newcastle.

112 Joicey, John, Tanfield Lea, Gateshead.

113 Jones, E., Lilleshall Iron Works, Sheffnall, Salop.

114 Jones, Alexander, Mine Agent, Prior's Lea, near Sheffnal, Shropshire.

115 Kerr, John, Hamilton's Gas Coal Works, Lismahagow, North Britain.

116 Kimpster, W., Quay, Newcastle.

117 Knowles, A., Crescent, Salford, Manchester, Lancashire.

118 Knowles, J., Pendlebury, Manchester, Lancashire.

119 Landale Andrew, Lochgelly, Fifeshire, North Britain.

120 Laws, J., Blyth, Northumberland.

121 Ledward, William J., South Bank Iron Works, Eston Junction, Middlesbro'-on-Tees

122 Levick, Jun., F., Cwm Celyn, Blaina and Colebrook Dale Iron Works, Newport, Monmouthshire

123 Lewes, T. Wm., Plymouth Iron Works, Merthyr Tydvil, Glamorganshire

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124 Liddell, J. R., Netherton Colliery, Morpeth.

125 Liddell, M., Benton Grange, Newcastle-on-Tyne.

126 Lishman, Wm., Etherley Colliery, Darlington.

127 Llewellin, Wm., Glanwern, Pontypool, Glamorganshire.

128 Locke, C., Rothwell Haigh, Wakefield, Yorkshire.

129 Locke, M.P., Jos., Westminster, London, S.W.

130 Longridge, J., 18, Abingdon Street, Westminster, London, S.W.

131 Love, Joseph, Brancepeth Colliery, Durham.

132 Low, Wm., Vron Colliery, Wrexham, Denbighshire.

133 Marley, John, Mining Offices, Darlington.

134 Marshall, Robt., Lugar Iron Works, Ayrshire, North Britain.

135 Matthews, Richd., South Hetton Colliery, Fence Houses.

136 Mc'Ghie, Thos., British Iron Works, Ruabon, Denbighshire.

137 Mercer, J., St. Helens, Lancashire.

138 Middleton, J., Davison's Hartley Office, Quay, Newcastle-on-Tyne.

139 Morris, Wm., Waldridge Colliery, Chester-le-Street, Fence Houses.

140 Morton, H., Lambton, Fence Houses.

141 Morton, H. J., 2, Basinghall, Leeds, Yorkshire.

142 Morton, H. T., Lambton, Fence Houses.

143 Mulcaster, H., Blackley Hurst Colliery, St. Helen's, Lancashire.

144 Mulvany, Wm. Thos., 1335, Carls Thor, Dusseldorf on the Rhine, Prussia.

145 Mundle, W., Ryton, Gateshead.

146 Murray, T., Chester-le-Street, Fence Houses.

147 Palmer, A. S., Malago Vale Colliery, Bedminster, Bristol.

148 Palmer, C. M., Quay, Newcastle-on-Tyne.

149 Palmer, J. B., Jarrow, South Shields.

150 Paton, Wm., Alloa Colliery, Alloa, North Britain.

151 Peace, Wm., Hague Cottage, Wigan, Lancashire.

152 Pearce, F. H., Bowling Iron Works, Bradford, Yorkshire.

153 Pease, J. Wm., Woodlands, Darlington.

154 Pilkington, Wm., St. Helens, Lancashire.

155 Plews, H. T., Bedale, Yorkshire.

156 Plummer, B., 5, Elswick Villas, Newcastle-on-Tyne.

157 Plummer, Jun., R., Quay, Newcastle-on-Tyne.

158 Potter, E., Cramlington, Newcastle-on-Tyne.

159 Potter, W. A., Mount Osborne Collieries, Barnsley, Yorkshire.

160 Powell, T., Newport, Monmouthshire.

161 Ramsay J., Walbottle Colliery, Newcastle-on-Tyne.

162 Ravenshaw, J. H., Seaham Harbour, Fence Houses

163 Reed R G., Newton Colliery, Felton, Northumberland.

164 Rees, Robert T., Lothy, Shenkin Colliery, Aberdare, Glamorganshire.

165 Reid P S., Pelton Colliery, Chester-le-Street, Fence Houses.

166 Richardson, Dr., Portland Place, Newcastle-on-Tyne.

167 Robinson, R., Stanley Colliery, Pease's West, Darlington.

168 Robson, J. G., Old Park Hall, Ferry Hill.

169 Robson, G., Tondu Iron Works, Bridge-end, Glamorganshire.

170 Robson, M. B., Field House, Borough Road, Sunderland.

171 Rockwell, Alfd. P., M.A., Norwich, Connecticut, United States, America.

172 Rogers, E., Abercarne Colliery, Newport, Monmouthshire.

173 Ross, A., Shipcote Colliery, Gateshead.

174 Rosser, Wm., Mineral Surveyor, Llanelly, Carmarthenshire, Wales.

175 Routledge, Jun., Wm., Shincliffe Colliery, Durham.

176 Rutherford, J., South Tyne Colliery, Haltwhistle, Northumberland.

177 Sanderson, Jun., R. B., West Jesmond, Newcastle-on-Tyne.

178 Sawyers, W. G., Whitehaven, Cumberland.

179 Shortreed, T., Newbottle Colliery, Fence Houses.

180 Shore, Isaac, Mineral Surveyor, Brymbo, near Wrexham, Denbighshire.

181 Simpson, L., Medomsley Colliery, Durham.

182 Simpson, R., 7, Quay, Newcastle-on-Tyne.

183 Sinclair, E., 8, Claremont Street, Edinburgh

184 Smith, C. F. S., Cinder Hill, near Nottingham.

185 Smith, F., Bridgewater Canal Office, Manchester, Lancashire.

186 Smith, Jun., J., Monkwearmouth Colliery, Sunderland,

187 Smith, Edmund Jas., 14, Whitehall Place, Westminster, London, S.W.

188 Sopwith, T., Allenhead, Haydon Bridge, Northumberland.

189 Southern, G. W., Springwell Colliery, Gateshead

190 Southern, J. M., Whickham Grange, Gateshead.

191 Spark, H. K., Darlington, County of Durham

192 Spencer, Jun., W., Corporation Road, Middlesbro'-on-Tees.

193 Steavenson, A. L., Woodifield Colliery, Crook, Darlington

194 Stenson, Jun., W., Whitwick Colliery, Ashby-de-la-Zouch, Leicestershire

195 Stephenson, M.P., Robt., 24, Great George Street, Westminster, London, S.W.

196 Stobart, H. S., Etherley, Darlington.

197 Stobart, Wm., Roker, Monkwearmouth, Sunderland.

198 Storey, T., St. Helen's Auckland, Bishop Auckland.

199 Stott, G., Ferry Hill, County of Durham.

200 Taylor, H., Earsdon, Newcastle-on-Tyne.

201 Taylor, M.P., H., Backworth Hall, Newcastle-on-Tyne.

202 Taylor, J., Haswell Colliery, Durham.

203 Taylor, T. J., Earsdon, Newcastle-on-Tyne.

204 Telford, W., Cramlington, Newcastle-on-Tyne.

205 Thomas, H. W., Pinchinthorpe, Northallerton, Yorkshire.

206 Thomas, J. T., Coleford, Gloucestershire.

207 Thompson, John, Marley Hill Colliery, Gateshead.

208 Thompson, T. C., Kirkhouse, Brampton, Cumberland.

209 Thorpe, R. C., Cockerham House, Barnsley, Yorkshire.

210 Tone, C.E, John F., Market Street, Newcastle-on-Tyne.

211 Trotter, J., Newnham, Gloucestershire.

212 Vaughan, J., Middlesbro'-on-Tees.

213 Vaughan, Thos., Middlesbro'-on-Tees.

214 Vaughan, William, Middlesbro'-on-Tees.

215 Wales, T. E., Abersychan Iron Works, Pontypool, Monmouthshire.

216 Walker, J. Lakelock, Wakefield, Yorkshire.

217 Walker, Jun., T., High Street, Maryport, Cumberland.

218 Ware, W. H., The Ashes, Stanhope, Weardale.

219 Watson, W., High Bridge, Newcastle-on-Tyne.

220 Watson, Joseph J. W., Ph. D. &c, The Knap, Charlton Kings, Cheltenham, Gloucestershire.

221 Webster, R. C., Hoyland Hall, Barnsley, Yorkshire.

222 Willis, Jas., West Auckland Colliery, Bishop Auckland.

223 Wilmer, F., Pensher Colliery, Fence Houses.

224 Wilson, J. B., Haydock Rope Works, Warrington, Lancashire.

225 Wilson, R., Flimby Colliery, Maryport, Cumberland.

226 Wilson, John Straker, West Cramlington, Newcastle.

227 Wood, C. L., Black Boy Colliery, Bishop Auckland.

228 Wood, Lindsay, Hetton Colliery, Fence Houses.

229 Wood, N., Hetton Hall, Fence Houses, County of Durham.

230 Wood, W. H., Coxhoe Hall, Ferry Hill.

231 Woodhouse, J. T., Midland Road, Derby, Derbyshire.

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Rules

1. - That the Members of this Society shall consist of Ordinary Members, Life Members, and Honorary Members.

2. - That the Annual Subscription of each Ordinary Member shall be £2 2s. 0d., payable in advance, and that the same shall be considered as due and payable on the first Saturday of August in each year.

3. - That all persons who shall at one time make a Donation of £20 or upwards, shall be Life Members.

4. - Honorary Members shall be persons who shall have distinguished themselves by their Literary or Scientific attainments, or made important communications to the Society.

5. - That a General Meeting of the Society shall be held on the first Thursday of every Month, at twelve o'clock noon, and the General Meeting in the month of August shall be the Annual Meeting, at which a report of the proceedings, and an abstract of the accounts of the previous year shall be presented by the Council. A special Meeting of the Society may be called whenever the Council shall think fit, and also on a requisition to the Council signed by ten or more Members.

6. - No alteration shall be made in any of the Laws, Rules, or Regulations of the Society, except at the Annual General Meeting, or at a Special Meeting; and the particulars of every alteration to be then proposed shall be announced at a previous General Meeting, and inserted in its minutes, and shall be exhibited in the Society's meeting-room fourteen days previously to such General Annual or Special Meeting.

7. - Every question which shall come before any Meeting of the Society shall be decided by the votes of the majority of the Ordinary and Life members then present and voting.

8. - Persons desirous of being admitted into the Society as Ordinary

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or Life Members, shall be proposed by three Ordinary or Life Members, or both, at a General Meeting. The proposition shall be in writing, and signed by the proposers, and shall state the name and residence of the individual proposed, whose election shall be ballotted for at the next following General Meeting, and during the interval notice of the proposition shall be exhibited in the Society's-room. Every person proposed as an Honorary Member must be recommended by at least five Members of the Society, and elected by ballot at the General Meeting next succeeding. A majority of votes shall determine every election.

9. - The Officers of the Society shall consist of a President, four Vice-Presidents, and twelve Members who shall constitute a Council for the direction and management of the affairs of the Society; and of a Treasurer and a Secretary; all of whom shall be elected at the Annual Meeting, and shall be re-eligible, with the exception of Three Councillors whose attendances have been fewest. Lists containing the names of all the persons eligible having been sent by the Secretary to the respective

Members, at least a month previously to the Annual Meeting; - the election shall take place by written lists, to be delivered by each voter in person to the Chairman, who shall appoint scrutineers of the lists ; and the scrutiny shall commence on the conclusion of the other business of the meeting. At meetings of the Council, five shall be a quorum, and the record of the Council's proceedings shall be at all times open to the inspection of the members of the Society.

10. - The Funds of the Society shall be deposited in the hands of the Treasurer, and shall be disbursed by him according to the direction of the Council.

11. - The Council shall have power to decide on the propriety of communicating to the Society any papers which may be received, and they shall be at liberty, when they think it desirable to do so, to direct that any paper read before the Society shall be printed. Intimation shall be given at the close of each General Meeting on the subject of the paper or papers to be read, and of the questions for discussion at the next meeting, and notice thereof shall be affixed in the Society's-room ten days previously. The reading of papers shall not be delayed beyond 3 o'clock, and if the election of members or other business should not be sooner despatched, the President may adjourn such business until after the discussion of the subject for the day.

12. - That the Copyright of all papers communicated to and

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accepted by the Institute, becomes vested in the Institute; and that such communications shall not be published for sale, or otherwise, without the permission of the Council.

13. - That the transmission of the Proceedings be withheld from members more than two years in arrear of their annual subscriptions.

* It was resolved at the Annual Meeting, 6th August, 1857, that an alteration of Rule 5 should be tried for the twelve months next ensuing; during which there shall be General Meetings only on the first Thursday, respectively, of the months of October, December, February, April, June, and August; an arrangement which was left unaltered by the Annual Meeting in August, 1858.

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ERRATA.

Page 19, 9th line from bottom, *for* "could pass" *read* would pass. „

" 39, 12th line from top, *for* "we confine" *read* we do not confine.

„ 41, 9th line from bottom, *for* "creeks" *read* creeps.

„ 53, 7th line from bottom, *for* "V" *read* Y.

„ 80, 7th line from top, *for* "minute" *read* second.

„ 80, 10th line from top, *for* "colliery, the whole of the" *read* colliery in the whole the.

„ 80, 14th line from top, *for* "that gas was" *read* that much gas was.

„ 100, 18th line from top, *for*, "560 yards in length and from 30 to 32 feet thick" *read* 560 yards in length, 400 yards in breadth, and from 30 to 32 feet thick,

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TO BINDER.

ERRATA IN PART VIII. CANCELS THE ERRATA SLIPS IN THE PREVIOUS Parts

DIRECTIONS FOR PLACING MAPS, PLANS, etc. - VOL. VII.

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NORTH OF ENGLAND INSTITUTE of MINING ENGINEERS.

General meeting, Thursday, October 7, 1858, in the rooms of the Institute, Westgate Street, Newcastle-upon-Tyne.

Nicholas Wood, Esq., President of the Institute, in the Chair.

The Secretary read the minutes of the Council meeting held on the 2nd October.

Mr. Thomas Bailes, jun., Thistleflat Colliery, Darlington, and Mr. Edmund James Smith, Whitehall Place, London, were elected members, and four other gentlemen were nominated for election at next meeting.

The President said - At the anniversary meeting, attention was directed to the expense of the publications, and there was a proposition made that the Council should take tenders for the printing of the works. There is, however, some difficulty in making out a specification so clear that printers might make a specific proposal. The subject has been under the consideration of the Council in the interval between the anniversary meeting and the present time. You will see that Mr. Reid attended the Council on Saturday, and gave explanations, when it appeared that the great expense was in the colouring of the plans, as, when plates are coloured, they are much more expensive than ordinary plates. The Council seemed to think it was very desirable that the style of the printing and of the works generally should be continued without any alteration. But, then the difficulty was in ascertaining whether Mr. Reid had made a fair charge or not; and, in order to ascertain this, the Council proposes that the opinion of some competent printer and publisher in London or Edinburgh should be taken for their guide. They go further, and say, that if Mr. Reid's charges, as tested by that information, are fair and reasonable, they will make no change,

inasmuch as the manner in which Mr. Reid has executed the work has been very satisfactory. What is, therefore, required at present, is your opinion as to the policy of taking the opinion of some competent person to guide the Council.

After some remarks by Mr. Barkus and Mr. Atkinson, it was resolved to take the opinion of some London publisher as to the charges made by Mr. Reid during the past year or two, on the publications of the Institute.

The President - The only two subjects before us at this meeting are the paper by Mr. Wales on Ventilation, with diagrams, &c.; and the discussion upon Mr. Hall's paper of February last, on the Production and Consumption of Coal in France.

The meeting decided to take Mr. Hall's paper first.

The President - As Mr. Hall is present, I will ask him if he has any further explanation to give?

Mr. Hall - No; I believe the paper will be found correct as far as it goes. Great pains were taken to make it correct from French documents. If they are correct, my paper is correct.

Mr. Dunn - It is very important, in presenting a paper of this description, that we should know that the quotations are drawn from authority. Mr. Hall does not give a single authority he quotes from. It is desirable to explain where the statistics are taken from - whether from government books, or published records by certain individuals; then we shall know how far they are entitled to credit. Is it competent for any one to present books of this sort to the society, consisting entirely of quotations from other works? I do not know that Mr. Hall could answer for these quotations of his own knowledge. He has not been much at the coal mines of France.

Mr. Hall - If Mr. Dunn wants any explanation I will endeavour to give it. Point out any one thing you think is in error. There is an excellent index on the French map you can refer to, the figures and letters of which correspond to those in the book. But, having to go through so many various books, errors may occur, and if one can be found let it be pointed out.

Mr. Dunn - I submit there is no authority given. Mr. Hall does not state that they are from documents published by the Prussian, Belgian, or French Governments.

Mr. Berkley - Mr. Hall says they are from official returns. We may assume they are printed by order of the respective governments?

Mr. Hall—Certainly, a portion of them were printed; but there is

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another portion for the last three years, these were not printed, at the time by the French authorities, but I have got them from a source on which I can rely. They will come out in print this very month, which will certify their correctness, and which will bring the information up to the end of the last year.

Mr. Dunn - Is it from the *Annales des Mines*?

Mr. Hall - No, France, from the *Résumé des Travaux Statistiques de l'Administration des Mines*, and other books, printed by the foreign governments, as already stated. The rest are from a party that collects them together, French agents. I took the trouble to collect them from the French

Government records, as before stated, and I believe they will turn out correct. I could easily write out a list of the different books I have referred to. There are also several remarks of my own.

The President - I think what is wanted is an explanation of the authorities from which the documents are derived. At the second paragraph Mr. Hall states, "I have taken care throughout these papers, as well in those which precede as in the present one, that all calculations should be based on official returns, wherever they were obtainable; and, in cases where I could not obtain the statistics from the coal-owners' returns themselves, or documents issued under the authority of the government, I have furnished liberal estimates, founded upon specified, and, I believe, accurate data." Mr. Hall says, "coal-owners' returns, published by authority of government." This is the conclusion I should come to on reading that paragraph. Take, first of all, "foreign statistics." Those, you say, are furnished from coal-owners, and published under the authority of government?

Mr. Hall - Yes, in France, as far as they were printed up to 1852. After that I had to get them from the Secretary of the Departments, to make my paper more complete. They were not then in print; they only come out once in three or four years, they will be printed this year.

The President - Up to 1852 there are published returns. In what shape were they published?

Mr. Hall - The French, the same as our government blue books. I got the Belgian returns from the like source.

The President - They were published by the government and accessible or purchasable by anyone?

Mr. Hall - Yes, the book containing the figures for the three years up to 1852 I got from the Secretary.

The President - This refers to foreign statistics. Then, there is a great deal in reference to England?

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Mr. Hall—I have referred to those documents which I have published from time to time. I have continued this as undisputed.

The President - From what source are the calculations derived? From the returns of Mr. Hunt, or from whom?

Mr. Hall - Partly from the Coal Trade Office, in Newcastle, and more particularly from papers which I read to this Institute in May, 1854, and December, 1855; the former on the Great Northern, and the latter on the Austrian, coal-field. The quantity of coals raised in Great Britain in 1850, or 1851, (I believe before Mr. Hunt's returns for coal were issued), was estimated at forty-three millions of tons annually for Great Britain; but, when I went into the matter, I found it upwards of fifty-three millions, or thirteen millions more; and Mr. Hunt, I find, in 1854, made it out upwards of sixty-three millions of tons annually. Since this period I have taken Mr. Hunt's book of Records for statistics relating to the production of Great Britain.

The President - It would be very satisfactory for the members to know from what source the statistics are procured, that they might either check them or satisfy themselves of their accuracy. If the statistics are accessible to the members generally, it is for the members themselves to ascertain whether they are correct or not. All we want, according to Mr. Dunn, is, if you can state from what publications they are obtained.

Mr. Hall - It would be very easy to do that. I think I have now done so.

The President - The remarks on the statistics are, of course, your own; but the statistics themselves, you say, are derived from the Coal Trade Office, or from Mr. Hunt's publications?

Mr. Hall - From my own research before Mr. Hunt's returns had been published; from the Coal Trade Office here; from other parties in the southern counties, where no general coal trade office is kept; and from France, Belgium, Austria, and Prussia. I believe they are correct. I find, subsequently, that Prussia, for instance, is increasing a little more rapidly in her production of coal and iron than I stated. I published a document on the Northern Coal Trade, and another on Austria, in 1856. These were undisputed by the members. I take it for granted they are as correct as anything yet published.

The President— The members do not give any opinion of any of your deductions from the statistics. The statistics themselves we may, I think, assume to be correct.

Mr. Dunn—I have several Belgian reports. Mr. Hall has stated

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generally to the Institute from whence he got the information. That is the answer to my question, and it enables parties to test the accuracy of the documents if they think proper.

The President - There is one foot note in Mr. Hall's Paper, which, passing through the Society, requires to he investigated a little. I mention this more particularly, having seen in the *Mining Journal*, last week, a statement that Mr. Hall had put forth certain facts with reference to the improvements introduced by him into the Great Northern Coal-field - that these had been read before the Institute, and, not having been contradicted by any one, the public must assume they were correct - I wish to direct the attention of the members to one or two statements, so that their accuracy may be investigated. These occur in a note at the bottom of page 60. Mr. Hall states: - "I allude here to the improvements introduced by me into the Great Northern Coal-field about this period. They consisted of the adoption of tubs, with carriages attached, in lieu of corves, and angle iron tram plates, for drawing coals from the extremities of the workings to the bottom of the shaft, and slides or guide rods for conducting them thence to the surface." Then he goes on to say - "The average saving per ton in England by the general adoption of these improvements, if taken at only 1s. 3d. per ton, 2s. 0¾ d. less than the sum given above, amounts to upwards of four million pounds per year upon the total produce, or upwards of one million sterling upon the present annual yield of the Northumberland and Durham coal-fields." This is a very important statement, and we could not very well pass it over without notice. The improvements stated by Mr. Hall as having been introduced by him are, first: - "the adoption of tubs with carriages attached", - second, "angle iron tram plates" - and third, "slides or guide rods." So far as my experience goes - and I should like to be corrected by gentlemen present, some of whom have been longer in the trade than I have been - I think the adoption of the tubs and slides had been in use in the southern coal-fields many years previously to their being introduced into this district [Mr. Dunn - Yes.] I have seen them in my visits to different collieries in the midland districts. I think Mr. Hall introduced them to South Hetton colliery in the first instance, which was, I believe, their first introduction into this district. Other gentlemen will be so good as mention their experience. Then, with reference to the tram plates, I think the introduction of them into the collieries in this district was prior to the date of Mr. Hall's birth.

Mr. Dunn - Mr. Curr used them.

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The President - Then the third is connected with the first. The introduction of tubs involved the introduction of guide rods. I think Mr. Hall had the credit of introducing them into this district. I am quite sure he had not the credit of introducing them into the southern district.

Mr. Hall - I quite agree with Mr. Wood, that things of this kind were in use by Mr. Curr, who went from Pontopike. He had those sorts of things, which were called skips, working in the shaft, it may be with slides, and so had others in the southern district. They were in practice in the neighbourhood of Sheffield. When I introduced the tubs I was not aware of this fact. But I was informed afterwards by the eminent men who went from this neighbourhood (and who took the Rev. Mr. Hodgson, of Heworth, with them), viz., Mr. Watson, Mr. Buddle, and others. They went to Sheffield, and on their return - as I have seen by the Rev. Mr. Hodgson's memorandum book - they reported that the south country system of drawing coals would never answer in this Northern Coal-field, where pits were so deep and coal seams were so thin, and where a great quantity of work was required to be drawn up one shaft. Consequently, they were never put in practice in this Northern Coal-field till I introduced them at South Hetton, in 1833. In the southern district, on Curr's principle, the work was done by such a slow process, and so small a quantity of coals was drawn to bank at one pit at that time, that it was doubtful whether they would ever come into practice in this neighbourhood. In respect to the introduction of bridge or edge rails, with carriages and bogie wheels, Mr. Easton attempted this at Hebburn Colliery in 1824. He did not succeed, because the corves were not fixed upon the carriages as tubs are now; whereas, when I commenced at South Hetton Colliery, I had carriages with bogie wheels, and tubs attached to them, suitable for bridge rails, so as to do away the tram plate. I at the same time introduced cages and guides into the shafts so as to bring the same to bank. For a year or so before this, I had the same carriages with bogie wheels on bridge or edge rails underground, emptying into large tubs near the shaft bottom. These large tubs were conveyed up the shaft by wooden slides, so as to prevent vibration of more than an inch in any direction, each carrying the coals of four of the smaller tubs, equal to about 1½ tons. After these had worked about twelve months, to my satisfaction, Messrs. Nicholas Wood and Johnson were called in by the owners to report upon the same. In consequence of their doubts whether the large

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quantity of coals going over the screen might not cause breakage, the large tubs were subsequently abandoned, but my new plan, called the tub and cage system, was tried, and in another pit, (the large tubs being continued in the shaft where they were originally placed, in the meantime) and the new plan is now in use at both pits. As far as regards the tram plate, I meant to say, that I succeeded in getting the use of it abolished both at South Hetton and in the colliery I was connected with after I left South Hetton. I also took the carriages, which travelled on bridge or edge rails, into the face of the workings, and then, on their return loaded to the bottom of the pit, had them taken out to the top of the shaft. This was another great advantage that did not previously exist. Before this, there was a rolley or horse way with corves on great bogies and horses—the horse way being bridge rail—at the termination of which there was a crane to lift the corves off on to a tram carriage, to go the working part of the mine, and return upon a tram plate way to where the horses took them from. This conjoint system was a very unwieldy method, which I afterwards entirely did away with, and the uniform plan now adopted aids steam machinery with wire ropes, which, compared with the old system of horses, saves a great deal of trouble and expense.

Mr. Dunn - Mr. Easton was the first person that used edge railing. That was about the year 1815.

Mr. Barkus - No, it was after that, about 1824.

Mr. Hall - I have no priority of claim in introducing either the common tram plate or bridge rail into our coal mines, as the foot-note in my paper at page 60, from an error of the press, might lead some to suppose; but I do claim the credit of substituting the bridge for the tram plate, whereby I did away with the expensive method of lifting the corves from the tram to the bogie by the crane, and of introducing a more economical and uniform system of tub carriage and bridge rail throughout the mine.

The President - Your meaning is, that you substituted tubs and slides in lieu of tubs and tram plates. I have only to say, in reference to the report alluded to by Mr. Hall, that Mr. Johnson and I were called on by the Owners of South Hetton to report whether it was proper to continue the large tubs or not. We reported against them. At this time the coals were brought out from the face of the workings and emptied out of the small tubs into those large tubs, in which they were brought to bank, and were then emptied out of the large tubs into the

screen. It was considered that the emptying of the small tubs into the large tubs, and then again from the tubs in large masses upon the screens at once, was prejudicial. It broke the coal, and we considered, likewise, that it was not well screened.

Mr. Hall - It is quite true they were emptied in large masses, but it was done by machinery, so that they might be emptied as gently as they pleased, consequently, they appeared to be as well screened as before. Nevertheless, as Messrs. Wood and Johnson reported that such might be an objection, I immediately set to work and adapted to the other pit a new principle. I gave the owners a report of the same, and the chance of seeing both plans at work in each of the pits at one time, and both were at work when I left the colliery. In place of the last-mentioned improvement in tubs and cages, the large tubs were introduced again by my successor into one pit. Afterwards, when I went to Towneley Main colliery, where I was part owner, I again introduced the other previously named improvement - the system of tubs and cages - and put as many coals per year out of that one shaft, seventy fathoms deep, with a twenty-horse engine, as the former owners, Messrs. Dunn, got out of two, and sometimes three pits per year, with the old corf system, with three engines. The like may be said at South Hetton, 1100 feet deep, as to extra quantities.

Mr. Dunn - The large tub has been in use in Belgium from time immemorial.

Mr. Hall - If so, it has been altered; for I have found, both in Belgium, France, Austria, and Prussia, in 1844, that my new system, with cage and tubs, was, and is now in operation in all those countries, the same as I introduced it at South Hetton and Towneley Main. In my travels I never saw one instance in which the large tub was used, even in America.

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ON THE WORKING AND VENTILATION OF THE COAL MINES IN THE COUNTIES OF NORTHUMBERLAND AND DURHAM

By Mr. JOHN WALES.

It will be in the remembrance of some of the members of the Institute, that I brought before their notice, a short while ago, a paper on the subject of Ventilation - referring more especially to the systems at present in use in the Midland districts; and that, on that occasion, I undertook at some future time to make a few remarks on the same subject, making particular reference to the various modes of ventilation practised in Northumberland and Durham, from the earliest periods up to the present time.

I submit the following observations to your notice, with a view of redeeming my promise - premising that, as before, I shall address myself more particularly to the younger members of the profession, and that I shall confine my remarks mainly to the explanation of the diagrams, which I have prepared on a large scale, for the elucidation of this important subject.

In my former paper, I made no mention of the various means in use for the purpose of producing a current of air in a mine, and I shall, therefore, with your permission, make a few prefatory remarks on this lead before going into the general subject.

You are, of course, all fully aware that there are various means at

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present in use for this purpose, such as the furnace, steam jet, fans, pumping apparatus, &c. I shall, however, restrict my remarks altogether to the subject of the furnace, and its different application under various circumstances, it being found in practice more simple and more effective in its operation than any other source of ventilation hitherto introduced into this district.

There are three points of view from which the application of the furnace may be regarded:—

I. *In collieries where the workings are properly arranged, and the ventilation is efficient*. In this case, under ordinary circumstances, the total quantity of air, after ventilating the various workings of the colliery, and previously uniting in one current, may safely be brought over the furnace.

II. *Where the goaves give off gas to such an extent that, after mixing with the air from the whole mine, it is still considered unsafe to bring it over the furnace*. Here the air from the goaves is carried into the upcast shaft by a dumb drift, without coming in contact with the furnace, and the remaining portion of air passing from the whole mine, where naked lights are used, may also with safety be brought to the furnace.

III. *In mines of very great depth, where the sinking of the shafts involves a great expenditure of capital.* Under such circumstances, the workings are unavoidably carried to a great distance from the shaft - more gas may be expected to be given off, and the same quantity of air cannot with equal facility be obtained as in mines of less extent. The whole of the air, in such a case, might possibly have to pass along the dumb drift without coming into contact with the furnace, and the latter requires to be fed with fresh air from the downcast shaft, in order that the necessary temperature for the proper ventilation of the mine may be attained in the upcast shaft.

Having thus pointed out the various ways in which the furnace may be applied for the purpose of ventilation, we shall next consider the relative advantage when the total quantity of air which passes through the mine is brought over the furnace.

The whole of the air is, of course, rarified in proportion to the heat given off from the furnace; and where only the current of air from the whole mine, or say only half the quantity, as in the former case, passes over the furnace, the ventilating power will be proportionately diminished, the whole of the air not being rarified to such an extent as before.

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[Diagram No. 1]

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In connection with this part of the subject, it is scarcely necessary to remind you that, where none of the air which passes through the mine is brought over the furnace, and the latter is fed with fresh air, a much larger shaft and greater degree of ventilating power will be required to produce the same quantity of air, than in either of the above cases.

Before proceeding to explain the diagrams, I would wish to point out the necessity which exists of our having the air-courses of sufficient sectional area, particularly where the currents of air meet, or where they are of great length. In shorter currents of air, this matter is not of so much importance.

I shall now proceed to explain the diagrams which I have prepared for the illustration of mining ventilation, and let us take, first of all,

Diagram No. I.

Diagram No. I. shows the system of working and ventilating coal mines at a very remote date. At that period, the coal worked lay near the surface, and the pillars of coal left were, therefore, of very small dimensions, being barely sufficient to support the roof. The method of working away the pillars appears to have been neither understood nor practised at that time. Subsequent experience, however, made it evident to those who had the management of mines, that it was not only a great sacrifice on the part of the coalowner, but a serious diminution of our national wealth, to allow the pillars of coal to be entirely wasted or lost that had been left. The first plan of extracting the coal from the remaining pillars appears to have been, to take a portion off each end of the pillar, the other part being allowed to remain, which nevertheless occasioned a large per centage of coal to be left. This mode of workup continued for a great number of years, in proof of which we may refer to the fact that a great number of collieries have been re-opened in this neighbourhood to work the portions of coal which had been left in former years. Gradually the practicability of taking away the whole of the pillars became manifest; and it was plain that, instead of leaving the pillars so small, they would require to be increased in size, to resist the weight of the superincumbent strata while taking the pillars off in the second working. But even after this had been done, I am led to believe that a long time elapsed before anything approaching to a proper system of working away the pillars was attained. I shall treat of this more fully in explaining the other diagrams. The ventilation at this date, as will be seen on referring to the diagram, was very simple.

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The air was conducted to the extreme point, and then along the working face, in one unbroken current, leaving a small portion of air to escape at each stopping (commonly called a scale), which passed into the back pillars. This was the only method, at that period, of ventilating the waste, or old pillars which were left.

Diagram No. II.

The system shown on this diagram does not differ from the former with respect to the mode of working, but it is assumed that the coal has now reached a greater depth, and inflammable gas is consequently given off. It is, therefore, necessary that some other method of ventilating the back pillars should be resorted to different from that indicated on diagram No. I. At this particular period in the history of mining Mr. Spedding, of Whitehaven - a gentleman of some celebrity in the mining profession - invented a plan of clearing all the old pillars of inflammable or noxious gas. This would be a desideratum at that period, as at that time there were no safety-lamps, and the result was attained by coursing or carrying the air up and down in the old workings, as shown by the darts on the diagrams. It will be observed that, according to this mode of ventilation, a great number of doors were required to keep the air in its proper course. In the event of anything occurring to one of these doors, the air would naturally pass on to the next door, in which case the pillars which ought to have been ventilated by this deranged door might probably be filled with gas; and when the door was shut, and the ventilation again restored, the gas would be brought into contact with the miners' candles in the boards, the consequences of which are only too well known. It will also be observed that the current of air requires to travel a great distance up and down in those pillars, and it is then brought up to the working face. The consequence of the air travelling through the old workings to such an extent as this, would be that it might become very impure, and in time prove highly injurious to the health of the miner.

Diagram No. III.

We have now arrived at a period when experience suggested the propriety of working away the old pillars, either wholly or partially, and it was an important consideration that this should be done with safety, for it must be remembered that that invaluable invention, the safety-lamp, had not then been introduced. The great requirement,

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[Diagram No. II]

[Diagram No. III]

[Diagram No. IV]

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therefore, was, that the ventilation should be so arranged that the air might be kept in its proper course. It will be seen, on referring to the diagram that so soon as the bearing-down stoppings marled A are interfered with or rendered useless by working off the pillars of coal in which they are situated respectively, the air would at once pass direct across at the top of the panel - this being the nearest route to the upcast pit - and would, as a natural consequence, leave the wastes without ventilation.

It is evident that at this period there had been no dangerous excess of inflammable gas to contend with, otherwise this mode of working with the naked lights could not have been pursued. It will also be obvious to every one that, by taking away the pillars of coal according to the system above described, the loss of coal, at a reasonable calculation, must have amounted to 30 per cent., which would certainly be a great sacrifice.

Diagram No. IV.

Represents a different system of ventilation, and a different system of working away the pillars of coal.

The ventilation, it will be observed, is now arranged by inserting stoppings in the various boards, so that the air can now be conducted across the panel or district of workings from A to B. After this, it is brought into the waste, and conducted up and down in the courses, until it arrives at C, passing down into the return, having thus ventilated the whole district of waste. By this arrangement of ventilation the whole of the pillars may be removed and entirely worked off.

The method of working the pillars at that time was commonly called "working the broken mine." In the first place, a "jenkin" or opening from six to eight feet wide was driven into the middle of the pillar. And the fact of six to eight of those jenkins requiring, according to this system, to be in operation at one time, in one "headways course," will at once show how costly a plan this must have been. Another objection to this system will be seen in the excessive consumption of timber which inevitably occurs, by having so many working places adjoining each other in one headways course, added to which, it will be observed that, by driving the jenkins in the heart or middle of the pillar, the strongest coal is taken out, and that on either side of the jenkin would be crushed, and rendered almost unmerchantable

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Diagram No. V

Presents to us a view of ventilation altogether different from the plans which have already been brought before your notice. I mentioned, in a former part of this paper, the improvement made in the ventilation of coal mines by Mr. Spedding, with regard to the coursing of the air behind the working face. We have now arrived at an epoch when experience has taught us how to divide, by means of a regulator, the currents of air in such a manner that we are enabled, with the greatest precision, to give one district a greater quantity of air and another a less quantity, just as the circumstances of the case may require. This, it is well known, is a great advantage over the old system of carrying one unbroken current of air through a mine; for let us suppose that, according to the old system, a single current of air had to traverse a distance of six miles through the circuitous passages of a mine. Now, if we make two divisions in this current of air, and allow the distance for each division to travel to be about three miles (instead of six), then we shall have reduced the frictional resistance to about half of what it was before.

Allow me to make a few remarks here as to the improvement which was made in respect to divisions or splits in the air. The division of the air was accomplished by the application of what is commonly termed an air-crossing, which was adopted for the purpose of carrying one current of air over or underneath another current which might be passing in a different direction - one current generally being supposed to be fresh atmospheric air, and the other impure or vitiated air.

I am not positively aware to whom the invention of the air-crossing belongs, but I have heard it attributed to the late Mr. Buddle - a gentleman whose name will ever be remembered in connection with the science of mining.

The importance of dividing the air into separate currents or splits, and the benefits derivable from such a mode of conducting air through the various districts of a coal mine, are now sufficiently obvious; and I shall proceed to make a few remarks, necessarily as brief as possible, on the requisite distribution of the various currents of air. To take the diagram No. V. It is here shown that the air is divided at A, one-half going to the right (to B), and the other (from A to D) to the left portion of the district or "panel." Let us follow the portion going to the right. It will be seen that the air is conducted from A to the far board B by

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[Diagram No. V]

[Diagram No. VI]

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stoppings placed in the various boards. By this method of carrying the air along the working face, the miners are enabled to inhale the fresh air before it reaches the waste. After arriving at B, the air, having ventilated the working face, then passes off into the back workings or waste, and, traversing the various courses, comes to the point marked C [here observe the advantage of the air-crossing], where it passes over the crossing which separates the ingoing and outcoming currents, uniting with the air (at E) which has ventilated the workings between A and D in a similar manner, and thence proceeding to the upcast.

It will be evident at a glance that this arrangement of ventilation is much superior to the former systems, which I merely introduced to your notice for the purpose of illustrating the various degrees of improvement which have been successively attained in the modes of carrying off air through a mine. By the system at present under notice, we are enabled to give the workmen the fresh air, and to dispense with all the doors in the headways course, which were formerly required to ventilate the waste. This is a great security to the air keeping its proper course; the ventilation of the mine is rendered more perfect, and there is less liability to accident or derangement. We shall now proceed to

Diagram No. VI.

This represents an efficient system of ventilation and a mode of working the coal in which the jenkins are not driven into the heart of the pillars, and we are enabled almost entirely to remove or work away the pillars of coal left in the first working of the mine. In this system, it will be seen that there are a range of stoppings [which must be substantial - they are generally of pillared stone, well and closely packed] from A to B on one side and from A to C on the other. The air is first taken direct to D, where it divides to the right and left in equal quantities, temporary stoppings being used to bear the air to the edges of the goaves. By a close inspection of this diagram, it will be seen that the air is never entirely confined to pass through the goaf. This is an advantage, for it is always desirable to have the stopping or door, as the case may be, which is adjoining the edge of the goaf, provided with a scale, - that is, an opening sufficiently large that we may always rely upon having a portion of air passing through it, however much the air may be contracted in the goaf. In the event, however, of the air passage through the goaf becoming so much contracted that the quantity

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going into the district is lessened, then it would evidently be desirable to enlarge the scale or opening, as before alluded to, in the stopping or door next the goaf, provided that there is a sufficient quantity of air passing through the goaf to prevent the appearance of gas at the edges. If we follow the course of the air in this diagram, we shall find it to be conducted down three boards, and up the same number, and so on, until it reaches the air-crossing E, where it crosses and unites (at F) with the air which has ventilated the opposite side, in a similar manner, after which it passes off into the return.

The question may here occur, to those who are not conversant with mining operations, "why the air is conducted alternately in three boards, in preference to any other number?" This is done because the current of air is shorter by this plan than it would be with a less number of boards, and, besides, there is not such a risk of the air being stopped or contracted when three boards are provided. I have said that three boards are required, but the exact number will very much depend upon the nature of the roof. One of these three, I may here mention, is generally appropriated as a "*travelling road*". The size of this will be proportionate to the length of the traverse and the number of passages, for it will be evident that to make large air-ways, where the length of the current of air is short, would entail a useless expenditure. This system of coursing air in a mine is now almost, if not altogether, abandoned; but, if time permit, this will be more fully treated in the course of our subsequent remarks.

Diagram No. VII.

Plan No. 1.— This shows another arrangement for the working and ventilation of a coal mine. It will be seen, on referring to this diagram, that the coal field to be worked is divided into separate panels or districts more particularly with this object - that should an explosion of gas occur, the element of destruction may be confined, as much as possible, to the district in which it originated. Another advantage derived from this mode of working is that, when the boards reach the barrier (indicated by the dotted lines), the removal of the pillars may be at once commenced with, thus obviating the necessity of the pillars standing so long as was the custom formerly, by which the coal was, therefore, much lessened in value ; for it is well known that when pillars of coal remain unworked for a great number of years, the coal, to a certain extent, becomes oxidised by exposure to the atmosphere, and the result

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[Diagram No. VII.

Plans 1, 2 and 3]

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is that a large portion of the seam, in such cases, is rendered almost worthless. Again, by working off the pillars immediately after the various districts have reached their respective barriers, the air-courses are done away with, the lengths of the traverse of the respective currents of air are materially diminished, and this is an important point to he attended to with regard to vigorous ventilation.

It will be seen that this district (No. 1) has reached the barrier; and the roof being of such a nature that the headways courses do not fall, after the timber is taken out, the working away of the pillars, therefore, can be commenced with at once. This is accomplished by laying the tram-road along the three headways next the barrier to the far side of the district. We then commence to work away the pillars, by taking away a "lift" or slice of coal, of say five or six yards in width, for half the length of the pillar on one side of the headways, and another similar lift on the opposite side, until the pillar is entirely worked off - one pillar after the other being removed in a similar manner.

In this case it is necessary, with pillars of such dimensions, that the workings in one headways should not be in a direct line with other headways courses, but be kept at a certain angle, similar to that from A to B on the plan. This prevents, to a great extent, the coal from being crushed on the occurrence of creep.

The ventilation on this plan is quite simple, the air being taken first direct to the face F, and from thence into the working places; and when it reaches D, it is then taken into the waste, and ventilates it until it passes the regulator E; and then, uniting with the other currents, it passes on to the upcast pit.

This is a good arrangement for working away pillars of coal where roof does not fall. The naked light can be kept a safe distance from the goaf.

Plan 2 differs from the former plan only as regards the system of working away the pillars. The roof is supposed to break down to a great extent after the timber is taken out, and this prevents the old headways course being applied to such advantage as in Plan 1. New openings have, therefore, to be made along each headways for tram-roads. The system adopted in working the broken mine is widely different from that pursued in No. 1. By a glance at Plan 2, we will at once see the manner in which the pillars are worked off. A jenkin is driven half-way up in one side of the pillar; a place is then driven through the middle of the pillar to the goaf, and then the lifts are taken off on the next side. This being done, the lower half of the pillar is removed in a similar manner, and so on until the whole of the pillar is worked off.

As this plan entails considerable expense in regard to the driving of Jenkins and ridding, &c, I shall shortly treat upon other methods of working away the pillars, by which the expense may be avoided.

Plan 3 is a district of workings in progress in the whole mine, showing the arrangements of ventilation.

In Diagram No. VIII.,

Which we shall now proceed to notice, the boards in the district are not driven to their respective barriers before the pillars are commenced to be taken away, as was the case in the last diagram; but the pillars, on the contrary, are commenced with so soon as the boards are driven to such a distance that the ventilation may admit of the safe introduction of candles in working the whole mine, while, at the same time, safety-lamps may be used in extracting the pillars in the broken mine.

The principal advantage of this system over the former consists in the fact that the same "establishment," in regard to timber, rails, &c, that was required for working the whole mine, can be used for the purpose of working the broken mine.

The timber is also allowed to remain, and this prevents the outlay incurred in ridding and driving jenkins to open out the old work; and therefore, in point of economy, this system is much superior to the former.

Let us now consider the mode of ventilating this particular system, and, in doing so, I will bring before your notice the different ways in which such a plan of working may be ventilated, pointing out, at the same time, their respective merits, to the best of my ability.

And, first, let me direct your attention, as briefly as possible, to *plan No*. 1.

The air is here taken direct to the working face, as shown by the darts. It is then conducted along the working face, and, as you will observe, ventilates the various boards in the whole mine as it passes along; after which it is taken down the far board to ventilate the broken mine.

There is an objection to this system, and it is this: - It will be seen that the doors which are placed at A are for the purpose of bearing the air around the working face; but if it should occur that those doors

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[Diagram No. VIII]

were neglected, then the ventilation between C and D would at once become suspended, and any gas which might happen to be at the edge of the goaf might probably "back" to the candle D.

According to *plan No*. 2, which we shall now proceed to notice, the air is taken up the rolley-way board to E, where it divides - one portion being taken to ventilate the boards in the whole mine, and the remaining portion to ventilate the workings in the broken mine. By this mode of ventilation the doors at A, in plan 1, are altogether dispensed with, there being a separate current of air from both the whole and broken mine.

Allow me to direct your attention to the utility of dividing the air at E. On referring to this plan, it will be observed that that portion of air which is taken to ventilate the broken mine is conducted across to the far board F, when it is taken to ventilate the edge of the goaf in its course to the upcast shaft. Now, so long as all the stoppings and doors remain as at present shown, plan No. 2 is, in my opinion, superior to plan No. 1. But if the doors G and H, in plan No. 2, are destroyed by working away the pillars, the air will then not be carried over to F as before - it only reaches G - and, consequently, the space between G and F will almost be stagnant.

It is quite possible, under these circumstances, that gas might come to the edge of the goaf; and if so, such an arrangement of ventilation would prove highly improper. The chances of accident would be greatly increased, inasmuch as the gas from the goaf might then back up against the stopping C, and press through it upon the naked light outside of the stopping. This will be evident, if we look at the point where the air divides. It will be seen that the distance from A to B and C is equal to that from A to D and C.

If we suppose the existence of a scale in the stopping C, the question would then arise, whether the air from the broken mine could pass through the scale at C upon the candle, or whether it would pass from the whole mine into the broken ? This is rather an important question, for, should the air from the goaf pass through the scale upon the naked light, it would certainly evidence a very deficient system of ventilation.

As we have already observed, the distance to the stopping C in each division is equal, and it will, therefore, be obvious that the two columns of air will be equally balanced. Should any obstruction, however, take place in either of these divisions of air, then the air at the stopping C

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would press into the division in which the obstruction occurred; but, everything remaining in order, the tendency of the air will be to press from the goaf into the whole mine, and such a mode of ventilation must, therefore, be admitted to be undoubtedly objectionable.

To take, in the next place, *No.* 3 *plan*.

The system of working shown here is similar to Nos. 1 and 2; but the arrangements are very different, so far as the ventilation is concerned. You will observe we have here recourse to doors at X again, as a passage for coals.

Let us compare this mode of ventilation with that shown on plans Nos. 1 and 2. The air here is, in the first place, taken direct to the working face, and after ventilating the whole mine, is then carried down the far board F, as far as one pillar, to A; after which it is conducted back along the headways to B, then down another pillar to C, and thence along the headways to D. It now passes on to the edge of the goaf, and ventilates the workings in operation in the broken mine. This system of ventilation is a great improvement upon that shown in plans 1 and 2, inasmuch as the distance between the naked lights and the safety-lamp is greatly increased, and the air will always press in the right direction, viz., from the whole mine into the broken. The doors are so arranged, it will be seen, that, when open at X, the gas cannot by any means back to the candle) while the doors at E remain shut.

It may be here observed, that accidents have occurred under the system of working coal mines shown on this diagram, and it is, therefore, worthy of your most serious attention to consider which is the best mode of application with regard to safety.

Of the three methods which I have brought before your notice, I would certainly myself recommend the last as the most safe under all circumstances.

Before concluding, however, with this diagram, I would wish to observe that, where the cover of the mine does not fall after the supports (props, &c.) are taken out, the pillars of coal may then be worked off without incurring any expense in rudding, &c. ; and I would, in such a case as this, approve of the panels being driven up to their respective barriers before commencing to take away the pillars. The coals, according to this method of working, would not stand long before the broken mine was commenced with - probably, in an ordinary seam, not more than three years - which, of course, would not, during that period,

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[Diagram No. IX]

at all deteriorate the merchantable value of the coals. This is, in my opinion, a safer system of working than the system of "following-up" with the broken as the boards progress in the whole mine. It is only in cases where the cover of the mine is composed of soft materials, and breaks down to a considerable extent, that, when the supports are removed, jenkins require to be driven across the pillars of coal, and by the side of the old pillars, &c, as shown on plan No. 2, diagram No. VII.

This, it must be obvious, is much more expensive than the system of removing the pillars before the cover or roof of the mine is allowed to fall.

Diagram No. IX.

Differs from the arrangement of working shown in the former diagrams, chiefly with regard to the position of the rolley-ways. Instead of having the rolley-way board on one side of the panel, with a barrier of coal on the opposite side, we have here the rolley-way formed in the middle of the district, and one rolley-way, as it were, serves the purpose of working away two districts. Twice the quantity of coals may thus be obtained by one rolley-way, as compared with what could be obtained according to the plans shown on former diagrams. It is, therefore, evident that where the outlay for making horse roads is great, one half the expense may be avoided by this arrangement of rolley-ways.

It is, of course, most desirable at all times to have the cheapest method of working a mine. So far as the workings of the whole mine are concerned, the less number of rolley-ways may serve every purpose very well; but, when the broken mine is commenced with, and the roof is so formed that great weight is to be sustained by the pillars of coal of ordinary size, then, when the goaves on either side approach each other, we find that, as the last pillars adjoining the rolley-way are being worked off, the coal is very much crushed, and, to a very great extent, deteriorated in value. Therefore, in laying out the districts for working the coal, we must not only have regard to the best and cheapest mode of conveying the coal, but we must also consider the best method in which the mine ought to be worked generally.

My reason for showing you this is simply to lay the various methods of arranging the districts before you. In the two cases above referred to, the chief consideration is, which arrangement, according to the circumstances of the case, is the least expensive. The boards in this case

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are so much fallen, that they would be very expensive to open for air-courses. The air is, therefore, carried back and forward in the headways course, instead of being coursed up and down the boards, as shown on the former diagrams.

Diagram No. X.

Plan 1. — The method suggested here is, that the pillars be taken off in close proximity to the working of the whole mine. It is assumed that the roof is composed of ordinary material. We have, therefore, so arranged the workings that every alternate board is driven narrow - *i.e.*, two yards in width - in order that the roof of the boards may stand until the workings in the whole mine C are carried beyond the barrier K.

By this arrangement the naked lights are placed at a much greater distance from the goaf than in any of the former cases, when working off the pillars, as shown in diagram No. VIII. And not only so, but in the case of accident in district L, the barrier K of solid coal will be a more reliable means of separation than " *board end stoppings*" would have been.

The same current of air which ventilates the whole mine is taken to ventilate the broken mine on its route to the upcast shaft. The doors placed at G are for the purpose of allowing a passage for coal work into district M. As regards the system of working the broken mine - assuming, as I have already done, that the narrow boards are driven for the purpose of working off the pillars in the most economical manner (by taking one-half the pillars from each side of the narrow boards) - you will at once see that this is a much better system than that shown on plan 2, diagram VII., where jenkins had to be driven, and, consequently, as lamps would be in use, the aid of gunpowder could not be taken advantage of in driving them.

The pillars, as you will see, are sixty yards long by twenty yards broad, and they are of this length for two reasons. First, in not having an opening through the middle of the pillar, as is the ordinary practice, the weight of the strata above is much easier resisted, and the coal is not crushed to such an extent; and besides, secondly, in working away the pillars in the broken mine, there are not so many of what are generally termed " last lifts." The last lift is the last portion of coal that is worked off the end or side of the pillar. These last lifts are, of course, much more dangerous to work off than any other portion of the pillar -

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[Diagram No. X. Plans 1 and 2]

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all other lifts having a solid side, except the last, and this is only supported by timber, to which it generally proves very destructive. When the coal is got, too, it is generally crushed to a very great extent. Hence the necessity of having as few such lifts as possible.

Plan 2 is supposed to represent the coal lying at a great depth from the surface, and the texture of the coal of an average strength. It differs materially from the former diagrams, both in regard to the size of the pillars and the ventilation of the mines.

The pillars under this system are made large, with the threefold object of resisting the pressure of the overlying strata, preventing the creep as much as practicable, and producing as large a per centage as possible of good merchantable coal. The roof of the mine is here supposed to consist of a strong material, therefore, after the timber or other supports are removed, the boards do not fall, and the pillars may accordingly remain until that part of the whole mine B is worked to such an extent beyond the barrier F that the district A may be ventilated with a separate and distinct current of air from that which ventilates district B, now in progress in the whole mine. This effectually prevents the air in the broken mines (where safety-lamps are exclusively used) from coming in contact with the candles in district B.

The roof of the mine being strong, (as I have already supposed) the boards or openings may be taken advantage of, for the purpose of removing the pillars of coal left in the first workings, and this is done by taking lifts half way across the pillar to the right hand and also to the left hand of each board.

It is right I should here observe that in deciding as to the requisite dimensions of the pillars for any particular colliery three considerations should be regarded : - *the depth of the seam form the surface - the nature of the seam, its texture and strength - the nature of the material upon which the coal rests.* The diagrams before us represent the size of the pillars to be sixty yards long by forty broad. In working the whole mine, however, where naked lights are used, gunpowder is generally required for the purpose of bringing the coal down after it has been properly undermined. The size of the pillar, therefore, does not materially affect the produce of the seam in the whole mine; but in the broken mine, where gunpowder is prohibited, and the safety-lamps are exclusively used, great care and consideration is required with respect to the size of the pillars. Where the pillars are too small, "creep ", as a natural consequence, will take place - the coal will be crushed and rendered of less value - the consumption of timber will, in all probability, be

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excessive - and the expense of maintaining the roads will be great - in short, the total cost of production will be increased to a very serious extent. If the pillars should be too large, so that the coal does not fall after being undermined, it will require to be brought down by the pick, and will, therefore, as in the former case, produce a large proportion of small coals. But if the pillars are of sufficient size to prevent the "creep," and the coal is properly undermined, so that the weight of the strata above will break off or split the coal, the produce will then be nearly as good as the coal produced from the whole mine. Great judgment ought, therefore, to be exercised in determining the size of the pillars for any particular colliery.

Diagram No. XI

Plan 1.—According to this plan, the water level drifts are, in the first place, driven out from M to A. A pair of headings are then driven at right angles to the water levels, which continue as far as B, being the extent of district C. Boards are driven from these headways to the extremity of the boundary; and this district being now completed in the first working, the naked lights are withdrawn, and the safety-lamp introduced instead. The taking off of the pillars may now be commenced with, and while this operation is being proceeded with, another pair of headways, similar to the former, are driven from D to E. Boards are then turned away from the headways, forming another district similar to C, in order that it may be completed in the first working, by the time that district C is all worked off. By this method of working, the men may use candles while working district F, in the whole mine, as it is ventilated by a separate current of air from that which ventilates the broken mine. By this arrangement, too, the naked lights may at all times be kept at a proper distance from the goaf.

The advantage in deep mines of going to the extremity of the coal field before commencing to take off the pillars, (thus more easily keeping the horse roads in order,) is too obvious to need any further remarks. I have observed, in mines where the seam is naturally tender, and the mode of working such as I have described on a former diagram, - that is, where the pillars are removed as the boards in the whole mine progress, - very great loss has been occasioned by the breaking of timber in the horse-roads, and the return air-courses are, from the same cause, very expensive to maintain. In some cases, under these circumstances, it is almost impossible to leave barriers of coal of sufficient size to prevent "creep" from taking place.

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[Plan 1. Plan 2.]

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Plan No. 2 brings before our notice another system of working, which differs materially from that shown on the former plan; for you will see on this diagram, that a few boards are driven with a view of procuring " pit room" to obtain the necessary quantity of coal for the supply of the pit as soon as possible after it has been sunk.

Generally speaking, the manager of a colliery is anxious to raise as much coal as possible after the pit has been "put down" to the seam, and it is with a view to the attainment of this object that I would propose the driving of a few boards in each district, to a limited extent. I merely offer this suggestion in order to show the possibility of raising a quantity of coals, under such circumstances, in a short time.

In the course of my experience I have seen very serious results follow from working away the coal to too great an extent near the shaft, main rolley-ways, and returns. I have known it cause a great obstacle to the proper working of the colliery, as " creeps," &c, have ensued, and, consequently, a continual expenditure has thus been created.

With regard to the question, how a sufficient quantity of coals, in the above case, may be procured for the supply of the pit? I believe that with a little care and attention, and a proper arrangement of districts, there is very little doubt of obtaining a sufficient supply. What I have, said respecting this plan applies principally to the case of deep mines. Where the mines are not of very great depth it is not of such importance to leave large pillars, and to go to the extremity of the districts before commencing to take off the pillars. The necessity of attention in this respect does not exist here, for the pressure of the overlying strata is not great, and, consequently, "creep" may not be anticipated; neither need we fear that the coal will be rendered at all unmerchantable.

I must now, however, draw my remarks to a conclusion, and, in doing so, I would repeat what I stated before, that in the preparation of this paper I have principally endeavoured to make my observations, instructive and interesting to the junior members of this important Institute. The thought occurred to me that if I could, to the best of my ability, bring before the notice of the younger members of the profession the advances that have been from time to time effected in this branch of national industry and interprise, it might have the tendency of stimulating them to use their endeavours to extend still further the improvements which have been attempted and carried out by those who have had greater experience than themselves.

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Should this desirable result be attained, in however small a degree, I shall certainly deem myself amply repaid for the time and trouble expended in the preparation of these papers.

This, and the preceding paper on the Lundhill Explosion, which I had the honour of bringing before your notice some time ago, have been, I must confess, rather hurriedly arranged. Yet I trust, notwithstanding all imperfections, that what has been said will prove more or less useful and interesting to all who are connected with the important profession of mining engineering.

If I have failed in this attempt I have failed through want of ability, and not from a want of disposition to do justice to this most important subject.

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NORTH OF ENGLAND INSTITUTE of MINING ENGINEERS.

GENERAL MEETING, THURSDAY, DECEMBER 2, 1858, IN THE ROOMS OF THE INSTITUTE, WESTGATE STREET, NEWCASTLE-UPON-TYNE.

Nicholas Wood, Esq., President of the Institute, in the Chair.

The Secretary, before reading the minutes, said it would be necessary to call the recollection of the gentlemen present to a resolution to have an opinion taken as to the propriety of Mr. Reid's charges for printing the proceedings. As this matter had been intrusted to him, he wrote to his own booksellers to recommend a person, or two persons, to give an estimate of the cost of the lithographing, including colouring, and the printing. He wrote to Messrs. Smith, Elder, & Co., who had published books for him, and they recommended Messrs. Spottiswoode & Co. He believed Mr. Spottiswoode was known to Mr. Wood, and he was also known to himself, (the Secretary). On being written to, he very readily undertook the estimate, and wrote as follows:—

New Square, London, Nov. 1, 1858.

Dear Sir,

We beg to send you herewith an estimate for printing 530 copies of the Transactions, Vol. V., as if from MS. copy, viz., £442 1s. 6d. It is almost impossible to say what ought to be the charge incurred by authors for corrections and alterations in a work of this kind, but it ought to be very little if the copy is well prepared.

Yours faithfully,

J. SPOTTISWOODE & Co.

Thomas Doubleday, Esq.

The President— It appears from this, that Mr. Reid charges £14

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less than Messrs. Spottiswoode's estimate, and, considering the trouble which he has in arranging those papers, and the mode in which the work is got up, which is very creditable to Mr. Reid, I think there is no question that it is desirable to continue the printing in his hands. However, this matter was taken into consideration by the Council, and Mr. Doubleday will read their opinion.

The Secretary read the minutes of the Council, to the effect that they did not deem it advisable to make any change.

The minute of the Council was confirmed.

The Secretary next read a resolution of the Council, that a certain number of copies of Mr. Wales' paper be distributed among the collieries subscribing to the Institute.

The President—It had been suggested to me, that as Mr. Wales' paper was of a practical nature, it might be useful to distribute some copies of it amongst the viewers and overmen of the different collieries. Not having an opportunity of consulting the Council, and it being necessary to decide whether any should be printed, I took it on myself to order 250 extra copies to be printed, which has, I believe, been done. The Council to-day have relieved me from the responsibility of having done so. It still, however, remains with the meeting how they should be distributed; the Council having come to a resolution that they should be distributed amongst the collieries in proportion to the amount of their subscriptions; thinking it was scarcely right to give them to the collieries that did not subscribe. We have, certainly, been much disappointed in the support we have received from the collieries generally, and which, from the good that we trust has been done by the Institute, we had a right to expect. To give those collieries which do not now subscribe copies, perhaps, might induce them to subscribe. It was suggested at the Council meeting, as desirable, to give to each of the members an extra copy, for distribution, but, there being 228 members, and only 250 extra copies ordered, that would, of course, take up nearly the whole of the extra copies. I do not know whether the press is in such a state that extra copies could be printed, if so, then they might be distributed more liberally. It seems desirable, that the paper having been written with the express view of being made useful to viewers, underviewers, and overmen, they should get copies. They could, of course, obtain copies at the end of the year by purchasing the whole volume at a guinea, but it would be a long time to wait, and an expensive mode of getting it.

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Mr. Hall said Mr. Wales' paper was a very useful one, and if double or treble the number were printed, they might be sold for what they cost.

Mr. Dunn concurred in the propriety of that suggestion. They might, in this instance, deviate from the general rule.

Mr. Hugh Taylor, who had just entered the room, was asked his opinion.

Mr. Taylor said, he should always pay deference to the decision of the Council. Had they not come to that decision, he should have thought it invidious to make a distinction between the collieries, the principals of which subscribed, and the rest, because it was desirable that all the viewers should have the benefit equally. As they had come to this decision, he might suggest the giving of one or two copies to each subscriber, to make use of in any manner he might think fit.

The President—I concur in what Mr. Taylor says. There is a deal of liberality in it which ought to belong to an Institute of this description. Still, I must repeat, that we have not received that support from the coal trade, which, I think, we are justly entitled to; out of nearly 200 collieries in the trade we have only about 60 subscribing to the Institute. We have laboured hard to make it useful, and, I believe, it has raised a degree of emulation amongst the managers of the mines which has been, and will be, beneficial to the trade at large. Our energies and our usefulness are very much crippled for want of models, books, &c, I trust, therefore, that when Mr. Taylor next presides over a meeting of the Coal Trade, he will impress upon them how very desirable it is that they should give the Institution more substantial support. I know the opinion of Mr. Taylor would have very great weight with the coal-owners. I hope, therefore, that we may shortly expect a more numerous list of subscribing collieries. I now find that it is quite practicable to order an additional number of copies of Mr. Wales' paper, as the type is still set up, we can, therefore, enlarge the number of copies already printed, and consequently embrace all the objects which have been suggested. He would, therefore, move that 750 additional copies be printed.

The President put the motion, which was carried unanimously.

The meeting proceeded to the election of new members: — Messrs. Edward Hedley, Dodsworth Road, Barnsley, Yorkshire; Alfred P. Rockwell, M.A., Norwich, Connecticut, U.S., North America; Andrew Landale, Lochgelly, Fifeshire, North Britain; and John Straker Wilson, West Cramlington, Newcastle-on-Tyne, were elected; and Mr. John

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Kerr, of Hamilton Gas-Coal Works, Lesmahagow, N.B., was proposed for election at the next meeting.

The President—The next proceeding is a statement as to what progress has been made towards the establishment of a Mining College. Mr. Thos. John Taylor and myself, by appointment, last week met the Warden of the University of Durham, and Mr. Chevallier, one of the professors, on the subject of connecting the Mining College with that University; and without going into details, which occupied two or three hours, I can only say the meeting was of an extremely satisfactory nature. I think, from what took place, the University, so far as they are able, will do everything in their power to aid the establishment of a Mining College. The conclusion was, that Mr. Taylor and I should put down in writing the views entertained by the Council of what was desirable, as much in detail as possible, and that we should meet at the first opportunity. I presume we shall be able to meet together very shortly; and before next meeting of the Institute, we shall have made such progress as to be able to lay before you the nature of the arrangement proposed for the establishment of a College.

Mr. Taylor—This is, of course, preliminary. After due preparation you can then bring the question before the Institute, and have a discussion upon it.

The President—This is all the business except the discussion on Mr. Atkinson's and Mr. Wales' papers.

Mr. Hall said, some slight mistakes had occurred in his paper which he would like to explain. It was on the French Coal-field. He found at page 73, the third line from the top, the area was set down at 920 square miles, and it ought to be 1,920 square miles. The error occurred in reducing the French kilometres. He would have corrected the error at the last meeting, but he happened to refer to Mr. Dunn's work on the coal trade, and he found Mr. Dunn set it down at 914 square miles, which confirmed him (Mr. H.), in his error. There is another error at page 98, the 18th line from the top, 11,760 should be 11,960 square yards.

Mr. Atkinson's paper on "splitting," which stood first for discussion, was postponed in consequence of the unavoidable absence of Mr. Thos. John Taylor, who wished to be present at the discussion.

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DISCUSSION ON MR. WALES' PAPER.

The President said, as this paper was intended to be printed and circulated separate from the proceedings of the Institute, it was desirable that the discussion thereon should be printed with the paper. He, therefore, proposed that they should take the discussion on the respective diagrams *seriatim*. He would, in such case, first ask Mr. Wales if he had any further explanation or additional remarks to make, and then go on with the discussion generally.

Remarks on Diagram No. I. were then read.

Mr. Wales—The members would observe, that in this diagram the air was taken to the further board or working place, and carried up such board to the face, and then carried across the face of all the working places, and so down the first board, and along the return drifts, to the upcast shaft. The board stoppings being removed forwards as the working places advanced.

Mr. Dunn—That is what you call "face airing," but at that time there were a course of stoppings left standing occasionally, at every fourth, fifth, or sixth pillar, to distribute the air in the waste, which was not a partial coursing of the air.

Mr. Wales—That was what was called "shething the air."

The President—Up to what date was that practised?

Mr. Wales—Previous to 1760.

Mr. Dunn—Mr. Spedding introduced the system of coursing the air at the Workington Colliery, in Cumberland, in or about the year 1760. The universal system of ventilating coal mines, previous to the coursing of the air, was either that the waste was not ventilated at all, or, that a partial distribution of the air in the waste took place, which was called "shething." In the latter cases there was no inflammable gas.

Mr. Barkus—In the years 1812 and 1815 in some collieries there was a great deal of "shething." More than "coursing" the air.

Mr. Atkinson — It would be interesting to know if any explosions resulted from the waste getting foul?

Mr. Barkus —No doubt; we can refer back to explosions having taken place when the waste became foul in consequence of its not being coursed, but only shethed. One of the pits at Harraton Colliery exploded from that cause. It was the general custom on the Wear to sheth the waste, and not to course it, when I first commenced practise in 1812, and even after that period.

Mr. Wales—They must have been very slow in their adoption of

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improvements on the Wear. Coursing having been introduced fifty years previously.

Mr. Dunn—Certain districts of the Wear produced no inflammable gas, and at these collieries "shething" only was practised, but at the collieries where gas was produced, "coursing" was in use, indeed, these collieries could not otherwise be worked, as there were no safety-lamps at that time.

The President—Then I think we have come to this conclusion, that up to 1760, if any air was taken into the waste, it was done by the system of "shething", and that this was the system applicable to diagram No. I. We shall come to the details of coursing the air in the discussion on the next diagram.

Previously, however, to leaving the subject of shething, it may be desirable to give a short description of this mode of ventilating the waste, if it can be called ventilation. This appeared to consist of building stone stoppings in every third wall, which operated to carry any air which might scale through the timber board stoppings along the face of the workings, back again to the workings; and a scale of air being allowed to pass them, and also through some portion of those stone stoppings, as circumstances pointed out or rendered necessary, the waste was, by that means, partially ventilated. It was not, however, by any means the practice to place these stone stoppings in every third wall, this depended entirely upon the state of the waste in each case. In some of the old wastes, large areas of pillars were found standing without any sheth stoppings, in cases where there are no discharge of inflammable or other gases.

Mr. Dunn and Mr. Barkus corroborated the foregoing statement.

The President—We have now, I think, exhausted the subject of ventilation in the early period of coal mining, let us now take a glance at the mode of working the coal at that period. If we are to take the diagram as an illustration of the mode of working, that represents about one-third of the coal taken away, and two-thirds left. My experience, derived from examining the old workings of several collieries, would show that, generally a much larger proportion of coal was taken away. My notion is, that the principle laid down was only to leave the pillars of sufficient size to support the superincumbent strata, the intention having been, in the very early period of working coal, to support the superincumbent strata, and to leave pillars only just sufficient to accomplish this, and in some cases mere shells of coal appear to have been left over large areas of workings.

Mr. Hall—In all the old seams I have examined, I find the old people

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have been very clever in getting away the coal. I have seen a large extent of pillars, of two or three yards in thickness, in a seam seven feet in height. I have not seen any pillars taken away entirely in such cases, but there had evidently been a second working.

The President—With regard to working pillars, I have taken some trouble to enquire into the facts regarding this operation. I find, that previous to 1730, pillars had been worked, and they were, moreover, worked as a system, by extracting all the coal and allowing the superstrata to fall. It appears to have been a very general practice, when it was supposed that the pillars left in the first working were more than sufficient to support the strata above, to reduce, or, as they termed it, to "rob" the pillars. And this appears to have been done, sometimes by taking away a portion of the ends of the pillars, and sometimes the sides, still leaving what was supposed to be sufficient to support the roof. At times this appears to have been carried on to such an extent as to render the pillars insufficient to support the superior strata, the result of which was, that they were crushed, and produced what was called a "thrust." The old records speaking of "thrusts" having occurred throughout a large extent of workings. The pillars in this case were crushed to pieces, and the roof fell to such an extent as to fill up all the openings made in extracting the coal in the first instance. This took place when the coal was worked at moderate depths. Still later, when the coal was worked at greater depths, what was called a "creep" took place, from the same robbing of pillars, the pillars being crushed, and the thill rising up and filling the openings in the boards. And the instructions to the viewers, at those periods when their opinion was asked, in consultations, as to any further working, to report "what portion of the pillars can be taken away so as not to produce a thrust, or creep," as the case might be. At that early period I find remonstrances from the landowners, of the surface being damaged by the pillars being "taken away," in contradistinction to the effects of a creep or thrust, which was complained of as producing cracks in the walls, and otherwise damaging the buildings. We may, therefore, assume that pillar working took place at a very early period of coal mining.

At that period, the reducing or entire removal of pillars was pursued without any reference to ventilation, the only exception being that the stoppings in the sheth walls were secured, but this occurred in very few cases. Where ventilation in the waste was required, it appears to have

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been the case, generally, not to remove all the pillars, but to leave them of sufficient dimensions to support the superincumbent strata, and to accomplish that object, except in cases where the pillars either had originally, or had been reduced, to make them insufficient for such a purpose, and where thrusts or creeps had been produced.

Remarks on Diagram No. II. were then read.

Mr. Wales—The improvement shown in this diagram is in coursing the air in the waste.

Mr. Dunn—You have drawn this plan three boards to the course. When there was not much gas the air was distributed into five boards, and, in some instances, the air was only taken up and down in one course, though there might be twenty boards in one sheth of workings.

The President—My experience is, that the air was more generally coursed two and two, the exceptions being where there was no inflammable gas, and no other noxious gas discharged. Then, as Mr. Dunn states, the air was distributed into three, four, or five boards, and, in some cases, the whole of the air was taken into the workings, then coursed down to the main road, back again up to the face, and then along the face and through the remaining waste to the return drift to the shaft. Mr. Spedding, as previously stated, introduced the mode of coursing the air in the year 1760.

Mr. Atkinson—When the length of the courses for the air to travel from the face to the main road was very great, it was, I believe, the custom to have board end stoppings, such as is shown in the diagram, placed about midway, or at any other distance from the face of the workings. The air was then coursed back and forward between the face and those board stoppings, and after ventilating the working places, the air then ventilated the waste between the board stoppings and the main roads.

Mr. Wales—-This mode would ensure fresh air to the working places, and not the contaminated air from the long courses of the waste; and the stone board stoppings would prevent the scale of air through the timber stoppings escaping into the return. It is an old idea amongst pitmen, that by shortening the courses more air was obtained. It was certainly more pure.

The President—They would observe, that at this time the safety-lamp was not invented, and that the only light used was candles. At Killingworth Colliery the discharge of gas in some of the working places

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was at one time such, that it was necessary to take the air, after leaving each working place, down the long courses, to "dash " it, or to mix the light inflammable gas with the bulk of atmospheric air, before it could be taken into the next board, and used with the naked candle; the light gas on the top of the current being inflammable without such admixture. This was, no doubt, a very ticklish operation, rendered still more so by having to test the explosive point of the air by the naked light, but it was unavoidable before the safety-lamp was invented.

Remarks on Diagram No. III. were then read.

Mr. Wales—This diagram shows the mode of working the pillars pursued previous to the use of the safety-lamp, which consists of the sheth wall not being interfered with, and the two intervening walls taken away. In this way one-third of the coal was left; but, by doing so, the ventilation was preserved.

Mr. Dunn—Mr. Wales had passed over the intermediate stage, viz., taking away a certain portion of every other pillar, and keeping the air going through the waste.

The President—There were various modes practised of taking away the pillars, and, at the same time, preserving the ventilation in the waste, depending upon the number of boards ventilated by such current, or, in pit language, in each course, the nature of the coal and other circumstances. In some cases the principle pursued was to take away a certain proportion of the pillars, leaving sufficient to support the roof, in which case the waste was left "upstanding;" in others, the pillars were entirely removed, which produced a " thrust" or "creep," and there were intermediate cases of working where a certain proportion of the pillars were left, not sufficient to support the superincumbent strata, and where a creep was produced, the coal so left being afterwards worked away, the thrust or the "metal ridges" of the boards forming the pillars to support the roof.

Remarks on Diagram No. IV. were then read.

Mr. Wales—The remarks accompanying the diagram explain this mode of ventilation and working. The air is taken along the face by board stoppings, and then the standing pillars are ventilated. The pillars are entirely taken away, producing either a thrust or creep, and destroying all ventilation in the waste, which then becomes what is called a goaf or goave.

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The President—The diagram shows the system of working the pillars by "jenkins" but it is not essential to this system of ventilation and working pillars that, in doing so, the walls should be pierced by jenkins. They may be removed by any other method hereafter described. The jenkin is only one mode of obtaining access to the coal comprising the pillar, with a view to its removal. Such access may be had by driving up one side of the pillar, or by taking away one-half to the end of the pillar, and removing the other half in returning.

Mr. Wales—By jenkining the pillar the remaining coal is partially injured or destroyed.

Mr. Taylor—I have seen it done without destroying the coal, in collieries at moderate depths from the surface, and when the coal was very strong.

Remarks on Diagram No. V were then read.

The President—Two very important elements of ventilation are introduced in this diagram, viz., *the splitting of the air*, or dividing the entire current of air produced by the furnace into one or more currents, and the apportionment of the quantity of air in each split or current to the use of regulators. *And the use of crossings*, - The splitting of the entire current of air into one or more separate currents of unequal lengths is generally accomplished by regulating stoppings being placed in the return air-courses of the shorter currents - openings being made in such regulating stoppings or "*regulators* " to admit only the requisite quantity of air through such openings, and to force the remainder along the longer passage. In the diagram, A B is the longer, and A D the shorter current. A regulator is therefore put in the latter current at R, to force the requisite quantity of air along the course of the longer current A B; and as it is necessary that a small quantity of air should pass along the passage B S C, a "scale" as it is called, is allowed through a stopping at S; or, in fact, a regulator, with a very small opening in it, is placed in such short current at S. This splitting of the air generally involves the crossing of one current over or underneath the other, and therefore a viaduct or crossing is used by which this is accomplished, shown at C, and the three splits of air meet at E. Consequently at A, where the splitting of the air takes place, and at E, where they again unite, the tension of the air is the same in all the currents. The friction or resistance opposed to the currents A D and B S C at the regulators R and S being equivalent to the increased resistance of the long current AB.

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Mr. Wales—It is important to observe the proper place where such regulator should be placed. It may be placed at D, but in this case the whole force of the pressure against the regulator would be thrown upon the board stoppings, and in case of injury to any one of them, it not only takes the air off the boards beyond, but may affect the air generally. Hence the necessity of having the regulator placed in a line with the permanent stoppings, as at R.

Mr. Barkus—In this mode of ventilation the board stoppings along the face of the workings A B and A D are to be removed every pillar. This is a great expense, and so far this system is objectionable. The air, in passing from A B to C and from A D to R, is coursed up and down the waste, there will therefore be a great tendency of such air to pass through the timber board stoppings, especially those near A, where the air on the one side of the stopping has upwards of four times the distance to travel to E that it has on the other; consequently there will be a great scale of air through these stoppings, and the air will be very languid at B and D. The air might be coursed up and down, and when the courses were long, permanent board stoppings might be put in to shorten the courses, and to give fresh air to the workmen.

Mr. Wales—Even in Mr. Barkus's mode he would require board stoppings, as, for instance, if the boards were coursed three and three, two of the boards would require board stoppings, as it would require stoppings in the two boards, on each side of the sheth wall, to carry the air to the furthest board in the downgoing course, therefore, only two-sixths of the stoppings would be saved. The stoppings are plastered with lime, and therefore there is not a great scale of air.

The President—You save doors along the face of the workings by such board stoppings, which is of some consequence, and which are necessary in Mr. Barkus's case, in the sheth walls.

Mr. Wales—I have rather understated the advantages of splitting the air. Suppose you have six miles of air-course, if divided in two you have only about half the resistance.

Mr. Atkinson—If, instead of one long current, you divided it into two, and if the same quantity of air still continues to pass along each portion, then the resistance is one-half in each division, but the aggregate distance is the same. But, if you divide the quantity of air into two portions likewise, then the resistance of each split will be only one-fourth of one-half or one-eighth, (the resistance being as the square of the quantity, it will be $1: 2^2 = 1\text{-}4\text{th}$), or, for the entire distance, one-fourth.

The President—Two important objects have, therefore, been obtained by the improvements shown in this diagram. We have the doors required in a pit very much superseded by the use of regulators, and you have, by a splitting of the air into two divisions, instead of one long current, the resistance reduced three-fourths; by which, with the same motive power of furnace, a much greater quantity of air may be obtained in any mine.

Remarks on Diagram No. VI. were then read.

The President—This diagram, like the others, comprises the ventilation and the mode of working; let us take the mode of working first.

Mr. Barkus—These pillars are taken off at an angle, when these angles meet in the centre at D, the pillars to be lastly taken off will have the whole weight of the superstrata upon them, with no lateral support, there being goaf on both sides. By this mode the coal of these pillars will be much crushed.

Mr. Wales—This mode is, no doubt, liable to this objection, but we shall find this provided for in the different modes hereafter to be described.

The President—It does not appear to me, that it follows as a matter of course, that this mode alone should be assumed as that which was always used in taking off pillars of this description. We must take into consideration that the time has arrived when the benefit of the use of the safety-lamp was resorted to, and that in cases where there was inflammable gas, such lamp would be used in taking off the pillars shown in this diagram. We must, likewise, bear in mind, that those are pillars which had been left as unworkable, previous to the invention of the safety-lamps, and that they were little more than sufficient to support the superstrata. Large tracts of such pillars having been left standing in the Counties of Northumberland and Durham. The question then was, which was the best mode of taking them off. No doubt, the mode described in this diagram has been most extensively pursued, but there are several modifications thereof, where circumstances admitted, by which such pillars were removed. We are, therefore, discussing a case which will not, in all probability, again occur. It may be assumed, that now we have the aid of the safety-lamp, some one or other of the several modes which will be hereafter pointed out, has, or will be, adopted, and that in the intermediate stage of forming pillars, to be thereafter taken away, they will be left of such dimensions, and that the workings will be so laid out or organized, as that the loss of coal by creeps, or thrusts, or insufficient strength of pillar to support the superincumbent strata, will be avoided. We may, I think, therefore, go on to the next consideration, that of ventilation. The mode of ventilation seems generally to be the same as that of the last diagram, viz., taking the whole of the air up the main road to the face, and dividing it right and left into two splits, keeping it up to the face of the workings by a row of board stoppings from A to B and A to C.

Mr. Wales—I have already described such stoppings, which I prefer being built with small coal, or stone, in preference to brick, which is apt to be crushed or broken down by the weight. S S in the diagram are scales which are left for the purpose of allowing a portion of the air which is forced by the stoppings upon the working places to traverse the back pillars. As a principle, we confine the whole of the air into the working places and goaf. If there were no scales in cases of falls, &c, the general air of the pit might be diminished. The scale allows a certain portion to pass through the stoppings in such cases.

The President—These scales are, in fact, a description of regulators.

Mr. Taylor—You have no such thing as a self-acting regulator? Suppose the pressure to increase, and no air going through the goaf, would it not be desirable to have a self-acting regulator to give way in such a case, and to allow the air to pass through it?

Mr. Daglish—You might have canvass doors, or a common swing door, which would yield to a sudden or even continued pressure, and so allow the air to pass through them.

Mr. Wales—Something of this kind could no doubt be contrived.

Remarks on Diagram No. VII. were then read.

Mr. Dunn—This is the system of panel working introduced by Mr. Buddie in the year 1815.

The President—The safety-lamp having been introduced, an entire alteration of the system of working the coal took place. It was no longer necessary that the working places should be kept constantly free from inflammable gas, or that the current of atmospheric air should be always made to sweep away the gas, so as to admit the use of candles, The safety-lamp could be used in cases where this was impracticable. The whole of the pillars could likewise be taken away, and goaves formed, even where inflammable gas existed, and the working of pillars could be carried on even though such goaves were filled with inflammable gas, it being only necessary that the edges of such goaves should be ventilated so as not to allow the working places to reach the inflammable

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point. Admitting, therefore, that gas was thus allowed to accumulate in large quantities, and in extensive and continuous districts, it was clear that if an explosion did occur, (which might be the case by accident to the safety-lamps by falls of stone or otherwise, or by carelessness or wilfulness on the part of the workmen in damaging the lamps) it would be extremely disastrous. Mr. Buddle, therefore, adopted the system of panel working, as shewn in the diagram, by which each district of working was insulated from the adjoining district, so that any explosion occurring in any particular district, would not be communicated to another. There was also another advantage in panel working - candles could be used in working the whole mine, where the ventilation was perfect, and when pillar working was commenced, and the ventilation destroyed by goaves being formed, then safety-lamps could be exclusively used, and candles excluded from the working within such panel entirely.

Mr. Barkus—I see by the diagrams that the pillar working is shewn as commencing on the right-hand corner of the panel, which results in the roof having to be broken down afresh at the commencement of taking away the pillars in each panel, consequently, laying an increased pressure on such pillars, and crushing the coal; would it not be advisable that the working should commence on the left-hand side of the panel where the roof is already broken down by the pillars working off the previous panel, such pressure would not then exist, and the coal would not be so crushed.

The President—If you commence working on the left hand side of the panel, in order to obviate a second break of the superior strata, you must begin the working by removing the barrier, and so communicate with the goaf of the panel previously worked, this would destroy the insulation of each panel, and be in direct opposition to the principle laid down by Mr. Buddle. The advantage gained on the one hand, would be the saving of coals by the crush of the pillars in the second break, while, on the other hand, you have the disadvantage of a communication between the goaves of the two panels, and the probable large discharge of gas from the goaf of the abandoned panel.

Mr. Barkus—You would have to take away the barrier at sometime or other, when a communication would then be made. My experience is in favour of extending the old goaf, and not leaving any barrier between that and the goaf in the course of formation, by beginning on the left hand side, and pursuing the working towards the right hand. Com-

mencing on the right hand side there will be a large open space, and, consequently, a large accumulation of gas, before the roof breaks down, which is avoided when the working of pillars is continuous, as it would be if they were commenced on the left hand side. If an explosion did take place in the panel, plan 1, I think the effect would be more disastrous if the left hand barrier was complete, than if there was a communication between the two panels. In the former case it would he like shooting through two gun barrels (which the double headway would represent), whereas, if there was a communication through the barrier, or a portion of it taken away, the explosion would expand itself into such panel, and the effect would be less disastrous. He had, himself, been called in to examine an explosion which happened in a similar district to this, where all the horses and men were blown to the end of the barrier in a mass together.

Mr. Atkinson and Mr. Liddell concurred with Mr. Barkus in opinion that the effect of an explosion would be diminished by its being allowed, by the working away of a portion of the barrier, to expend itself into the old goaf. So far the working away of the barrier and commencing to work pillars on the left hand side might be advisable, though it would subject the working of the panel, plan 1, to all the discharge of gas from the old goaf.

Mr. Wales—It must be observed that the right hand side of the panel is the rise side, and it is always desirable to keep the goaf containing gas on the rise side of the workings. If the goaf is on the dip side, and the pillar working on the rise side, there is always the tendency of the gas to flow upon the working places.

The President—It was often remarked at the commencement of the panel working, that the pillars were left of insufficient size, and that the barriers around the panel were for the purpose of stopping creeks or thrusts which might be occasioned by an inadequate initial strength of such pillars to prevent such casualties, and that it would have been better to make the pillars themselves of adequate strength, and not have recourse to the barriers. But the use of barriers did not necessarily imply that the pillars should be of inadequate strength, though there is no doubt that, in the first instance, such was generally the case. Panel working, was, however, most extensively practised in the trade, particularly in the collieries under Mr. Buddle's superintendence.

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NORTH OF ENGLAND INSTITUTE of MINING ENGINEERS.

ADJOURNED MEETING, THURSDAY, JANUARY 6, 1859, IN THE ROOMS OF THE INSTITUTE, WESTGATE STREET, NEWCASTLE-UPON-TYNE.

Nicholas Wood, Esq., President of the Institute, in the Chair.

The President—This is an adjourned meeting for the purpose of concluding the discussion on Mr. Wales's paper on the Ventilation and Working of Mines. The discussion on the 2nd December last terminated diagram VII; we shall, therefore, commence with the discussion of diagram VIII.

Remarks on Diagram VIII. were then read.

Mr. Wales—Plan No. 1, diagram VIII., is a mode of working pillars at the same time that the boards in the whole mine advance, keeping in view that there should not be less than two pillars standing between the working with the lamps in the pillars and the naked lights m the whole mine. It will be observed that there are two doors placed at A for the purpose of allowing the coals to be brought out of the pillar working into the main rolley-way. If those doors are neglected or left open, then the air between D S and C might become stagnant, and in the case of there being a discharge of gas in the roof, it might back to the candle at D up the board C D.

Mr. Atkinson—If the gas was very pure, it might back along the roof above the small quantity of air which might, under such circumstances, be circulating from D to C at the time.

The President—Then this is considered an objectionable plan in

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consequence. We shall now go on with the discussion of plan No. 2, which, I understand, is a mode of working in which such an objection does not exist.

Mr. Wales—The arrangements of working in this plan are similar to No. 1, the only difference being in the ventilation. By referring to this plan, it will be observed that the air is divided at E, one portion going to ventilate the whole mine, and the other the pillar working. By this arrangement, the doors at A, No. 1, are dispensed with, and the risk obviated by such doors being neglected or left open. It is, therefore, safer in this respect than No. 1. But suppose any accident should occur to the doors G D and H D, by which they were destroyed or left open, or should the pillars in which they are placed be worked away before any arrangements had been made in the adjoining pillars to keep the air going along the headway from E to F, then the air between G D and F 0 would become stagnant, and the gas or inflammable air from the goaf might expand and back in the direction F B to the stopping at S C. It then becomes a question of some importance in what direction, under those circumstances, the air will press through the stoppings S C. If towards the whole mine current, in such a case the gas might be ignited by the candles, and the most disastrous result might be the consequence; - or if towards the pillar working or the current E F, then no harm would ensue, safety-lamps alone being supposed to be used. I have stated, as my opinion, that the air would press through the stopping S C from the pillar working into the whole mine, and if so, it is of the utmost importance that the air should always be constantly directed to the point F 0, and so, towards the pillar working.

Mr. Wales, at this part of the discussion, introduced a diagram for the purpose of establishing his opinion, that the air would press through the stopping S C towards the whole mine. He stated that, at equal distances from E (point of division of the currents), the densities in both the whole mine and the pillar current would be equal; and that, on referring to diagram VIII., such distance would be at A and X respectively, (X being a point in the whole mine current the same distance from A as B is from A, and which would be in the middle of the second board to the right of P, or to the middle of the third board to the left of S C, diagram VIII., No. 2), consequently at B there is an excess of density above that at S C, equal to the distance of X from S C, and, therefore, the air will press through the stopping C from the pillar working into the whole mine.

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Mr. Atkinson here handed in a paper, which he said was on this special point, which he had prepared since the last meeting, and which was read, as follows:—

Mr. Atkinson—In the paper now under discussion it is stated, in reference to plan 2, diagram VIII - "If we suppose the existence of a scale in the stopping C, the question would then arise, whether the air would pass through the scale at C upon the candle, or whether it would pass from the whole mine into the broken? This is rather an important question, for, should the air from the goaf pass through the scale upon the naked light, it would certainly evidence a very deficient system of ventilation.

"As we have already observed, the distance to the stopping C, in each division, is equal, and it will, therefore, be obvious that the two columns of air will be equally balanced. Should any obstruction, however, take place in either of these divisions of air, then the air at the stopping C would press into the division in which the obstruction occurred; but, everything remaining in order, the tendency of the air will be to press from the goaf into the whole mine, and such a mode of ventilation must, therefore, be admitted to be undoubtedly objectionable."

I think it desirable to make a few remarks on the above paragraph, because it seems to me to be at least ambiguous, if not self-contradictory.

If it were obvious, as is stated, that the two columns of air would be equally balanced, it is difficult to understand why the tendency of the air to press from the goaf into the whole mine should be greater than its tendency to press from the whole mine towards the goaf, in the opposite direction.

In the foregoing quotation it is also stated, in reference to obstructions taking place in either of the divisions of air, that the air at the stopping C would press into the division in which the obstruction occurred; but this is only true in cases of obstructions taking place in either split before the points C or B are reached, as the very opposite tendency would arise from obstructions occurring in subsequent portions of either of the splits.

An inference that might be drawn from the statements contained in the quotation is, that if the distances traversed by two currents of air, from the point where they are divided or split, to a stopping placed between them, are equal, then the pressure on each side of such stopping would be the same, and the air would have no tendency to press in either direction through a scale in such separating stopping; this is, however, by no means a matter of course, and several other conditions require to

be considered in addition to the mere distances from the splitting point to such stopping before it can be determined whether the air on each side of the stopping would be balanced, or on which side of it the greatest pressure would exist; and, in fact, the distances to be traversed by each split, after passing such a separating stopping, have as much influence in determining the question as those parts traversed by them before arriving at it.

The question as to the direction in which air will press through communications, connecting two splits or currents of air in a mine, is one of very great importance; and I, therefore, think it desirable that the general principles, on which all such questions depend for their solution, should be expounded in this discussion, so as to be circulated with the paper itself ; with this object in view, I beg to submit to the meeting a general rule on the subject, together with one or two suppositious cases, chosen to illustrate its application, before applying it to the particular case alluded to in the quotation already cited.

It may be regarded as a general rule, that, *when a communication is opened in a stopping, separating two splits of air in a mine, the air will press through the opening in such stopping from that split or current, which, on reaching the line of communication in which the stopping is placed, has encountered the smallest proportion of the total common resistance that has to be encountered by each of the splits in the airways traversed by them, after passing the point where the split takes place, and before reaching the point where they again re-unite*.

It arises from the principles involved in the above general rule, that the question as to the direction in which air will press through an opening in a stopping, connecting two splits of air in a mine, depends, more or less, on all the following conditions:—

1st.—The distance from the splitting point to the line of communication in which the stopping is placed, by the route which each of the splits traverses.

2nd—The distance in the routes of each of the splits from the line of communication in which the stopping is placed to the point where the splits are re-united.

3rd—The areas and perimeters of the sections of each of the air-ways, traversed by each of the splits, and the nature of the material composing the internal surfaces of the air-ways in each of the splits, as regards the frictional resistance they offered to the air in them.

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4th—The number, dimensions, and positions of the regulators in each of the splits.

5th—The temperature of the currents of air in each of the splits, in all parts of the air-ways where ascents or descents are met with, together with the extent of each of such ascents and descents.

6th—The quantities of air belonging to other splits by which either of the splits under notice are accompanied in any parts of their route.

This question is affected by a few other conditions, but, as their effects on the ventilation of mines are generally insignificant, it is not necessary further to advert to them in this discussion.

In the examples it is now proposed to give, in illustration of the general rule which has been enunciated, let the following conditions be presumed to exist.

1st—That each split is confined to a single air-way over the whole of its route, reckoning from the splitting point to the point where the currents meet again.

2nd—That the air-ways of each of the splits have one common area and perimeter of section over the whole extent of their routes, so that the resistances are proportional to the distances traversed by the air in the respective splits.

3rd—That no regulators, or other local obstructions, exist in either of the splits.

4th—That no other currents of air unite or mix with the currents of either of the splits, while in their separate state.

5th.—That the whole of the routes pursued by each of the splits are situated in the same horizontal plane.

Let Fig. 1 represent a part of the workings of a mine in which the above conditions exist.

[Fig. 1]

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Then, if a current of air was divided into two splits or currents at the point A, one split passing on from A to B C D E, and under a crossing at F, to Z, and there reuniting with the other split; which, after separating from it at A, had passed through F G H I K L M, and to the same point of reunion at Z; and suppose the dotted line C H to be the line of communication, and the stopping C; then, although the distance from A to B and C, and thence through the assumed communication to H, as shown by the dotted line C H, is exactly the same as the distance from A, through F and G, to the same point H by the route of the other current or split; yet it would be found, on opening the new communication from C to H, that these two currents or columns would not balance each other, but that considerably more motive tension would prevail at the point H, in the split having the longest subsequent route, than in the split having the shortest subsequent route; and that, therefore, a current of air would be established from the point H towards the point C, as soon as the communication was opened between them.

This result is clearly accounted for, on the principles indicated by the general law before alluded to, in the following manner: - The air of the long route A F G H has, at the point H, encountered a *smaller proportion of the entire common resistance* due to each of the splits, in its passage from A to Z, than has been encountered by the air of the other split A B C H, in passing from A through B to the point G H. On examining the distances, it will be seen that the air in the long split has, at the point H, passed over a distance of 15 parts, out of a total of 54 (the total distance by the long route from A to Z); while, in the short split, the point H has been reached by passing over 15 parts out of a total of 28 (the total distance by the short route from A to Z); so that the former current has expended 15-54ths, and the latter only 15-28ths of the entire common resistance, due to the whole of their respective routes, in passing from A to Z; and therefore the former has 39-54ths of the motive tension due to the splits in reserve at the point H, while the latter has only 15-28ths of the same common motive tension in reserve at the same point, and hence the former must overcome the latter, and create a current from H to C through the proposed communication.

It is, perhaps, superfluous to remark here that the entire resistance encountered by each of the splits, in passing from A to Z, must of necessity be the same, seeing that the two currents have each the same common tension at the splitting point A, and also at the point of reunion Z, where they form a common aeriform medium.

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In the ordinary pit phraseology of this district, the result just noticed would be expressed by saying that the air going from A, through F and G, to H, would naturally return by the shortest route, through C D and F, to Z, in preference to following the longer route, from H, through I K L and M, to Z; but this mode of accounting for the result is objectionable, inasmuch as it attributes it to an imaginary cause, different from the true one, and it is easy to point out circumstances or conditions under which air will press from a split having a shorter, to another having a longer, return; and the following example will suffice to show this.

In Fig. 2, let the general conditions, already stated, be presumed to prevail:—

[Fig. 2]

Suppose the intake air after passing from M to A, to be split into two currents, one passing through B C D E F and G, to the point of reunion at Z, while the other split, starting from the same point, A, passes through H I and K, to the same point of reunion at Z; then, if a communication were opened from the point I to D, or a stopping C, as in the diagram, the result would be that a current of air would press from I to D, where it would join the split passing from A through B and C, and with it would return from D through E F and G to Z, the point of general reunion, where it would be joined by the air passing from I through K, to the same point Z, and with it would pass out in the general return through L.

In this example we have air pressing from I, through D E F and G to Z, a distance of 16, in preference to passing only a distance of 4 from I through K to Z, and the air passing from A B and C, on reaching D has also two routes offered to it, one a distance of 14, through E F and G to Z, and the other a distance of only 6, from D through I and K to the same point Z; and yet it will pass along the longer route, and avoid the shorter one. This is strictly in accordance with the general rule already laid down, and is explained in the following manner.

The short split in passing from A, through H to I, has, on reaching I, overcome the resistance due to 2 parts out of 6, (the entire resistance it has to encounter in passing from A to Z;) and therefore, it has in reserve at the line of communication, I, 4 parts out of 6 of the total

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motive tension required to propel it from A to Z; while, on the other hand the air of the longer split, on reaching the line of communication at D has encountered 16 parts out of 30, (the entire resistance that has to be encountered in passing from A to Z) and has, consequently, in reserve, at the point D, only 14-30th parts of the common resistance, (to be encountered by each of the splits in common), which, being less than the 4-6th, or what is the same, 20-30th parts of the same resistance in reserve at the point I, in the shorter route, it necessarily follows that a current will be established from the latter towards the former, through the line of communication.

The general law which has been stated to operate in deciding the direction in which air will press through a communication connecting two splits of air in a mine is not limited in its application to cases where any particular series of conditions happen to exist, but is of universal application to all cases, provided only that the conditions before enumerated as necessary to the determination of the question are known.

Before, however, it is possible to decide as to the direction in which air would press through a scale in the stopping C, shown in plan 2, diagram VIII, of the paper under discussion, it is absolutely necessary to assume certain conditions, as to which the paper itself is silent, because by altering the conditions alluded to, the direction in which the air would press might become reversed.

Let it, therefore, in this case be assumed -

1st - That each of the galleries or air-ways have the same uniform area and perimeter of section and the same character of internal surface throughout their extent, so that equal distances in single air-ways offer equal resistances to equal quantities of air passing in the unit of time.

2nd - That the goaf and adjoining air-ways, taken together, present, over equal distances, the same resistances as would be due to the air passing along a single air-way of the standard dimensions. This is equivalent to presuming the air-ways in the broken to require the area offered to the air by the edge of the goaf in order to reduce their resistances to that of a standard air-way.

3rd - That the temperature of the air in each of the splits is the same throughout their extent, or, on the other hand, that their routes are both perfectly level.

4th - That the regulators X and Y are so opened that equal quantities of air pass into each of the two splits.

5th—That the air returning from the district lying to the right hand

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of or beyond No. 2, be exactly such as to occupy one of the two returns at the foot of the panel, leaving the other return free for the whole way air, which may thus be treated as being confined to a single air-course throughout its extent. Then the following are the distances traversed by each of the splits, and the results in accordance with the foregoing rules:—

"*Whole Coal-way Air*"

[Calculations]

"*Pillar Working Way*"

[Calculations]

But since the conditions, in this case, are, that the openings in the

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regulators X and Y are such as to cause equal quantities of air to pass in each of the splits, it is necessary to suppose the opening in the regulator at X to he so much less than that at Y, as to create an excess of resistance equal to that of an air-way of the standard size, having a length equal to the differences of the virtual distances, or of 4750 - 2831 = 1919 links.

On the supposition just noticed, we find that the whole way air has, on reaching the point C, encountered 1550/ 4750 parts of its resistance, while, at the point B, at the other end of the line of communication, the broken way split has only encountered 431/ 4750 parts of the same resistance; so that the whole way air only has in reserve at C 3200 parts, while the broken air at B has a reserve of 4319 parts, out of 4750 parts, of the common resistance due alike to each of the splits; and hence the air would pass from the broken into the whole, or from B to C, if there was any escape or scale of air at the stopping C.

Mr. Wales expressed general concurrence in the views enunciated in Mr. Atkinson's observations, and took occasion at the same time to repeat his former observations with regard to the pressure at the stopping C, viz., that if an obstruction occurred at P the pressure would be in the direction of the whole mine, but if at Q the pressure would then be towards the pillar working.

The President—Mr. Atkinson's exposition of the principles which regulate the scales or pressure of air from one current to another in different parts of the route from the downcast to the upcast, or from the splitting of currents to the points where they again reunite, is quite clear. In the case of one single current from the downcast through the workings to the upcast (in furnace ventilation), we have a certain tension of the air at the bottom of the downcast and a less degree of tension at the bottom of the upcast, (the difference of the weight of the two columns being the motive power), the degree of tension at any other point in the route will be in proportion to the distance traversed by such current of air from the bottom of the downcast towards the upcast shaft; and, if we have two currents separated, or a split at the bottom of the downcast shaft, and reuniting again at or near the bottom of the upcast, then the degree of tension of each current in any part of the route is, as before stated, proportional to the distance traversed by each current from the bottom of the downcast as compared with the entire distance. If we,

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therefore, make a communication between those two currents in any part of the route, that current which has traversed the shortest comparative distance from the downcast to the upcast, will press towards that current which at that point has traversed the greatest comparative distance - supposing the resistance in both cases throughout the respective routes is uniform throughout the entire distance. But suppose one current to have a shorter distance to travel than the other, then the pressure of air will be towards the short current, until the increased velocity, and, of course, increased resistance of the air along the short current compensate for the extra length of the longer current, when they will become balanced. But if we wish the velocity of the air to be the same in both cases, then a regulator must be placed in the short current, when the increased resistance of the air through such regulator will compensate and balance the extra length of the long current, and the tension of the air of the short current at the regulator, or before passing through it, will be the same as the tension of the air of the long current, at the same distance from the downcast shaft, or from the split of the two currents in both cases, and the tension of the air of the short current from the regulator to the upcast, or to the reunion of the two currents, will be precisely the same as that of the long current at the same distance from those points; the obstruction of the regulator being equal to the resistance of the difference of the length of the two currents. Then the direction of the scale of air through any stopping separating two currents of air, may be easily determined by attention to those principles.

A discussion of some length ensued amongst the members present, both on the case of the diagram before them and also on suppositious cases, but as all the conclusions resulted in the recognition of the principles laid down by Mr. Atkinson, it is not necessary to give the details of such discussion.

Mr. Dunn—Let us discuss the practicability of bringing a portion of the air of the whole mine current, after leaving those workings into the pillar working, through the stopping B, No. 2, diagram VIII., instead of allowing it to pass to the upcast shaft by the regulator V.

Mr. Wales—This cannot be done if the quantities are to remain the same in both currents.

Mr. Dunn—Assuming you have 10,000 cubic feet of air per minute passing into the whole mine, along the headway A D P, and 10,000 cubic feet into the pillar working from A to B, could we not, by opening the stopping at the right hand of B, closing the regulator Y to a certain

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extent, and opening the regulator X a similar extent, abstract 5,000 cubic feet from the current A D P, cause it to pass through the stopping B, and so join the pillar current of air, which would give 10,000 passing through the whole mine, 15,000 through the pillar working and regulator X, and 5,000 through the regulator Y, making the aggregate quantity at E as before, 20,000 cubic feet per minute?

Mr. Wales—I do not think this can be accomplished and allow the currents A D P and A B to be equal, viz., 10,000 cubic feet each, the total quantity at E being 20,000 cubic feet per minute. Let us consider how matters stand before we commence. It is quite clear the pressure of air through the stopping B is from the pillar working towards the whole mine current. We must, first of all, therefore, balance this, which will be done by contracting the regulator Y, and opening the regulator X. Such obstruction of Y and opening of X, will, of course, have the effect of diminishing the whole mine current and increasing the pillar current, the latter being the shorter current, probably 1,000 cubic feet would balance the two currents at B ; then we should have 9,000 through the whole mine, and 11,000 through the pillar working. Now, if we wish an additional quantity, say 4,000 feet more, through the pillar working, then we must further close the regulator Y, and open the regulator X, and so allow 5,000 to pass Y, and 15,000 to pass X. Then if we open the stopping B, what will be the effect? You will not have 10,000 passing into the whole mine from A to D P and C, with 5,000 passing through B, and the remaining 5,000 through the regulator Y. But you will have a larger quantity passing through the pillar working, and a less quantity passing through the whole mine workings. For this reason, when 11,000 was passing through the former, and 9,000 through the latter, the resistance of both currents was supposed to be equal, or they were balanced; if we, therefore, abstract 4,000 from Y, and give that or 4,000 more to X, as both currents were previously balanced, it will divide itself between the two currents, in the ratio of the resistances of the two currents, and as the whole mine current is the longer one, the greater proportion of the 4,000 will pass along the pillar or shorter current.

Mr. Anderson—It is quite clear that the quantities of air will not divide equally at E if you make a communication between the two currents at B.

The President—I quite agree with Mr. Anderson, that you cannot have the same quantities in each division if you make a communication

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through the stopping at B. The case is that of two currents split at E and reuniting at B, the tension of the two currents being the same at E in splitting, and again in reuniting at B, and they are currents of unequal length; it is clear, therefore, the greater quantity of air will pass along the shorter current, according to the well-known principles of the resistance of air in mines.

Mr. Dunn-—If I place doors at Q, I can keep the requisite quantity of 10,000 cubic feet through the whole mine, 15,000 cubic feet through the pillar working, and 5,000 cubic feet through the regulator Y.

Mr. Wales—Certainly you can; but we presumed that no doors were to be allowed in this case, otherwise it would be the case of No. 1, diagram VIII., which has already been discussed and found objectionable.

After some further discussion, in which Mr. Hall, Mr. Potter, and other members took part, the portion of the remarks on No. 3, diagram VIII., was read.

Mr. Wales—Plan No. 3 differs from Nos. 1 and 2 in this respect, that the ventilation is so arranged that a greater distance intervenes between the workings with the candles and the workings with the lamps, with an intervening current of air interposed between those two sets of workings. The ventilation of No. 3 is much the same as No. 1 in principle, viz., the whole of the air is first of all passed through the whole mine, and then brought into the pillar working, and so to the upcast. In proceeding with the removal of the pillars behind the whole mine, great care should be exercised in having as great a distance between the naked lights and the lamps as possible. In this case the air is taken direct to the face of the whole mine, and after ventilating the various places, it arrives at F. It is then taken one pillar down to R, along the headway to B; then another pillar down to C, and thence along the headway to D; after which it is taken to ventilate the pillar working. By this arrangement the distance is greatly increased between the places where the naked lights are used at F in the whole mine, and where the safety-lamps are used in the pillar working, compared with plan 1, where the distance is only two pillars. The doors are likewise so arranged that if they are accidentally left open at X, the air is still kept on the edge of the goaf at S, which prevents the gas from backing out of the goaf. Plan No. 3 I consider a much safer system, as regards ventilation, than either No. 1 or No.2.

The President—I have always considered it absolutely necessary in forking the pillars immediately after the whole mine, that the distance should be such as to admit of an insulating current of air being inter-

posed between the whole mine or candle workings, and the pillar or lamp workings, in every case where gas exists. I recollect of giving very strong evidence of the necessity of such a precaution before a committee of Parliament, several years ago, and of pointing out that, unless there was interposed between the two, a current of air which no possible sudden outburst of gas from the goaf could back, or which could by possibility reach the candle workings, it was not safe to use candles in the whole coal working, the pillar working being followed up immediately afterwards. The double current in No. 3, however, accomplishes this desideratum.

Remarks on Diagram IX. were then read.

Mr. Wales—This diagram represents a field of coal lying in a horizontal position from A to B, the full rise being from B to C, and differs from diagram VIII. in the rolley-way being made in the middle, and working to the right and left - the objections to which are stated in the explanation of the diagram.

Mr. Barkus—I think the mode of working which involves the two goaves meeting in the centre of the panel very objectionable, as the coal is very much crushed and injured when the goaves approach each other.

Mr. Wales—It will be observed that the boards in this case have been driven to the barrier before the pillars are commenced to be removed. I think this a better system than that of following up the pillar working as the whole working proceeds, as in the last diagram, leaving only five or six walls for the protection of the air-courses and horse-roads, as such walls are often rendered useless by creeps. It will be observed that I have assumed a communication to be made through the barrier at C, for the purpose of allowing water to pass from the whole mine workings to the main water level A B; a question arises, as in diagram VIII., if the gas from the goaf will press through the stopping C, the distances from A B C and from A D C being equal, which would imply a balance of air. But as the seam rises from the goaf to C, there is no doubt that the light gas would rise towards and press through the stopping C towards the whole mine. If we, however, wish to use this communication for the purpose of allowing the escape of gas from the goaf, which, in some cases, it may be desirable to do, we have only to remove the stopping immediately above C to the left hand side of the dotted communication C, when the gas would go into the return air from the whole workings.

The President—We will discuss the question of going to the extre-

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mity of the royalty before commencing to work the pillars, and, also that of having the rolley-ways or main-roads laid out in the middle of the panel, or on one side, when we have considered the two next modes of working.

Remarks on Diagram X. were then read.

Mr. Wales—Plan 1 is supposed to represent a district of coal, the roof of which, is composed of soft material; and to avoid the expense of ridding and driving jenkins, every alternate board is driven narrow, so that the working of the pillars is performed more economically. With regard to the ventilation, it is similar to diagram VIII., No. 3, except that there is a solid barrier of coal between the whole and the pillar working, which is a greater security than stoppings.

Mr. Dunn—This would appear to be a costly system, having so much narrow work.

Mr. Wales—The pillars, you will observe, are 60 yards long, consequently, there is very little more narrow work in this case, than in the ordinary mode of working with pillars 30 yards long. In determining on the plan of working, we must not be guided solely by the first cost; we must take into consideration the comparative cost of narrow work, in the first instance, with the cost of ridding, jenkining, and the deterioration of the coal in size in jenkining.

The President—With regard to the system of working and ventilating, this appears to be substantially Mr. Buddle's system of panel working; each barrier K being the panel barrier, the working of the pillars being followed up from the end next the pit in plan 1. But it is not, I presume, essential to the system, that this should be done, the working of pillars being shown, in plan No. 2, as taking place at the top of the panel. With regard to the propriety of driving every other board narrow, that would not of course, be done, unless such a mode was justified by the circumstances pointed out by Mr. Wales. I may observe, that the letter M should be substituted for L in reading the description of the diagram.

Mr. Wales—The plan No. 2, is supposed to have a strong roof, consequently, the boards are all driven wide; the pillars, as in plan 1, being 60 yards in length. In this plan the ventilation is, however, different; a current of fresh air being taken to both the whole and pillar working, whereas in plan 1, the air from the whole mine is taken into the pillar working.

Mr. Barkus—I do not think it necessary to have a separate division

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of fresh air for the whole and for the pillar working, when you use the safety-lamp in the latter case.

Mr. Wales—I think if there is sufficient air in the pit, it is desirable that the workmen should have fresh air in both; besides, the ventilation of the pillar working in No. 2 does not depend upon the doors D D G, as in plan 1.

Mr. Barkus—The plan of working in No. 2, is the same as diagram IX., viz., the two goaves uniting in one point, which, I consider, very objectionable.

Mr. Wales—In this case the pillars are made of sufficient dimensions to resist any pressure brought upon them by the operation of working. I do not think, therefore, that with pillars of such strength, and with coal of the ordinary strength, there would be much fear of creep; but should the coal be of a tender nature, I would proceed as in plan 1; commencing at the low end, and continue the working until the goaf reaches the barrier F. But, in this case, another crossing would be required at M No. 2, similar to L; the opening G would have to be closed, the stoppings at H and M taken out, and the air divided at H. I here beg to remind you that plans Nos. 1 and 2 are supposed to be on the dip side of the shaft, and the workings proceeding towards the dip, and that an opening is made in the stoppings S S, on the rise side of each panel, for the purpose of allowing the gas to escape from the goaf into the return.

Remarks on Diagram XI. were then read.

Mr. Wales—This diagram differs from the former in the workings being driven to the boundary before commencing to work pillars, and the ventilation is also somewhat different as shown by the darts.

Mr. Barkus—Such a system of going to the extremity before commencing to work pillars would require great patience, and would not, I think, be generally applicable.

Mr. Wales—In pits of great depth, and where the coal is naturally tender, it is almost impossible to leave barriers of sufficient strength (between the goaf and the rolley-way) to resist the pressure of the superincumbent strata. In such cases, and where the boundary does not extend more than three-quarters of a mile from the shaft, I would certainly recommend diagram XI. in preference to diagram X. It would, no doubt, require patience to prosecute the workings to the boundary first, but, ultimately, great advantages would, in my opinion, be derived by such a mode of working a coal mine.

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The President—We are now in a condition to arrive at some conclusion as to the policy of going to the extremity of the royalty before commencing to work pillars, or to work pillars as the exploring drifts progress thereto. Diagram No. XI. shows a mode of pushing the main roads to the boundary and then commencing to work pillars; and diagram X. a mode of taking off the pillars in the progress of approaching the extremity of the royalty. The objection to the former mode is, that a considerable time must elapse, especially in large royalties, before the leading places reach the extremity of the royalty; and that it is necessary, in a commercial point of view, to have the colliery in full working at as early a period as possible. The objection to the latter mode is, as stated by Mr. Wales, that in pits of great depth it is difficult to leave barriers or walls of sufficient strength to prevent them being crushed, and so to waste and diminish in value a considerable portion of coal. The measure, therefore, of the policy of adopting the latter mode depends upon the nature of the coal, its depth from the surface, the description of roof, &c. But, if it be practicable to leave pillars of sufficient strength to resist the superincumbent pressure without crushing the coal, then the objection to working pillars in the progress of pursuing the workings towards the extremity of the royalty, in preference to reaching that point before commencing to work pillars, substantially vanishes, and we obtain what is generally advisable, a full pit working of coal at a much earlier period. The propriety, therefore, of adopting either the one or other of these modes, or modifications thereof, must be left to the viewer in charge of the mine. With regard to the propriety or otherwise of having the main roads or rolley-ways carried forward in the middle of a sheth of workings, as shown on diagram IX., or carried forward on the one side thereof, depends upon the same contingencies, viz., the practicability of adopting the former mode without injury to the pillars of coal required to be left on each side of such roads for their support. It is, no doubt, desirable, if it can be accomplished, as concentrating the workings, and having one line of main roads instead of two.

Mr. Daglish—At Seaton colliery, which is upwards of 200 fathoms in depth, we have to leave two walls on each side, of 40 yards each, to prevent the pressure from affecting the horse-roads and the two air-ways, one on each side, notwithstanding which, we find the air-ways affected by the pressure.

Mr. Wales —In the main roads to the dip of the Eppleton pit, at Hetton Colliery, we have the main rolley-way and two return air-ways

on each side, and we leave the walls between the main way and the return drifts 22 yards thick, in addition to which a barrier of 60 yards on each side, next the goaf - the depth being about 200 fathoms - and we find the coal not crushed when the pillars are taken away on each side.

The President congratulated the meeting on the utility, in a practical point of view, of such papers and discussions, and he trusted that the members would contribute to the Institution any matters of such a character elicited in the course of their avocations.

The meeting then adjourned.

NORTH OF ENGLAND INSTITUTE

of

MINING ENGINEERS.

GENERAL MEETING, THURSDAY, FEBRUARY 3, 1859, IN THE ROOMS OF THE INSTITUTE, WESTGATE STREET, NEWCASTLE-UPON-TYNE.

Nicholas Wood, Esq., President of the Institute, in the Chair.

The Secretary having read the minutes of the Council, the following gentlemen were elected members of the Institute: - Mr. John Kerr, Duke of Hamilton's Coal-works, Lismahagow, N.B.; Mr. Thomas Hall, West Hetton Colliery, Ferryhill; Mr. John S. Arthur, West Hetton Colliery Office, Ferryhill; Mr. Wm. Morris [alteration made by hand], Waldridge Colliery, Chester-le-Street; Mr. Alex. Jones, Prior's Lea, Shiffnel, Salop; Mr. Isaac Shore, Brymbo, Wrexham, Denbighshire; Mr. Mark Fryer, Eighton Moor Colliery, Durham.

The President then stated that the first business of the meeting was the discussion on Mr. Atkinson's Paper, read at the Meeting of May 13, 1858, and asked if Mr. Atkinson had any further remarks to make on the subject.

Mr. Atkinson—The paper alluded to, and now under discussion, was intended as the solution of a question that arose in discussing the practicability of ventilating the workings of coal mines, by different splits of air traversing routes of different lengths, by means of regulators placed in those routes offering, in themselves, the smallest resistances to the passage of the requisite quantities of air through them, in order to avoid the use of a great many of the principal doors employed in the more ordinary system of ventilation. - Page 163, Vol. VI.

The question was, whether, in the event of the gross quantity of air

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circulating in the unit of time, in a mine so ventilated, becoming reduced in amount, while the air-ways, forming the whole of the splits, together with any regulators placed in them, remained unaltered, those splits having the longest routes to traverse would or would not continue to be traversed by as large a proportion of the reduced gross quantity of air, as they had obtained of the original gross quantity, previous to such reduction in its amount.

As an answer to this question the reasoning in the paper was intended to show -

1st - That if the whole of the workings of a mine, so ventilated, were situated in the same horizontal plane, the whole of the splits, whatever might be their relative lengths, would continue to be traversed by the same constant proportion of the gross quantity of air circulating, whatever might be its amount, so long as the air-ways forming the splits remained unaltered.

2nd - That when the air in the intakes is cooler and denser than that in the returns, those routes having the greatest amount of rise will obtain a decreased share or proportion of the gross quantity of air circulating in a mine, in the unit of time, as it becomes reduced in amount, and, consequently, an increased share or proportion of the whole as it becomes increased in amount, so long as the air-ways and regulators of all the splits remain unaltered; and that the very opposite results would ensue if the air in the intakes was warmer and less dense than that in the returns.

3rd - That when the air in the intakes is cooler and denser than that in the returns, those routes having the greatest amount of dip will obtain an increased share or proportion of the gross quantity of air circulating in a mine, in the unit of time, as it becomes reduced in amount, and, consequently, a decreased share or proportion of the gross quantity, as it becomes increased in amount, provided the airways and regulators of all the splits remain unaltered ; and that the very opposite results would ensue if the air in the intakes was warmer and less dense than that in the returns.

The President - The following experiments have been made at Hetton Colliery, to test Mr. Atkinson's theory, by Messrs. Wales and Lindsay Wood, at some of which Mr. Atkinson was also present. The experiments were conducted much in the same manner as those made by Mr. Berkley at Crook Bank Colliery, and given at page 183, vol. VI, of the Society's " Transactions," and also as those made by Mr.

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Cooper at the Grange Colliery, and given at page 229 in the same volume. The experiments were, as those of Mr. Berkley, extreme cases, not likely to occur in practice, but made to test the accuracy, or otherwise, of Mr. Atkinson's theory. The details will, of course, be given in each experiment, but it may be stated generally, as the principle on which all the experiments were conducted, that portions of each pit were selected as near the downcast and upcast shafts as possible, and where a very short route could be obtained between the intake and return air; to contrast the effect by such short route with the longer route of the passage of the air along extensive drifts and workings between the point of splitting of such air from the short current and its reunion again with the same current. A regulator was put in the short route, so that the quantity of air passing into both routes could be varied at pleasure; and a main regulator was placed between the downcast shaft and the point where the air was split in the intake current to vary the aggregate quantity of air passing into the mine or into both splits. Places were selected in both routes, where the passage through which the air passed along both was contracted so as to make it precisely the same area in each route, and within such area the anemometer was placed to measure the velocity of the air. Generally, the regulator placed in the short route was so adjusted as to cause the same quantity of air to pass along both routes, the main regulator being full open. The main regulator was then successively closed to cause a less aggregate quantity of air to pass into the mine, and so to test the effect with such variable quantities of air passing into the two routes. The quantity of air passing was ascertained by Biram's anemometers, the revolutions of which, are proportional to the velocity of the current. Several experiments were made on different occasions, but the following are those which presented the most uniform results, and which were carefully performed.

Figures Nos. I., II., III., and IV. are diagrams which show the distances which the air travelled in the course of the experiments. They do not show the whole of the passages or drifts through which the air passed, more particularly in the return current; but they show the relative distances between the short and long routes. The following short description of the several cases will, however, give a general idea of the variations from a single route, and enable any one desirous of making the requisite calculations to estimate the effect of such variations upon the theoretical results.

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EXPERIMENT No. I.

In the intake current of this experiment the whole of the air is in one current, comprising four splits, with a single passage from the downcast shaft to *A*, at *A* the short split proceeds to the upcast shaft along *A B*. From *A* to *a*, there are, as shown on the diagram, three currents, at *a*, two of these currents proceed to *b*, where they separate, one passing to *x*, and the other to *y*. The other split at a proceeds to *y*. All those splits or currents are single, and confined to one ingoing passage respectively, except that from *b* to *W* this split is single about half way, and double the remaining distance, but no bords, from the shaft to *W x* and *y*. The return currents may be explained as follows : - The current from *y*, is split at the extremity, one current proceeding to the right and the other to the left, as shown on the diagram : the right hand current has three openings or air-courses, and the edge of the goaf to return along to *a*, and the left hand current has two air-courses and the edge of the goaf until it reaches the same point, when both currents join the return currents from *W* and *x* at *c*. The return air from *W*, has three or four bords or air-courses and the edge of the goaves from *W* to *d*, where it meets the return currents from *x*. The return air from *x*, is divided at the extremity, one to the right, the other to the left. In both cases the return air-courses consist of two walls and the edge of the goaf from *x*, to *d*, where they join the current *W*. These united currents then proceed to *G*, and so travel to the upcast *U*. In their passage thither they have from three to five bords, and the edge of the goaf to spread within, but there is no coursing by stoppings. The long split has a considerable rise, (as shown by the dip and rise darts on the diagram) the short one being nearly level.

EXPERIMENT No. II.

In this experiment the air-course is single from the place of splitting at *A* to the extremity at *x*, the several intake drifts being closed by stoppings; the intake air was, therefore, a single current from *A* to *x*. The return air is split right and left at the extremity, having two main air-courses on each side, direct to the upcast shaft, as shown on the drawing. There were workings on each side of those drifts, and though the intake drifts into those workings were effectually closed by stoppings plastered with lime, there were no stoppings placed in the return drifts, any air flowing out of the workings into the main return drifts could, therefore, only be by the tension of the air in the workings expanding itself when

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[Diagrams]

Experiment No. I: Elemore Colliery

Experiment No. II: Jane Pit at Eppleton Colliery

Experiment No. III: Blossom Pit

Experiment No. IV: East Minor Pit

the pressure was removed, and which would cease at a certain time, the entrance of any air being effectually prevented by the stoppings in the intake drifts. The dip and rise arrows show the inclination of the seam; the long split having a considerable dip, and the short one nearly level.

EXPERIMENT No. III.

The intake current in this experiment, as shown in the drawing, is composed of three splits from the downcast to *a*, exclusive of the short split from *A* to *B*; then two from *a* to *b*, and single currents from *a* to *y*, *b* to *x*, and from *b* to *W* respectively. The return current from *W*, until it reaches *c*, is a single passage, without any connection with any workings. The return current from *x* has one air-course and the edge of the goaf until it reaches *c* and joins *W*. These two splits, up to *e*, have two air-courses from *c* to *e*. The return air from *y* is split right and left, and as there are about fifteen bords on each side from *y* to *e* and *f*, the air from *y* is carried around the face of these boards and the edge of the goaf (about 200 yards distant from *y e* and *y f*) to *e* and *f*. The three splits then proceed together to the shaft, across a panel of workings extending about ten pillars from *f U*, but those boards are much fallen, and the air is not forced into them, the air being taken along special air-courses of two or three openings to the shaft. The short split, which is nearly level, only passes from *A* to *B*. In the long split there is a considerable rise to *x* and *y*; *W* being nearly level with the splitting point.

EXPERIMENT No. IV.

The intake current in this case is a single air-course from the downcast shaft *U* to *a* and *x*. The return current has liberty to pass along three or four headways, and a narrow sheth of workings (not coursed) from *x* to *a*; then single for about 500 yards; and then across a panel of workings (not coursed) having three or four bords to spread itself, and so to the upcast shaft. The long split has a considerable dip as shown by the dip and rise darts in the diagram, the short split being nearly level.

In all the diagrams *R* represents the main regulator, or where the aggregate quantity of air admitted, into the various routes was regulated; *Θ Θ* the places where the quantity of air passing into the respective routes was measured; and *A B* the length of the short route from the point of splitting to the reunion of the two routes; *D* being the downcast shaft and *U* the upcast shaft.

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NO. I. SET OF EXPERIMENTS

Experiment made in the Hutton Seam, at Elemore Colliery, January, 1859.

Depth of Pit, 134 Fathoms.

[Table of results]

NO. II. SET OF EXPERIMENTS.

Experiments made in the Jane Pit, Hutton Seam, Eppleton Colliery, January 29th, 1859. –Depth of Pit, 174 Fathoms

[Table of results]

NO. III. SET OF EXPERIMENTS.

Experiments made in the Blossom Pit Main Coal Seam at Hetton Colliery, February 1st., 1859. – Depth of Pit, 110 Fathoms.

[Table of results]

NO. IV. SET OF EXPERIMENTS,

Experiments made in the Minor Pit, at Hetton Colliery, in the Hutton Seam, December 27th, 1858, and January 15th, 1859. – Depth of Pit, 150 Fathoms.

[Table of results]

The President—I shall now explain, as shortly as I can, the result of the foregoing experiments. It will be seen by Experiment I., that the regulator of the short current was so adjusted, that the velocity of the air passing around the long current, and that passing along the short current were precisely the same, viz., 400 feet per minute, which, through the area of 25 square feet, where measured in both currents, was 10,000 cubic feet of air per minute. This took place when the area of the main regulator was 45 square feet. The area of this regulator was then reduced to 16 square feet, when the relative velocities was 375 short current, and 350 long current : - at 6 feet area, which diminished the total quantity of air passing into the mine about one-half, viz., from 20,000 cubic feet, to 11,425 cubic feet, or as 1 : .567, the relative velocities, were, as, 242 : 215, or, as 1 : .9 ; or, in other words, when the quantity of air in the mine is diminished one-half, the quantity of air passing into the workings, or along the long route, is only diminished one-tenth, below that passing through the shaft door regulator, or along the short route. When the quantity of air passing into the pit is still further diminished, a greater disproportion exists; and, when only 2,750 cubic feet, or about one-fourth of the original quantity is allowed to pass into the pit, the result gives no perceptible current into the workings or long route, while the whole of the air passes along the short return, to the upcast shaft. Mr. Atkinson accounts for this stagnation of the air, in the long rise split, by saying, that the main regulator was so nearly closed, and, consequently, absorbed so large a part of the entire ventilating pressure, that the remaining part of such pressure, applicable to the splits, was no greater in amount than the opposing pressure, arising from the excess of the outward gravitation of the cool dense air, in the ascending intake, over the gravitation of the warmer and lighter air, in the descending return; and that the small pressure remaining due to the splits could no longer overcome it.

And it is easy to perceive that, by sufficiently closing the main regulator, it could be made to absorb so large a proportion of the ventilating pressure, that the remainder, left applicable to the air-ways forming the splits, should be less in amount than the opposing force, (in a rise way), due to the difference of the gravitation of the denser intake, and that of the lighter return ; and this would lead to the ventilating pressure being entirely overcome by the opposing force, so that the air-current would be made to flow in the reverse direction, and serve out of the intake. Besides, previously to the main regulator being reduced in area at each experiment, the tension of the air was at a certain standard,

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and that it required a lapse of time for that tension to adjust itself before the correct result was ascertained. Thus, when the main regulator was open at 6 square feet, the tension of the long current may be said to be represented by a pressure capable of producing a velocity of 215; it would, therefore, be some time before the tension of the air in the long current was reduced to the lesser pressure required to produce a velocity of only 110, the result of the 5th Experiment, and that the real relative quantities passing into the respective long and short routes could not be corrected until such an equilibrium existed; and, there can be no doubt, that until sufficient time elapsed, the experiment could not be said to be conclusive.

The opposite result seems to follow in Experiment II., the long split being in this case a dip-way; but, in this experiment, the quantity of air was not diminished more than about one-half; and, when the total quantity of air is diminished about one-half, the diminution in the short current is about one-sixth. In Experiment III., the diminution of quantity in the long route is somewhat more rapid, as when the total quantity of air is reduced one-half the diminution in the long-route is about one-eighth. The same result in all these cases, however, follows, viz., that when the total quantity of air passing into the mine is reduced to about one-eighth, the greater portion passes along the short route to the upcast shaft, and, on an average of the three cases, only about one-tenth passes into the workings. Mr. Atkinson accounts for these apparent discrepancies on theoretical principles, deductions of such cases by the difference of level in the various routes, and by the variation of temperature in the different parts of those routes acting upon such variations of level; and, therefore, until we have experiments made when such variations do not occur, where the passages are nearly level, and where the difference of temperature is inoperative, we cannot accurately test the theoretic conclusions apart from these causes by experiment. Considered, however, in a practical point of view, we have arrived at this result, which appears to me very important, viz., that when the various splits of air in a mine is regulated with the maximum quantity of air existing, that quantity may be reduced one-half without any greater diminution of air in the longest currents over that passing along the short routes than about one-ninth of the original quantity; and this result, observe, only occurs in extreme cases, where the length of the short route from the point of splitting to the point of reunion again, in the four experiments, is an average of 78

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yards only, while the lengths of the long route is an average of 2,700 yards of intake, and the same distance in returning, which makes the ratio 1 in 70 nearly.

The above result takes place when the regulators are adjusted with the maximum quantity of air passing into the mine; if they are adjusted with a diminished quantity of air, a contrary result, as might have been expected, follows. In Experiment I., when about three-eighths the quantity, (155,155), or 7,750 cubic feet per minute is passing into the mine when the regulators are adjusted, and when increased quantities (330,445) are made to pass to the extent of 19,375 cubic feet, the long current receives 11,125 and the short current only 8,250 cubic feet. And in Experiment III., the regulators being adjusted when (7,880), or 2,212 cubic feet is passing into the mine, and when that quantity is increased to (305,590), or 22,375 cubic feet, the long route receives 14,750, and the short route only 7,625 cubic feet.

Mr. Wales—Let us suppose that we have a mine comprising ten districts, and that each district is ventilated with a separate and distinct current of air. For this purpose nine regulators will be required, i.e., a separate regulator for every district except the farthest. Let it be assumed that each district receives 6,000 cubic feet per minute, making a total quantity of 60,000 cubic feet of air per minute to ventilate the mine.

Now, so long as all things remain equal, we have the above quantities passing into each district in their due proportions. But, how would the case stand, if it be assumed that some change occurs in the mine which reduces the gross quantity of air from 60,000 cubic feet per minute to 40,000 cubic feet per minute. Occurrences of a very ordinary character will produce this effect. For instance, owing to the pit being off from repairs going on in the upcast shaft, or the furnace not being properly attended to, then, under such circumstances, the gross quantity of air circulating in the mine would be materially diminished.

It is absolutely necessary, therefore, that great attention should be given to every district in the mine, and, more especially, should such attention be devoted to the longer currents, and, in particular, to the longest one of all, for it is found by practical experience, as indicated in the above results, that the far-off districts receive a less proportion, as compared with the nearer districts, whether the same be situated in the dip, rise, or level portions of a mine.

An exception to this rule, however occurs, as will be seen in the case

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of Eppleton Colliery, No. II. set of experiments, where the gross quantity of air is so much reduced that the influence of the increased temperature in the deep workings causes a greater quantity of air to circulate through the longer route, as compared with that which circulates in the shorter, a principle which holds good so long as we continue the reduction of the regulator below nine inches. But when the regulator is increased in size, as in the succeeding parts of the experiments, the opposite result is shown, and, therefore, Mr. Atkinson's theory is only substantiated in the exceptional instance noted above.

Mr. Atkinson—A comparison of the results of these experiments, which have been made on the subject, with the deductions, which I have previously enumerated, will serve to test the general accuracy, or the contrary, of the principles on which the paper under discussion is founded, as all the experiments agree with the theory except No. IV. in the Minor pit, and it was probably connected with the Eppleton pit workings, which would account for its not doing so.

The discussion from which the paper took its origin bore reference to the ordinary splits of air in mines, and to such changes in the gross quantity of air circulating as might arise in the practice of ventilation; yet many of the experiments that have been made to test the correctness of the conclusions contained in it have embraced extreme and unusual discrepancies, both as regards the relative lengths of the routes of the splits - some of which have been about a hundred times as long as those they have been compared with - and also as regards the changes effected in the amounts of the ventilating pressure applicable to the splits, some of which have been so great as to reduce the quantity of air circulating from a brisk current to a mere breathing of air, and, in some instances, to such an extreme extent have these changes been carried, that the air has actually reversed itself, and come outwards in the natural intakes. And, although such extreme conditions were scarcely contemplated in preparing the paper, they are fully accounted for on the principles adopted in it.

Thus, when a fixed quantity of air is circulating in a rise split, having the air of its intake cooler and denser than that of its descending return, there is a constant amount of pressure, arising from the excess of the gravitating force of the dense air of the ascending intake over that of the lighter air gravitating in the descending return (the gravitation of the intake air opposing, and that of the return aiding, the general ventilating pressure) operating in opposing the general ventilating pressure

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applied to the air-ways traversed by the splits, and, under the conditions stated, it is only the excess of the ventilating pressure, applicable to the splits, over the opposing pressure arising from the unequal gravitation of the air in the intake and that in the return, which is effectual in overcoming the frictional resistances, and causing the air to circulate in such rise split in the right direction; and it is evident that, by reducing the furnace power to a sufficient extent (or, more easily, by suddenly removing so much of the ventilating pressure by means of a regulator in any of the air-ways traversed by the air before reaching the splitting-point), the portion of the ventilating pressure remaining applicable to the splits may be reduced till it is less than the opposing force, arising from the unequal gravitation of the air in the intake and that in the return of the rise split, and when this takes place, the former will overcome the latter, and reverse the air in the rise split.

If, however, a sufficiently long time were allowed for the equalization of the temperatures and densities of the air in the intake and return, the ventilating pressure would again set the current in motion in the original direction, but, of course, the current would be smaller than before the reduction of the ventilating pressure applicable to the splits.

The results would also be temporarily modified by the expansion or contraction of the air in the splits, arising from the change in the pressure operating upon it. Contraction would ensue if the regulator that was closed was situated in the air-ways lying between the point of reunion and the top of the upcast shaft; and expansion, if it were in the downcast shaft, or in the air-ways extending from it to the splitting-point.

At the Elemore Colliery, No. I. set of experiments, where the long split had a rise of 110 feet, and was 8,000 yards in length) the short split being only 110 yards, and nearly level, and with which it was compared, it was found that, by reducing the main regulator placed in the air-way extending from the bottom of the downcast shaft to the splitting point, till the opening in it was only half a foot in area, the air in the long split was reversed, while a moderate quantity continued to pass through the small opening in the main regulator, and, along with the slight current serving out of the natural intake of the long rise split, passed along the short split, and up the upcast shaft.

The intake, in this case, was cooler than the return, the relative temperatures being 56° and 64° respectively, so that the result might have been anticipated on the principles alluded to; but the experiment was

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not continued until the temperatures became sufficiently equalized in the intake and return to allow the current to be converted into its original direction.

The above is an instance of the reversal of the air of a long split in a rise way ; while, on the other hand, on two different occasions, and under two distinct arrangements, at the Eppleton Colliery, one of them being the same as Experiment No. II., the other arrangement not being named amongst those described, the air in a short split was reversed by closing, to a sufficient extent, a regulator placed in the main intake leading from the downcast shaft to the splitting point, while a considerable quantity of air passed through the main regulator, and, (along with the reversed air, coming outwards in the short split,) into the long one, which had a great amount of dip (in one case 204 feet), and its intake air cooler (57°) than that of its return (68°), showing that the excess of the gravitation of the cool air of the dipping intake over that of the air in the ascending return from the long split, was sufficient to overcome the ventilating pressure applicable to the splits, and to cause an eddy, forcing its return quite through the short split, in direct opposition to the ventilating pressure. The experiments in this case, also, were not continued sufficiently long for the temperatures and densities of the intakes and returns to become sufficiently equalized to cause the air to again reverse itself, and flow in its original direction in the short split.

Two sets of experiments made by Mr. Berkley, at Crook Bank Colliery, and another set made by Mr. Cooper, at the Grange Colliery, are already published in the "Transactions," Vol. VI., pages 183 and 229, and appear to agree, to a satisfactory extent, with the principles adopted in the paper under discussion.

One set of experiments have more recently been made by Mr. Cooper, and another by that gentleman and Mr. Wales, at the Grange Colliery, and these both agree, to a satisfactory extent, with the principles adopted in the paper. Although, in these experiments, the same proportions were nearly maintained, under different gross quantities of air, yet, it was found that the long run obtained a slightly increasing, and the short one a slightly decreasing, quantity; on reducing the gross quantity of air; and this is fully accounted for by the long run having a slight dip of about half-an-inch to the yard. The short run, in this case, was formed by setting open the shaft or separation doors, and was an extreme case.

The experiments at Elemore Colliery, (No. I. set) already alluded to,

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agreed in principle with the paper, as the current having the greatest rise obtained a decreasing proportion of the gross quantity of air as it became reduced in amount, the intake being cooler and denser than the return.

Another set of experiments, the particulars of which are not given, were made at the Elemore Colliery, where the length of the runs, and the ascents of their routes were very nearly equal to each other, and the results satisfactorily agreed with the principles on which the paper is founded, as the proportions of air in the splits continued the same when the gross quantity of air circulating was altered in amount.

In the experiments previously alluded to as made at the Eppleton Colliery, the short split got a decreasing, and the long split an increasing, share of the gross quantity of air, as it was reduced in amount, and as the long split had the greatest amount of dip, and the temperatures of the intakes was less than those of the returns, the result is agreeable to the principles of the paper, notwithstanding there being a very great discrepancy in the lengths of the routes of the splits.

The general results of these experiments, in my view of the case, all accord with the general principles on which the paper under discussion is based.

In an experiment made at Hetton Colliery, in the minor pit, Hutton seam, where the route of the long split was 1,700 yards, and the length of the short split was 12 yards, and where the long split has a considerable dip, viz., 700 yards, of 1½ inches per yard, and 1,000 yards nearly level, (Experiment IV.,) and the short one is nearly level, it was found that the short split obtained an increasing, and the long one a decreasing share of the gross quantity of air circulating, as the gross quantity of air became reduced in amount, notwithstanding that the temperature of the intake was lower than that of the return ; a result contrary to the principles contained in the paper under discussion, and which I am unable at present to account for, although I have not very closely investigated the case. I am, however, aware that there is an opening from the extremity of the long split into the workings of the Eppleton Pit, and any leakage at this opening would have a tendency to make the results such as they really were; and, as this is the only experiment that has come to my notice, which does not accord with the principles in my paper, I am inclined to suspect that some leakage had really existed at this place.

Mr. Thos. John Taylor—Would you not, in estimating the effect

produced on different currents of air, have recourse to the circumstance of a diminished pressure on the air-column every step you take from the downcast to the upcast shaft? Every particle of air becomes lighter, whether passing to the dip or the rise.

Mr. Atkinson—I say, this is a feasible explanation, to a very limited extent only, inasmuch, as the difference of the temperature between the intakes and returns is immensely greater in its effects in the matter we are discussing.

Mr. Thos. John Taylor—We are assuming, that the temperature is the same. Putting that out of consideration, I ask, whether you consider the other circumstance is not only something, but very considerable? At the downcast shaft, you have the whole pressure of the ventilating column; in the upcast, it amounts to nothing.

Mr. Atkinson—I do not agree with you, to the full extent. It was once before discussed by you and I, and we did not agree then. I have reconsidered the matter, and come to my own conclusion. I quite agree with you, it is a fact, that such a cause exists. Take level workings, and suppose the temperature uniform, in passing from the bottom of the downcast to the upcast, the air is more expanded and less dense at every step. That is a cause which will operate in the same manner, and in the same direction as the returns, being hotter than the intakes; but, this is trivial in amount, compared with the difference of temperature.

The President—Take the density of the air at the bottom of the downcast shaft, and the density of the air at the bottom of the upcast shaft - the difference between the two, is the ventilating power. This will admit of no argument, considered in a statical point of view; but we are considering its dynamical effects ; we are dealing with a moving current of air, at a certain density at the bottom of the downcast shaft, and diminishing in tension, until it arrives at the bottom of the upcast shaft, where it is of a much less density, and the question is, what effect such constant and gradual diminution of the tension of the air between the downcast and upcast shafts has upon the general ventilating power, produced by the statical difference of density of the air of the two shafts ?

Mr. T. J. Taylor still thought that the chief cause of the difference of density between the intake and return air of mines was to be found in the gradual expenditure of ventilating pressure on overcoming the resistances encountered by the air, giving rise to a gradual reduction of the tension and density of the air as it circulated, below that which

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it would have possessed had it been in a state of rest, and the pressure a statical one. He thought that any ordinary difference of temperature between the intake and the return air of mines would be found to have a much smaller influence in reducing the density of the return air below that of the intake, than the cause to which he had alluded.

Mr. Atkinson, in reply, stated, that he had considered the matter referred to by Mr. Taylor, but had come to a very different conclusion from that gentleman, inasmuch as he believed that if the temperature of return air only exceeded that of the intake by a single degree, the expansion arising from it would cause as great a reduction in the density of the return air below that of the intake as would have resulted from the gradual expenditure of about one and a half inches of water gauge as ventilating pressure, on the resistances offered by the air-ways ; and this was, perhaps, more than the average amount of ventilating pressure expended upon the workings of coal mines; and it was from a conviction, that the reduction of density, arising from the source alluded to by Mr. Taylor, was generally so small, in comparison with that resulting from differences of temperature, that in the paper under discussion, he had thought it was not worth while specially adverting to it, any more than to some other generally unimportant causes, which, nevertheless, to a small extent, co-operate with changes of temperature in producing changes of density in the air of mines.

If this discussion had to be resumed at any future day, he would be glad to bring forward his reasons for coming to this conclusion.

Mr. Taylor—I shall also write a paper specially on the subject, which I will endeavour to have ready for the next meeting of the Institute.

The President—It would be very desirable to have the parts of the subject fully investigated, and I would, therefore, beg of the gentlemen to favour the Institute with papers on this interesting and important part of the theory of ventilation at their next meeting. Reverting to the experiments made at Hetton Colliery, it would be well, perhaps, to continue the discussion and endeavour to explain the apparent anomaly in the two classes of experiments, viz., when, after the relative quantities of air made to pass into the long and short routes respectively are nearly equal, and when increasing quantities of air are made to pass into both routes by successively opening the main regulator; and then, on the other hand, when the relative quantities passing into each route are again made nearly equal, and diminished quantities of air are made to

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pass along such routes. The particular split which, in the former case, gets an increasing proportion of the air, always, in the latter case, receives a decreasing proportion of it, and vice versa.

Mr. Atkinson—This result is in perfect accordance with the principles of my paper. See page 164, vol. VI., of the "Transactions," where, after explaining the causes which operate to diminish or increase the quantity of air, in cases "where the gross quantity of air becomes reduced in amount, owing to any decrease in the general ventilating pressure, or, owing to any contraction of the shaft, &c," I add, " of course, the opposite effects would, in each of the conditions stated, take place on increasing instead of diminishing the gross quantity of air circulating, while the air-ways forming the splits and the regulators in them remained unaltered."

A discussion, of considerable length, then took place on the apparent discrepancies between the theoretical deductions in Mr. Atkinson's paper and the practical result as developed by the experiments made at Hetton Colliery, in which Messrs. Taylor, Atkinson, Wales, Daglish, Berkley, and Boyd took part, the result of which was, that as the experiments made at Hetton Colliery were in routes either not level, and therefore influenced by the variations of temperature of the intake and return currents, or were influenced by the return air being at liberty to pass into other workings adjacent to the direct routes; and as it was supposed that there were leakages of air through the stoppings from the adjacent workings into the direct return route of the experiments, especially when the main regulator was much contracted, it appeared desirable to postpone the discussion until the next meeting of the Institute, and, in the meantime, to fix upon some cases where experiments could be made, free from such objections.

It was stated, that as the regulator in the short route was an aperture in a thin timber stopping, less than an inch in thickness, this might affect the result, from the well-known fact that fluids passing through such apertures presented a different result from fluids passing through even short pipes of the same area, and Mr. Tone gave an instance of this in the case of water, at the Newcastle Water Works. It seemed, therefore, desirable that experiments should be made, by causing the air of the short currents to pass through boxes, or along a passage of a small area, instead of through the thin partition of a timber regulator.

It being likewise observed, that the velocity of the long route in all Cases having been taken into the intake current, it was desirable that the

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velocity should, also, be taken in the return current, to ascertain if a perfect equilibrium existed in the tension of the air in such current from the change of pressure or quantity of air in each experiment, and also to test the leakage of air into the return current.

The discussion was, therefore, adjourned until the next meeting of the Institute, and the reading of Mr. Ross's paper was proceeded with.

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[Diagram]

Plan No. 1: Shewing part of the workings in Springwell Colliery with gas drifts.

ON GAS DRIFTS FOB DRAINING GOAVES IN COAL MINES, and ASCENSIONAL VENTILATION.

By Mr. ALEXANDER ROSS.

I would remark, in the first place, with respect to the first part of this subject, that I propose to illustrate it by giving an account of the successful application of such drifts in practice, which was done on a somewhat extensive scale at the Springwell Colliery, in the year 1843, and has also been continued from that period up to the present time, and I shall, afterwards, attempt to deduce from such results, reasons for its application in other cases where the circumstances are similar.

I may state, that in the year 1843, the Hutton seam at Springwell colliery was in course of working. The particular district referred to was the third panel of workings on the south side of the main east drifts or engine plane. The coal seam here dips to the east at the rate of about one in twelve. The accompanying sketch shows the position of the workings referred to. Plan No. I.

The mode pursued in working was to drive the bords from the south levels (which are driven water level from the engine plane,) to the rise, that is, nearly due west, and after they had proceeded a certain distance, generally four pillars. The pillars were then removed. The air was, first of all, conveyed up the first four bords (the fourth bord being the waggon-way bord), to the face, by stoppings on the south side of the waggon-way bord. It was then taken along the face of, and thus ventilated the whole bords, by means of bord stoppings, and then returned

through, and ventilated the pillar or broken workings, sweeping close by the edge of the goaf, and returning to the east and the water levels down two bords, between the waggon-way and the goaf.

The quantity of air circulating through this panel of workings was generally about 10,000 cubic feet per minute, and as the average area of the air-way was about thirty-two feet, the velocity of the air was 5.2 feet per minute. This was found to be quite sufficient for some time to keep the workings in both the whole and broken in a clean and workable state, and it may be stated, as a general rule, that, in working this colliery, the *whole* of the quantity of gas given off was not very large. It was only in cases where fissures or troubles were met with, and where, in some instances, blowers discharging large quantities of gas were met with, some of which have continued to give off gas in considerable quantities for a number of years, that gas was generally given off.

But where the working of pillars was proceeded with, and the roof became broken, followed by heavy falls, large quantities of gas were invariably discharged. This gas was generally supposed to be given off from a small seam of coal six fathoms above the Hutton seam. From whatever part, however, of the strata it was given off, it is evident it must exist in a high state of tension, *in situ*, if we take into consideration the large quantity of gas which has been given off from this goaf for a number of years. When the pillar workings, in the particular district referred to, had proceeded to some extent, it became very difficult to keep the working places in a clean state, owing to the large quantity of gas pressing out from the goaf, and which, from the current of air descending, it was necessary to force down to the dip by the current of air.

When the whole bords reached the extremity of the panel, and when a barrier was left between this district and the next, and when some of the pillars were removed up to this barrier, the foul air collected against it, and backed out into the working places in such quantities that it was found impossible to keep the working places free and in a clean state, so as to get the pillars off, so much so that the operation became both difficult and dangerous, and occasioned, in some cases, the loss of some of the pillars. And here it may be remarked, that in working other districts in a similar manner, particularly in taking the pillars off from the rise to the dip, great difficulty had been experienced from the same cause.

Now, as the return air-course of the district above, (that is the second district,) was only separated from the goaf alluded to by the barrier, it

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only required a holing to be made through it to procure a natural escape to the rise for the gas ; this holing was, therefore made, as shown on the plan, and the gas escaped into the return air from the district above, and mixed with and was taken with such air to the upcast shaft.

The best effects resulted from this operation, and, subsequently, the working of the pillars was proceeded with on this principle, and the remaining pillars worked down to the dip, without experiencing any obstruction whatever from foul air.

The good effect of this plan suggested a different system of working, which has since that time been pursued, viz., to drive the bords up the whole length of the panel to the barrier before commencing pillar working; to hole a gas drift into the district above, as in the instance alluded to; and then to work all the pillars off to the dip. By this method of working, the working places were kept perfectly free from gas, although the quantity given off by the different goaves was always large. Plan No. I. shows this mode of working with the gas drift through the barrier.

It was found, also, that the goaves did not, as was generally expected, become close, the gas always continuing to pass upwards through those gas drifts, mixed with a little air which also passed that way; but, which was regulated by the regulator *R* in the gas drift.

When coal seams are similarly situated to the instance here given, and which produce gas in a similar manner, (that is, principally from the roof, which frequently occurs), there can be little doubt that the plan pursued in this case may be made use of with much advantage, especially in new collieries, where the workings can be laid out in districts with the view of its application ; and this, it may be observed, may be done where long work is the system employed as well as in pillar and stall workings.

It must be borne in mind, that it is necessary that the returns, &c, be suitably laid out for the purpose. The returns, into which these gas drifts are holed) should be entirely shut off from any working parts, and at a considerable distance from the upcast shaft, and the gas should also be mixed with other air currents, so as to render the whole return air harmless before arriving near the furnace, supposing that to be the ventilating power applied.

But where a dumb drift is used, the system will be applied with more security. It must be remarked, that in the case of Springwell colliery, the return air ascends towards the upcast, which is to the rise of these workings, but, although this is the case, the ventilating power of the

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furnace shaft is not more materially diminished than it would be if placed at the dip, owing to the fact that the *surface* rises in the same direction as the coal seam. All this, then, is in favour of the application of gas drifts, and also of ascensional ventilation.

But it has been brought against this system that the placing of the upcast shaft at the extreme rise of the workings would, in many cases, seriously reduce the ventilating power of such upcast. It may, however, be a question worth discussing how far this objection may be balanced or overcome by other advantages connected with it, as a safe and effective ventilation is what is required. I would submit that this may be secured, in many cases, by means of ascensional ventilation, and the use of gas drifts, even where the aggregate quantity of air would be much less than it would be by placing the upcast shaft on the same level as the downcast, or on the dip of the workings.

I shall, therefore, proceed to illustrate this by taking the plan of the workings in the Lundhill colliery. In this case the coal is got by a sort of long work, and as the narrow bords or bord-gates are driven to the rise from the main levels, the intake air is ascensional, and the return air descends. It will be seen on a reference to the Lundhill plan, given in Vol. V. of the Institute "Transactions," there are ten working faces or districts, fully won out, I will presume that each district is ventilated by a distinct split of air, of 8,000 cubic feet per minute, making a total of 80,000 cubic per minute in the colliery. 40,000 cubic feet of air passing to the north, along the main levels, and also the same quantity passing to the south, and so passing downwards to the upcast shaft, which, as shown on the plan, is at the extreme dip of the workings. The mode of thus dividing the air is not difficult, but as it is not a part of the system of ascensional ventilation, which is the object of this inquiry, it is not necessary to give it here.

We have, we may suppose, all the advantages of the most improved system of ventilation, but we have the disadvantage, in the case alluded to, of the gas produced in the goaves and faces having to be forced down to the dip, by the current of air, to reach the upcast shaft. And as much gas is generated in the roof above the coal seam, quite similar to the instance given at Springwell colliery, we apprehend there will be much difficulty in keeping the working places clear of gas.

This would, especially, be the case in removing the barriers, after reaching the boundary of the colliery to the rise. We presume that much of the coal contained in those barriers would indeed be lost.

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We, therefore submit, that in such a case the coal would be got much better, in every respect, if the main drifts were, first of all, pushed to the extremity of the boundary to the rise, and then commencing to work the whole and pillars simultaneously, as in the long wall system; and by placing the upcast shaft to the extreme rise of the royalty, and thus to make the ventilation ascensional, instead of forcing the lighter gas and air from the goaves and the return air generally to an upcast shaft placed at the extreme dip of the royalty, this would, in my opinion, be the best mode of working.

In such a case, suppose the downcast or drawing shaft placed at the extreme dip, and in the centre of the royalty, and the upcast in a line therewith, but to the extreme rise; I would then drive two main drifts direct from the downcast shaft to the upcast shaft, which would be roads for the conveyance of the coals, and also intakes for the air currents, the workings being divided into districts on each side of these main roads, and suppose each district to be ventilated by a separate current, as previously explained.

I would then drive two other drifts, parallel to the main roads, one on each side, which would act as return air drifts for any workings which might exist along the water levels on each side of the downcast shaft, or in any other part of the coal-field between the downcast and upcast shafts; the air in such return drifts being ascensional towards the upcast shaft.

Suppose a set of workings to be in operation, consisting of five panels on each side of the main roads as at Lundhill colliery, and shown in Vol V. of the "Transactions" of the Institute.

I would drive a pair of gas-drifts water level from the upcast shaft, right across the face of the workings on each side, to the extremity of the boundary; and, I would make openings from the goaves of each of the panels of workings into the gas-drifts - the same as in the case of the Springwell colliery workings, by which the gas from these several goaves would pass off to the upcast shaft, together with the return air from each of the several panel workings.

Whatever number of panels there might be on each side of the main roads, I propose each panel should be aired by a separate current, so that if there are five on each side, and 8,000 or 10,000 cubic feet of air required for each panel, there would be required to be 80,000 or 100,000 cubic feet of air in the upcast shaft, but any other quantity might be used.

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It will be seen, by this arrangement, that the length of the air currents are very much reduced, which embraces two very important advantages. 1st.—It reduces the friction and resistance of the currents, and thus increases the amount of ventilation; being ascensional, also, an amount of advantage is gained, equivalent to the difference between forcing the light and heated column of gas and air downward to the upcast at the dip of the royalty, over the ascensional power of such a column of light air ascending to the upcast at the extreme rise of the colliery. And 2nd.—The cost of maintaining air-ways will also be reduced. If we take the currents at the extremity of the panels, and supposing them brought back to the upcast at the low level, and on the same plane as the downcast, the distance traversed will be nearly one-fourth greater than by passing such currents into the proposed gas drifts and so to the rise upcast. This mode of ventilation, of course, applies equally to the pillar and stall working, as to the long wall mode of working. The gas drift in both cases draining the goaf as it is formed.

I may now remark, in conclusion, that the drainage of goaves of gas by natural means, appears to be a subject of much importance, and can be effected by other means than this pointed out. In some cases, the working of an overlying coal-seam will accomplish this object; and this was well illustrated in the case of Springwell colliery, where the great source of gas is in the upper thin beds of coal. In the eastern part of the colliery, where the overlying seams are not worked, we are subject to sudden and great discharges of gas; but to the west, where the upper seams are worked, the lower bed is comparatively free from gas, and not at all subject to the sudden irruptions of gas met with in the eastern district.

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Plan No. 6 - Transverse Section of the Vale of Rosedale.

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ON THE DEPOSIT OF MAGNETIC IRONSTONE IN ROSEDALE.

By MR. NICHOLAS WOOD, President of the Institute.

In Mr. John Marley's very elaborate and very able account of the Cleveland Ironstone District, communicated to this Institute at its meeting of June, 1857, and published in vol. V. of the "Transactions," he states, page 207: "The only special district to which I think necessary now to allude is the Rosedale Abbey district, the ironstone from which has attracted a large amount of attention, on account of the large per centage, immense deposit, and magnetic properties."

Mr. Marley then gives a history of the discovery of this bed of ironstone, its position in the series, as well as in the district generally, and adds all the information which had then been elicited with regard to the particular features and character of such deposit, which he illustrates by a diagram, showing the explorations which had been made by drifts and pits towards such elucidation; and he then concludes by saying: - "I have no doubt that this seam is the same as the seam at the point *A* on the plan (No. 6), as also the same as that found on the east side of Rosedale, in Capt. Vardon's property, of varied thickness, as well as the seam as that at Grosmont, Fryupdale, Swainby, and Boltby, known as *the top seam* of Cleveland - the nine inches of coal in the pit sunk agreeing with Beckhole, near Grosmont, in particular; so that the only doubtful point is as to the portion from the outcrop at *A* to the

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so-called magnetic quarry; the most feasible solution being that it is a disjointed patch of the regular seam, known as the *top seam*, and not a vein, as has been said; and, with all deference to the parties who have had more opportunity for examining this district than I have, I propose leaving the *extent* of the *magnetic and extra per centage* tract as an *unsolved problem*, as it may vary from one or two acres to any indefinite extent, not being at all proved to the south."

This is a very clear and correct account of the information then existing on this deposit, Mr. Marley's opinion being that it represented the top seam, as developed at Grosmont, Fryupdale, Swainby, and Boltby.

A section of the strata at Grosmont is given by Mr. Marley, as follows:—

[Table]

Another section near Grosmont gives the top seam 11 feet 6 inches, then 187 feet of shale and ironstone, and then the Cleveland band.

The section at Fryupdale is as follows:—

[Table]

The section at Swainby is as follows:—

[Table]

And at Felix Kirk, near Boltby, the section is:—

[Table]

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Section of strata in the hills at Swainby Mines.

[Table]

Section of the strata at Eston Nab, showing the top seam, and tile main or Cleveland band, where the latter is in perfection.

[Table]

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[Table continued]

The main features of the sections given by Mr. Marley as assimilating to the Rosedale bed, are,

1st. The top seam, varying from 12 to 23 feet in thickness.

2nd. Lias shale and other strata, 132 feet to 220 feet.

3rd. The Cleveland main band, 9 to 12 feet.

But in all these sections there are no beds of ironstone between the top seam and the Cleveland main band.

Mr. Bewick, in a paper presented to the Institute, and printed in Vol. VI. of the " Transactions," gives drawings, and an account of the deposit of Rosedale, and concludes with those remarks, - " My object in thus troubling the members of this Institution with the foregoing remarks is twofold. First, to show that the iron ore of Rosedale, instead of being a large mineral field as was first asserted, and still believed to be so by many, is nothing more than a volcanic dyke; and, secondly, that the ironstone lately opened out in this locality is not, as it is reputed to be, the main seam now being worked in Cleveland and Grosmont districts,

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Plan No. 1, Rosedale Ironstone.

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but it is my opinion, if Mr. Marley will permit me to say so, the top seam."

I shall now give an account of the operations concluded by Professor Phillips and myself towards the investigation and development of this bed of ironstone.

The first discovery of this deposit of ironstone was at a quarry on the south-west side of the valley of Rosedale, about a mile south from Rosedale Abbey, and shown on plan No. 1. When this quarry was opened out it was found to consist of apparently a confused mass of ironstone boulders of an ellipsoidal structure, and of gigantic size, often three or four feet in diameter. The central part of these boulders being generally blue, and consisting of a solid dark oolitic magnetic iron ore, with, in many cases, sandy and solid ironstone crusts around it, and, in receding from the centre, the iron ore becomes paler, alternating with dark brown purplish layers, the layer then becomes pale brown, and the magnetic quality is lost. In most cases, however, the nodules are quite solid, and a slight stratification exists, though very obscure; and in several cases, likewise, the oolitic structure is merged into compact brown iron ore. In some parts also, where exposed to the water and to the weather, the iron ore is partly washed away and a gritty ferruginous crust remains. These great variations do not occur where the ironstone is under cover, or covered by other strata, but appears to assume those different phases in consequence of its extreme susceptibility to change by exposure to air and water; and it is somewhat remarkable that the magnetic property is strongest where the mass is thickest, and scarcely shows any magnetism in places where it is thin, or where it has little cover, and, consequently, more exposed to decomposition or change.

The great characteristic difference of composition between this ironstone and the top and main band of Cleveland is, the entire absence of shells, the structure being entirely of an oolitic character, being entirely composed of small round concretions of iron ore, cemented together with extremely thin silicious or arenaceous films, and in its magnetic properties exhibiting polarity, and likewise in its greater richness than the binary ironstone of Cleveland.

This quarry has been excavated, so as to form a face of 60 feet in thickness; to which must be added, 11 feet of blue magnetic stone, 2½ red ironstone, slightly magnetic, bored down below the bottom in magpie stone, and three feet of shale.

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Soon after the quarry was discovered, it was thought advisable to drive a drift into the side of the hill, to ascertain the extent of this deposit, the quarry being about 600 feet from the bottom of the valley, and about 300 feet below the utmost level range, or plateau of moors, lying on the south-west side of the valley. This drift, together with a pit sunk upon it, is shown by a drawing in Mr. Marley's paper. Since then, it has been driven to a much greater distance, and three bore-holes have been put down from the surface to the Rosedale bed of ironstone.

Plan No. 1, shows the position of this drift, the distance and direction in which it has been driven into the hill; and also the position of the three bore-holes and the quarry. And fig. I, plan No. 2, also shows the section of the same drifts, and the section of the borings, together with their depths from the surface, and the beds of ironstone which they have proved. I have carried such section across the valley, for the purpose of shewing the position of the ironstone band on the opposite side of the valley.

Fig. I, plan No. 3, shows, on a larger scale, the strata bored through in the three bore-holes above alluded to, and the ironstone beds which they have proved; and fig. II, Nos. 1, 2, 3 boreholes, 4 facing drift, and 5 side drift, shows the thickness of the lower bed of ironstone in the several boreholes in the face of the drift, and also in the side of the drift.

It is necessary to remark, that where the drift was first set away in the side of the hill, it met with shale, and it continued in shale for a distance of about 80 yards, when the ironstone was found. The drift continued in the ironstone for a distance of 180 yards further, making a total distance of 260 yards from the face of the hill. No. 4, plan 2, is a section of the ironstone at the face or furthest extremity of the drift shewing an entire thickness of 32 feet of ironstone, viz., 6 feet 2 inches of drift, 11 feet 9 inches above the drift, and 14 feet 0 inches below it. And what is important to mention, the ironstone was here distinctly stratified, as shown by the lines across the section No. 4.

400 yards in advance of the extreme end of the drift, and 660 yards from the side of the hill, a borehole No. 2, fig II, was put down, and at right angles to the line of this borehole from the drift two other boreholes were put down from the surface, as shown on plans, fig. I and II, each 200 yards distant from No. 2 borehole, or 400 yards separate; and the following are sections of the strata passed through in these boreholes.

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Plan No. 2, Fig. I.

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Account of the Boring No. 1, or South Bore-hole on Rosedale Moor. - 1858.

[Table]

Account of the Boring, the Middle Hole, or No. 2, on Rosedale Moor. - 1857

[Table]

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Account of the Boring No. 3 Hole, or North Hole, on Rosedale Moor. - 1858

[Table]

It will be seen, therefore, that for a distance of 580 yards from the pit, No. 1 on the section, plan 2, to the boring No. 2 on the same section, the thickness of this bed of ironstone is nearly the same, and that this is the case likewise at the other two boreholes Nos. 1 and 3, at right angles to the above line of section, the respective thicknesses being as follows:—

	Ft. In.
Drift.................................	32 0

No. 1 borehole................32 0

No. 2 ditto 32 0

No. 3 ditto 29 3

These borings and sections show two distinct beds of ironstone, stratified with great regularity, and they prove most conclusively that neither of them are at all like what Mr. Bewick terms "nothing more than a volcanic dyke."

It will be seen by the map of the district, plan No. 1, that a red coloring is traced around the edge of the valley; this is undoubtedly the out-crop of what is called the " top seam " of ironstone, as it can be traced south and east into Eskdale, and towards Grosmont and Fryupdale; and also north towards Swainby and Boltby, in which localities Mr. Marley has given sections of the top seam, and also of the Cleveland main band. Supposing this out-crop in the Rosedale valley to be the "top seam,"

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Plan No. 3, Figs. I & II.

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then the upper bed in the sections Nos. 2 and 3, is unquestionably the top seam likewise; and we there have a bed of ironstone upwards of 30 feet thick, lying parallel to, and strictly conformable with the "top seam" (and separated therefrom only by a thin bed of shale) of an entirely different character from either such top seam, or the main band of Cleveland.

I have laid down, on plan No. 2, a section of the strata given by Mr. Marley, at Grosmont to the south-east, and at the Swainby mines to the north, and I have added the section at Eston Nab. It should be observed, also, in corroboration of the upper bed of ironstone in plan No. 2 being the top seam, that a bed, or rather, three or four beds of ironstone intermixed with shale occurs in the brook of Rosedale, and crops out in the bank, which is generally believed to be the representative of the Cleveland main band, though the ironstone is very inferior and not workable. I have laid on the section No, 1, plan 2, the position of this bed of ironstone, which agrees pretty well with its position in the other sections, making allowance for the variation in the thickness of the lias shale as found in the several localities.

I have likewise, on plans No. 1 and No. 2, shown the position of the quarry, which appears to have slipped down below the level of the beds, as shown by the drift and borings. This appears to have been occasioned by a slip dyke which crosses the drift near the pit, as shown on the plan No. 1. It will be seen by this plan that the drift passed through alluvial soil and shale up to near the pit, when this dyke was crossed and the ironstone cut, as shown on the plan. This dyke is supposed to run in the direction shown on the plan, crossing the drift near the pit, and throwing the strata down on the south-west side, and, consequently, the strata comprising the quarry; and it appears that the quarry itself is much broken, and has very much the appearance of a disjointed slip, the elliptical nodules being in a mass of confusion, as shown on the plan.

It has been supposed by some parties that this dyke has given the magnetic character to the ironstone, but it is well known that the character of the ore must be changed from a peroxide to a protoxide to become magnetic, which the crossing of the dyke through the strata could scarcely accomplish; and then we have the entire absence of shells in the lower bed, while the matrix of the upper bed or top seam is entirely calcareous and filled with shells. The concretionary nature of the stone, and the much greater per centage of iron produced by this deposit over that of either the top seam or the Cleveland main band, are also charac-

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teristic of this bed of ironstone. The analysis given by Mr. Marley of the Rosedale stone being upwards of 50 per cent, of metallic iron, while the top seam and main band are about 32 to 35 per cent. ; and the produce of a large quantity smelted at Consett gave 55 per cent. from the calcined ore, and 45 per cent, from the raw stone.

Whatever opinion may, therefore, be formed of the cause of this deposit, we certainly have the fact, that for a width of 400 yards and a length of 580 yards, we have a bed of ironstone highly magnetic, of an almost entirely uniform thickness, totally different in its mineralogical character from the ordinary stone of the district, and yielding in produce nearly 20 per cent. more iron in the furnace. To what extent this bed may exist beyond the extent already proved will be the subject of further investigation, but it will be a very extraordinary anomaly in geology if a bed of such uniform thickness should not extend to considerable distances. It has been stated that a similar bed has been discovered in other and distant localities; not being myself cognisant of the facts, and my information not being very precise, I abstain from giving such information at present. The importance of such discoveries are of too great interest in the district, and too valuable in a commercial point of view to remain long unexplored, and therefore we may hope, that at some future period the Institute will be favoured with an account of such deposits.

The President's paper on the Rosedale Ironstone, having been read, a discussion thereon was taken.

Mr. Bewick said the magnetic ore in the quarry was a casual deposit in the shape of a dyke or vein.

Mr. Marley—I understand, since I was at Rosedale Abbey, that which the President stated to be the top seam had been discovered in a regular stratified state on the south side of the magnetic quarry. At the last discussion we had on the subject, I admitted if that bed of ironstone had been discovered keeping on its uniform rise and dip, from the north side of the quarry to the south, I had been mistaken in supposing the magnetic seam to be the same as that of the seam then discovered on the north side of the quarry. Then, as to whether it was a vein or a bed, or whether what I supposed at the last meeting it was an overflowing between soft strata, similar to "flats" in lead veins, I had not an opportunity of forming an opinion, for want of the three bore holes, which have now been given'

The President—What you stated was quite correct. The top seam had not then been found on the south side of the quarry. It is now found

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on the south side as well as the north side; but I do not think we have yet discovered the magnetic stone on the south side of the quarry, except in the drifting and borings.

Mr. Marley—When I made my examination, preparatory to reading my paper, the top seam at the point A on the plan therein referred to was lost, and no continuation was found south of the magnetic quarry; but, by competent witnesses, I have been informed it is now found south of the said magnetic quarry. But, if the magnetic stone is a bed, it is extraordinary so large an extent of country should give no trace of it, as at Grosmont and other places we have not the slightest trace of it. At Ingleby they are putting three bore-holes down, with a view of proving the existence or otherwise of the magnetic ironstone there. They are now, I believe, past the top seam position, but have got nothing but shale yet. These borings will prove about 100 fathoms of strata. I have hitherto been of opinion that the round particles, in the specimens of magnetic ore, are oolitic shells.

The President—No. I believe they are iron, with a silicious matrix.

Mr. Marley—Has one of those globules ever been analysed by itself and found to be pure iron?

The President —I do not know; but I believe there is no calcareous matter in those particles which there would be if it were shells.

Mr. Marley—Unless it is some peculiar formation.

The President—Then the shell is gone, and the iron left.

Mr. Boyd—The chemical part of the shell remains in the Cleveland stone.

Mr. Marley—The magnetic stone is not in analogy with the Cleveland.

The President—It has changed its character from a peroxide to a protoxide.

Mr. Marley—I acknowledge the magnetic stone is free from "pectens."

Mr. Bewick—After hearing what has been stated by our President, I am bound to say our opinions are as much opposed as ever, and I shall endeavour to shew you that the ironstone beds they have bored through at Rosedale Abbey are not the same as the magnetic ore and top bed found by the side of the valley, that, in fact, the borings have not reached those deposits by several feet, and that, therefore, *they have not as yet proved anything more respecting them*. The strata they have bored through are quite above them, and you will find on looking at the table of the borings, published with the July discussion, that an important member of

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the series, which immediately overlies the top bed, is wanting. I allude to the great sandstone rock, which is seldom under 50, and sometimes met with 100 feet thick. This rock does not appear in the borings at all.

The President—Yes, it does, viz.:—

	Fms	Ft.	In.	
Brown freestone, No. 1 bore-hole......	12	5	6)	
Brown and grey post, 2 do.......	12	1	0)	with other beds of post, mixed with shale.

Brown post........ 3 do....... 11 5 0)

Mr. Bewick—That is not the sandstone I allude to. That rock is found higher in the series, and belongs to the coal measures, which your boreholes have gone through, but, as I have just said, they have not yet reached the other sandstone, and cannot, therefore, have touched the top bed. In this section, plan 5, you have, in my opinion, a type of the ironstone you have gone through in your borings. The seams here are thin and divided, and the shale between them is interspersed with iron nodules; and, as you admit the seams are split in the last bore-hole, it but serves to confirm my opinion that they are one and the same. They occupy the same geological position in the series - that is, they intervene the great sandstone rock and the coal measures in the oolitic series.

The President—Do you purpose giving the sections for publication?

Mr. Bewick—Yes; I intend leaving the whole of the sections with you for that purpose. The thickness of every stratum, in the diagram representing a cross section of the vale of Rosedale, is taken from the table of the borings before referred to, in which I may here observe there is an error of 3 fathoms 2 feet. The total ought to be 48 fathoms 2 feet, instead of 51 fathoms 4 feet, and if you take from this 1 fathom 1 foot for the grey shale they have left off in, below the ironstone, it leaves 47 fathoms 1 foot, from the top of the bore-hole to the bottom of the ironstone. I am thus particular because I have taken a line of levels, commencing at the south drift, by the side of the hill, and terminating at the *south bore-hole*; and I find there is a difference in the height of the level, and the depth of No. 2 *bore-hole*, of 64 feet, fully corroborating what I before stated, viz. - That the bore-holes have not yet reached the sandstone which overlies the top bed, and if you will allow me to explain my sections, I think they will prove to you that the ironstone they have cut through belongs to that which we call the oolitic beds, and which are found in different localities in the Grosmont district, not so thick, it is true, neither are they magnetic; but they are found, as I before I stated, occupying the same geological position, and accompanied by the same description of strata. Section plan 5 is taken between Goathland Mill

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Plan No. 4, Section of the strata on the west side of Rosedale. Showing the position and supposed formation of the Magnetic Ore.

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and Beckhole, near Grosmont, which, you will observe, contains the same alternating strata of sandstone, shale, coal, and ironstone, as you see in section plan 6, which is a transverse section of the vale of Rosedale, representing the strata they have bored through there. The ironstone beds *a* and *c*, in section plan 5, are, in my opinion, the same as those marked *a* and *c* in section plan 6. The bed *c* is very irregularly diffused throughout this portion of the oolitic district. It is found in the nodular form. In some places you find it of considerable thickness, and then, again, entirely wanting. Sometimes of good quality, but more frequently coarse and inferior, and gradually alternating with the sandstones. The bed a is more regular, but thinner, and of very good quality; its upper portion consists of a nodular bed averaging from 3 to 6 inches; and the lower portion a bed averaging from 12 to 18 inches in thickness. Where-ever I have met with those beds, however, I have always found them so variable, both in extent and thickness, as to afford no reasonable prospect of their paying for working. They may certainly be found different at Rosedale, but I would just observe that I consider boring a most fallacious mode of proving ironstone deposits in strata, such as that which these borings have gone through, you are so liable to mistake a nodule for a bed, or a portion of a bed. I shall be much surprised if you do not find the section of your shaft, should you sink one, very different from the section of your bore-hole.

The President—Then it is a question of policy, in Mr. Bewick's view of the case, commercially considered, whether the borings should not be continued. With regard to the identity of the position in the series of the bed of ironstone ranging around the Rosedale valley, as shown in plan No. 4, and also colored in plan No. 1, it appears to be undoubtedly the " top bed " of Cleveland. All parties admit this. Then the question is, is the bed of ironstone proved at the pit No. 1, plan 2., fig. I; and the bed corresponding therewith and proved in the bore-holes Nos. 1, 2, and 3, and therein designated by me as the top seam, the same bed of lronstone ? Mr. Bewick thinks not, and that the borings have not yet reached this bed. I can, of course, only refer to the borings, driftings, and the section of pit No. 1, and I must add that there appears to me no doubt whatever on the subject; and the fact that, according to Mr. Bewick's plan No. 4, we have the top bed on both sides of the magnetic quarry, ranging as accurately as can be conceived with this bed in the borings, confirms this supposition in my opinion. It is true that this bed is at a lower level at the south or left hand drift than on the north side, but this is clearly the effect of the dyke shown on plan No. 1, which

throws down the strata in that direction. With regard to the supposed want of what Mr. Bewick calls the thick sandstone strata immediately above the top bed of ironstone, and shown on the section, plan No. 5, to be 100 feet thick, I have looked carefully over the sections given in Mr. Marley's paper, and I do not find in any one of them, except at Eston Nab, the extreme northern point of the district, any bed of sandstone approaching to that thickness, and there the section given is

	Ft. In.
Freestone..	60 0
Shivery post, patches of jet, and fireclay	54 0
Top seam, exclusive of shale bands	1 3

At Rosedale Cliffs, between Staiths and Runswick Bay, we have

	Ft. In.
Freestone...............................	26 0
Fireclay..................................	4 6
Freestone shale......................	5 5
Blue shale................................	0 10

Top seam, exclusive of shale bands...	4 7

Still further south, the sandstone at Wreck Hill is only 10 feet, with 2 feet 6 inches of shale covering the top seam; and at Grosmont, Mr. Marley gives 25 feet of sandstone, and another section at 58 feet 6 inches, which he says varies in thickness and quality. At Fryupdale, the thickness of sandstone is given at 55 feet, and at another place, viz., Swainby, the following is the section—

	Ft. In.	
Soil, &c..........................	3 0	
Freestone.........................	24 0	Near the limekiln this is 100, with 9-inch iron-Stone balls in it.

	Ft. In.	
Slaty coal...................	0 9	
Shale	1 0	
Sandstone	4 0	
Slaty coal...................	0 9	
	6 6	
Shale	5 0	
Coarse freestone.............	3 6	
Shale, with occasional nodules of ironstone..	13 0	
Top seam.............		28 0

Considering, therefore, that in the borings there is about 60 feet of sandstone, there does not appear to me any substantial difference between the shale in those borings, and in the other parts of the district to justify the supposition that the upper bed of ironstone is not the top seam. Mr. Bewick thinks the boreholes have not reached the sandstone he describes. If so, he should like to ask Mr. Bewick what seam of ironstone that is in the district which has been bored to?

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Plan No. 5, Section of a portion of the strata near Grosmont, corresponding with that bored through at Rosedale.

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Mr. Bewick—It is, in my opinion, as I have previously stated, the ironstone found in the oolitic series.

The President—Where does it occur in the other districts? Where do you find another similar deposit in Mr. Marley's sections?

Mr. Marley—Dr. Verity gives a variety of ironstone seams. If we refer to the plate in page 206 of my paper, you will find there are several ironstone seams lying over the seam, which we agree to be the top seam of Cleveland. Professor Phillips said, that with the exception of the classification of names, this section was practically correct.

The President—Do you think the ironstone which crops out all around the valley of Rosedale is the top seam?

Mr. Bewick—I think so; I have no doubt about it.

The President—If we are agreed that the deposit of ironstone found cropping out around the valley of Rosedale, as shown in the different plans, then there can be no difficulty in tracing the sandstone overlying that bed to the sandstone first of all sunk through at the pit No. 1, on the plan No. 2, and thence to the boring Nos. 1, 2, and 3; and these borings having passed through the upper bed of ironstone, below such sandstone, and then through the magnetic bed, there cannot be the least doubt of the geological position of these beds. With reference to the levels there is no discrepancy whatever in that respect, there is a rise in beds in the line of the drift, and in the extension of that line to the borings, and the direction of the line between the borings seems to be nearly water level at that part. There is not, therefore, the least discrepancy on this point. I have taken the ordnance maps as my guide as regards the levels, and have no doubt they are correct. Whatever opinion may, therefore, be arrived at with respect to the comparison of the beds proved in the borings and in the pit, with the beds at Grosmont, &c, there appears no doubt in my mind that the mass of ironstone of the quarry is a detached portion of the thick or lower bed of ironstone, and that such bed exists *in situ* for a considerable, and, of course, at present, for an unknown extent in the locality of Rosedale.

Mr. Bewick—If our President means by pit No. 1, the air-shaft sunk on the main drift, I quite agree with him that the sandstone found in that shaft is the same as that which overlies the top seam; but, I beg to say, I entirely differ with him in supposing it to be the same as that they have gone through in the borings. I am also opposed to his opinion with reference to the direction of the dip and rise of the strata. There can be no doubt, I think, but the strata on the west side of Rosedale, *and to the south of the crown*, - that is the point from whence the strata

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dip in contrary directions - are dipping in a south-westerly direction, as shown in my section plan 4, and still more clearly proved by the drift commenced on the south side of the magnetic dyke, and driven in a line with the south bore-hole, running nearly west, but which has been discontinued, owing to the top seam, in which the drift was commenced, dipping so much in that direction, instead of rising, as our President supposes, as, at the distance of not many yards, to be completely under water-level. With reference to the slip-dyke or fault mentioned by that gentleman, I can only state that I have never yet been able to discover any dislocation or disturbance of the strata, other than what has been occasioned by the dyke of magnetic ore in its immediate vicinity. Then, as to the extent of the magnetic ore, all I can say is, I have paid several visits to Rosedale solely for the purpose of examining the strata in that neighbourhood, the many deep ravines which abound there affording ample opportunity for doing so, but I have never been able to trace the magnetic ore beyond the vicinity of the quarry, and every visit only serves to convince me that it is a casual deposit, in the shape of a dyke or vein. A bed, however, of 560 yards in length, and from 30 to 32 feet thick, cannot be identified with a casual deposit, nevertheless, I think, very probably there may be a mistake in supposing you have a solid mass of ironstone 32 feet thick, This may have occurred from the borers having cut through nodules or irregular patches of ironstone, and also from the shale in which it is found being very hard and of the same color as the ironstone. From these circumstances it is an easy matter to be misled by borings.

The President—Whatever may have been the result of investigations on the surface, I do not think I can add any further information to that already given and shown on the plans, to prove that a thick bed of ironstone of about 32 feet, exists over a space of upwards of 560 yards in length, and 200 yards in width, with not the least indication of any change or termination of such deposit. It would, indeed, be a most extraordinary occurrence in the annals of boring, to suppose that occasional nodules, or irregular patches of ironstone should have produced the result recorded in these borings. The boring through the ironstone beds was performed under the immediate inspection of Mr. Stott, a well-known experienced borer, who kept the specimens brought up the borehole; and I can add, that I examined a great many of the specimens myself with a magnet, and found them magnetic. There is not the least pretence of supposing that shale could be mistaken for ironstone. Have you seen any nodular magnetic ironstone in the Grosmont district?

Mr. Bewick—Never. You must remember, (addressing the President,)

that you stated at the October discussion in 1857, that Professor Phillips and yourself, had discovered the magnetic ore in "two localities two miles apart," viz., at Sheriffs drift and at the Quarry, and again in the July discussion of last year, you stated the stone in the drift south of the dyke was magnetic, but on examining it I found this not to be the case as regards both the drifts. I believe the reason why there are so many conflicting opinions with reference to the nature and extent of the magnetic ore, is owing to the difficulty there is in distinguishing the ore from the top bed - that is, in separating the igneous portions from the sedimentary, for although they are both frequently magnetic in the immediate vicinity of the dyke, there is yet a vast difference between them. The igneous portion is harder, heavier, and more compact than the sedimentary, and the former appears to have acted upon the latter whilst in a heated condition, much in the same way as a magnet acts upon a piece of common iron, imparting to it a portion of its peculiar magnetic properties, much in the same way as the magnet imparts to the common bar of iron a portion of its magnetism. I may here be permitted to add, that whilst I believe this ore to have been subject to a heat sufficient to evolve the different gases it contained, I yet do not think the heat has been of that intensity so as entirely to expel it. We need not, therefore, be surprised at traces of carbonic acid being found in the chemical analysis of this ore. Here is a specimen of the igneous portion, which I took from the bottom of the quarry, and, after examining it, no one can doubt I think of its having been subjected to heat.

The President—There is no doubt, as stated by Mr. Bewick, that portions of the top bed in Rosedale are occasionally magnetic, and it was this property which led to the mistake, if they are mistakes, in supposing the magnetic bed to have been discovered at Sheriffs drift, and at the drift south of the magnetic quarry. The explorations at that time had not been sufficiently extended, nor have they yet been prosecuted to such an extent as to ascertain if the magnetic bed exists in those localities. Finding part of the ironstone partaking of magnetic influence led to a supposition that this bed did exist in those localities, and the subsequent explorations have not been prosecuted to an extent to ascertain the fact either one way or the other. (To Mr. Bewick)— From what part of the Quarry did you take this specimen?

Mr. Bewick—It is from the floor of the quarry. This (showing another specimen), is a sample of the top bed which appears to have been partially burnt, and you will at once be able to detect the difference

between them. These (showing other specimens), are samples of the ironstone found in the oolitic rocks, in the neighbourhood of Grosmont, some of the nodules of which are amongst the richest of the clay or calcareous ironstones. I omitted to state that, with the exception of the first 60 feet, where the ground was so steep that I could not fix my instrument, and from which there may be some slight inaccuracies, I took my levels with a good and safe instrument, and the operation was performed in the ordinary way of back and foresights. I find the difference between my levels and what I suppose the correct position of the top bed of ironstone, and that shown by the bore-holes, to be 64 feet.

The President—The question of the difference of the levels rests entirely upon the assumed inclination of the beds; a difference of level of 64 feet in a distance of 400 yards, accords, in my opinion, with what may be supposed to be the regular inclination of the beds.

Mr. Bewick—Yes; but in your section you connect two sandstones which have nothing to do with each other, viz., the sandstone found in the air shaft immediately overlying the top bed, and the sandstone found in the bore-hole, between which there are several feet of *alternating strata*, and to do which you must of necessity raise your level line, and show the strata to be rising in that direction, but the drift you have driven some distance into the side of the hill, and at the same point as my line of levels, shows the strata to be *dipping in that direction*. I may mention, too, that had another bed of 32 feet thick really been met with in the bore-hole, it must have been found along the sides of the valley, which are intersected in so many places with mountain streams, all of which have been searched by persons having a fair knowledge of the geology of the immediate neighbourhood, but without the least trace of it having been met with.

The President—I cannot think that there is the least doubt that the sandstone in the pit, No. 1 section, plan No. 2, is the same sandstone as that proved in the borings; all the appearances on the surface, as well as the general rise and dip of the strata, prove this. Extending the line of section across the valley, it is clear there is a general rise of strata along the line of section. No doubt the strata in the drift dip towards the west, but that is no doubt influenced by the slip dyke which crosses it. I would observe, that taking the line of section along the face of the valley in plan No. 1, in the direction of the dotted line *a b*, and applying the inclination of the top bed of ironstone, shown on plan No. 4, to that line, and not to the curved or projecting line along the face of the hill, the posi-

tion of the bed would be rising from a towards *b*, and it would require a slip dyke, shown on plan No. 1, to throw the bed into its proper position along the face of the valley to the west of the magnetic quarry. On examining plan No. 1, it will be seen that the magnetic quarry and the top bed of ironstone, as shown in plan No. 4, project considerably to the east of the general line of the side of the valley, which being towards the dip of the strata, shows the top bed at a lower level than if the section had been continued in a more direct line, or in the direction *a b*. Whatever conclusion, therefore, may be arrived at after all the explanations given, we have the fact of an almost horizontal bed of ironstone, and of nearly an uniform thickness, distinct in character from the ordinary beds of the district, extending over a length of 568 yards and a width of 200 yards, which clearly proves that it is not a vein. How much greater distance it extends, must be left to future explorations to prove; but it would certainly be an extraordinary anomaly in geology for such a thickness of strata to disappear altogether in a short distance. If it extends across the valley, as shown in Mr. Bewick's plan No. 5, then there is no reason to suppose that it may not extend to the same distance to the north; and if, according to Mr. Bewick, the borings have not yet reached to the top bed of ironstone, then the deposit of ironstone, in the valley of Rosedale, is richer in ore than either Professor Phillips or myself have set forth. The correct extent must, however, be left to future explorers to discover. Enough has been proved to show a most extraordinary deposit of a very peculiar and rich ironstone, and well worth further investigation.

Mr. Bewick—There is a section of the cross drift, shown on plan No. 2, driven at right angles from the main drift to prove the breadth of the dyke, and which, at the distance of 16 yards, cuts the shale, and apparently touches the top seam at the same time. At the distance of 6 yards, the stone in this drift ceases to be magnetic. It is, therefore, incomprehensible to me how it can again become so at the distance of 200 yards from this point. Of course, you have a right to infer from the information that reached you that such is the case. Still I would strongly recommend that the borings should be continued to prove whether the sandstone below you or not ; to ascertain which could not fail to give great satisfaction to all concerned, the cost would not be great, as the bottom of your borings must be near the top of that rock.

The President—The cross drift was not sufficiently extended to the west to prove the dyke, but, as there was a considerable rise of the strata in that direction, no doubt such an inclination has been occasioned by the

proximity of the dyke, shown on the plans No. 1 and 2. All the facts show that the slip dyke has been a dislocation subsequent to the formation and consolidation of the various beds affected by it; and, consequently, such dyke could not, we can scarcely conceive, have any influence on the character of the ironstone bed itself, especially as it is not contended, I believe, that such dyke is either of a basaltic or mineral character, there being no appearances, in my judgment, to justify such a conclusion.

NORTH OF ENGLAND INSTITUTE of MINING ENGINEERS.

ADJOURNED MEETING, THURSDAY, MAY 5, 1859, IN THE ROOMS OF THE INSTITUTE, WESTGATE STREET, NEWCASTLE-UPON-TYNE.

Matthew Liddell, Esq., in the Chair.

The Secretary having read the minutes of the Council,

The following gentlemen were elected members of the Institute: —

Joseph J. W. Watson, Ph. D., &c, The Knap, Cheltenham; and Richard George Coke, Ankerbold, Chesterfield.

Mr. Berkley said, Mr. Greenwell had sent some remarks on the object of regulators.

The Secretary said, Mr. Thos. John Taylor had told him to communicate to the meeting that he was unavoidably absent, but he would be quite ready to enter into the subject for discussion at the meeting in June. If this paper of Mr. Greenwell's bore on the subject, the whole Matter might be postponed till the next meeting.

After some further conversation the Chairman requested Mr. Berkley to read the following remarks by Mr. Greenwell:—

I think it necessary to make a few remarks upon the following observation contained in Mr. Atkinson's paper, read in June, 1858, p. 165:—

"The opinion here to be advocated is, that except in so far as the difference of density in the air, in different parts of the air-ways in different splits, where the course followed by the circulating air may dip or rise, may give rise to discrepancies, air will divide itself in the same proportions over any number of routes or splits, whatever may be the gross quantity of air circulating in the unit of time."

On reference to page 160, it will be observed that I expressed before the Institute an opinion at variance with the above.

Experiments have since been tried, and the results, read at the meeting held on February 3rd, seem practically to corroborate the view taken by myself, to such an extent, at least, as to demand the careful examination of each division of air, in the event of any sudden depression of the ventilating power.

My more particular object, however, in making these few with the following remarks, is to endeavour, if possible, to show why the practical result is at variance with the theoretical opinion; and I make them with considerable diffidence, because they involve some deviation from the law governing the circulation of currents of air in mines, as now generally accepted.

The present law treats of air as a fluid passing along a channel of uniform dimensions, similarly to water or air passing along a pipe; and this is a position which I now assail. If this were correct, I should I agree with the opinion expressed by Mr. Atkinson, but in consequence, I as I think, of the premises being inaccurate, the results, as proved in practice, are equally so.

And I do not think that the observations on these experiments, contained in page 77 of the present volume, at all affect the value of the lesson which they teach, for "routes not being level, " return air being at liberty to pass into other workings," "leakages of air," &c, &c, are such occurrences as are constant in practice, and fact as we find it, must be our guide, and not fact as it theoretically ought to be.

An air-current passing along an air-course does not pass along an uniform pipe, but it passes along a channel the area of which is in some parts capacious, but in others comparatively speaking contracted. In fact, to confine ourselves, for perspicuity's sake, to one very short and one very long current, the short air-course may consist of 100 yards of fifty square feet in area, and the long one of 10,000 yards of the same dimensions. For one inch in length of the short current, the area is perhaps only one square foot, and there are probably several places in the long air-course, where the area for short distances may not exceed twenty-five feet.

As much, therefore, as the inch in length of the one air-course, rather than its full length of 100 yards, is the ruler of its quantity of air; so much are the contracted spaces existing in the other air-course, rather than its full length of 10,000 yards, the ruler of its proportion.

We will now suppose that each division consists of 10,000 feet of air, this amount being adjusted by experiment. If we double the exhausting power we do not affect the two air-courses equally, because, in the one case we increase the ventilating pressure upon an area of one square foot, and in the other upon twenty-five, checked in the former case by the friction along 100 yards of 50 feet area, and one inch of one foot area, and in the other by 10,000 yards, chiefly of 50 feet area, but in places only 25. We, in this case, by increasing the exhausting power, increase the quantity circulating through the long division in a greater ratio than that passing through the short one. If, on the other hand, we reduce the exhausting power, we produce a diminished effect upon the short division as compared with the long one, the ruling area acted upon by the exhausting power being smaller.

I have not gone at any length into this question, as it strikes me that its importance is such that it must give rise to considerable investigation and discussion, rendering further comment unnecessary at present. I would simply observe against the accuracy of any conclusion that will be arrived at, by taking an average area of any air-course as a basis of calculation of the resistance met with by ventilating currents.

The Chairman then said, no one would take the average size of the area as a principle.

Mr. Atkinson said, there was nothing like an average area assumed in anything he had said or written. He had drawn up a few remarks, connected with his former paper on splitting air, which he had intended to be introduced as part of the discussion.

The Chairman—If this is merely a part of the discussion, it could scarcely be printed till the discussion comes on.

Mr. Atkinson—Either that, or let each of these papers go as the discussion held to-day, and then adjourn the discussion till next meeting. If it was thought well to receive this as a short discussion, the President and others might read it before the next meeting.

The Chairman—How would that affect Mr. Taylor? if he has gone to the trouble to make experiments.

Mr. Atkinson—It would be an advantage to him. He would have an opportunity of considering it previous to the discussion.

The Chairman—Under these circumstances the paper may be read.

Mr. Berkley—It would be an advantage.

Mr. Atkinson then read the following remarks:—

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There are one or two statements contained in the preceding part of the discussion of this subject as to which I beg to make a few remarks.

At page 70, vol. VII. of " Transactions," the late Mr. Wales, whose recent death we have to deplore, is reported as intimating that the experiments he had previously given (see pp. 66 and 67 of "Transactions,'' vol. VII.) indicated that far off splits of air receive a smaller proportion of the gross quantity circulating than the shorter splits, as such gross quantity becomes reduced in amount, whether the far off splits happen to be rise, dip, or level ones. And in the next following page, he appears to have signified that the principles advocated in the paper under discussion had only been confirmed in the exceptional case of his No. II. set of experiments, made in the Eppleton pit.

These statements, I submit, with all due respect for the memory of the departed, are not supported by the experiments to which they allude, nor by any others that have come under my notice or been laid before the members of the Institute.

In No. I. set of experiments, Mr. Wales shows by his diagram, and admitted in this room, that the far off split was a rise split, while the near hand one was virtually level, so that the results are in perfect keeping with the principles of the paper under discussion.

In No. II. set of experiments, Mr. Wales himself admits that the principles of the paper are borne out, as the long dip split obtains an increased share of the gross quantity of air as it becomes reduced in amount. And I may remark that, on two previous occasions, (making-three in all,) under different arrangements, in the presence of Mr. Wales and myself, in the same pit, precisely similar results were obtained. On one of these occasions, the waste steam from the underground boilers, instead of passing direct to and up the upcast shaft, was reversed and carried inbye in the long dip split, when the main regulator was nearly closed; and on the other occasion, although this evil was provided against by placing the main regulator on the inbye side of the boilers, yet we found the air in the short split was reversed when the main regulator was reduced to a very small area, while a considerable quantity of air continued to circulate in the long split - the return air from the long dip split overcoming the total ventilating pressure, and passing outwards, through the route of the short split, into the main intake of the long or far off split, when the total quantity was small.

As regards No. III. set of experiments (page 67, vol. VII. of " Transactions"), made in the Blossom pit, at Hetton Colliery, Mr. Wales

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admitted in this room, and, indeed, shows in his diagram, that the air in the long split had access to rise workings, while the short split was virtually level, so that these results are admittedly in accordance with the principles laid down in the paper under discussion. Since the February meeting, however, Mr. Wales and myself made a series of experiments in the same pit, with the rise workings of the long split partially closed off, and although, when we reduced the gross quantity of air by closing the main regulator in the main intake, we found that a reduced proportion of air passed into the long split, we at the same time found ,that an increased proportion came out of it; and although more pains had, on that occasion, been taken to prevent leakage from the Eppleton pit than when No. III. set of experiments were made, still Mr. Wales himself was constrained to admit that this leakage was such as to render even these experiments useless. And on a still more recent occasion, with the rise workings of the long split partially shut off, and still greater precautions against leakage from the Eppleton pit, we found that when the gross quantity of air was reduced to its lowest amount, and none passed inwards in the main intake, there was still as much (or a little more air) coming out of the long split as passed through the short split, still indicating that, from leakage, the experiments were quite inconclusive; and as the two last-named trials were instituted with the view of testing the principles of the paper in perfectly level splits, it is the lamented decease of Mr. Wales that has delayed further and more satisfactory trials being made in this pit. The source of error will act in directly opposite directions, if the trials are first made by placing the main regulator in the intake, and afterwards repeated with it in the main return, and they would be nearly eliminated by reducing the furnace power without employing a main regulator in either the main intake or main return; and I propose to have, with the aid of Mr. Daglish, one or both of these modes tried before another meeting, if the question is allowed to remain open till then.

We see, therefore, that in No. III. set of experiments there is nothing contrary to the principles of the paper under discussion.

In No. IV. set of experiments I have already admitted there is an apparent contradiction to the principles of the paper, as a long dip split there appears to obtain a decreasing proportion of the gross quantity of air as it becomes reduced in amount. That it is only an apparent and not a real departure from the principles of the paper I am satisfied, and have previously (see page 74, Vol. VII., "Transactions,") pointed out

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the probable source of this apparent discrepancy between the theory and observed result, as arising from a leakage from the Eppleton pit into the long split, which leakage would become greater and greater in amount as the gross quantity of air became less and less in amount, by closing the main intake regulator, and thereby taking the pressure off the Minor pit side of the holing, and thus introducing Eppleton pit air into the long dip split, owing to which the quantity of air passing inwards in the intake, to that split from the Minor pit, would necessarily be reduced in amount, and suppressed, as indicated by the results of the experiments, while the real quantity in the long split would, at the same time, be an increased one.

It would be highly satisfactory to myself, and, I think, not without interest to the members, if Mr. Daglish could spare time to repeat these as well as the Blossom pit experiments, with increased precautions against errors and leakage, to lay before some future meeting.

In further confirmation of the general accuracy of the theory adopted in the paper under discussion, which maintains that in dip-ways, when the intakes have lower average temperatures than the returns, as is usual in mines, the long dip splits will get a continually increasing share or proportion of the gross quantity of air circulating, as it becomes reduced in amount, and confirmatory of the results obtained on three separate occasions in the Eppleton pit, as already alluded to, I have been favored with the result of another set of experiments, instituted at the Springwell colliery, by Mr. Southern, in which the far off, or long split, had a very considerable dip.

The accompanying sketch shows the general position of the air-ways, and the rise and dip of the strata; *R f* is a main regulator, placed in a part of the main intake traversed by the air before reaching the splitting point *A; R b* is a similar regulator, placed in a part of the main return traversed by the air of the splits after they have been again united at the point *B*, and, by means of either of these regulators, the gross quantity of air circulating can be altered in amount, while the air-ways of the splits, and the regulator *c*, in the short one, remain unaltered. At the point *e* in the long split, and at the point *d* in the short one, are frames, each having an opening 3 feet x 3 feet, or 9 feet area, where the number of revolutions per minute performed by an anemometer can be tried.

The short split was merely the length of a stenting from *A* to *B,* a distance of 20 yards, and was nearly level, while the long split traversed

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Plan showing the ventilation of part of the workings of Springwell Colliery, May 1859.

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sundry districts of workings in the form of sub-splits, all, however, lying very considerably to the dip of the short split, being the whole of the workings down the engine bank of the colliery.

At *c*, at a distance of 18 yards from the measuring place in the short level split, a regulator was so fixed as, at the commencement of the experiments, to equalize the currents in the long and short splits, as indicated by the same anemometer, at the measuring places *e* and *d* in the respective splits, each of which were nine feet in area, and this regulator was allowed to remain in the same state during the whole of the experiments.

In the first series 1, 2, 3, and 4, the main regulator in the return was not made use of, but was standing wide open while the gross quantity of air was reduced, by closing more and more the main regulator f in the main intake, and the results were as follows :—temperature, return 65°, intake 56°.

SPRINGWELL COLLIERY

[Table of Results]

From these experiments, we perceive that the far off split, owing to its being a dip-way, and having a higher temperature in the returns than in the intakes, obtains a continually increasing share or proportion of the gross quantity of air circulating, as it becomes reduced in amount by closing a main regulator either in the intake or return air-ways, and

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thus being in accordance with the principles adopted in the paper under discussion, and opposed to the very common opinion to the contrary, to the effect that the opposite results would ensue, whether the splits were rise, dip, or level ones, and to which opinion our lamented friend Mr. Wales appears to have clung even after being acquainted with three different sets of experiments, made at Eppleton colliery, all giving the contrary results.

As the quantities of air in the long and short splits, in No. II. set of experiments, made at Eppleton colliery, were never so regulated as to be equal in amount, and as the short run regulator was never altered during the experiments, the extreme cases may be stated as follows (see page 66, vol. VII. of "Transactions ") :—

[Table]

But, in order to compare the results, let us alter each of the largest quantities to 1000, and ascertain what the lesser quantities would become when altered in the same ratio.

As 1220 : 1000 :: 485 : 397.54 the proportionate number of revolutions per minute on reducing the greatest number to 1000 in the short split; and

As 1140 : 1000 :: 545 : 478.07 the proportionate number of revolutions per minute on reducing the greatest number to 1000 in the far off or long split, so that these results may be viewed thus—

[Table]

So that, on proportioning the largest quantities as equal to each other, we find that, on reducing till the revolutions are only 40 to 50 per cent, of their original amount, the far off split obtains an increase of about one-fifth greater number of revolutions per minute than the near hand or short split.

In conclusion, I would merely submit the following experiments on this subject, which have been sent to me by Mr. Cooper, who made them at the Grange Colliery. I give them because the reduced quantity of air was effected by reducing the furnace power, and not, as in most of the other cases, by means of regulators; the measurements of the air being made by timing powder smoke.

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Experiments on Splitting Air.

Grange Colliery, 27th August, 1858.

The furnace was fired well all day on Friday, up to 6 p.m., and then ceased entirely to 6 p.m. on Saturday.

No I. Experiment at 6 p.m., Friday, August 27.

No. II. " at 6 a.m., Saturday, August 28.

No. III. " at 6 p.m., " "

The heat arising from steam pipes in the pit and workings seemed to keep the temperature at 70° in the upcast, and the downcast temperature was near about 53½º.

[Table]

Mr. Dunn observed that Mr. Ross's paper stood for discussion.

The Chairman said he had not seen Mr. Ross's paper, and as there were so few members present, he suggested that the discussion be postponed. It was an important subject, and, in justice to Mr. Ross, it might be as well to postpone it, to give an opportunity to those, who, like himself, had not read the paper, of considering it.

Mr. Ross consented to this arrangement, after which the meeting was adjourned.

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ON THE RELATIVE IMPORTANCE OF CERTAIN CAUSES

In PRODUCING CHANGES OF DENSITY

IN THE AIR OF MINES,

AS IT PROGRESSES IN CIRCULATING.

By Mr. JOHN J. ATKINSON.

At a recent meeting of the members of this Institute, some difference of opinion was expressed as to the comparative importance of two causes, each operating in expanding, and thereby in reducing the density of the air in the return air-ways, below the density of that in the corresponding intake air-ways, under the ordinary conditions prevailing in mines. See Transactions, vol. VII., page 75, &c.

I then stated that I considered that the higher temperature usually prevailing in the air in the return air-ways, over that existing in the air in the intake air-ways, had, under the ordinary conditions of mines, a much greater effect in expanding the volume, and thereby reducing the density of the air of the returns below that of the air of the corresponding stakes, than arose from the reduced barometrical pressure prevailing in the returns compared with that prevailing in the intakes, owing to the gradual expenditure of the motive column, or ventilating pressure, as the air progressed in its circulation through a mine - a cause which I held to be (under the ordinary conditions of mines) one of small importance in comparison with the former.

I beg to submit the following investigation of the matter to the notice of the meeting.

The expansion which air (under a constant pressure) undergoes by an

increase of temperature is such, that any volume of air at 0°, or zero of Fahrenheit's scale, is increased in bulk by 1-459th part of such volume for each degree of heat added, so that the *comparative volumes*, assumed by a given quantity of air, at different temperatures, so long as the pressure remains constant, are directly proportional to the sum of the temperature and the constant number 459; thus, air under a constant pressure at 40°, 50°, and 60°, has volumes respectively proportional to 499, 509, and 519. And since the densities of air are evidently inversely proportional to the spaces occupied by a given quantity or weight, it follows that at the temperatures 40°, 50°, and 60°, under a constant pressure, the densities are respectively proportional to the fractions 1/499, 1/509, 1/519.

From what has been stated, it is evident that if by t we denote the average temperature of the intake air, and by T that of the return air, then, under a constant pressure, will 459 + t and 459 + T represent numbers proportional to the volume assumed by any given weight of the air in the intake and return respectively; and the fractions 1/ 459+ t and 1/459+ T or the numbers 459 + T and 459 + t will be proportional to the densities of the air in the intake and return respectively.

On the data just given, and assuming the pressure to be constant, if we take the average temperature of the intake air at 40°, and that of the return air at 41° (being only one degree more), we should have the relative volumes in the intake and return proportional to 499 and 500 respectively; and taking the density of the intake air to be represented by 500, that of the return air would be 499; being a reduction of 1-500th part below that of the intake air.

Again, if the average temperature of the intake air were 50°, and that of the return air 51°, the volumes would be respectively proportional to the numbers 509 and 510; and, therefore, the density of the intake air being assumed at 510, that of the return air would be 509; or 1-510th less.

And, similarly, if the average temperature of the return air were 60*, and that of the intake air 61°, the volumes would be respectively proportional to the numbers 519 and 520.

And assuming the density of the intake at 520, that of the return would be 519, which is 1-520th part less than the density of the intake air.

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Let us now proceed to consider what amount of ventilating pressure (as represented by a water gauge connecting the intake and return airways near the bottoms of the downcast and upcast shafts), gradually expended on overcoming the resistances offered by the air-ways extending from the bottom of the downcast shaft to that of the upcast shaft, would be required to give rise to equal amounts of differences of densities between the air of the intakes and that of the returns, to those differences of densities we have already seen to arise from an average difference of 1° of heat at different temperatures; presuming, for that purpose, that the temperature of the air remains uniform or constant throughout all parts of the intake and return air-ways. Taking the atmosphere at the surface of the earth, or the tops of the pits (and presuming them to be on the same level), at 30 inches of mercury (taken at 52°); then, since the density of mercury at 32° is 13.596 times as great as that of water at its greatest density, and since mercury expands about 1-9990th part of its bulk at 32°, for each degree of temperature added, this pressure at the tops of the pits would be equal to

[Calculation] = 407.07 inches

of such water column; but taking the pressure at the bottom of the downcast pit at 31 inches of mercury (at 52°), this would give a pressure of 1-30th more, or of 420.64 inches of water column, taken at its greatest density; while the intermediate pressure of 30.5 inches of mercury, at the same temperature, would be equal to a pressure of 413.85 inches of water column at its greatest density, and the latter is, perhaps, more nearly the average pressure of the atmosphere in mines than the former.

Now, supposing that a water gauge connecting the intake and return airways of a mine, near the bottoms of the pits, indicates a pressure, in inches, of water column represented by m, as being expended on the distances of the air-ways extending from the bottom of the downcast to that of the upcast shaft; and, for perspicuity, let it be further presumed that the whole of the air-ways are situated in the same horizontal plane, and that the air goes direct to the face in the intake, and returns direct back to the upcast shaft in the return, - equal distances of its entire route presenting equal resistances, - then the barometrical pressure at the bottom of the downcast shaft, being taken at 31 inches of mercury at 52° or 420.64 inches of water column, would be reduced to 420.64 - m/2

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at the face, or extremity of the intake, because m/2, or half of the motive column, will have been gradually expended on overcoming the resistances of the intake air-way before reaching that point, making the average pressure of the intake air

[Formula]

The pressure at the bottom of the downcast shaft would, of course be 420.64 — m, and that at the face being 420.64 - m/2, gives the average pressure of the air in the return, as

[Formula]

the average pressure in the return, is, therefore, less than that in the intake, by [calculation]* or half of the ventilating pressure expended on the resistances of the workings.

Now, the fraction of the pressure of the intake air, by which the pressure of the return air is less than it, is evidently

[Formula]

and, since the densities are proportional to the pressures, this fraction (2 m/1682.56 - m) is also the fractional part of the average density of the intake air, by which it exceeds that of the return air; and, therefore, in any case, we have only to establish an equation between this fraction and that representing the reduction of density, arising from an increase of a degree of temperature, and, from it, to ascertain the value of m, in order to determine the necessary expenditure of ventilating pressure on the resistances of the air-ways required to produce the same amount of difference of density between the air in the return and that in the intake, as is due to the assumed difference of temperature of one degree.

We have already seen that a difference of 1° of temperature between

* And hence in all cases m is proportional to, or varies as, the *difference* between the average pressures and densities of the air in the intake and that in the return, so far as it arises from this cause.

the intake and the return airs creates a difference of 1-500th part in the densities, when the air of the intake has a temperature of 40°, and that of the return one of 41°, and, by putting

[Calculation]

we find m = 1.681 inches of water-gauge as necessary to cause the same average difference of density, between the air in the intake and that in the return, as would arise from a difference of 1° of temperature, on supposing the temperature of the intake to be 40°, and that of the return to be 41°, and the pressure to be constant.

And, if we presume the average temperature of the intake air to be 50°, and that of the return to be 51°, then, since the density will be altered by 1-510th part, from this source, we have

[Calculation]

and, therefore, m = 1.648 inches of water gauge ventilating pressure, as necessary to cause an equal difference of density to that arising from a difference of a single degree of heat at these temperatures.

Again, taking the intake temperature at 60°, and that of the return at 61°, the reduction of density in the return is 1/520th, and putting

[Calculation]

we get m = 1.616 inches of water gauge as necessary to be expended on the resistances of the workings, to give rise to a difference of density (between the air in the intake and that in the return), equal to that which is due to a difference of only 1° in the average temperatures, when that of the intake is 60° and that of the return 61°.

An examination of the formula that has been employed will show that if the barometrical pressure of the air at the bottom of the downcast shaft were less than 31 inches of mercury, or 420.64 inches of water column, then the water gauge column, or ventilating pressure, required to give rise to equal differences between the density of the intake and that of the return air, to those already ascertained as arising from 1° of temperature, would not be so great as those that have been found as to the greater barometrical pressure, in exactly the same ratio that the barometrical pressure itself might be less than that just mentioned. And, by applying the formula, we obtain the results specified in the following table, as due to barometrical pressures of 31, 30½, and 30 inches of mercury.

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[Table]

It is, perhaps, needless to remark that it rarely happens, in practice, that there is a greater amount of ventilating pressure expended on the resistances of the workings of mines than even the least of those contained in the above table, as being required to give rise to the same difference of density between the air in the intake and that in the return, as would result from a difference of only one degree of temperature at the respective barometrical pressures and average temperatures indicated in the table; it may, therefore, be concluded, that even under the extreme conditions of the average temperature of the intake air being as high as 60°, and the barometrical pressure at the bottom of the downcast pit being as low as 30 inches of mercury, it would still require as much as 1.564, or upwards of 1½ inches of water gauge column to be gradually expended on overcoming the resistances of the workings of such a mine, in order to produce the same disparity of density between the air of the intake and that of the return, as would result from a difference of only a single degree in their average temperatures.

As the average temperature of the air in the return airways of coal mines generally exceeds that of the air in the intake air-ways by several degrees, and as few mines exhibit the amounts of water gauge indicated in the foregoing table, it is submitted that the reduction of density; arising from the gradual expenditure of ventilating pressure, is, under ordinary conditions, very much less in amount than that arising from the average temperatures of the air in the return air-ways of mines being higher than those of the air in the corresponding intake air-ways, and

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that, therefore, the effects arising from the former cause are generally of small importance in comparison with those arising from the latter.

The average effective differences of density, arising from the causes under consideration, are much the same in rise or dip workings as they are in level workings; for, in traversing rise workings, although the absolute density of the air would become less and less (quite independent of the expenditure of the ventilating pressure), owing to its being compressed by less and less of air column as it ascends ; still, there is a further reduction of density, arising from the gradual expenditure of ventilating pressure on the resistances of the air-ways, as it progresses in its circulation, and also from the expansion due to any gradual increase of temperature that may happen to take place as it travels.

And, in like manner, although the absolute pressure and density of air going into dip workings may become greater and greater as it proceeds, owing to its becoming compressed by a continually increasing atmospheric column, still this increase is less than it would have been but for the gradual expenditure of ventilating pressure on overcoming the resistances of the air-ways as they are traversed, or but for the gradual expansion of the air, by its increasing temperature, as it progresses in its route.

Were it not for the changes of density in the air, arising from changes of temperature, or from the gradual reduction of pressure due to the expenditure of ventilating pressure on the resistances encountered, or from other causes, no forces would be created, even in rise or dip workings, either in favour of or opposed to the general ventilating pressure, because, in the case of rise workings, the gradual reduction of density due to the air as it ascended the intake, and thereby became subject to a lessened superincumbent column of air, would have been exactly counterbalanced by the gradually increasing density as it descended again in the return, and thereby became subject to the pressure of a correspondingly increasing superincumbent column of air.

And, similarly, in dip workings, the gradual increase of density due to the air as it descended in the intake, thereby becoming subjected to the pressure of an increased height of air column, would have been exactly counterbalanced by the gradually decreasing density as it ascended again in the return, and thereby become subject to the pressure of a correspondingly reduced superincumbent column of air.

It may be added, that the effects of the gradual expenditure of ventilating pressure in expanding the air in the returns over that in the

intakes would become less and less in amount as the quantity of air circulating in the unit of time became itself reduced, if the reduction arose from a reduced ventilating pressure, or from the absorption of an increased share of such ventilating pressure by any main regulator placed in the air-ways traversed by the air before reaching or after passing the splits under comparison, inasmuch as the pressure expended on forcing the air round the splits would thereby become less and less in amount ; indeed, the pressure favourable to ventilation in dip workings, and retarding it in rise workings, so far as it arose from this source, would become reduced in a much higher ratio than the corresponding reduction in the quantity of air circulating in the unit of time, the total pressure varying as the square of the quantity of air circulating in the unit of time.*

Thus, if 2.5 inches of water gauge caused 50,000 feet of air to circulate per minute, on reducing it to 40,000 the water gauge would only indicate 1.6 inches of column; on further reducing the air to 30,000 feet, the water column would fall to 0.9 inches; and, on reducing the air to 20,000, it would fall to 0.4 inches; while, on reducing the air to one-fifth of its original quantity, or to 10,000 feet per minute, the water gauge would be reduced to 1-25th of its original amount, or to 0.1 inch; so that this is far from being a constant force, either in favor of or opposing ventilation.

The following example is submitted, to illustrate the general conclusion that has been arrived at in the foregoing pages.

Suppose a mine to have two pairs of drifts, driven perfectly level, to the extent of 1,000 yards, or 3,000 feet, on opposite sides of the shafts, with a stenting or communication connecting each pair at their extremities

* Since the squares of the quantities of air circulating in the unit of time, in the same unaltered splits, are directly proportional to the motive column expended on overcoming the frictional resistances; and since the amount of the difference between the average barometrical pressure or tension of the air in the intakes, and of that in the returns of the respective splits, is also proportional to the same motive column expended on the frictional resistances; and since such differences between the pressure or tension of the air of the intakes and that of the returns of the respective splits are, at the same time, proportional to the differences that they create between the densities of the air of the intakes and that of their respective returns, and also to the local forces or pressures to which the latter, in turn, give rise (operating in aid of the general ventilating pressure in dip workings, and in direct opposition to it in rise workings), it results that these local forces, arising from such differences of density, themselves vary in the same ratio as the general ventilating pressure applied to the frictional resistances, and therefore, like itself, are proportional to the square of the quantity of air circulating in the unit of time; and hence, although they do affect the absolute quantities of air circulating to the small extent shown in this article, they cannot alter the proportions in which it distributes itself over a series of common splits, as it becomes altered in amount, so long as the air-ways forming such splits remain unaltered, and the temperature of the circulating air is the same throughout their extent.—See Note, page 118.

one of each pair being connected with the downcast shaft, and used as an intake, and the other connected with the upcast shaft, and used as a return air-way. Also, suppose the same mine to have two other pairs of drifts, of the same length as the level ones, one pair extending 3,000 feet, to the extreme rise, and the other a similar distance to the extreme dip of the strata; the dip and rise being assumed at 1 in 20, each of the four pairs of drifts being ventilated by a distinct split of air.

Let it be further assumed that the temperature of the air over the entire extent of each split is 62°. Taking the barometrical pressure of the air at the bottom of the downcast shaft at 30½ inches of mercury, of the density due to the temperature of 32°.

Now, supposing the ventilating pressure expended on overcoming the resistances met with by the air in traversing the drifts composing the workings of the mine, to be equivalent to the pressure of 3 inches of water column, taken at its greatest density, (which is due to a temperature of 39°.1, according to Joule and Dr. Lyon Playfair), as indicated by a water gauge, connecting the intake and return air-ways at the bottom of the shafts, and, therefore, not exhibiting the pressure due to the resistances encountered in the shafts, this ventilating pressure would be equivalent to a column of mercury, of the density due to a temperature of 32°, of [calculation] = 0.220653 inches in height.

Now, it has been found by M. Regnault, that mercury is 10,517.3 times denser than air, at a temperature of 32° in each case, the air being under a pressure of .760 metres, or 29.92196 inches of mercury. But the density of air at 32°, is to that of air at 62°, as 459 + 62 is to 459 + 32, or as 521 is to 491; so that the ratio of the weight of mercury at 32°, to that of air at 62°, under a pressure of 30½ inches of mercury, (also reduced to 32°) is as [Further calculations] = 10,948.4 to 1; and hence the barometrical pressure at the bottom of the downcast shaft, reckoned in air column, of the density due to a temperature of 62°, is

[Further calculations] = 27,827.2 feet.

The ventilating pressure expended on the workings, exclusive of that expended on the resistance in the shafts, is equivalent to an air column

[Further calculations] = 201.3 feet in height.

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[Further calculations]

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[Further calculations]

And since it is

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not many mines that afford more than one-half of the motive column of three inches of water to be expended on the workings, we may conclude that, in the most of our mines, an average difference of a single degree of temperature between the air of the intakes and that of the returns would produce equal effects in reducing the density of the air of the returns below that of the intakes, with those that arise from the cause we have been considering.

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NORTH OF ENGLAND INSTITUTE of MINING ENGINEERS.

GENERAL MEETING, THURSDAY, JUNE 2, 1859, IN THE ROOMS OF THE INSTITUTE, WESTGATE STREET, NEWCASTLE-UPON-TYNE.

Nicholas Wood, Esq., President of the Institute, in the Chair.

The Secretary having read the minutes of the Council, the following gentleman was elected a member of the Institute:—Mr. Samuel Bailey, Ward Collieries, near Walsall, Staffordshire.

The President then said, the Council had suggested that it was desirable that the discussion on ventilation should be deferred until a meeting to be called for that purpose in July. The members of the Institute would recollect that the original subject of discussion was, the Splitting of Air in Mines, and the proportions which each Current of Air would distribute itself along Routes of different Lengths, when subjected to different resistances, by alterations in the ventilating pressures, or by variations in the gross quantity of air circulating in any unit of time; supposing the regulators distributing such currents remain unaltered during such variation of quantity, or alteration in the motive column or ventilating pressure.

It was assumed by some gentlemen, and by Mr. Atkinson, in particular, that, excepting so far as the gravitation of different temperatures and densities in ascending and in descending portions of the air-ways forming the different routes, might effect the result; air would divide itself over any series of splits in the same proportions, whatever might be the amount of ventilating pressure or the gross quantity of air circuiting in any unit of time, the regulators remaining unaltered.

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Other gentlemen, and Mr. Wales, in particular, contended that practically, the regulators in a pit remaining unaltered, and the relative quantities of air traversing long and short splits being adjusted, any increase in the ventilating pressure, or variation of the quantity of air circulating the mine, would give a comparatively increased proportion of the altered quantity of air, in the long routes, and a diminished proportion of it in the short routes; while a diminution of the aggregate quantity of air would give, on the contrary, a diminished share or proportion in the long route, and an increased one in the short route.

A great many experiments have been made to test those opinions. Mr. Atkinson had favoured the meeting with elaborate investigations on the theoretical consideration of the questions, and he (the President) understood that Mr. Atkinson intended laying before the meeting to-day some further experiments; and, also, some observations on the result of the several experiments which had been made. Mr. Thomas John Taylor, while agreeing with Mr. Atkinson's general conclusions on the subject, (see pages 75 and 76 of the present volume of "Transactions,") still held that he had omitted to include amongst the causes assigned for his conclusions, one of a highly important nature, and particularly at the meeting of February last, thinking that he had not paid sufficient attention to the difference of density between the intake and return air of mines, so far as it arose from the expenditure of the motive column, and promised that he would write a paper, specially on the subject, which they expected Mr. Taylor would have produced at this meeting. Since he had taken the chair, he had received a letter from Mr. Taylor, expressing his regret that a pressure of business had prevented him from being present at the meeting; but, that he had forwarded a few preliminary observations of his views on the subject, and stated that he hoped at the next meeting of the Institute, he would be able to lay before them a more elaborate paper on the subject, and be present to offer any explanation thereon. He would, therefore, propose, that the following preliminary observations by Mr. Thomas John Taylor be read.

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ON THE CAUSES OF THE VARIATION OF THE DENSITY OF AIR CIRCULATING IN COAL MINES.

By THOMAS JOHN TAYLOR.

In the question now under discussion by the Institute, a wrong impression appears to have arisen respecting the real points at issue. This will be, perhaps, best understood by making a few comments upon the following passage in Mr. Atkinson's paper, read at the last meeting.

Referring to a former communication Mr. Atkinson says, "I considered that the higher temperature usually prevailing in the air in the return air-ways, over that existing in the air in the intake air-ways, had, under ordinary conditions of mines, a much greater effect in expanding the volume, and thereby reducing the density of the air of the returns below that of the air of the corresponding intakes, than arose from the reduced barometrical pressure prevailing in the returns compared with that prevailing in the intakes, owing to the gradual expenditure of the motive column, or ventilating pressure, as the air progresses in its circulation through a mine, a cause which I hold to be (under the ordinary conditions of mines) one of comparatively small importance in comparison with the former."

Mr. Atkinson then proceeds, with great clearness, to establish the position thus laid down. It is, however, submitted that the effect of local differences of temperature upon the ventilation of mines has never been denied, and, least of all, by myself. On the contrary, the difficulty I have experienced, while allowing their full influence to those differences, is to account for the circumstance of their proving, not too little, but too much, as regards their influence upon ventilation, especially in the case where the airing of rise districts is compared with that of others lying to the deep of the same upcast shaft.

To show my meaning more perspicuously, I shall here quote some

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experiments made first by myself, and afterwards repeated, on my request, by Mr. George Greenwell, some years ago, at Backworth Colliery, that mine having been chosen on account of its great differences of level, which at the time amounted to ninety-one fathoms between the extreme dip and rise.

[Tables of Results: I, Rise Air; II, Deep Air]

The remark made upon these experiments is as follows:

"In both returns the air is warmer than in the respective ingates: and this alone, on a first glance, would seem to furnish a solution to the fact that dip workings are better to ventilate than rise ones ; for lighter air in a return from the dip constitutes a part of the ascensional column, while in a return from the rise, its tendency is the contrary way; the principle is the same as that well-known one of the difficulty of forcing light inflammable gas downhill, contrasted with the facility of its ascent, circumstances well-known, not only in dealing with the mixed atmosphere of mines, but also by gas-makers, who are in the habit of allowing 1-10th of an inch, more or less, water pressure, for every ten feet of rise or fall in the level of the pipes."

But the solution thus suggested does not go far enough.

1—We are to ask, whether all returns are warmer than their ingates? In the case of dip workings it is easily understood that they are so, but why in rise workings? There are, in fact, cases where the temperature of the return from rise workings does not exceed by a degree that of the ingate. Yet such cases are not excluded from the category under consideration.

2.—We must also consider the effect of the variable amount of hygrometric moisture in the air. Experiments for determining the moisture contained in the air at various points did not accompany the foregoing

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observations on temperature, but those made since have shown that in this mine the dew point follows up within about 2½ ° of the dry bulb temperature, and as the first observation, made near the bottom of the downcast pit, gave a temperature of 47¾ °, the dew point may be put here at 45°, and at the upper station (58¾ °) at 56°.

Then 30 + .458 tension of vapour at 56° = 30.458

And 30 + .316 tension of vapour at 45° = 30.316

being as 1000 to 1005 nearly for the ratios of density due to this cause.

Variations in the volume and density of air from temperature, pressure, or vapour, are all to be brought in as elements for explaining the real point at issue, which is not, it is conceived, one of temperature versus motive column, but how to give a right explanation of all the circumstances. *Temperature obviously does not account for every case, it will not carry us through, either as regards dip or rise workings*; nor, so far as my experiments go, will hygrometric difference do so, but the notable feature of the increase and diminution of ventilating column must necessarily find admission as one assignable cause at least into every example.

Experiments require so much care and time in making and collecting, that I have not yet been able to bring a sufficient number to bear on the question, and would be sorry, nor would it indeed be right, to lay any crude conclusions before the Institute respecting it. At the next meeting I shall read to the Institute a complete paper on the subject, as well as on that of regulating stoppings, and other topics connected with ventilation which have been recently brought forward.

After the reading of Mr. Taylor's paper had been concluded, a discussion arose, as to the best course of proceeding. It was then suggested that the President should write Mr. Taylor, urging upon him to have the paper containing his additional observations ready for a meeting in July; that such observations should then be printed and circulated amongst the members; and, that the general discussion should be taken at the meeting in August.

Mr. Atkinson then laid the following account of an experiment at Haswell Colliery before the meeting.

On the 7th of the present month an experiment was made at Haswell Colliery, to ascertain whether a long dip split, or a short level one, would obtain an increased proportion of air, as the gross quantity became less and less in amount.

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The accompanying sketch shews the air-ways; the extreme length of the long split, from the splitting point at *A* to the point of reunion, was about 7,000 yards; while the length of the short split to the first place, *B*, where it met a part of the air returning from the long split, was only about 16 yards, being the thickness of a stenting wall; and to where it joined a second portion of the long split return, at *C*, the distance was 430 yards; the remainder being met at a little greater distance, at the point *E*, as shewn by the sketch.

The experiments were tried in a manner exactly similar to those reported to the Institute, and described by the President, at page 63 of the present volume of "Transactions."

The results obtained were as follows:—

[Table of Results]

These results are in perfect accordance with the conclusions come to in the paper, and opposed to those come to on the common opinion or view of this part of ventilation.

The President then said, that it would be in the recollection of the members, that at the meeting in May last, Mr. Berkley read some observations, communicated by Mr. Greenwell, (who was not able to be present at the meeting) on the subject of Mr. Atkinson's conclusions, on the theoretical consideration of the enquiry. Mr. Atkinson informed him, that he had made some remarks on the observations which Mr. Greenwell had communicated to the Institute; with the permission of the meeting, he would now propose that the remarks of Mr. Atkinson, on Mr. Taylor’s objection, as well as on those raised by Mr. Greenwell, should be read. It was then agreed, that the remarks should be read and printed with the proceedings, after being put into the form of an ordinary paper.

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Plan shewing part of the ventilation at Haswell Colliery - May, 1859.]

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REVIEW OF THE RESULTS

OF THE

EXPERIMENTS THAT HAVE BEEN MADE TO TEST

AND OF THE objections that have been advanced against certain arguments employed, and conclusions arrived at, in two papers, (by the writer) recently read to and now under discussion by the Northern Institute of Mining Engineers, on certain matters relative to the

VENTILATION OF MINES.

By JOHN J. ATKINSON.

The substance of this paper was jotted down from time to time at leisure intervals in the form of notes intended to be employed in the discussion of the papers alluded to in the above title, which was fixed to take place at the general meeting of the 2nd of June, 1859; but in consequence of the absence of a member - who, while agreeing generally with the conclusions arrived at in the first of these papers, had, in a previous part of the discussion, stated that he anticipated their arrival, chiefly, at any rate, from an alleged cause, not specially noticed in the paper itself, and in support of which view he was expected to have attended the meeting— they were not so used.

The second of the two papers alluded to in the title, " On the Relative Importance of certain Causes in producing Changes of Density in the Air of Mines as it progresses in Circulating," was written entirely with the view of meeting the particular objection to which allusion has been made.

After certain preliminary observations in reference to this particular objection (as well as to others) had been read to the meeting, for which

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purpose they had been forwarded by the member who advanced it, it was decided that the notes which I had prepared for introduction into the discussion, should also be read, and that I should, afterwards, put them into a form suited for their appearing in the "Transactions," as an ordinary paper, if possible, in time for being printed, and in the hands of the members, prior to the adjourned meeting, appointed for the general discussion of the whole subject.

I proceed to do this:—

I will first refer to the objection urged by Mr. T. J. Taylor, at pp. 74, 75, and 76, of the present volume of "Transactions," being the subject of the second of the two papers named in the title of this article.

Although I am of opinion that the *absolute quantities* of air circulating in the rise splits are less in amount, and that those in the dip splits are greater in amount in consequence of the pressures arising from the changes of density (to the extent indicated in the paper on the relative importance of certain causes in producing them) in the circulating air, arising from the gradual expenditure of the ventilating pressure upon the resistances successively encountered by the air, as it circulates through the air-ways of mines; because these pressures always operate against the general ventilating pressure in rise splits, and in aid of it in dip splits, and therefore require to be deducted from it in the former, and added to it in the latter case, in order to obtain the effectual pressure operating upon and overcoming the frictional resistances of the respective air-ways; nevertheless, I am also of opinion that the pressures or forces arising from this source, do not alter or affect the *proportions* in which any *increased* or *reduced* gross quantity of air that may be allowed to divide itself over any series of rise, dip, or level splits in a mine by altering the general ventilating pressure, while the air-ways forming the splits remain unaltered; whether such alteration of the pressure applicable to the splits be made by increasing or diminishing the general ventilating pressure itself; as by driving a furnace to different extents, or allowing it to go out; or whether it be altered by opening or closing a main regulator in either the intake or return air-ways, traversed by the *whole of the air applicable to the splits*, either before reaching the splitting point, or after passing the point of reunion; for the purpose of causing it to absorb more or less of any uniform ventilating pressure that may be employed, by thus increasing or diminishing the resistances presented by such regulator, and so applying a greater or smaller portion

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of such constant general ventilating pressure to overcome the resistances met with by the air in the splits themselves, and thereby increasing or diminishing it in quantity.

The following are my reasons for coming to the conclusions just mentioned:—

The density of air (temperature and other conditions being constant) is directly proportional to the pressure under which it exists, and therefore the difference between the density of the air of a split, in any one part of its route, and the density of that in any other part of its route, is directly proportional to the expenditure of ventilating pressure in the intervening part of the route; at least in so far as such difference of density arises from the expenditure of this pressure; and we are not here considering other causes of change of density. Since this is true of the air in all the parts of its route, it is also true of the average densities in the whole of the route, or any portions of it, considered separately; and, therefore, the *difference* between the *average density* of the air of the intakes, and *that* of *the returns*, of any split, is directly proportional to the expenditure of ventilating pressure, or the resistances of the splits, in so far as such difference arises from such expenditure of ventilating pressure, as a cause, however such ventilating pressure may be made to vary in amount, other things being the same.

But since the gross pressures *on all the sections of columns of air*, whether such columns are upright or inclined at any angle, are directly proportional to the vertical heights of the columns, to the areas of the sections, and to the average densities of the air over equal vertical heights in such columns, it follows that the differences between the pressures produced by columns of constant heights, with proportional average densities over equal vertical heights, as in the air in the air-ways of mines, where the sectional areas are sensibly constant, must (so far as they are affected by the cause under notice) be always proportional to the densities themselves. And these are the pressures which, from this cause, require to be added to the general ventilating pressure in dip, and to be deducted from it in rise splits, in order to obtain the effectual pressures.

Seeing, therefore, that the pressures arising from this cause, are proportional to the ventilating pressure that gives rise to them, it follows that, *in a dip split*, we (so far as this cause is concerned) have *to add* to the ventilating pressure another pressure, arising from its expen-

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diture, which always varies in exactly the same proportion as itself, in order to obtain the effectual pressure applicable to the resistances ; and have to deduct a quantity or pressure that, in the case of a rise split, is also constantly proportional to the pressure from which it is thus to be taken ; and hence, in both cases, the resulting effectual pressures, operating on the resistances, are constantly proportional, in each of the separate splits of a mine, to the amount of the ventilating pressure employed; and will not, therefore, alter the proportions in which the air divides itself amongst them, however much it may affect their real amounts; and this is so, whether the quantities happen to vary as the *square roots*, or as any other roots or powers of the pressures, while the air-ways remain in the same state; contrary to the opinion which they were adduced to support.

Having noticed the only point of difference in opinion, between Mr. Taylor and myself, mentioned by that gentleman previous to the reading of the remarks he has sent to the present meeting, I now turn to notice other causes, operating in producing changes of density in the air in mines ; avoiding, however, the effects of changes of temperature in producing changes of density, as being those chiefly insisted upon in the paper itself; and, also, that which I have just discussed, as being of no effect on the question as to the proportions in which air divides itself over different splits in a mine under different ventilating pressures. In doing this, I will merely take the conditions assumed by Mr. Taylor himself, at pages 352, &c, of vol. III., in criticising another paper, which I read to the Institute, on the Theory of Ventilation; but, before applying it to the matter in hand, I would here observe (owing to my not having had a suitable chance of doing so before) that the principal item which is there introduced with the view of shewing that the density of the air in an upcast shaft, is somewhat different to what I had, in the paper on the theory, treated it as being, is one that really was inapplicable to the case; inasmuch as I had, in the paper itself, embraced its effects. I here allude to the temperature imparted to the circulating air by the mine; the effects of which, on the density of the upcast column, are, of course, embraced in taking the average temperature of the upcast shaft, a fact which was perhaps overlooked by Mr. Taylor, in writing the remarks upon the paper alluded to.

The statement given at page 355, volume III. of " Transactions" on the data there given, ought to have been as follows, omitting the matter I have named as being included by the paper itself: -

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[Various calculations, including asterisked note]

Now, since 0.989 is the specific gravity of the resulting mixture in the upcast column, even on the grounds assumed, as in some respects an extremely unfavourable case for the paper on the theory, (taking the density of the air in the mine as the unit for comparison) we find that the difference 1 - 0.989 = 0.011, or, 1 1/10 per cent, is the entire difference of density between the upcast column, as assumed by myself in the theory, (for the sake of avoiding intricacy) and the column, as it is assumed to be, in the extreme conditions taken by Mr. Taylor in his

* This is on the supposition that the prevailing barometrical pressure is 30.615 inches of mercury; and not 30 inches, as assumed by Mr. Taylor. The former number, 30.615 inches, is probably nearer the average pressure in mines, than the latter.

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critique, in order to render palpable the acknowledged (see pages 87 and 88, vol. III. of "Transactions"' and other parts of the same paper) absence of *rigid accuracy* in the paper he is there reviewing.

His mistake of including the expansion arising from the heat imparted to the air, in its passage through the mine (really allowed for in the paper under review), *appears*, in the *critique*, to increase this discrepancy to 1 — 0.963 = 0.037, or 3 7/10 per cent., in lieu of 1 1/10 per cent.; the ultimate mean density given in the remarks of Mr. Taylor comes out as 0.8724, to that of the air in the downcast shaft, taken as unity, by embracing the effects of expansion due to the mean temperature of the upcast shaft, making a difference between the upcast and downcast densities, of 1 - 0.8724 = 0.1276, or upwards of 12¾ per cent; but the average temperature of the air in each of the shafts being, itself, taken as a datum in my paper on the theory, the only difference that is here shewn, as a want of accuracy in it, amounts to no more than 1 1/10 per cent, in the extreme case assumed; and, since the quantities of air circulating, are, in the paper on "The Theory," taken as being proportional to the square roots of the pressures, such a difference as that mentioned, would only appear to give rise to one of about $\sqrt{1} - \sqrt{.989} = 0.0055$ to unity, being a shade more than a-half per cent, in the absolute quantity of air circulating in the unit of time; even under the extreme conditions assumed by Mr. Taylor, as stated by that gentleman, at page 354 of Vol. III. of "Transactions."

And it should be borne in mind that, in the third chapter of the paper on the theory, commencing at page 86 of the same volume, it was stated that the conclusions come to in such paper, were not given as being rigidly accurate, although they might, perhaps, be regarded as possessing all the accuracy which could be considered desirable, under the usual conditions prevailing in mines ; and surely the discrepancy just alluded to, cannot reasonably be regarded as being of much importance in such a matter.

Leaving this digression, I return to consider how far the causes alluded to, happen to give rise to differences between the densities of the air of intakes and that of returns, considered apart from those causes that are recognized and discussed in the two papers mentioned in the title of this article; taking, as a general guide, in doing so, the case cited by Mr. Taylor, as just mentioned.

We thus obtain the following results:—

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[Table of results]

Now, since 0.9875 is the specific gravity of the resulting mixture, even in the extreme condition of the gas given off being as much as 2½ per cent., or 1-40th part of the air circulating (a mixture of light carburetted hydrogen gas and atmospheric air, in the proportion of one volume of gas to 14 volumes of air, is explosive, so that if the air were reduced to one-third of its amount in such a mine, the whole of the return air would become explosive), it follows that the difference 1 - 0.9875= 0.0125, or 1¼ per cent., is the difference between the density of the air in the intakes and that in the returns of a mine under these conditions, if we even admit that no gas is given off in the intakes or returns, but that the entire quantity is assumed to be given off at the face.

The difference of density just mentioned is such as to cause the air of the returns to be lighter than that of the intakes, to the extent of 0.0125/1 = 1-80th part of its own density.

This difference is equal to one that would arise from a difference of an average temperature in the intakes and returns, to be determined by the equation

[Equation 1]

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Where t denotes the average temperature of the intake air, and t^1 that of the air in the return; from which equation we find

[Equation 2]

And if we assume the average intake temperature to be 40°, we find by the formula [2] that the average return temperature t^1, must be equal to 46°.32, in order to give rise to the same amount of difference of density; so that the difference of density arising from this cause would, under these conditions, be equivalent to a difference of 46.32 - 40 = 6.32 degrees of average temperature between the air of the intakes and that of the returns.

If we assume the average intake temperature to have been 50°, we find, by a similar application of the formula, that it would require the average return temperature to be 56°.44, being an excess of 56.44 — 50 = 6°.44 above the average temperature of the intake, in order to give rise to the same differences of density that are due to the mixture of gas with the return air, to the extent that has been assumed.

And if we presume the average temperature of the intake air to have been 60°, we similarly find that it would require the return air to have had an average temperature of 66°.57 or an excess over that of the return of 66°.57 — 60 = 6°.57, in order to give rise to the same difference of density between the air of the intakes and that of the returns, that would arise from the mixture of gas to the extent, and under the hygrometrical conditions that have been assumed.

Now, while it is freely admitted that the differences of temperature just stated, (as being necessary to give rise to changes of density equal to those that would arise from a mixture of fire-damp with the return air, to the extent of one-third of the amount required to render the whole of a pit's air explosive, and on the further supposition that no part of such gas was given off in either the intakes or the returns, but entirely in the extreme workings), are considerable in amount, it is submitted that the quantity of gas is never, under what is taken in the paper on splitting' air, to be the " ordinary conditions of mines," greater than, say, one-half of the amount that has been assumed, inasmuch as I do not believe that there is a colliery deserving the name of being moderately well ventilated, which would give off sufficient gas to render the whole of the return air explosive on the quantity of air being reduced to one-third of its ordinary amount. Indeed, I am acquainted with collieries that generate large quantities of fire-damp, where the returns remain at least

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this far removed from the firing point, when the air circulating is reduced to one-half of its ordinary amount; and it must be borne in mind that the assumed conditions on which the calculations have been made, involve the necessity of the *entire return* being rendered explosive, in the event of the air circulating being reduced to one-third of its ordinary amount, as has been stated.

Under what may fairly be termed the ordinary conditions of mines, (as stated in the paper I am here defending), I do not, therefore, think that more than one-half of the gas assumed in these calculations can be presumed to be given off; " under the ordinary conditions of mines," and, in addition to this, if we consider that at least a part of the generated gas will be given off in the intake and return air-courses, and, therefore, I have a less effect in creating differences between the average densities of the intakes and those of the returns, than under the assumption that has been adopted, of the entire quantity being given off at the extreme face of the workings, we may, I think, reasonably reduce the assumed amount of difference between these average densities, so far as it arises from gases, to one-third of the amount at which it has been assumed, in order to bring it within what may be called the ordinary conditions of mines, so often stated in the paper, to be the conditions to which its general conclusions were confined.

If we therefore presume the quantity of gas given off to have been only one-third of that previously taken, it would be reduced to 1-120th part of the circulating air, and the quantity of mixed air and gas escaping from the mine would become 32,124⅓ cubic feet per minute, and the density of the mixture would be 0.9938, taking that of the air at the temperature of the mine as unity, and presuming the whole of the gas to traverse the whole of the return, and no part of it to be given off in the intake, we should, by a series of calculations, similar to those already made, find that the return air would be lighter than that of the intakes, to the extent of [calculation] part of its own density, and the differences between the average densities of the air of the intakes and those of the returns, would be found to be such as to require the temperature of the return to exceed that of the intake by 3°.11, when the intake temperature is taken at 40° ; by 3°.17 when it is taken at 50°; and by 3°.24 when it is assumed at 60°; in order to give rise to the same amounts of differences between the average densities of the air of the intakes, and that of the returns, as would result from the hygrometrical state of the air, and from the mixture of gases to the extent and in the manner we have presumed.

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The temperature just stated, as being equivalent to the combined effects of the causes we are considering, are, I acknowledge, somewhat greater than I anticipated before I made the calculations. The differences of temperature represent the number of degrees of heat that, under the conditions we have assumed, would require to be added to the observed temperatures of the returns, in reasoning upon the effects that would be produced on the proportions in which air would distribute itself, over the splits of a mine, on altering the gross quantity circulating in the unit of time - supposing, of course, that the assumed conditions, as to the hygrometrical state of the air, and as to the proportion of gases given off by the mine really prevailed, and were not otherwise allowed for.

It would take a larger proportion of carbonic acid gas to be given off, by such a mine, in order to produce effects equivalent in amount (but in the opposite direction) to those that we have calculated to be due to firedamp; indeed, a small proportion of carbonic acid gas would be required to be given off by a mine, in order to counteract the effects on the density of the air in such a case, produced by its hygrometrical condition in different parts of its route.

If such a mine, instead of giving off fire-damp, were supposed to give off carbonic acid gas to the extent of 2½ per cent, of the volume of air circulated through the mine during the time of its being generated, then, still adhering to the specific gravity of 1.548, as adopted by Mr. Taylor (but which is doubtless too high), we find, by a series of calculations similar to those already given, that this quantity of gas would cause the return, on reaching the furnace, to have a density about 1-97th part of the density of the fresh air, greater than would be due to it in the absence of gases ; a departure of less amount, from the standard density of fresh air, than we have already seen to be due to the presence of an equal proportion of fire-damp ; the difference being now in the contrary direction.

If there was a mixture of these two gases, they would tend to annihilate each others effects on the density of the return.

We must, however, bear in mind, that the gases might be given off partly in the intakes, partly in the faces of the splits, and partly in the returns; and, hence, it might be improper to consider the full amount of these calculated differences of density, as prevailing on the average of the entire lengths of the intakes, and of the returns.

In addition to this, if we consider that lights burn badly where there is only twice the assumed proportion of carbonic acid gas mixed with the air, or 5 per cent, by volume; and that the mixture becomes dangerous to human life, if breathed, when it contains about thrice the assumed

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proportion, or 7½ to 8 per cent.; and that, therefore, lights would burn badly in the return if the air of a mine, having no better ventilation than we have assumed, if it were reduced to one-half of its ordinary amount; and that it would become dangerous to human life on being reduced to only one-third of its original amount; I think we may, as in the former case, conclude that an excess of only about 3° of temperature being required in the air of the intake over that of the air of the return, to produce an equal difference in their densities, is rather an outside quantity, than one arising under the ordinary conditions of moderately well ventilated mines.

I would here add, that allusion is made at the commencement of the paper on splitting (page 16, vol. VII. of " Transactions") to the effects of gases, as well as of heat, on changing the density of the air in mines, and the paper comes to its conclusions quite independent of the particular causes which produced such a change of density in the circulating air.

I now turn to the remarks of Mr. Greenwell, read at the last meeting, in reference to the paper then under discussion, which appear to be founded upon some misconception on his part, as to the nature of the premises on which the paper is based. See page 105, vol. VII. of Trans. for these remarks.

Before, however, that gentleman enters upon what he terms the more particular object of his remarks, he makes a quotation from the paper in question, and calls attention to the fact of his having previously expressed an opinion at variance with that held by myself, as set forth in such quotation ; and afterwards alludes to the results of the four sets of experiments given at pages 66 and 67 of the present volume of " Transactions," as corroborating the particular opinion held by himself; and therefore, as might be inferred, disproving the opinion to the contrary, set forth in the paper to which his remarks bear reference, to the extent, he states, of demanding " the careful examination of each split of air, in the event of any sudden depression in the ventilating power."

As, however, it formed no part of the opinion expressed by Mr. Greenwell that a careful examination of each split of air ought to be made in the event of a sudden depression of the ventilating power, any more than of that expressed by myself in the paper he is objecting to, it follows that if the experiments do prove the necessity of making such examination, in doing so they no more corroborate that gentleman's opinion than my own. The general opinion held, before the paper was written, by Mr. Greenwell and others, was to the effect, that, in the event of a sud-

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den depression of the ventilating power, the long splits would receive a reduced proportion of the lessened quantity of air; and the short splits an increased one; thus making the proportions depend upon the relative lengths of the splits, and taking no notice of whether they happened to be rise, dip, or level ones; while the opinion maintained by myself, in the paper alluded to, on the contrary, was to the effect that the mere lengths of the splits had nothing whatever to do with the proportions of any altered quantity of air they would receive ;and that it depended chiefly upon the amount of rise or dip, together with difference between the temperatures and densities of the air in the intakes and that of the returns of the several splits; and these are the contrary opinions that the experiments were made to test ; and they were not made with any special view of ascertaining whether it was necessary to make a careful examination of each of the splits, in the event of a sudden depression of the ventilating power, as this remark appears to imply ; the necessity of which I have never doubted any more than Mr. Greenwell himself.

Let us glance at the whole of the experiments at present before the Institute, in order to see which of these opinions are practically corroborated by them; and, on the other hand, which of the opinions are disproved by their results.

The following is a summary of the results of the experiments at present before the members:—

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[Table of experimental results]

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On looking over the results of the twelve experiments, as just stated, we perceive that the first nine, and also the last two (comprising four sets of experiments where all the workings were nearly level, two sets where the long splits ventilated rise workings, and five sets where the long splits ventilated dip workings), being eleven out of the twelve, all agree with the conclusions come to in the paper under notice; while the first four sets, made where the workings were nearly level, and also five out of the six experiments made where the long splits ventilated dip workings, being no less than nine out of ten as to the expected results of which there was any difference of opinion, directly contradict the common opinion which Mr. Greenwell is defending. (See page 160, vol. VI.) The only experiment (in which, however, the long split had only about eight fathoms of dip) having even the appearance of giving results opposed to the conclusions arrived at in the paper under discussion, being the Minor pit experiment, No. 10 in the foregoing list; and the result of even this solitary exception is at variance with those of the remaining five of the sets that were made under similar conditions, so far as conditions were specified in the paper I am defending. I allude to those where the long splits ventilated workings lying more to the dip than those ventilated by the short ones. Now, as my conclusion in the paper was, that the common opinion Mr. Greenwell is defending in his remarks, would only hold true in the cases where the longer splits happened to ventilate rise workings (I mean rise, when compared with the position of those ventilated by the shorter splits), and on the supposition of the air of the returns being less dense than that of the intakes; and as I can prove that the experiment No. 10 was, by the conditions stated in my paper, excluded from being classed as one embraced in its general conclusions, while the whole of the results of the others are in accordance with them, I submit that these experiments *altogether* agree with and confirm the opinion set forth in the paper, and equally disprove and refute the common opinion to the contrary; a conclusion directly opposed to that set forth in the remarks I am now reviewing, as given at page 160, vol. VI. "Transactions."

If leakage existed to such an extent as to bring about results bearing the outward appearance of contradicting those anticipated in my paper (and I have stated this to have been my opinion in a previous part of the discussion, in reference to experiment No. 10 in the list), then the conditions required by the paper, to form a part of what was necessary its conclusions, have not prevailed, and, therefore, the truth of the conclusions remain unaffected by it.

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That any such amount of leakage as I have mentioned is excluded from the conditions, required by the paper, in order to justify classing the case under its general conclusions, appears from the following ext tracts from the paper itself:—

"Owing to any contraction of the shafts or general air-ways where all the air circulates in one mass or body." - See page 164, vol. VI. "Trans."

“Or from a contraction introduced into the shafts or into any part of the air ways where the gross quantity of air circulates."   See page 165, vol. VI.

This, taken in connection with the fact that any leakages, or even open air-ways themselves, occurring between any two parts of the particular splits that are *completely* operated upon by the contraction, or by the reduced ventilating power, do not affect the proportions (except as stated in the paper) in which the air will divide itself over them, when it is thus altered in amount; as such leakages or the currents in such connecting and open air-ways, will, themselves, be affected by the contraction or reduced ventilating pressure, in a manner similar to the main air currents - these considerations, I say, plainly demonstrate that such cases as do not agree with the general conclusions of the paper, owing to leakage, are cases that the paper itself does not class under such conclusions; and, therefore, even No. 10 experiment itself, is not really at variance with the paper, if the results were indeed rendered by leakage the reverse of what they would have been, in its absence.

I will now prove this to have been the case.

The result of experiments made on the 21st of May, by Mr. Daglish, Mr. Lindsay Wood, and myself, in the Minor pit at Hetton Colliery, under the same arrangements as to splitting the air, and in the same place that No. IV. set of experiments were made (the results of which are given by Mr. Wales at page 67 of the present volume), clearly prove that the apparent disagreement between that set of experiments and the principles of the paper under discussion is more in appearance than in reality; inasmuch as they prove that the *workings* ventilated by the long split did actually obtain an *increasing*, and not a *decreasing*, proportion of the air, as it became reduced in amount; and that it was merely owing to that split obtaining (as the general quantity reaching the splitting point became reduced,) a gradually *increasing* supply of air through another channel, that it happened to get a reduced proportion through

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the particular part of the channel where the measurements were taken, when Mr. Wales made his experiments.

In the first place, in preparing to make these experiments, as a check to the former ones, the gross quantity of air passing in by the ordinary intake, after coming through the main regulator at *R*, on the accompanying plan, was divided, as nearly as possible, equally between the long and the short splits, by means of the regulator at *C*, in the latter; and this regulator was then allowed to remain in the same state during the whole of the time of making the experiments : the main regulator at *R*, from being wide open at the time of equalizing the splits of air, was successively more and more reduced in area, in order to lessen the quantity of air passing through it, in exactly the same manner as was done in the experiment reported by Mr. Wales; and in the meantime the air was simultaneously measured at the entrance to the long split intake, and also in the workings ventilated by that split, at a point marked *E* on the plan, where the air-way was single; and likewise in the short split at *H*. The following results were obtained, satisfactorily confirming the opinion expressed by me at page 74 of the present volume of "Transactions," and again named at page 77, as mentioned in Mr. Greenwell's remarks.

Experiments made in the Minor Pit, at Hetton Colliery, in the Hutton Coal Seam, 21*st May*, 1859.

[Table of results]

*** The results of these experiments can be better seen by reducing the number of revolutions at the head of each column to 100, and also reducing each of the numbers standing beneath it, in the same column, in the same ratio; so that they will represent the per centages, of the original quantity in each measuring place, that continues to pass it, after reducing the area of the opening in the main regulator.

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Plan of East Minor pit, Hetton Colliery.

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From this statement we perceive that the per centage of the original quantity of air passing through the measuring place at *E*, in the workings of the long split, is, in every instance, greater than that passing through the short split, as we successively reduce the gross quantity.

An examination of these results plainly shows, that although the proportion of air passing the measuring place at the outer end of the long run became reduced, in comparison with that passing through the short split, as the gross quantity reaching the splitting point, itself became less and less in amount; in the same manner as in the experiment reported by Mr. Wales, at page 67 of the present volume ; yet it proves that at the same time, an *increased* and not a *decreased* proportion of air traversed the workings ventilated by the long split current, in agreement with the result anticipated in the paper under discussion; the falling off at the *outer end* of the intake, merely arising from the fact of an increased quantity of air, passing into these workings through other channels, than the one in question, as the main regulator at *R* became more and more reduced in area.

If, in these experiments, we presume each revolution to represent a velocity in the air of one foot per minute, as they profess, and very nearly do, at high velocities, we shall find that only a portion of the air that passed the measuring place at *G*, at the outer end of the long split, really traversed the long split workings (a part of it escaping through some other channel) even before the experiments were commenced; for on multiplying, 415, the number of revolutions per minute at *G*, by 15 94/144, the area of the place in which the instrument revolved, we obtain 6,496 cubic feet of air per minute, as the quantity passing through this measuring place; and at the same time we have only 393x16 = 6288 cubic feet, traversing the *workings* of the long split, as indicated by the observations at the measuring place in these workings; whereas, on reducing more and more the outward pressure on the channels of *escape*, (by reducing the area of the main regulator at *R*, in the main intake,) we, on the other hand, obtain a continually increasing supply of air from

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these channels, *into* the workings of the long split; which, by helping to increase the resistance of the air in the long split, naturally prevents the otherwise due quantity from passing in by way of the measuring place. And it is evident that had the gross quantity been reduced by a regulator at *F*, in the main return, instead of by that at *R*, in the main intake, we should have had, on the contrary, a continually increasing proportion of air passing through the measuring place *G*, at the outer end of the long split; owing to an increasing proportion of it escaping by the same channels, arising from our thus necessarily increasing the outward pressure upon them, as we successively reduced the area of the main regulator in the return; the motive column necessary to overcome resistance at all regulators being conserved in the air in all the preceding portions of its route; but in that case we should have had a continually decreasing proportion of the gross quantity of air in the long split workings, as compared with that in the short split, as we reduced the opening in the main regulator, as the escaping air would, of course, increase the resistance encountered by the rest, as far as it accompanied it in the main intake, and leave a lessened pressure to overcome the resistances of the remainder of the route, and thereby lessen the quantity of air traversing it.

Anticipating the results just described, a main regulator was placed in the main return at *F*, and the following series of experiments were made, by gradually closing it, while the short split regulator remained in the self-same position as in the experiments just cited.

Experiments made in the Minor Pit, at Hetton Colliery, in the Hutton Coal Seam, 21*st May*, 1859.

[Table of results]

* These areas should be considered as being: much larger than those given, owing to leakage through the adjoining workings. *See Plan*.

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The main regulator at *R*, in the intake, was wide open during the time of making the above experiments. In this case, on comparing the quantities at *G*, in the long split intake, with those at *H*, in the short split, we perceive that, in every successive reduction of the main regulator, there is a greater proportion of the original quantity of air passing into the intake of the long run, (at the same point that we had the reduced proportion in the experiments 2, 3, and 4), in comparison with the proportion that goes into the short one, of *its* original quantity, as was anticipated.

On looking at the numbers of revolutions at *E*, in the workings ventilated by the long split, we perceive a falling off in the proportion of air compared with the quantities passing through the short split, in all the experiments, as compared with No. 1.

The results of these experiments may be stated in per centages of the original quantities at each of the measuring places as below:—

[Table of results]

These results are of a very similar description to those recently obtained by Mr. Berkley and myself at Crook Bank Colliery, under similar conditions and arrangements, there being a good deal of leakage in the long split in that case, as in this. We see, however, that in each of the other 11 experiments to which I have alluded, the results come out in accordance with the conclusions arrived at in the paper in spite of leakage, although at least in every experiment that has been given by Mr. Wales and myself, as made at the Hetton and Eppleton Collieries, I know that leakage existed between the long run and the workings of adjoining collieries; and as the short run has in no case been affected by such leakage, and in every instance, except that just named, the main regulator has been placed in the main intake, and not in the return, it follows that, to the extent of their operation, they have always been made to act against

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the long run maintaining its proportion at the measuring places, on reducing the gross quantity of air; and, consequently, in opposition to the opinion held by myself, and in favour of that held by my opponents; but would have had the opposite effect in each of the cases, had the obstruction, or main regulator, been placed in the main return. Leakage can only affect the question at issue, owing to each split influenced by it, obtaining an increased share or proportion of its otherwise due quantity of air, in one part of its route, and a consequently lessened share or proportion in the other part of its route; and it therefore depends entirely upon the situation of the place of measurement, in relation to the position of the leakages, as to whether it would, in experiments, appear in any case to operate in favour of my own opinion, or that of my opponents; besides, it is evident that leakage might occur in a short, as well as in a long run, if the splits were such as were contemplated in the paper, the ordinary splits in a working pit; and in addition to this, whichever split they occurred in, they could be made to *appear* to operate in favour of either the opinion of myself or that of my opponents, wherever the measuring places might be, depending upon whether an obstruction was placed in the main intake or in the main return, to reduce the quantity of air.

But in the event of the quantity of air being reduced by the slackening of the furnace, or by a depression in any other ventilating power that might be employed, then the leakage existing between the various splits of air in the same mine would not affect the proportions in which the reduced quantity of air would circulate *in each part of each split* into which it might be divided; as they would all be simultaneously and similarly affected by the resulting change of pressure ; and the effects of columns of air of different densities in the various ascending and descending parts of the air-ways, as set forth in the paper under discussion, would, under ordinary circumstances, in that case, be the only matter worthy of notice that would prevail in determining the new proportions they would assume, not only on the average, but also in *every portion of the route* of each split. If, however, there was a communication between some of the splits and the workings of a distinct pit, the conditions mentioned in the paper would be departed from, and consequently it would be unreasonable to anticipate the same results as if they had been complied with; and although the paper happens to be perfectly silent in reference to the effect of leaks in modifying the proportions in which air will divide itself over a series of splits in the same mine (although the extracts shew that their effects are not cases in the paper), when the

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quantity is reduced by means of obstructions in the general intakes or returns, and not by reducing the ventilating pressure, it seems probable that in the ordinary conditions of mines, where these effects are small, that they would rarely prevent the arrival of the results anticipated in the paper, on the assumption of their absence. And it should also be borne in mind, that, so far as their effects extend, they are equally liable to operate, and to the same extent, against the common opinion (to the effect that it is the relative lengths of the splits that determines the pro- portions in which air will divide itself amongst them, when it is reduced in amount), as it can possibly be to operate against the opinion set forth in the paper, to the effect that the mere lengths of the splits have nothing to do with the question, and that it depends upon whether they are rise, dip, or level ones; so that I am quite at a loss to know how their effects can be taken to corroborate the opinion of Mr. Greenwell, as that gentleman assumes.

Mr. Greenwell states that his more particular object in making the remarks, however, was to endeavour to show why the practical result is at variance with the theoretical opinion; but as the experiments show that there was a much wider field for explanation, on the contrary, as to why the *practical result*, in nine cases out of the ten that I have quoted in the outset, is at variance with his own, the popular opinion; as there is, at most, but one out of these twelve experiments that has even the semblance of being at variance with the opinion advocated in my paper; and that one, to say the very least, has been proved by the recent experiments I have named, to be just as much at variance with Mr. Greenwell's opinion as my own, if, indeed, it be so with either. And as to the two remaining cases, where the long split ventilated rise workings, there never was any difference of opinion respecting them; and, therefore, I might have left the matter here, but that I have often heard an opinion expressed to the effect, that the application of theory to considerations touching the ventilation of mines does no good, but rather harm; and, as I believe that this opinion is as pernicious and erroneous, as it is prevalent, inasmuch as it tends to retard the progress of our knowledge of the laws of ventilation; while, for my own part, notwithstanding that I have had a considerable amount and variety of practical experience in the matter of the ventilation of mines, yet, I frequently find myself greatly assisted in deciding otherwise doubtful points, by combining theory, *based upon experiments*, (as in these papers), along with my practical knowledge, and I, therefore, think it worth while

extending these remarks, in order to prove that those of Mr. Greenwell, in reference to the theory involved in these papers, are founded upon an entire misconception of its nature; and that the conclusions he arrives at, touching this part of the subject, are as erroneous as his ideas appear to have been respecting the general bearing of the results of the experiments, in reference to the two opinions they were made to test.

Following the course of the remarks, therefore, I next find it stated that the writer of them does not think that the observations on the experiments, (given at pages 60 and 67), contained in page 77 of the present volume of " Transactions," at all affect the value of the *lesson that they teach*, because, he states, that " routes not being level," " return air being at liberty to pass into other workings," " leakage of air, &c, &c. (?), are such occurrences as are constant in practice, and because fact, as we find it, and not fact as it theoretically ought to be, must be our guide." I am not aware of what particular lesson is here meant, but the most striking lesson that I gather from the experiments, is one that teaches me that the common opinion, advocated by Mr. Greenwell, has been erroneous, and that the opposite one, set forth in the first of these papers, (at a time when I had a perfect knowledge of the prevalence of the former), has been substantially corroborated by such experiments.

If, however, the lesson here alluded to, be the necessity of examining each split of air, in the event of a sudden depression of the ventilating power, I have satisfactorily shown that, as I have not held any opinion to the contrary, and as the experiments I have alluded to, with a single exception at most (if it be taken to be such, but I cannot admit it to be so), confirm the views set forth in the paper, such a lesson was, perhaps, more needed by those who maintained the opposite and erroneous opinion, that, in the event of a sudden depression in the ventilating power, the shorter splits would obtain an increased share of the lessened gross quantity of air (without reference to their being rise, dip, or level ones, or to the prevailing average densities), than by myself.

It is because *facts as they are* must be our guide, that I have attributed the results of the experiments almost solely to the amount of rise and dip in the various splits, considered in connection with the relative densities of the air in them; although these causes, *in opposition to fact as we find it*, are ignored by the contrary opinion, in reference to their effects on this question.

I think the results of the experiments made on this subject will be found to demonstrate that facts as they were hypothetically thought to

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be, and not facts as we find them, have been the guide, in this matter , of those who held the opinion that the mere relative lengths of the splits was all that had to do with the question.

I here pass over the succeeding remarks as to leakages, as I have already said enough on the subject, and their effects on the question here debated; and come at length to the leading subject of these remarks. I find it stated that "the present law treats of air as a fluid passing along a channel of uniform dimensions, similarly to water or air passing along a pipe;" and this position is attacked by the writer of the remarks.

The assumed position here assailed by Mr. Greenwell is most undoubtedly an untenable one, and must have been surrendered to him, had it been held or employed in my papers, which, however, it most surely is not. As (owing, I suppose, to some misapprehension) these remarks conclude by needlessly protesting against the accuracy of any conclusion arrived at by taking an *average* area of any air-course as a basis of calculation of the resistances met with by ventilating currents, it may be useful to show what is the actual basis of the conclusions arrived at in the papers, and, at the same time, to point out how satisfactorily both abstract ideas, and general, and common-place experience, in this instance, agree with and confirm *the theory obtained by classifying and generalizing the results of experiments.*

The chief and almost the only positions on which the conclusions contained in the paper are founded, are the following:—

1st.—That the pressures required to overcome the frictional resistances of currents of air, in the irregular air-ways of mines, are proportional to the squares of the quantities of air circulating through the same unaltered air-ways, in the unit of time.

Now, although this assumed law *happens* to be *identical* with that which Mr. Greenwell himself appears to think prevails in the case of pipes having an uniform area and form of section throughout their extent, it would be erroneous to suppose that it is merely adopted in these papers as being, *in consequence of this*, also applicable to the irregular air-ways of mines. On the contrary, no less than seven different experiments, made with no view of maintaining this law, and, therefore, all the more reliable in the support they give to it, are set forth in the first of these papers, as the reason of its being assumed to prevail in the air-ways of mines, having areas and forms of section varying more or less at every step; viz., one at Moorsley pit of North Hetton colliery, two at Belmont colliery, two at Castle Eden colliery, made with furnaces, engine fires,

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and steam jets; and two at Gelly Gare colliery, made by a ventilating machine; all of which are adduced in the paper itself, as indicating the prevalence of the law in question; and an 8th experiment is given at page 235, vol. VI. of the " Transactions" (made by means of a ventilating machine), the whole of which, with one exception, where round numbers only appear to be employed in stating the quantities of air (I mean the case of Moorsley), give results agreeing within 1 to 3½ per cent, of those that would have resulted from the prevalence of the law in question; and as even these small variations all occur in the particular direction that indicates them to have arisen from the peculiar increase of resistance, due, not to the mere resulting increased quantity of air circulating, but to the additional degree of expansion which any increased quantity of air, in the same air-ways, necessarily experiences, under the increased exhausting power required to put it into circulation; as is stated in the paper itself, and as both theory and these and other experiments alike indicate that they ought to do. I avoided encumbering the paper with the intricate formulae that would have been required in any attempt to demonstrate that the slight deviations of the results of these experiments, from the assumed law, were to the precise extent that theory indicated; as they were what I looked upon as small in amount, considering the difficulties that attend the determination of the exact quantities of air passing along a gallery in a mine in any given time. But really, after all, the truth of the conclusions arrived at in the paper would remain equally valid, if the resistances, instead of being exactly proportional to the *squares* of the quantities of air circulating in the unit of time, through the same unaltered air-ways, were proved to be, in fact, proportional to any other power or root, whatsoever, of the quantity; as will be found if the paper be read, accompanied by the substitution, in it, of any literal symbol (say n) indicating any quantity whatever, in lieu of the *square* (in that case, of course, also using the n th root instead of the square root) in the general reasoning contained in it. And hence it was the less necessary to establish, more nearly than was done in the paper, the prevalence of the law in question. Yet I am satisfied, both by the experiments I give, and by many others, that it is near enough the truth for all practical purposes.

And this flexible premise was, in truth, not stated in the paper to be an indisputably established principle; but was merely put forth as resting upon the experiments adduced in support of it, in the paper itself. - See pages 165, 166, &c, Vol. VI., "Transactions."

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As Mr. Greenwell states that he would be of my own opinion if the same law prevailed in reference to air passing along the air-ways in mines, as obtains in the case of a fluid passing along a channel of uniform dimensions (and the above law is the only one that I can imagine to have called forth this statement, and, so far as the arguments in the paper are concerned, I have shewn that its being the true law is not absolutely necessary to the validity of the arguments employed, and conclusions arrived at, in the paper) I will, in the sequel, endeavour to shew that the theory applicable to uniform pipes, itself indicates, in a very high degree, that at least this particular law would be equally applicable to channels having varying and irregular sections and, indeed, that uniform pipes and the irregular air-ways of mines, only form two particular cases, each embraced by the same general laws.

Having, as I think, shewn the futility of the whole of Mr. Greenwell's general objections to, and arguments against the opinion advocated in the paper, as well as the unsoundness of the common opinion that they are put forth in support of (both by shewing the fallaciousness of the reasoning employed in them, and by an appeal to the results of the experiments before the meeting); little more remains than for me to point out the mistaken conclusions arrived at in the example which is introduced to illustrate the arguments employed in the remarks.

Admitting, for the sake of argument, that the general principles adopted by Mr. Greenwell in treating the example, are correct, still we only have to follow them to their legitimate conclusion, to find results in accordance with the opinion advocated by myself and at variance with that in support of which they are adduced.

The supposed case is one where there are two connected splits of air in a mine; the short one having to traverse a channel only 100 yards in length, and 50 feet in area, but having in it a regulator occupying one inch (by its thickness) of the length of the channel; the opening in it, on being so adjusted as to render the currents in the longer split and the shorter, exactly equal to each other, being only 1 foot in area. The long split is assumed to be 10,000 yards in length, having the same general sectional area as the shorter one, of 50 feet, but, in some places, one of half that area, or 25 superficial feet. When the assumed adjustment of the short split regulator is made, 10,000 cubic feet of air per minute, are supposed to pass along each of the splits.

It is stated in the remarks, that, under these conditions, "if we double the exhausting power, we do not affect the two air-courses equally;

because in the one case we increase the ventilating pressure upon an area of one square foot, and in the other, upon twenty-five; checked, in the former case, by the friction along 100 yards of 50 feet area, and along one inch of 1 foot area; and in the other by 10,000 yards, chiefly of 50 feet area, but in places only 25." And the conclusion professedly drawn from this, is, that " We, in this case, by increasing the exhausting power, increase the quantity circulating through the long division, in a greater ratio than that passing through the shorter one ;" a conclusion which, I submit, is neither warranted nor supported by the premises : the legitimate conclusion being, I imagine, that since it has been assumed that a common exhausting power or ventilating pressure was required to operate upon an area in the long run, greater than that operated upon, by it, in the short run, in the proportion of 25 to 1, in order that equal quantities of air might pass through them, in a given time, in the first instance; it follows that, if we allow the air-ways to remain in the same state, equal quantities will continue to pass through the splits, however we may increase or decrease the always common ventilating pressure; because whatever may be its amount, it will always continue to operate upon an area in the long run, greater than that operated upon, by it, in the short run, in the very same proportion as at first, when we have presumed the quantities to have been equal.

Mr. Greenwell's own premises inevitably lead to, and necessitate this conclusion, if the quantity of air circulating in the unit of time, through any unaltered air-way, is proportional to any root or power of the pressure required to put it into circulation; whatever that root or power may happen to be (omitting, as Mr. Greenwell himself has done, to notice here, any effect due to changes of density in the air), and it is by no means necessary that this proportion should be the square root, in order to establish this conclusion; a conclusion directly at variance with that which Mr. Greenwell has, by some wrong inference, professed to draw from the same premises; the true conclusion being in accordance with the arguments employed in my paper; and directly opposed to the common and contrary opinion, the example was intended to have supported.

The fallacy, in this case, is similar to one that a person would be falling into, if, having an engine of one-horse power, drawing a load of one ton, at a given speed; and another engine of twenty-five-horse power, drawing a load of twenty-five tons, at the same speed; he should imagine that by exactly doubling the power of each of these engines, he would thereby increase the speed of the load of twenty-five tons, drawn by the larger

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one, in some higher ratio than he would increase the speed of the load of ; one ton, drawn by the smaller one, because he had added more power to the former than to the latter.

Without wishing to give any countenance to this somewhat imperfect mode of dealing with the general question, it will be found to give exactly similar results whether we employ it in considering the pressure as distributed over the largest, smallest, or any other intermediate sectional area, occurring in any part of either, or both of the splits. It is proper that I should here add, that the simultaneous velocities of the air passing along a gallery or split in a mine, must, sensibly, vary in the same proportion as the quantity of air passing through it, in the unit of time; inasmuch as the same quantity of air must pass over all the different sections (however great or small they may be) in the same time : every velocity, therefore, varies sensibly in the same ratio as every other velocity in the same air-way; and the first of the laws, as to pressure and resistances, being proportional to some root or power of the velocity, and, therefore, of the quantity of air passing in a given time, may be taken, thus far, as being intended of all or any of the velocities that happen to arise from the different areas of the sections of the air-ways of a mine.

I will next endeavour to show that the theory applicable to fluids flowing through pipes of uniform section, is of such a character as to render it, at least, highly probable that the velocities of air passing, in a given time, through any air-way, are, in each and every part of the same |air-way, sensibly proportional to the square roots of the *resistances encountered by it*; and, therefore, *also* to the square roots of what is but a measure of such resistances - *the pressures putting the air into circulation*; as they doubtless are, at the same time, sensibly proportional to the quantities of air passing in the unit of time. I have previously stated that the truth of the conclusions in the paper does not depend upon the establishment of this point.

And I merely do this, as an endeavour to remove an erroneous popular opinion, that attributes an evil influence to theory, which, in my humble opinion, it is free from.

The theory of the matter is to the effect that, in all cases, the gross *pressure required to overcome the resistance* of a fluid moving along a pipe of uniform dimensions is, *ceteris paribus*, directly proportional -

1st, *To the rubbing surface of the moving column of air*. In a channel of uniform section, this surface is found by multiplying its

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length by the perimeter of its section. And when we reflect that the pressure exerted by the moving air is generally about the same on every unit of surface exposed by it, we must conclude that this law is at least what might have been expected.

2nd, The gross pressure is similarly proportional to *the square of the velocity of the air in the pipe*. And when we consider that not only is the *number of particles* of air that pass through the pipe in each moment of time (and so encounter the resistances offered by it) directly proportional to the velocity; but also, in addition, that the particular velocity, and hence the momentum, with which each particle encounters these resistances, also varies in the same proportion; it seems to be but reasonable to conclude that the gross pressure required to overcome the resistance should, from these two circumstances, be proportional, not simply to the velocity itself, but to its square; as, on doubling the velocity, the particles of air fly with double their original momentum; and, in addition to this, twice the number of particles encounter obstacles each moment of time; making, together, a fourfold increase, for a double quantity, and so on for any other increase of quantity - as its square.

3rd, In uniform pipes the gross pressure required *is the SAME, whatever may be the area of the section, other things being constant*; and, as a consequence, the pressure per unit of surface, or the head of motive column, must be less in the same ratio that this area is greater; and must, on the other hand, be greater as this area is less : that is to say, it must vary inversely as the area of the section, in order to give rise to the same amount of gross pressure applicable to the resistances; just as we obtain the same moving force by doubling the pressure on the piston of an engine, if, at the same time, we reduce its area to one-half, and *vice versa*.

The above are all the matters that enter into the theory of air passing along pipes of uniform section, if we omit to notice the effects of changes of density, and the pressures required to generate velocity, which are not matters that require our notice at this place.

Now, if we admit these laws as prevailing to the extent necessary to satisfy the ordinary requirements of practice, and I believe Mr. Greenwell himself does so; then, on considering the case of a number of pipes of different lengths, each having a uniform but different area of section, joined together, end to end, we must of necessity allow that, since the pressures required to overcome the resistances of each, are proportional, *ceteris paribus*, to the squares of the quantities of air passing through them in the unit of time, so also must be the pressure required to overcome the sum of their resistances, as presented by them, when united together in the manner just described; unless, indeed, their union gives rise to new resistances (or does away with some of the sources of resistance presented by them in their separate state), varying in their amount after a different law from that embracing their resistances when separate.

And, if we admit, as has been assumed, that the rubbing surfaces, multiplied by the squares of the velocities of the air are the true elements of the theory (as numerous and varied experiments appear to indicate), then it is natural to suppose, that however different, and multiplied in number, may be the forms, sizes, and areas of the sections presented by the different parts of an air-way in a mine, still (so long as it remains in the same state), whatever may be the altered quantity of air forced through it in a given time, the form and dimensions of the section of the air in motion, in each particular part of such air-way, *and therefore the rubbing surf ace presented by it*, will, on the whole, remain sensibly the same, notwithstanding any alteration in velocities. The velocity over each section increasing or decreasing in direct proportion to the quantity of air passing through it in the unit of time. And, if this be the case, we are led by these theoretical considerations to the same conclusions as those indicated by the experiments quoted in the paper under discussion; to the effect that the pressure required to overcome the resistances, of even the irregular air-ways of mines, are very nearly, if not accurately, proportional to the square of the quantity of air passing through them in the unit of time. In dismissing these remarks I would just state, in addition, that both theory and experiment indicate that the pressures due to the generation of velocities (considered apart from frictional resistances), are, *ceteris paribus*, proportional to the squares of the velocities themselves, and therefore vary in the *same proportion* as the other resistances already mentioned.

I would next proceed, in a more explicit manner, to correct an error, fallen into by the late Mr. Wales, at page 71 of the present volume of " Transactions," and corrected by myself at page 77, in the same day's discussion; my reason for wishing to make this matter more plain is, that I have good reason to think that a few persons fail to understand and appreciate the explanation already given, at the place mentioned above.

Notwithstanding that so long as the same causes continue to operate, in other respects, it follows, as *a matter of course*, that the very contrary results must ensue from increasing the gross quantity of air to those

that result from reducing it; I took the pains to state, in the paper, in so many words, that *of course* the opposite effects would, in each of the conditions stated, take place, on *increasing* instead of *diminishing* the gross quantity of air circulating, while the air-ways forming the splits, and the regulators in them remained unaltered; yet, after all, since these results have been found to come out in the experiments, they have been repeatedly urged to myself, as arguments against the conclusions arrived at in the paper; and hence my wish to make the matter clear and intelligible, by once more explaining it.

For this purpose let us just take the results of what are termed No. I. set of experiments at Elemore colliery, an account of which are given at page 66 of the present volume, but which series, nevertheless, consists of two distinct sets, for all the purposes for which they were instituted; inasmuch as the short split regulator was altered between No. 5 and 6 of the series.

In this instance, the quantities of air in the long and short splits were equalized by means of the short run regulator in No. 1, and the gross quantity was gradually more and more reduced in Nos. 2, 3, 4, and 5 experiments.

Again, the quantities of air in the two splits were equalized in No. 6 experiment by altering the short split regulator, and the gross quantities were successively increased by means of the main regulator.

In order to see that the changes in each case were *in the same direction*, as to proportions, we have only to place the two sets in juxtaposition, giving precedence, in each instance, to the largest quantities, and to the next least in succession.

[Table of results]

But in order to render the matter more clear still, let us reduce the

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first number of revolutions, in each of these two sets of experiments, and in each of the respective splits, to 100, and see the per centage of such revolutions performed in the same splits, in each case, as the gross quantity of air becomes reduced in amount; in this way we obtain -

[Tables of results]

From which we clearly perceive that, in each of these sets of experiments, in every instance the short split obtains a larger share or proportion of the gross quantity circulating, as such gross quantity itself becomes less and less in amount.

This result is of the precise nature that was anticipated in the first of the papers named in the title of this article, inasmuch as in this case the long split had a considerable rise, the short one being sensibly level, while the ordinary conditions of the averaged temperature of the air being higher than that of the intakes also prevailed.

What has been stated may serve to shew how the results of experiments that have, by some, been supposed to exhibit a mysterious discordancy, even amongst each other, are, after all, perfectly congruous; and, at the same time, in harmony with the results anticipated in the papers.

A few persons who are really interested in the general question, discussed in the first of the two papers alluded to in the title of the present article, from a lack of mathematical knowledge, have found a difficulty of following the reasoning contained in the paper itself; and I therefore think it worth while exhibiting in a more popular style, and without employing algebraical symbols, how constant pressures, operating against the ventilating pressure in rise, and in favour of it in dip ways, happen to cause the split with the greatest amount of rise, to obtain a lessened, and that having the greatest amount of dip, to obtain an increased, share or

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proportion of the gross quantity of air, as it becomes reduced in amount; and, on the other hand, why the very reverse of these alterations in the proportions come round, under similar conditions, when the gross quantity of air is *increased* instead of being *diminished* in amount, while the air-ways forming the splits remain in the same state - when the air of the returns happens to be lighter than that of the intakes, either from having a higher average temperature, or from any other cause; - from which it will be easy to perceive why the opposite results should ensue, in the event of the air of the returns being denser than that of the intakes. Let us suppose that the general ventilating pressure applicable to the airways of the splits, is represented by 100 when the quantities going to the rise and dip splits are equal to each other; and that 10 represents the constant pressure (arising from the air in the returns being less dense than that in the intakes), operating against the general ventilating pressure in the rise split; and that the same number represents the pressure, arising from the same cause, operating in aid of the general ventilating pressure in the dip one, at the same time; we should then have an effectual pressure of 100 - 10 = 90 in the rise split; and one of 100 + 10 = 110 in the dip split; indicating that the specific resistance of the air-ways in the rise split are less than that of those in the dip one, in the ratio of 90 in the former, to 110 in the latter; seeing that equal quantities of air are passing through them, when these pressures are required to overcome the resistances.

On decreasing the general ventilating pressure applicable to the splits, from 100 to 50, we should have an effectual pressure of 50 - 10 = 40, in the rise split; and an effectual pressure of 50+10 = 60, in the dip one; and on further reducing this general ventilating pressure applicable to the splits, from 50 to 10, we should have no effectual pressure at all in the rise split, because 10 - 10 = 0; while there would still be one of 10 + 10 = 20, in the dip one; or upwards of 18 per cent, of its amount, when the quantities were equal in the two splits; and, as the quantity of air decreases at a much slower rate of progression than the effectual pressure, we should continue to have upwards of 40 per cent of the original quantity of air in the dip split, when that in the rise one was brought to a stand-still, in this case : where I have assumed the original constant pressures in each, to be equal to 1/10th, or 10 per cent, of the general ventilating pressure, applicable, alike, to each of the splits, at the time when the quantities of air were assumed to be the same in each.

It is evident that if we further reduced the general ventilating pressure, say to 5, we should, in the dip split, have an effectual pressure of

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10 + 5 = 15, accompanied, however, by a n*egative* pressure of 5 - 10 = -5 in the long split; and this means a pressure of 5 in the opposite direction; which ought, therefore, to cause the air in the rise split intake to be reversed, and come outwards and join the fresh air (coming through the main intake), at what had previously been the splitting point, and accompany it in traversing the route or the dip split; a result that I have repeatedly witnessed during the course of making the experiments on the subject under our notice. I have also seen the constant pressure of dip splits exceed the reduced amount of the general ventilating pressure, applicable to the splits, and so force the return air out of the long dip split return, from the original point of reunion to the original splitting point,

where it joined the fresh air coming into, and accompanied it in its course in traversing the workings forming the long split (thus forming, in each case, an eddy), notwithstanding the long split being *very* long, and the short one, on the contrary, *very* short.

These results are, under the conditions set forth in the paper, such as were to be expected, only so long as the conditions as to the densities of the air in the original intakes and returns should continue to prevail, and are evidently merely temporary; inasmuch as the return air must, when the reversal takes place, gradually be forced back into the intake, and in the course of time alter the relative densities previously prevailing in the intake and the return respectively; and that, to such an extent, as ultimately to bring the eddy to a stand-still; after which the temperatures and densities in the intakes and returns would slowly approximate towards equality, and hence these local pressures would gradually become reduced in their amount, and allow the general ventilating pressure to re-establish a current in each of the splits, in the original direction taken by the currents of such splits.

Where the split is a long one, this re-establishment of currents, in the directions pursued by the original ones, is a long time in coming to pass; and in none of these experiments has it been allowed to come around, for want of the necessary time.

If we next suppose the general ventilating pressure applied to the splits (only) to become greater instead of smaller, than it was when the quantities in the two splits were regulated so as to be equal to each other, we obtain results of the opposite class to those already named - that split which, on reducing this pressure, obtained an increasing share of effectual pressure (in the former case) now obtaining a decreasing one. For instance, if the general ventilating pressure applied to the splits be

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increased successively above the point at which the quantities were equal, in the very same proportions that we have already conceived them to be reduced below that point, we find that, when the general pressure becomes 200, the effective pressure in the rise split becomes 200 - 10 = 190, and in the dip one 200 + 10 = 210; when it becomes 1,000, the effectual pressure in the rise split is 1,000 - 10 = 990, and in the dip one 1,000 + 10 = 1,010; and when it becomes 2,000, the effectual pressure in the rise split becomes 2,000 - 10 = 1,990, and in the dip one 2,010; the same split that, on *increasing* the general pressure, got a continually *increasing share* or proportion of effectual pressure, in comparison with what it had when the quantities of air were regulated so as to be equal in each split, now, on the other hand, obtains a *decreasing* proportion, and that which then got a decreasing, now gets an increasing one.

The results are stated in the following table:—

[Table of results]

On looking at the case where the quantities of air circulating, were equal to each other, in the above table; and then, first upwards, and afterwards downwards, from that point; it will be seen that the rise split gets a continually increasing, and the dip one a continually decreasing effectual pressure, in comparison with each other, as we proceed upwards; and that, on the other hand, the rise split obtains a continually diminishing, and the dip one an increasing, effectual pressure, as we

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glance downwards from the same point, making a similar comparison; the actual quantities of air, given by the experiments, at pages 66 and 67 of the present volume of "Transactions," follow the same laws in general; as was predicted in the paper on the proportions in which air will divide itself over different splits under different ventilating pressures : notwithstanding the fact that this very result has been urged as a proof of a want of correctness in the conclusions arrived at in the paper. See present volume of "Transactions," page 71.

If we presume the squares of the quantities of air circulating in each of the splits, in any unit of time, to have been directly proportional to the effectual pressures putting them into circulation, and at the same time, inversely proportional to the resistances offered by the respective splits to the circulation of a given quantity of air in any unit of time ; then, the quantities of air circulating in each of the splits, under the different pressures exhibited in the above table, would have been proportional to the numbers contained in the following table :—

[Table of results]

* This quantity of air would be coming outwards in the long split intake, and would unite with 1,237 of fresh air, from the main intake ; and, with it, traverse the route of the short run, so long as the conditions assumed continued to prevail.

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PROPOSED COLLEGE

OF

MINING, ENGINEERING, AND MANUFACTURING SCIENCE.

The President then stated, he had great pleasure in bringing before them some proceedings regarding the establishment of the proposed Mining and Engineering College. The members of the Institute would recollect that, in the first instance, it was proposed that the College should be erected in Newcastle-upon-Tyne, or some other suitable locality, and that it should be an entirely independent institution, unconnected with any existing establishment, and erected, endowed, and supported by subscriptions and donors; and they would likewise recollect the munificent proposition which his Grace the Duke of Northumberland had made for the endowment of such a College. The committee appointed for the purpose of promoting its establishment had appealed to the Coal Trade generally, to the noblemen and gentlemen individually connected with, and interested, both locally and generally, in such trade, and also to those noblemen and gentlemen who were otherwise connected with the two counties of Northumberland and Durham, and they had likewise appealed to the manufacturing and commercial interests of the district; but they regretted that they were obliged to arrive at the conclusion, that it was hopeless to expect to be able to raise the necessary funds to establish, endow, and support a College of an entirely independent character, and unconnected with any other institution.

The committee then turned their attention to the University of Durham, and made inquiries if it were practicable to engraft upon that University the proposed College, which, though connected with that institution, should at the same time be of a purely practical nature, and which should be under the management and control of a council elected by the body of subscribers, and by the Mining, Engineering, and Manu-

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facturing interests, by whom it was supported; and the members would recollect that at the meeting of the Mining Institute of August last, and recorded in the proceedings at page 200, vol. VI, of the "Transactions," some correspondence with the University on the subject was laid before them. Since that period meetings have been held with the authorities of the University; and he had now the pleasure of laying before the Institute a report of the College Committee, which embodied propositions for the establishment of the proposed Mining and Engineering College in connection with the University of Durham.

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REPORT OF COMMITTEE.

Mining Institute Rooms, Newcastle, May 20, 1859.

The Committee for the establishment of a Mining College have to report that, in pursuance with the course agreed to be adopted, they have very fully brought the subject before the Coal Trade of Durham and Northumberland, as well as that of the kingdom at large.

While several lessors and lessees of mines have very liberally offered their support to the proposed measure, it must be admitted that the amount of encouragement received by the committee does not offer, on the whole, a prospect of the successful erection and endowment of so desirable an institution in a separate form.

Under these circumstances the committee, anxious to carry out the object for which they were appointed, and acting under the authority and with the advice of the Mining Institute, to whom the various proceedings have from time to time been fully explained, have endeavoured to realise by means of an existing establishment those great objects which they doubt not will be fully appreciated by the Mining interest as soon as their practical and economical results in the improvement of Mining operations, and also in diminishing the waste of life, come to be understood and developed.

The Institution referred to is the University of Durham, the authorities of which have shewn great willingness to meet the views of the Committee.

In the several interviews which have been held with them, the following heads for the establishment of a Mining College within the precincts of the University have been provisionally arranged :—

“Durham, Feb. 15, 1859.

"Heads of a provisional arrangement for the establishment of a Mining and Engineering College, as agreed to by the Warden and Senate of the University of Durham.

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"1. A College of Mining and Engineering shall be established at Durham, in connection with the University of Durham.

"2. The College shall be managed by a council, consisting of a Principal, the Professors who give Lectures in this department, and a number of persons not exceeding three, nominated by the subscribers towards the funds of the Institution.

"3. The University of Durham shall provide two Professors, namely:—

"1. The Professor of Mathematics.

"2. A Professor of Natural Philosophy and Applied Mechanics.

"The Mining and Engineering College shall provide three Professors, viz. .—

"1. Of Mineralogy, Geology, and Working Mines.

"2. Of Chemistry.

"3. Of Plan Drawing, Levelling, Surveying, and Practical Engineering.

"The three last-named Professors shall be nominated by the Mining and Engineering College, and shall be approved by Convocation.

"All the Professors shall be Officers both of the University and of the Mining and Engineering College.

"4. The Principal shall be nominated by the Mining College, and approved by convocation, and may or may not be one of the Professors.

"5. The Principal shall be charged with the superintendence of the students, unless they are members of some College, Hall, or house in the University; and, together with the council of the College, shall arrange the various lectures to be delivered.

"6. Lecture Rooms shall be provided by the University for the five Professors. Chemical Laboratories and Workshops, if necessary, shall be provided by the Mining and Engineering College.

"7. Students in Mining and Civil Engineering shall be of two classes, matriculated and non-matriculated; matriculated students shall reside in some College, Hall, or House licensed for that purpose by the University.

"Non-matriculated students, if not resident with their parents, shall reside in lodging-houses licensed by the Principal of the

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Mining College, and approved by convocation; or, in special cases, with the consent of their parents, in such other houses as the Principal may approve.

"8. Matriculated students shall be admissible to the academical rank of Mining Engineer and Civil Engineer according to the regulations passed in January, 1855.

"Non-matriculated students shall be admissible to such title of distinction as shall be agreed upon by the University and the Mining and Engineering College.

"All students who attend only a part of the course of study shall receive certificates of competency in those subjects in which they have passed the requisite examinations.

"9. Instruction shall be provided for increasing the usefulness of Schoolmasters in the Mining Districts.

"10. Arrangements shall be made, if possible, for enabling the students to inspect Mines and obtain instruction in practical work.

"11. All non-matriculated students shall be subject to such discipline as shall be determined by the University and the Mining and Engineering College.

"12. Every matriculated student shall pay an admission fee of £2, and a terminal fee of £5. The admission fee shall be divided equally between the University and the Mining and Engineering College. The terminal fee shall be divided, two-fifths to the University and three-fifths to the Mining and Engineering College.

"The fees thus paid shall entitle any Mining or Engineering Student to attend, with the consent of the Lecturer, any lecture given in the University, and in like manner the fees paid by Students in Art or other faculties in the University shall entitle them to attend, with the consent of the Lecturer, any lectures given by Professors of the Mining and Engineering College.

"Non-matriculated students shall pay fees according to the lectures which they attend. Civil and Mining Engineers, Under-Viewers, Overlookers of Mines, and Manufacturers and other persons desirous of attending any course or courses of lectures, shall be admissible, with the consent of the respective

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Lecturers, on the payment of such fees as shall be from time to time determined by the council of the Mining and Engineering College.

"13. The fees received either by the University or by the Mining and Engineering College shall be applied partly to the payment of the Lecturer, and partly to general purposes connected with Civil Engineering and Mining."

It will be seen from the foregoing minute that the Mining and Engineering College, though intended to be locally situated within the University, is yet to be conducted upon independent principles, and to be managed by its own governing body.

With regard to the endowment of the proposed College, the authorities of the University of Durham, while they offer the gratuitous accommodation of Lecture Rooms, thus saving the large outlay that would be required for a separate building, have yet no power to divert their funds from the purposes to which they were originally appropriated. They have, however, agreed to pay the salaries of two of the Professors, on the ground of their services being useful to both institutions. Beyond this the Committee entertain a confident hope that the Mining interest will contribute towards the permanent endowment of the proposed College, and have also reason to believe that Government are favourable to the project.

The Committee therefore, approving of the foregoing provisional arrangement, deem it advisable to appoint a Sub-Committee, consisting of Messrs. N. Wood, T. J. Taylor, and I. L. Bell, and request those gentlemen to confer with the authorities of the University of Durham, and also to bring the project under the notice of Government, and to take such other preliminary steps as shall in their judgment be expedient or necessary for the successful establishment of the proposed College, reporting the result of their labours as early as they conveniently can to this general Committee.

NICHOLAS WOOD, Chairman.

The President—The members of the Institute would perceive from the foregoing outline of the proposed arrangements that the independence of the Mining and Engineering College has been strictly adhered to; that the principle on which it is proposed to be established is of a purely practical character; and, that its management is to be entirely in the

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control of its own supporters, subject only to those regulations which are necessary between a College and a University, in order to provide for the requisite discipline, and to render the whole as perfect a seminary for the instruction of Mining and Civil Engineers, and others intended for manufacturing pursuits, as possible.

The objects of the proposed College has been set forth in detail in the various proceedings of the committee, from time to time, and have been embodied in the Transactions of the Institute, and also in the shape of separate pamphlets which have been published; it would, therefore, be superfluous in me to trouble you with many observations on this occasion. If you think it advisable to adopt the recommendation of the committee, and to sanction the appointment of the sub-committee, those gentlemen will no doubt take immediate steps to confer with the authorities of the University, and with the Government, and other persons, who may give their aid in the establishment of so important an institution. Looking at the manner in which the authorities of the University of Durham have met the subject, there can be no apprehension that any difficulty will exist in the requisite details for completing the arrangement between the University and the promoters of the College: - the only difficulty which presents itself is, that of funds, - for the erection of such buildings as are not proposed to be provided by the University - for the annual payment of Professors - -and for the current expenses of the establishment ; - to that object the sub-committee will first of all have to devote their attention; and, taking into consideration the importance of such an institution, it is to be hoped they will be successful. It is to be trusted, likewise, that when something of a practical nature is laid before them, the lessors of the vast coal mining districts of Northumberland and Durham, and of their lessees (who are more immediately connected with the benefits likely to arise from such an institution), will contribute towards the requisite funds. The Government, it is trusted, will likewise see the propriety of appropriating some grant towards the establishment of an institution which will, unquestionably, more than any other, contribute towards efficiency in the management of mines, and towards the more important object of saving of life, for the promotion of which an annual expenditure of upwards of £12,000 is contributed in the shape of allowances to the Inspectors of Mines. Those beneficent feelings, also, which induced his Grace the Duke of Northumberland to make such a munificent proposition for the endowment of a College at Newcastle, may, it is trusted, if the Institution of Durham

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be established to the satisfaction of his Grace, be transferred to the promotion of the Institution in that locality; and we may likewise venture to hope that some of the funds, exhibitions, &c, of the University of Durham may, with the permission of Parliament, be appropriated towards the support of a College in that University.

There is only one other subject connected with the establishment of the proposed College which I think it necessary at this time to mention, and which though, in my opinion, of great importance towards the utility and efficacy of the institution, is only slightly noticed in the minutes of the proposed arrangements, and which is, the establishment of Schools in connection with the College in the different mining districts, and the training of Schoolmasters in the College for such schools. It has been suggested that Schools might be established in the different colliery districts of the kingdom, and that at such schools instruction adapted to the education of boys intended as Managers of Mines, or for Engineering or Manufacturing pursuits, should be given - that such schools should be in connection with the proposed College - that they should be, in fact, as it were, preparatory schools for pupils to be sent to the proposed College, and who should, on being sent to the College, have some privileges granted to them; and that schoolmasters educated and prepared at the College for such a course of education as would be suitable for the pupils to be sent to the College, would be peculiarly adapted as teachers for such schools. There would then be a system of preparatory schools in the several districts for such College, and those schools being presided over by masters trained at the College, the practical result would be, that a system of Mining and Engineering instruction would be taught at such schools as would be of infinite advantage to the rising generation in those localities, and to those who were intended to conduct the Mining operations at a future time. The great bulk of the pupils of such schools would probably acquire full and complete instruction, for the several gradations of duties with which they might thereafter be entrusted, and it is very probable that very few of the pupils would aspire to the additional knowledge to be acquired at the College. The system would, however, admit of any number being sent, and, as I have already named, would present inducements for their entering upon a College instruction. But the great utility of such a system would be, that it would embrace the education of all classes and grades of Mining and Engineering students, from the highest and most efficient instruction which an University would

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afford, to that of a local school peculiarly and exclusively devoted to the education (and at the cheapest rate) of the lowest grade of Colliery Manager, Civil Engineer, or Manufacturing pupil.

A discussion then took place on the several heads of arrangement proposed to be made with the University of Durham, and several practical suggestions were made; these, however, having subsequently been submitted to the authorities of the University, and agreed to by them, have been embodied in the preceding document.

The President then put a resolution that the sub-committee named by the committee be appointed, and that they should be desired to carry into effect the recommendation of the committee with as little delay as possible; which was carried unanimously.

The meeting then adjourned.

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NORTH OF ENGLAND INSTITUTE of MINING ENGINEERS.

ANNIVERSARY MEETING, THURSDAY, AUGUST 4, 1859, IN THE ROOMS OF THE INSTITUTE, WESTGATE STREET, NEWCASTLE-UPGN-TYNE.

Nicholas Wood, Esq., President of the Institute, in the Chair.

The proceedings of the Council having been read,

The following gentleman was elected a member of the Institute :— Mr. Robt. T. Rees, Lothy Shenkin Colliery, Aberdare, Glamorganshire.

The President then said they would take the resolutions of the Council, recommended to be considered at the general meeting first in rotation, and stated they would perceive that Mr. T. Y. Hall had made a claim of £189 19s. 6d. for diagrams and illustrations to his papers, and that the Council had referred the claim to the general meeting for consideration. After a protracted discussion on this claim, from which it appeared Mr. Hall had incurred this amount of expence in the illustrations to his paper, while, on the other hand, no orders or authority had been given by the Council for the payment, it was ultimately arranged that the matter should be referred back to the Council, with power for them to arrange with Mr. Hall as they might deem expedient, and in accordance with the rules of the Institute; and the following resolution, proposed by Mr. Potter, and seconded by Mr. Reid, was passed unanimously, with the concurrence of Mr. Hall:—

"That the claim of Mr. T. Y. Hall be again referred to the Council for arrangement, it being understood that Mr. Hall is willing to leave the settlement thereof to the Council."

The Council having reported that vol. I. was out of print, and that it

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was desirable that 200 additional copies be printed; and that, in their opinion, it was desirable that an increase in the number of copies printed annually of the proceedings of the Institute be made.

It was Resolved—"That 200 additional copies of vol. I. be printed; and that 600 copies of the "Transactions" be printed in future, instead of 530 copies as heretofore."

A discussion then arose as to a portion of the Vice-Presidents being ineligible for re-election, in accordance with a resolution passed at the last annual meeting of the Institute, when

It was Resolved—" That the resolution at the last annual meeting, of the ineligibility of some of the Vice-Presidents for re-election at this meeting, be postponed until the next annual meeting of the Institute."

Attention was directed by the President to a resolution at the last annual meeting of a catalogue of the books and specimens belonging to the Institute, which, it appears, had not been accomplished,

It was therefore resolved—" That the Council prepare a catalogue of the books and other property belonging to the Institute, in time to be printed with the next year's proceedings."

The resolution of the last annual meeting that a copy of the rules, together with any modifications therein made at the different meetings of the Institute, be made out by the Council, and printed with the proceedings was continued.

Mr. Reid then stated that the printing of the additional copies of Mr. Wales's paper had cost £71. 5s., and that copies had only been sold to the amount of £42 10s. 6d., leaving a balance of £28 14s. 6d. against the funds of the Institute.

The President said the only way in which the deficiency could be fairly met, except by the funds of the Institute, would be by the coal trade subscribing to that amount to the Institute. The additional copies of that paper were printed for express distribution amongst the members of the trade, and was, therefore, exclusively for its benefit; and he believed that no paper had issued from the Institute of more practical utility than that paper, especially to those practically engaged in the management of mines. As they were honoured with the presence of the Chairman of the Coal Trade of Northumberland and Durham, he hoped

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that gentleman would recommend the trade to contribute at least that amount to the funds of the Institute, which would be received as a proof of the support of the trade to the Institute.

The report of the Finance Committee and the Annual Report of the Council were read, adopted, and ordered to be printed with the annual proceedings.

The President then brought before the meeting a circular on a testimonial proposed to be presented to Mr. Hunt, of the Museum of Practical Geology, in Jermyn Street, for his valuable services in the preparation of the annual statistical returns of the production of mines, &c, when, after several remarks by different members of the Institute pointing out the value to the coal, lead, copper, tin, &c, trades of such statistics, the following resolution was passed:—

"That the sum of £10 10s. be subscribed towards the Hunt Testimonial, as a proof of the approbation of the Institute of his valuable and laborious services in preparing the annual mineral statistical returns, which are so useful to the mineral interests of the kingdom."

Mr. Daglish having presented a book of photographic prints of collieries, machinery, &c, to the Institute,

It was Resolved—That the thanks of the Institute be given to Mr. Daglish for his handsome present of photographic prints.

The question of the arrears of subscriptions was then brought before the meeting and led to a long discussion, whether it might be advisable to alter so much of the constitution of the Institute as to divide the subscribers into members and associates, similar to that adopted by the Civil Engineers, with a view of making the Institute and its publications more approachable to the class of underviewers and other sub-managers of mines and manufactories, and with that view to admit such persons at a reduced annual subscription, and, consequently, to enlarge its utility to the mining interests of the country. After considerable discussion the following resolution was passed—

" That it be referred to the Council to consider and report whether it may be desirable to have members and associate members of the Institute, and what difference in the amount of annual subscription they would in that case recommend."

And a resolution was likewise passed—

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"That the transmission of the proceedings be withheld from members more than two years in arrear of their annual subscriptions."

The President then communicated to the meeting the steps which had been taken since the last meeting of the Institute, in the promotion of the establishment of a Mining College. He alluded to the report of the Council with reference to the interview which the sub-committee had with the Secretary of the Home Department, and stated, although that gentleman did not, as they scarcely expected he would, give them any specific promise of support, he did what he believed was as much as was usually conceded in such cases, promise to give the subject his careful attention. Considering, however, that the Mine Inspection Bill terminated next session of Parliament - that it would be necessary to renew the bill - that the inspectors had reported that increased intelligence in the overlookers of mines generally would tend materially to diminish accident - and that a committee of Parliament had recommended the establishment of practical schools for that purpose; he did hope that Government might be disposed, when the Inspection Bill came before Parliament, to give them a grant of an annual sum towards the endowment of a central College, and of the establishment and support of local schools associated therewith in the different mining districts of the kingdom. The authorities of Durham University had met the subject in a most gratifying manner. They had offered to appropriate lecture rooms, which would save a considerable sum in the erection of buildings if the College was established elsewhere; they had promised to pay the salaries of two out of the five professors, which it was thought might suffice for opening the College; and they only regretted that they were not enabled by their constitution to devote any further appropriation of their funds for the support of the College. As, however, it was pretty evident that parliamentary powers would be requisite to obtain the necessary funds, and as the renewal of the Inspection Bill offered an opportunity of obtaining such powers without expence, it might be that arrangements might be made to provide the Durham University with additional funds which might be applied towards an extension of that Institution, and to comprise in its constitution the addition of a Mining College. And last, and not least, he (the President) did hope that they might look with some confidence to the mining interests of the kingdom for pecuniary support, considering the benefits which were likely to result from the establishment of such an

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Institution, not only in a commercial point of view, but likewise in the saving of life.

Mr. Hugh Taylor (Chairman of the Coal Trade), expressed satisfaction at the statement which had been made by the President. The trade was indebted to him and to the Council for the attention they had paid to the affairs of the Institute. With regard to the College they should follow up their application to the Secretary of State. The £3,000 a year was a very small amount out of £800,000 a year voted annually for educational purposes, and, in his opinion, no extension of education could be more beneficial to the state, than the education of the managers of mines and manufactories, as well in the saving of life, as in the working and obtaining the mineral products of the kingdom, which he had no doubt would accrue from its establishment. He supposed it would be so constituted that its benefits would apply to the entire kingdom.

The President—That is the intention.

Mr. Hugh Taylor—Then I think the Government should provide the whole of the funds. If it had been confined to this locality, then he believed the coal trade of this district would be disposed, and he trusted would find the means to establish such an institution. But he apprehended a central establishment and district schools, embracing the education of the managers of mines for the entire kingdom, would be infinitely more useful and effective; and, therefore, the support should come from the Government, and from the entire trade and manufacturing interests of the kingdom.

The President said he might add that already petitions had been presented to Parliament by the workmen, praying that a number of sub-inspectors of a lower class in the profession than the present inspectors should be appointed. It was extremely desirable that the workmen who incurred the risk of the explosions and accidents in the mines, should have confidence in the management of the collieries, which he was afraid those petitions showed they had not; and, he (the President,) thought every person engaged in the trade would be of opinion, that a system of management founded upon the unquestionable competency of the responsible operative managers of the mines, would be more likely to prevent accidents and explosions, and to ensure satisfaction to the owners and workmen, than a set of sub-inspectors overlooking less competent and uneducated operative managers, which latter system he thought would tend to produce inextricable confusion.

The appointment of the Officers of the Institute for the ensuing year having been made,

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The President stated that the routine business of the annual meeting was then concluded, and that the next business before the meeting was the termination, if practicable, of the discussion of the several papers on the Splitting of Air,—on the Question of Regulators, and,—on the Variation or otherwise of Currents of Air of different lengths when regulated, in cases of Variation in the Ventilating Power of the Mine. He, (the President), named the termination of the discussion, as the investigation and discussion of those questions had now been prolonged for a very long period, and had occupied a considerable portion of the meetings during this year, to the exclusion he believed of other papers of importance. He was duly impressed with the importance of these investigations on the practical ventilation of mines, but the most important subjects must have an end, and he would, therefore, propose that the discussion, if practicable, should be brought to a close at this meeting of the Institute. He said that a paper on the subject was expected from Mr. Thomas John Taylor on one section of the enquiry, but he understood, from that gentleman, that important Parliamentary business had prevented him from having the paper ready for the meeting to-day. Mr. Taylor, however, thought he could have it ready in a few days, and that it might possibly be printed with the proceedings of this meeting, so that all the papers and discussions on the subject might appear in this year's "Transactions." If this arrangement met the approbation of the meeting, they would then go on with the discussion, and Mr. Taylor's paper would, if possible, be printed with the proceedings.

Mr. Atkinson—I am quite agreeable to any course that may be adopted; I merely suggest that I have made some remarks in reference to Mr. Taylor's preliminary paper, read at the last meeting, which has, I believe, precedence to my reply to Mr. Greenwell’s remarks on my papers.

It was then arranged that Mr. Atkinson's notes on Mr. Taylor's remarks should be read, which were as follows.

REMARKS on MR. T. J. TAYLOR'S PRELIMINARY PAPER,

Entitled

"THE CAUSES OF THE VARIATIONS OF THE DENSITY OF AIR CIRCULATING IN COAL MINES."

By Mr. JOHN J. ATKINSON.

I beg to offer a few remarks in reference to a communication, made by Mr. T. J. Taylor to the last meeting, relative to the questions appointed for final discussion this day.

Mr. Taylor states, in his communication, that the effects of local differences of temperature upon the ventilation of mines have never been denied, and least of all by himself.

Their effects were surely virtually denied or ignored by those who held that the mere relative lengths of the routes of the different splits of air in a mine was the only matter requiring consideration in determining which of them would obtain an increased, and which of them a reduced proportion, or share, of any altered gross quantity of air circulating in the mine in a given time. And, although Mr. Taylor was not of those who thus failed to recognize these effects of local differences of temperature in modifying the proportions in which different gross quantities of air would be distributed amongst the various splits of a mine, yet that gentleman, at pages 74, 75, and

76 of the present volume of "Transactions," states that, in his opinion, under the ordinary conditions of mines, these proportions would be affected to a greater extent by those particular changes of density in the air of rise and dip splits that are caused by the gradual expenditure of motive column on overcoming the resistances successively encountered by the air as it progresses in circulating, than

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they would be affected by those other changes of density that arise from the variations of temperature generally experienced by the air as it circulates.

Indeed, it was simply owing to Mr. Taylor having expressed this opinion that I produced the paper on the relative importance of these two particular causes in the production of changes of density in the air of mines as it progresses in circulating.

Mr. Taylor admits, in the remarks I am here reviewing, that I established the position I had assumed in reference to this matter.

I have, however, not only shown that the changes of density arising from the gradual expenditure of motive column are, under the ordinary conditions of mines, of small amount in comparison with the alterations of density arising from change of temperature; but also, that however much those changes of density that arise from this particular cause may modify and affect the actual quantities or proportions of air circulating in the several splits of a mine, still, since they vary in the self-same proportion as the motive column giving rise to them, they cannot, under ordinary circumstances, be supposed to have any sensible effect towards disturbing or altering these proportions when an altered motive column puts a new gross quantity of air into circulation in the same splits.

This peculiarity in the amount of pressure arising from the gradual expenditure of motive column, and operating in favour of, and in addition to, it in dip workings, and in opposition to it in rise workings, prevents its being included amongst the causes tending to vary the proportions in which different gross quantities of air will be distributed over a series of splits in the same mine; while, at the same time, it is necessarily one of the causes operating to render dip workings more easy of ventilation than rise ones ; showing that the two questions are distinct, and ought not to be treated as being identical, however similar to each other they may appear; a mistake which Mr. Taylor seems to have fallen into in the remarks under notice.

Mr. Taylor next proceeds to quote some experiments having reference to the relative temperatures of the air in the rise and dip workings of the Back worth Colliery, from which we learn that the average temperature of the return air of the rise workings exceeded that of its corresponding intake by 5°; and that the average temperature of the return air of the dip workings exceeded that of its corresponding intake by 4°. After this follows some remarks relative to the tendency that these higher temperatures in the returns than in their corresponding intakes have to render dip workings easier of ventilation than rise ones.

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In all this there is nothing opposed to the views set forth in my own papers.

Mr. Taylor, in the remarks I am noticing, next proceeds to state that that part of the solution of the question, as to why dip workings are more easy to ventilate than rise ones, which is to be found in the higher average temperatures of the air of the returns, than those of their corresponding intakes, does not go far enough; and that we are to ask whether all returns are warmer than their corresponding intakes; and why, in particular, this should be so in rise working.

Now, however closely allied may be the question as to why dip workings are more easily ventilated than rise ones, to the other question, as to which of a series of splits of air will obtain an increased, and which a decreased proportion of any gross quantity of air circulating, on altering its amount from any assumed standard, it is evident that they are, nevertheless, two separate questions; and at least one cause (the local pressure arising from the expenditure of motive column) has been pointed out as operating to its full extent in influencing the former question, although exercising little if any influence upon the latter.

I have not assumed differences of temperature to be the only cause operating to produce changes of density, and so to influence the question, as to the particular proportions in which any altered gross quantity of air will distribute itself over any series of splits in a mine; nor even to be the only cause operating to render dip workings more easy of ventilation than rise ones. Neither have I assumed that all returns are warmer than their corresponding intakes, even in dip, and much less in rise workings, and hence I do not exactly perceive the relevancy of this portion of these remarks to the matter at issue.

I have concluded that in the event of the density of the return air being less than that of its intake then that by altering the gross quantity of air, certain results would ensue in rise ways, and the very opposite ones in dip ways; and that the results just alluded to would be exactly reversed if the density of the air of the intakes was less than that of the returns; altogether independent of the particular causes which might operate in producing the relative densities of the returns and intakes.

If Mr. Taylor favours this meeting with a paper and investigations on the nature and comparative importance of the various causes contributing to alter the density of the air in mines, perhaps no one will feel more interest in it than myself; but, judging from the nature of the remarks of that gentleman, read to the last meeting, I feel it to be due to myself to

state that, whatever may be the results of such an investigation, they cannot controvert anything I assumed in the chief paper, commencing at page 163, vol. VI., of "Transactions," "On the proportions in which air in mines distributes itself over several splits or routes having different lengths, and offering different resistances to currents of air passing through them," inasmuch as I did not, in that paper, enter into any investigation of this part of the subject ; and it can only be one of my more recent papers or communications that can be affected or controverted by an investigation of this nature,— I mean either the paper commencing at page 115 of the present volume of "Transactions," or that read to the last meeting.

Mr. Taylor remarks, that "variations in the volume and density of air from temperature, pressure, and vapour, are all to be brought in as elements for explaining the real point at issue, which is not, it is conceived, one of temperature *versus* motive column, but how to give a right explanation of all the circumstances. *Temperature does not account for every case; it will not carry us through either as regards dip or rise workings*; nor, so far as my experiments go, will hygrometric difference do so, but the notable feature of the increase and diminution of ventilating column must necessarily find admission, as one assignable cause at least, into every example."

Now, at the time when these remarks were communicated to the Institute, I had not entered into any general investigation of the causes tending to render the air of the returns more or less dense than that of the intakes, and I am at a loss to know how the matters named could be assumed as elements requiring to be brought in to explain any point then at issue ; for, whatever might be their effects in altering or affecting the density of the air circulating, the real point at issue would remain as before, viz., as to whether the mere relative lengths of the splits, on the one hand, or whether the relative densities of the air of intakes and returns of rise and dip ways, on the other hand, were the real causes determining which particular splits would receive an increased, and which a decreased share or proportion of any altered gross quantity of air put into circulation, in the same unaltered mine or part of a mine.

I am not aware that I have anywhere assumed that the question was one of temperature *versus* motive column, except when writing a special paper on that very question, owing to Mr. Taylor himself having, in a previous part of the discussion of the first of the series of papers, expressed an opinion in reference to the relative importance of these two

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particular causes in producing changes of density in the air of mines, at variance with the opinion entertained by myself on that particular point; neither have I pretended or assumed that temperature alone would account for every case, or I should not have alluded to the effects of a mixture of gas with the air in producing changes of density, as I have done in the first of the series of papers, at page 164, vol. VI. of the "Transactions."

I have, on two former occasions, (see note, page 122, and also pages 134, 135, and 136 of the present volume of " Transactions") shown that the expenditure of ventilating pressure is trivial in its effects on the density of air in mines, and, therefore, also in its effects in modifying the proportions of air going into the different splits of a mine, as compared with the amount of the effects of the same nature produced by the differences of temperature usually prevailing as between intake and return air.

I have in these places further proved that these effects, arising from changes of density, produced by the expenditure of ventilating pressure, while of very small amount in fixing the relative proportions of any gross quantity of air going into the various splits of a mine, since they are always proportional to the motive column giving rise to them, would have no effect whatever in modifying the proportions in which any altered gross quantity of air would distribute itself over the same unaltered splits of a mine, if we suppose that no other disturbing causes existed; as, in that case, whatever might be the gross quantity of air circulating, the proportions in which it would be distributed would continue to be the same; and need not, therefore, necessarily find admission as an assignable cause into every example of the real question, in reference to which these papers have been written, as might be inferred from Mr. Taylor's remarks at page 131 of the present volume of "Transactions."

Looking back to the paper, commencing at page 163, of volume VI, of "Transactions," on the relative proportions in which different gross quantities of air distribute themselves over the various splits of a mine, out of which the more recent papers intended for discussion at present have all arisen, I conceive that whatever may be proved as to the nature, variety, and extent of the various causes operating to produce changes of density in the air of mines as it progresses in circulating, they cannot be urged as controverting anything set forth in that paper, inasmuch as the conclusions arrived at in it are come to on the mere assumption that the densities of the air of the intakes are, on the one hand, greater, or,

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on the other hand, less than those of their respective returns; quite independent of the number, nature, or magnitude of the separate causes that may contribute towards the production of such differences of density.

Hence I conceive that it is by a mere extension of a particular branch of the general subject (generally recognized in the paper itself, but intentionally left uninvestigated), that these particular matters, brought forward by Mr. Taylor, can find admission into the discussion of the original point.

The President—We shall now go on with the discussion generally, and as Mr. Atkinson has answered some remarks of Mr. Greenwell in a paper read at the last meeting, and, as Mr. Greenwell is now present, I think we should first of all hear that gentleman's answer to Mr. Atkinson's remarks.

Mr. Greenwell—The question to be considered is the difference that would take place in the relative quantity of air that would go in long and short runs, by the difference made in the ventilating power. I had a model made, representing the upcast and downcast shafts, three feet high and four inches square. The communication between the bottom of each of these shafts was direct. It also represented a pair of drifts four feet long, from the bottom of the downcast, returning to the bottom of the upcast. In the short run there was a door placed, and in the face of the drift I placed a small lighted taper, which was observable by a little glass frame put opposite. I had a spirit lamp at the bottom of the upcast, and when the door was shut the whole of the current of air went round the face; but when the door was opened I still found that though there was a diminution in the quantity of air the diminution was very little, compared with what might be expected, as there was such a short run from the bottom of the one shaft to the bottom of the other; but when I partially closed the top of the upcast I found the effect much greater on the candle placed in the face, and that a certain contraction of the upcast produced a stagnation of the air in the long split. As I gradually closed the upcast the flame of the candle became perfectly vertical, the air all passing along the short course. I conclude this would take place in practice.

Mr. Hall—What is the object of contracting the upcast shaft, instead of contracting the intake?

Mr. Greenwell—To reduce the gross current of air.

Mr. Atkinson—With regard to the question asked by Mr. Hall, there

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is nothing, that I am aware of, in air-tight places, against contracting the upcast any more than the downcast ; or in favour of or against using the furnace power, as compared with any other power; but if there was any leakage in the boxes in Mr. Greenwell's experiments, then the difference is important, as it would materially affect the results. If perfectly tight it is a legitimate experiment; I presume the boxes were horizontal?

Mr. Greenwell—Yes.

Mr. Atkinson—If on reducing the gross quantity of air you got a greater proportion of diminution in the long split than you did in the short one, it is contrary to all my personal observations. It is contrary to all the practical experiments made in mines that have come to my knowledge; as well as contrary to what I believe to be the true theory of the case; and, of course, I am at a loss to account for it. Was the result really obtained?

Mr. Greenwell—This is an experiment easily tried by any one.

The President—You did not ascertain the quantity of air at the upcast, as compared with the quantity at the downcast, to prove that the boxes were tight.

Mr. Greenwell—The air must have gone from the bottom of one shaft to the other - there was nothing in the face, the candle was upright.

The President—You diminished by the contraction the quantity of air; if the theory is correct, it is correct with respect to any quantity of air, circumstances being alike; if there is no alteration of the door, if the area of the two passages, the long passage and the short one, are the same, the same relative quantity of air should go through both places, whatever may be the aggregate quantity; that is the theory of Mr. Atkinson.

Mr. Atkinson—In level workings that is the theory.

The President—To upset that theory Mr. Greenwell's boxes must be perfectly tight, and the area constant during the experiments; there must be no doubt on these requisites.

Mr. Greenwell further explained the form and working of his model, which was composed of boxes, which he believed, were perfectly air-tight.

Mr. Atkinson—Is it not possible there was leakage? I have seen experiments tried by boxes. With boxes Mr. Daglish got results very similar to those of Mr. Greenwell. The short split in his experiments got a considerably increased proportion of air on reducing its quantity by contracting the intake. I went down the pit one day with

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Mr. Daglish after he had tried his experiments. He said he believed the boxes were air-tight. I said, I don't think they are so; but we will try it by putting the regulator at the opposite end. Accordingly we reduced the area of general airway at the exit end of the boxes, and we could scarcely distinguish that there was any falling off in the long split, though there was a great falling off in the short one. There was scarcely a twentieth part indicated by the anemometer as a reduction from the original quantity passing in the long split, while according to the anemometer there was only about one half of the air in the short run. What was the meaning of it? Simply the leakage of the boxes. The leakage in Mr. Daglish's trials had operated in favour of the short run, and against the long run. In the trials made on my visit, I believe, there was only a twentieth diminution in the long run quantity, when in the short one there was a reduction of about one half of the quantity, the difference being due to the leakage; by contracting the outlet end of the boxes we increase the pressure in the boxes; and the excess of the leakage of the long split boxes over that of the short split boxes, arising from the increased pressure, had to pass over the anemometer in the long split. The boxes were placed in the return—the air coming direct to them from the downcast—and this would make the pressure in the boxes much greater than the external pressure upon them, in the return. By contracting the intake end of the boxes the internal pressure would be reduced and this would reduce the escape of air from the boxes, and thus unduly lessen the air going into that split which had the most joints and leakage—in this case, the long split.

Mr. Daglish—It takes a sensible current to turn a candle.

Mr. Dickinson—Though the remarks Mr. Atkinson has made are important as showing the attention that should be paid to leakage, these experiments of Mr. Greenwell's were only made with a model candle, which he had burning in the far end upright; still there might be a sensible current there, and he did not ascertain the quantity of air going through the short course; therefore, the diminution of the whole quantity might have affected both splits in proportion, and this is the real point at issue.

President, addressing himself to Mr. Greenwell—You do not generally get boxes very tight,—everything depends on their being tight,—you are still of opinion that the theory is not correct?

Mr. Greenwell—I am; whether it arises from some particular point not taken into consideration, I believe the theory, as it now stands, will not be found borne out by fact.

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President—The boxes represent shafts of uniform size. Was the drift between the shafts of the same area?

Mr. Greenwell—Yes.

President—The fact of the drifts or currents being made to pass along routes inclined to the horizon, and not perfectly level, may influence the theoretical results; and, though in this experiment the tubes representing the shafts are perfectly vertical, and the boxes representing the drifts are perfectly level, and consequently nothing to influence the theoretical result, still you think, in this case, little or no air traversed the long run when the gross quantity was reduced?

Mr. Greenwell—Until the contraction becomes so much greater as to give the upcast shaft power enough to supply itself from the far off place.

President—The friction of the air at the same velocity in the short run, is less than the friction of the air passing round the long run, supposing the areas of both to be equal ?

Mr. Greenwell—Yes.

President—Then it would depend on the contraction or regulator placed in the short route making the velocity, and consequently the friction greater in the short route, to compensate for the increase of friction in the long route; and, if the one balances the other, then in all cases will they not be equally balanced. You think in a diminished aggregate quantity of air you could get a considerable velocity through the short run (the two being connected), while the air in the long run remains perfectly stagnant, though with a greater aggregate quantity of air, there would be a definite and regulated quantity of air passing along each route?

Mr. Greenwell—Yes. A considerable velocity in the short one, and the long one stagnant. Take the case of Haswell Colliery in the shaft workings, the doors were thrown open, and the overman sent into the workings to observe the effect on the air currents in the dip and rise currents. After waiting a long time he came back and said, have you not opened the doors yet? He had found scarcely any difference, such was the power of the upcast shaft that it could not obtain a sufficient supply of air through the doors, but it still continued to draw air round the workings. I will rest the whole question on that experiment.

Mr Dickinson—-You took no means to ascertain the quantity going through the short cut?

Mr. Greenwell—It was evident.

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The President—The resistance of the air in the passages is as the square of the velocity and the length. Any increase or diminution of the aggregate quantity of air should not effect such a result. In all cases the relative quantities of air should pass along the routes, whether long or short?

Mr. Greenwell—If the theory is really correct. No doubt if, in treating air, you were treating a body such as water, which is almost incompressible, the theories of the motion of fluids and of air would be similar; but I am not sure whether the packing of the air itself, in forcing it round long columns, has not considerable effect.

Mr. Dunn—Suppose the door was of equal capacity with the pipe, what would be the effect?

Mr. Greenwell—It is the case. The communications and the door are all the same size, except the thickness of the door itself.

The President—We have, on the one hand, the theory, and a great many experiments on a large scale, in direct contradiction to Mr. Greenwell's experiment. I think gentlemen will be very chary in arriving at a conclusion which would upset the generally received law of the motion of air and those experiments, from the result of this one experiment with boxes, which may or may not have been perfect. If there was any leakage, it would materially affect the result, and no dependence could be placed on the experiment.

Mr. Greenwell—It is easily tried.

Mr. Daglish—I have tried boxes, but could never make them tight.

Mr. Atkinson—Mr. Greenwell supposes that the packing or elasticity of the air has some effect, although he has himself objected to my introducing theoretical considerations. The theory I have employed is based on numerous and varied experiments; while this hypothesis of Mr. Greenwell's is based on an idea of his own mind. I believe, when we come to refer to the experiments made in mines, we shall find that these experiments completely bear out the paper under discussion. In such of the experiments, cited at page 145 of the present volume of " Transactions," as were made in situations where the routes of the splits were rise or dip ones, the gases given off, the expenditure of motive column, and, in all probability, the hygrometrical state of the air also, each contributed to the effects of differences of temperature, in rendering the density of the returns less than that of the intakes; and the whole of the experiments give results in perfect agreement with the theory, on the presumption that such a difference of density really

existed. The very opposite results, in these cases, ought, according to the theory, to have been obtained, had the density of the returns, on the other hand, been greater than that of the intakes; a condition that might have been created by the admixture of a sufficiently large proportion of carbonic acid gas, in lieu of fire-damp, with the returns.

The President then said—I think I must now endeavour to give a summary of the various conclusions which appear to result from the papers and discussions on the subject before us. The question, first of all, originated in a difference of opinion amongst some members of the Institute, as to the proportions in which the air in mines would distribute itself in routes or splits of different lengths, in cases of a variation in the ventilating pressure, or in the aggregate quantity of air in the mine. Mr. Wales having stated that practically, when the aggregate quantity of air circulating in a pit was diminished, the regulators not being altered, there was a greater diminution of quantity in the long, than in the short splits; while Mr. Atkinson, on the other hand, contended that, all things remaining the same, viz., the regulators and air-ways of all the splits remaining unaltered, " except in so far as the gravitation of air of different temperatures and densities in ascending and descending parts of the air-ways forming the different splits or routes might affect the result; air would divide itself over any series of splits in the same proportions, whatever might be the general ventilating pressure employed, or the gross quantity of air circulating in a unit of time."

In order to test his opinion by experiment, Mr. Wales made a great many experiments at the different collieries, and Mr. Atkinson did the same, at some of which both gentlemen were present. It is not necessary for me to give more than the general result of these experiments, for the purpose of arriving at the conclusions which they manifest, as the details are given in the "Transactions," as well as the comments made upon them from time to time in the different discussions. It is of course, extremely desirable, in the practical operation of ventilating mines, to know how far theoretical deductions are borne out by the practical results, and, therefore, the enquiry is one of great importance in coalmine engineering; and the Institute is deeply indebted to the late Mr. Wales, and to Mr. Atkinson for the exertions and trouble they have taken in the elucidation of this important subject.

Adhering, therefore, to my intention of deducing from these experiments and theoretical disquisitions, the practical operative results, so that we may know in practice, how far an increase or diminution of the

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ventilating pressure, or of the aggregate quantity of air circulating in any mine, operates to increase or diminish the respective quantities of air in long and short routes or splits, the regulators by which such quantities are governed remaining unaltered; or, whether it is necessary in practice, in cases of any sudden diminution in the quantity of air circulating in a mine, to attend to or alter the regulators so that the same relative quantities of air may circulate through all the various routes or splits, whether long or short. I shall proceed, therefore, first of all, to give the result of the experiments adduced by Mr. Atkinson, to prove the accuracy of his theory when tested in practice.

The first of these experiments were made by Mr. Berkley at Crook Bank Colliery, and are given in detail in vol. VI. of the " Transactions" of the Institute, pages 183-6. The result of these experiments, in splits of the respective lengths of 72, 134, and 129 chains, the workings being nearly level, as stated by Mr. Atkinson in page 186, is as follows:—"In the first experiment the difference between observation and calculation amounts to 1/9th per cent, in one case, and reaches to between 5 and 6 per cent, in another case; but, in the second experiment, the least error is less than ¾ per cent., and the greatest is less than 2 per cent." And at the meeting of July, 1858, Mr. Atkinson presented two sets of experiments, made at the Grange Colliery, vol. VI,, page 229, the difference between the theoretical and practical results being about 2½ per cent., which Mr. Atkinson attributed to the "difficulty of measuring the precise quantities of air in each split, and not to any error in the theory." The respective lengths of each split being 1490, 2142, and 3260 yards.

These results led to an extensive set of experiments being made at the Hetton Colliery by the late Mr. Wales and my son, at some of which Mr. Atkinson was present. I have collected those into the form of a tabular statement, showing the practical effect upon long and short routes, where the disparity in the lengths is very great, and I have also added the theoretical results that would have been expected if the splits had been level, or what ought to have been the respective quantities of air in each split if the air had divided itself over the two splits in the respective proportions of the increased or diminished aggregate quantities of air circulating in the mine. I have also added two experiments made, at the suggestion of Mr. Atkinson, at the Springwell and Haswell Collieries. The following tables will show the results. Table I. is experiments made when the ventilating pressure was reduced during the experiments, and Table II. where the ventilating pressure was increased

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during the experiments; the regulators in each case being so adjusted that the currents in the long and short splits were nearly equal, and were undisturbed during the experiments.

[Table I]

[Table II]

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It is necessary to explain that the workings, or routes along which the air passes in these experiments, were not level, the seam of coal lying generally at an angle of 1 in 15 to 1 in 12. That the experiments 1, 2, 5, and 6 in the tables were made in routes the inclination of which were ascending from the downcast pit, and descending towards the upcast pit; consequently, the ingoing air in the long routes had to pass along an ascent or rise to the extremity of the workings, and had then to descend towards the upcast, while the short routes were nearly level. And the experiments 3, 4, 7, 8, 9, and 10, were made, where in the long routes the ingoing air had to descend, and the return air towards the upcast, had to ascend, the short routes being level.

It is likewise necessary to explain that the regulators were adjusted so that the quantities of air passing along the long and short routes or splits should be nearly equal, as in columns (1) (1). In column (2), Table I., the ventilating pressure was *reduced*, and, consequently, the aggregate quantity of air passing through the mine was diminished, when the quantity of air distributed itself in the long and short routes, as shown in column (2), Table I. The quantities shown in column (3), Table I., being those which ought to have passed along the long and short routes respectively, if such routes had been level, and if the disturbing causes of gravitation had not existed in consequence of the inclined position of such routes. In Table II. the same process was followed, the relative quantities of air in the long and short routes were regulated as nearly equal as possible, as in column (1); the ventilating pressure and the aggregate quantity of air was *increased* when the quantities shown in column (2) passed along the long and short routes respectively; and, as in Table I., column (3) shows the quantities which ought to have passed if those routes had been level. Comparing, therefore, the practical results shown in columns (2) (2), Tables I. and II., with the theoretical results as shown in columns (3) (3) of those tables, the difference will show the effect in practice as contrasted with what, but for the rise or dip of the splits, would be the theoretical result.

The result of such a comparison will show that, in all the experiments when the regulators were so arranged that they produced a maximum ventilating pressure, or a *full aggregate quantity* of air in the mine, and when that pressure was *reduced*, and, consequently, the aggregate quantity of air in the mine was diminished, the *long splits* got a comparatively *lesser* quantity of air in the ascending routes, viz., 1 and 5, while in the descending routes, viz., 3, 9, and 10, the long splits had a comparatively *greater* quantity of air, the *short* splits in the former case having a

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greater, and in the latter case a *lesser* quantity of air, as shown in Table 1. On the contrary, when the regulators were adjusted with a minimum ventilating pressure, or a *diminished aggregate quantity* of air, and when the quantity was *increased* the *long splits* in the ascending routes 2 and 6 got an *increased* quantity of air, the *short splits* having a *less* quantity, while in the descending routes 4 the *long splits* obtained a *less* and the *short split* a *greater* quantity of air.

This is strictly in accordance with the theory propounded by Mr. Atkinson, and explained with great clearness and perspicuity in his papers, and in the discussions. The air circulating in the long routes receives, as it proceeds, the natural heat of the mine throughout the whole of its passage, and, also, any gases discharged in its course, the difference of temperature between the ingoing and return air being several degrees, depending upon the length of the routes and other circumstances affecting the temperature of the mine; and if inflammable gas is evolved the levity of such gas, likewise, tends to increase the rarity of the return current, consequently, the column of air, from the downcast shaft to the extremity of the workings, is necessarily of a much lower temperature than the column from the extremity of the workings to the upcast shaft; and the tension or density of air being in proportion to its temperature, it follows, that that half of the column of air, from the downcast to the extremity of the workings, is of greater tension or density than the half from the extremity to the upcast, independently of the effect of the light gas. Hence, in ascending routes, the heavy column has to ascend and the lighter column has to be forced downwards; while, in descending routes the heavy column descends and the lighter column ascends to the upcast shaft, and, consequently, we have the results shown in the experiments.

The experiments 7 and 8 are, however, an exception, and it was the experiments in this pit which led the late Mr. Wales to doubt the accuracy of the theory. Mr. Atkinson has, however, thoroughly investigated this case, and subsequent experiments show that there were causes operating in this case which satisfactorily accounted for the apparent anomaly between the practical result and the theory.

It having been clearly, and to my mind satisfactorily, established, that dependence may be placed on the theoretical deductions expounded by Mr. Atkinson, as a guide to us in the practical operation of the ventilation of coal mines, in cases of the splitting of air by regulators,

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making proper allowances for the disturbing causes which have been so thoroughly investigated in the papers and discussions of the Institute, it may not, then, be uninteresting, or without practical utility, to see how far those disturbing causes operate in practice as obstacles in carrying out the system of using and of depending upon regulators, undisturbed, in the splitting of air in large and extensive mines.

The experiments have been made in extreme cases to test the accuracy of the theory, where the disparity between the lengths of the long and short routes were extreme, much beyond what occurs in practice. If, therefore, it is ascertained that the regulators may remain undisturbed in such cases as these, with regard to the comparative lengths in the routes, and, if we also adopt the almost equal disparity of the ventilating pressure, or diminution in the aggregate quantity of air in the mine, then we may, I presume, rely upon the system in the less extreme, disparity in the lengths of the routes, and in the more equal ventilating pressure, occurring in practice.

Referring to Table I., the diminution in the quantity of air in the long route, less than an equal distribution, is only as 215 : 228.5, or less than 6 per cent., the difference in the lengths of the two routes being as 3500 : 110. The diminution in the aggregate quantity of air being as 800 : 457. In experiment 3 the reduction in the quantity of air in the short route is only as 485 : 523, or a little more than 7 per cent. The disparity of the length of the routes being as 2850 : 60, and the diminution in the aggregate quantity of air in the mine being as 1827 : 1030. In experiment 5 the reduction in the long route is as 160 : 193, equal to 17 per cent. The disparity of lengths of the routes being as 2538 : 60, and the diminution of air in the mine being as 1154 : 390. In experiment 9 the reduction in the short route is as 522 : 661 and 262 : 461 respectively; the disparity of lengths being as 3520 : 218, and the diminution of the air in the mine respectively, as 2700 : 1322 and 2237 : 937. And in experiment 10 the reduction in the short route is as 257 : 295, or about 13 per cent. The disparity of lengths being as 7000 : 430, and the diminution of the air being as 827 : 591. I have omitted experiment 7 for the reasons previously stated. With the exception of the second experiment of 9, the variation from the regular proportionate diminution of air in the split, compared with the aggregate diminution, is from 6 to 20 per cent., the reduction of the aggregate quantity being from 50 to 60 per cent. In the second trial of the expe-

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riment 9, the regulator reducing the ventilating pressure was placed in the return, which, if there was leakage in the stoppings, might affect the result.

Let us now see whether in practice any cases ever occur to reduce the aggregate quantity of air in a mine 50 to 60 per cent., and if so, under what circumstances, or what are the causes which produce such a state of things; and if, in such cases, a diminution of from 6 to 20 per cent. of the quantity of air in any particular split would, under such circumstances, be attended with danger during the existence of such a reduction in the aggregate quantity of air usually ventilating any mine.

The prominent natural causes which operate in producing a diminution or increase in the ventilating power, is the variation in the density, temperature, and hygrometrical state of the atmosphere, the most important being the density indicated by the rise and fall of the barometer, hence it is a practice in all well regulated collieries to have a barometer in each pit, and to make daily records of the state of the atmospheric pressure. Beyond this all is artificial as regards the state of the ventilation of mines, the state of the air-ways, of the ventilating apparatus, whether mechanical or by rarefaction, all depends upon the care and attention of the workmen, and on the system of ventilating power employed. Admitting that all is in order, so far as the ventilating power and the state of the air-ways are concerned, we have, then, only the several variations in the state of the atmosphere, previously alluded to, to contend with. These operate, practically, two-fold.—1st, in a diminished pressure of the atmosphere, indicated by a fall in the barometer; and 2ndly, in the increased discharge of deleterious gases, the effect of such diminished pressure. Against the last, which cannot be controlled, we have only to endeavour to dilute it, and render it harmless, by an increase of ventilation, which is incompatible with the natural effect of the first state of things. It is, therefore, of importance to see what effect the variation of the atmospherical pressure has upon the practical ventilation of collieries generally, and particularly on the several divisions thereof, the subject of this enquiry.

Perhaps this would be best shown by giving a daily account of the aggregate quantity of air circulating in a colliery for a year, including, of course, all the variation of atmospheric pressure and temperature for that period. This, however, appeared unnecessary to the entire extent, and I have, therefore, abstracted a fortnightly account, comprising the extremes of both these influences.

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Register of Barometer and Thermometer, and Quantities of Air in the Minor and Blossom Pits at Hetton Colliery, for 1858.

[Table]

It will be seen, from the foregoing table, that the aggregate quantity of air circulating in the Hetton Colliery, in the year 1858, and produced by furnace ventilation, has not been effected at all by the variations in the atmospheric pressure, and this I believe to be the result generally with furnace ventilation; and for this reason, we know that more or less air, or ventilating power, is produced by the state or intensity of the furnace, an apparatus or anemometer, indicating the velocity of the current of air, is placed near the furnace, and the instructions given to the furnace-man is, to keep the apparatus showing the velocity of the air always at the same rate of speed. If, therefore, any cause occurs which would otherwise diminish the quantity of air circulating in the mine, the furnace-man adds more fuel to the furnace and so keeps up the same velocity of the current, as indicated by the apparatus showing the velocity. Practically considered, therefore, the variations of atmos-

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pheric pressure have no effect in producing such a general deficiency of air as will, when the regulators are once arranged, cause almost any irregularity in the currents of the different routes. The amount of variation at the Hetton Colliery, as exhibited by the foregoing table, below the average quantity of air during the year, is about 8 per cent.; this, on an examination of the tables hereinbefore given, will be found not to produce such an amount of variation in the currents of the different routes as to be productive of any practical inconvenience, or of any practical injurious result.

In the ordinary course of working we may, therefore, conclude that when the regulators are once arranged; there is no occasion, when everything is going right, to alter them. Accidents may happen to diminish the aggregate quantity of air. No rules can be laid down in such cases. It will, however, be of utility to know the extent of variation in the relative quantities of air which may be apprehended in specific cases of diminution in the aggregate quantity of air circulating in the mine, and these experiments will, it is trusted, furnish such information.

There are, however, cases that occur in practice regularly, in certain collieries, which it may be useful to investigate, viz., the extinguishing or damping of the furnace, in order that an examination may be made and the necessary repairs performed in the upcast shafts, which cannot be done while the furnace is in full operation. In these cases a very considerable diminution is produced in the aggregate current. It is true the workmen are withdrawn from the mine during the operation, and every care resorted to, in order to prevent any injurious effects or accidents occurring. Still, I presume, it may be useful to ascertain what effect is produced in some of those cases upon the general current of the mine, and, consequently, on the currents of the different routes.

I have, therefore, selected a case where it is necessary that the upcast shaft should be thoroughly examined once a fortnight, and for that purpose the furnace is practically extinguished for about twelve hours, during which time no workman is allowed to remain in the pit, except the men working in the shaft, who, when necessary, use safety-lamps exclusively.

The following is the register of the water gauge of the Seaton Colliery for two fortnights, showing the ventilating pressure previously to the damping of the furnace,—during the time it is practically extinguished, —and the pressure when the furnace is again in operation. This table is also useful in showing the hourly variation of the water gauge, pro-

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duced by the variation in the intensity of the furnace, previously to and during the time when the furnace is slackened, in order that the workmen may be enabled to examine the shaft at particular times when the pit is not at work. I have also added two tables showing the temperature and barometrical pressure at the top and bottom of the pit, the depth being 260 fathoms.

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Meteorological Observations at Seaton Colliery. *Register of Water Gauge during No. 2 Pay, and ending January 31st., 1858*.

[Table]

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Meteorological Observations at Seaton Colliery. *Register of Water Gauge during No. 12 Pay and ending June 12th., 1858]*

[Table]

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Meteorological observations at Seaton Colliery. *Register of Barometer and Thermometer during No. 15 Pay, and ending July 24th, 1858.*

[Table]

Meteorological observations at Seaton Colliery. *Register of Barometer and Thermometer during No*

26 Pay, and ending December 25th, 1858.

[Table]

The following table will show the comparative quantities of air passing

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through the different parts of the Seaton Pit, in cubic feet per minute when the furnace was damped, and when in full operation, from experiments made by Mr. Daglish.

[Table]

It will be seen, from the above experiments, that when the furnace was extinguished, the water-gauge or ventilating pressure was reduced from 1.7 in., 1.6 in., and 1.5 in. respectively, to 0.5 in.; and that generally, the diminution of quantity of air was about one-half, or 50 per cent., or as nearly as possible, as the square roots of the water-gauge or ventilating pressure.

Taking the main intake current of air in the several cases, the theoretical and practical results are as follows:—

[Table of Results]

These experiments show a remarkable coincidence between the theoretical deductions, and the practical results of the velocity of air in mines, with reference to the ventilating power employed; the velocities being almost strictly proportioned to the square roots of the water gauge, or ventilating pressures respectively.

It may be useful to compare the velocities of the air in the respective currents of long and short routes, as exhibited in the workings of a colliery, and not as experimental results only. Taking the case of Seaton Col-

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liery, as shown in the foregoing tables, the following table will show the several velocities of the currents in the different sets of workings, when the ventilating pressure was reduced as compared with the theory—the subject of this investigation.

[Table]

The result of these observations confirm the conclusions previously arrived at, viz., that, except in so far as the difference of density in the air and different parts of the airways, arising from the dip or rise of the strata, or other circumstances producing a difference of density, air will divide itself in the same proportions over any number of splits or routes, whatever may be the gross quantity of air circulating in the unit of time.

Whilst the proceedings were in course of preparation for the press, Mr. Greenwell made some further experiments with the boxes described in these discussions, the result of which showed that, as anticipated, the boxes had not been air-tight. Much to his credit he communicated the fact to the President, in the following letter:—

Radstock, September 6*th*, 1859.

Dear Sir,—I have carefully repeated the experiment alluded to in the last discussion of Mr. Atkinson's paper. I have also varied the experiments by means of regulators. The boxes were of the same size as those described. I find that the result formerly arrived at must have been occasioned by the boxes not having been tight, as suggested by you, for I now find that my experiments instead of being at variance with Mr. Atkinson's views corroborate them.

Trusting that this discussion and its consequences may have contributed to the more firm establishment of a useful principle.

I am, dear Sir,

Yours very truly,

G. C. GREENWELL.

Nicholas Wood, Esq.

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EXPERIMENTS ON THE STRENGTH OF WIRE ROPES AND CHAINS.

By Mr. J. DAGLISH.

The following experiments were made for the purpose of obtaining some authentic data on the strength of Wire Ropes and Chains of various descriptions and under different states of working condition.

The wire rope used was flat rope, or portions of it, weighing about 321bs. per fathom, made up of six round ropes stitched together, containing four strands of six wires each, of about No. 11 wire gauge. Considerable difficulty was at first encountered in properly securing the ends, all the ropes yielding at this part on strains considerably below what they should have borne, viz.:—

Series, No. I

[Table]

Although this series of experiments entirely failed to test the ultimate

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strength of the ropes, they are given as showing two points worthy of observation: - 1st., the loss of strength by heating the wire; and 2nd., the weakness of the ordinary socket joints compared with the other part of the rope. To test the actual loss owing to the heating of the wire, the following series of experiments were made: -

Series, No. II

[Table]

It would seem, therefore, that the loss by heating to redness amounts, in the case of this particular wire, to about one-half, but no difference was observed by different degrees of heating, nor by cooling immediately in water; at the same time the toughness of the wire is greatly increased by heating.

The cause of the weakness of the socket is very apparent when the manner in which the rope breaks is observed; in no case in the above experiments did the rope yield in the socket in the least, until fractured, and this always occurred at the first or second row of rivets; where, in fact, the rivets became a point of resistance, at which a cross in place of a direct strain was encountered. In ordinary practice, however, the socket joint, where it is not passed over any sheave or pulley, and is properly fitted on, seldom fails, and may be considered practically to be of equal strength with the rope; because although at first only one-half the strength, it is still greatly in excess for the actual strain, and it remains thus, whilst the rope by wear and oxidization rapidly becomes reduced in strength.

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In the testing of the flat ropes, Nos. 5 to 8, the fracture was always accompanied by a flash of light.

The next series was made for the purpose of finding the loss of strength in splicing (each series, A B C, being made with similar quality and size of wire); in the ordinary long splice the wires do not overlap except for an inch or two, as this would increase the bulk of the rope; there are, therefore, points near to where the ends of the ropes meet, which are of necessity slightly weaker than the remaining parts, without the splice itself yielding at all. The apparent difference between the strength of strands Nos. 1, 8 and 12, arises from a slight difference in the size of the wires and in the different testing apparatus used.

Series, No. III.

[Table]

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But as a hydraulic testing machine is not exactly adapted for such accuracy in small weights, the following were broken by suspending actual weights to them.

Series, No. IV.

[Table]

In this latter series, 15 to 21, the weight was sustained entirely by the friction of the wires, which made three turns in the length of strand experimented on, and the results varied considerably, according as the

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strand was greased or rough; in the last trial it was rather rusted, and one wire, already cut in one place, broke again rather than pull through; this increased the weight carried greatly.

It will be seen from the above, that to a direct strain the actual loss of strength by splicing is not great; but the amount depends, to a certain extent, on the length of the splice and goodness of the workmanship. In none of the above experiments was the splice ever noticed to fail in the least until the fracture occurred, which was always sudden, and without change or warning. It will be noticed that the average strength of single wires (experiments 1, 2, and 3, of Series II.) was 8 cwts., and a strand of 6 of same wires (experiment 1, Series IV.) was 48 cwts., that is, the aggregate strength of 6 wires equal to the strength of the strand of 6 wires. This differs considerably from previous experiments on hemp strands, where the loss by combining and twisting is stated to be as much as 30 per cent., but it may arise from the different nature of the material.

STEEL WIRE ROPES.

The use of steel wire for ropes has been recently introduced, and is attracting considerable attention; it possesses the great advantage of being considerably lighter than iron wire with equal strength.

Series No. V.

[Table]

CHAINS.

The following experiments are selected as fair examples from a large number made from various makes of iron in use in this district, some from north country iron and some from iron from the south. A wide difference is noticeable between the extremes of 24 tons and 8 tons. The proof strain for chains of this size is about 8 tons, so that No. 1

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bore more than three burthens, whilst No. 12 hardly stood the proof strain. The first pulled into a rigid bar before the fracture, whilst the latter flew like glass immediately on increasing the strain beyond a certain point. At some experiments made a short time since by some members of the Institute of Mechanical Engineers, a piece of ¾ inch chain made from iron, specially manufactured for the purpose, bore a strain of 28 tons.

Series No. VI.

[Table]

No. 9 was made from a ¾ inch bar of carefully forged iron, but although of excellent quality, this make of iron does not seem to be so well adapted for resistance to a direct strain as rolled bar iron - in which the fibre is elongated.

No. 11 was made from old wire rope welded into ¾ inch bar; though supposed to be charcoal iron, its power of resistance was not great.

No. 14, made from a bar of ⅞ths inch homogenous iron, failed in all trials at the weld - when made into chain, it seems to require workmen experienced in its use.

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It will be observed, that in some cases chains gave way at strains lower than what they had previously borne; frequently from the attachments yielding, or from the machine breaking, they had to be tested more than once, and several times they broke before the previous strain had been arrived at, thus - No. 5 broke at 16 tons, after having borne 18 tons; and in the next Series (VII.) No. 2 broke at 20 tons after having borne 22.

The heating of pit chains occasionally for increased facility in examination has been advocated by several gentlemen, whilst others have been of opinion that this would reduce the strength of the iron; and in the case of wire, the before-mentioned experiments on this point would seem to support this latter view; but in the case of chains it is not so, and heating does not seem to have any weakening effect, as the following experiments will show:—

Series No. VII.

[Table]

These were made with new chains, but to test the effect of heat further on working chains the following were made: -

Series No. VIII.

[Table]

These were portions of the same chain, the old had been at work about 12 months.

In making many of these experiments I have been greatly indebted to the kind assistance of Mr. Spencer, Messrs. Bagnall, and other gentlemen.

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